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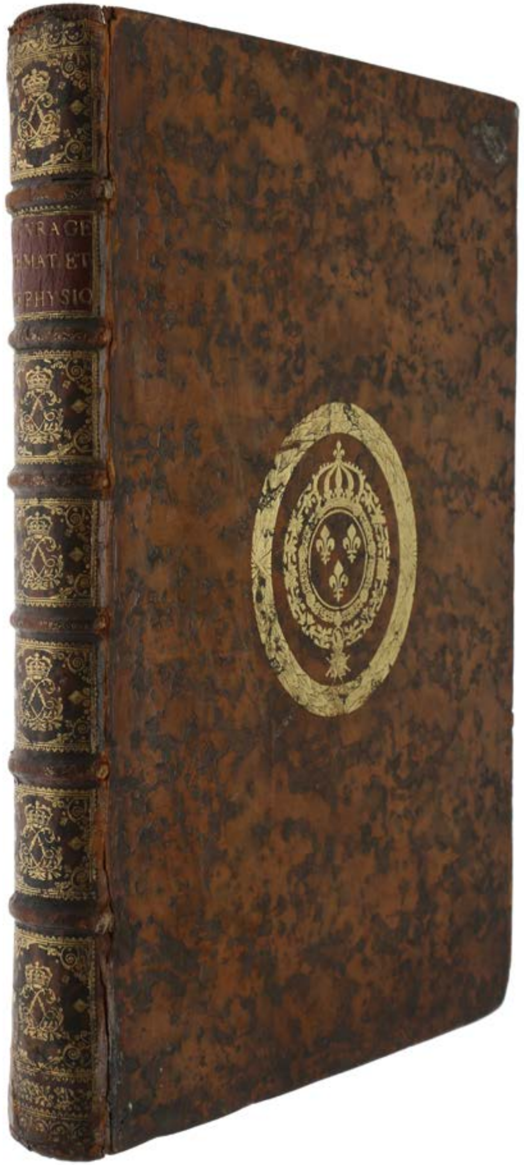
ENTIRELY UNRESTORED IN PRESENTATION BINDING

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. 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(as was Huygens), and without the assistance of the Académie it is likely that many of their works would have remained unpublished (only two works by Frenicle and two by Roberval were published in their lifetimes).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 …

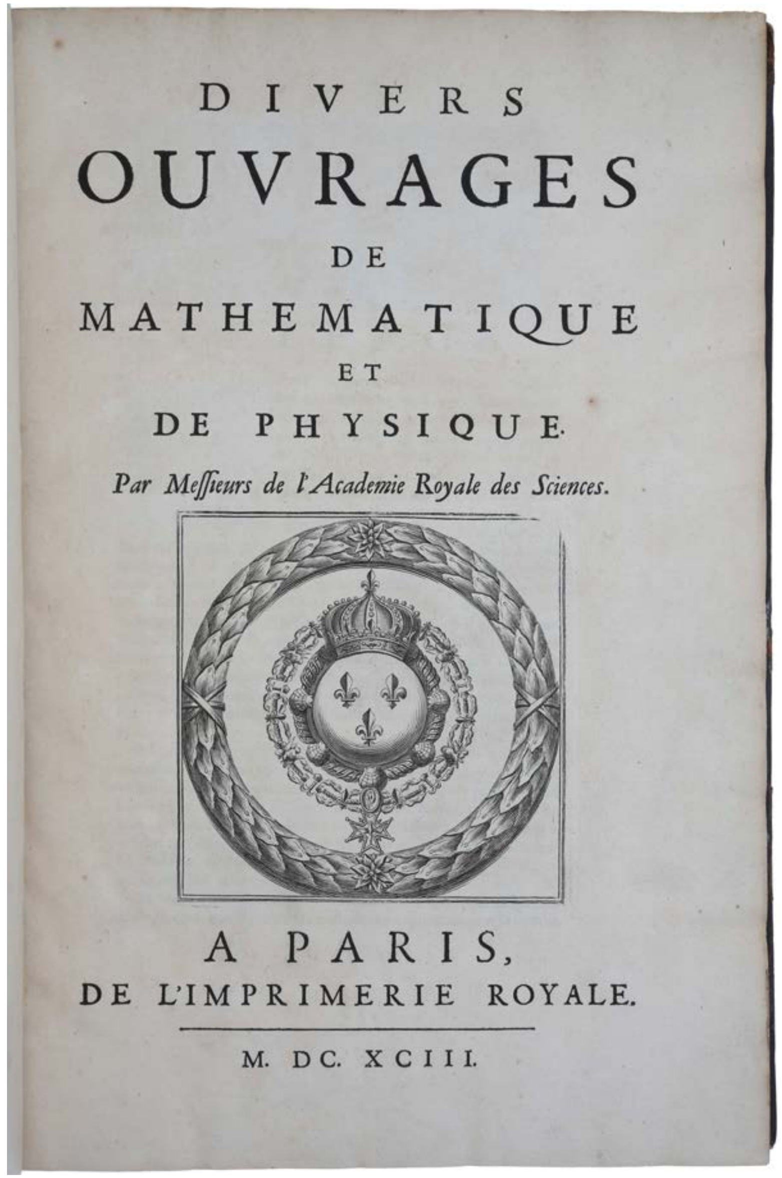
"On account of his method of the "composition of Movements" Roberval may be called the founder of kinematic geometry. This procedure had three applications—the fundamental and most famous being the construction of tangents. "By means of the specific properties of the curved line," he stated, "examine 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.

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).

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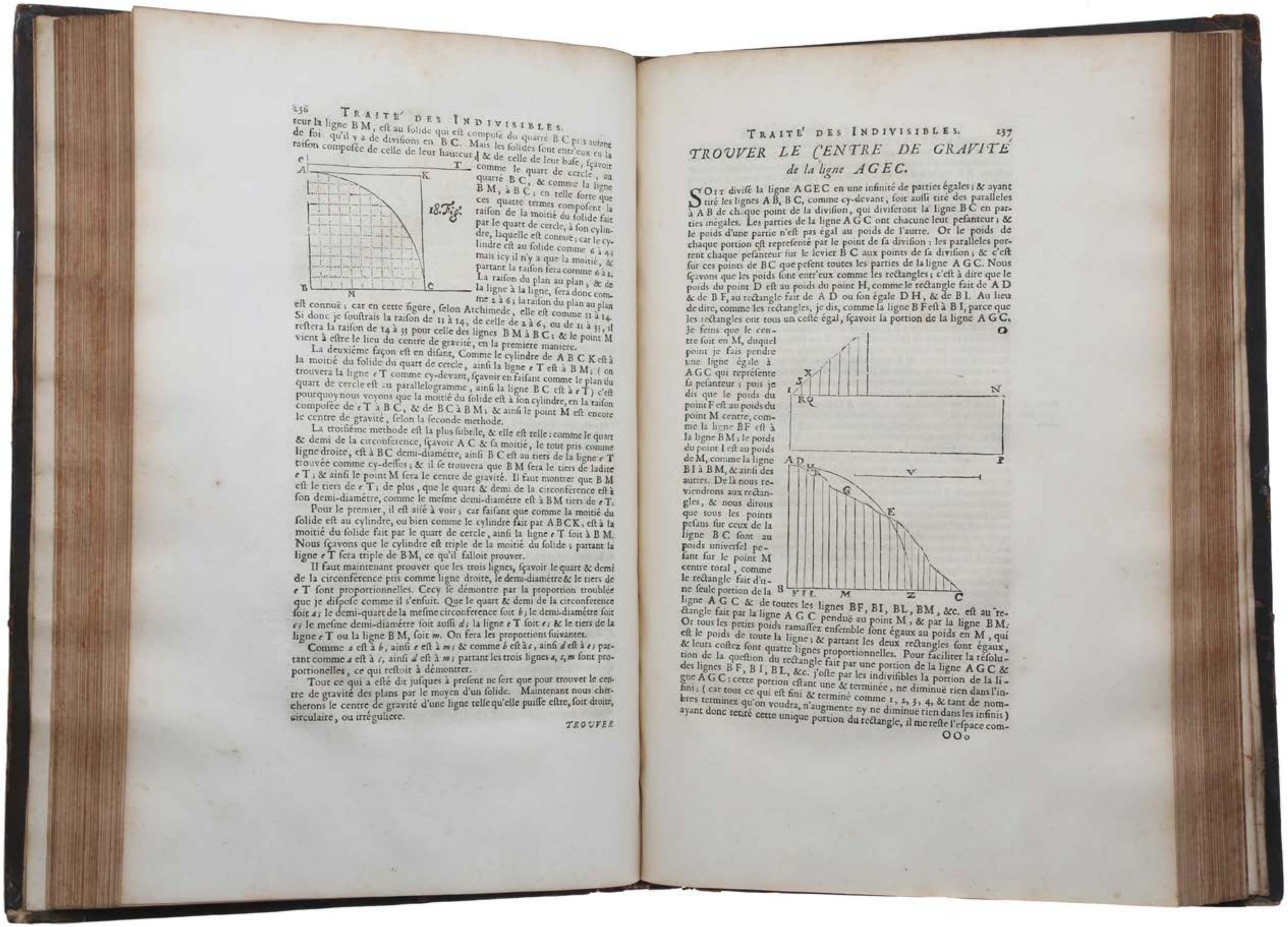


teur la ligne B M, est au solide qui est composé du quarré B C pris autant de fois qu'il y a de divisions en B C. Mais les solides sont entr'eux en la raison composée de celle de leur hauteur, & de celle de leur base, sçavoir comme le quart de cercle, au quarré B C, & comme la ligne B M, à B C; en telle 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 B C 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 B M 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 B M, 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

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 BM; le poids du point I est au poids de M, comme la ligne BI à BM, & 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 BM: 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

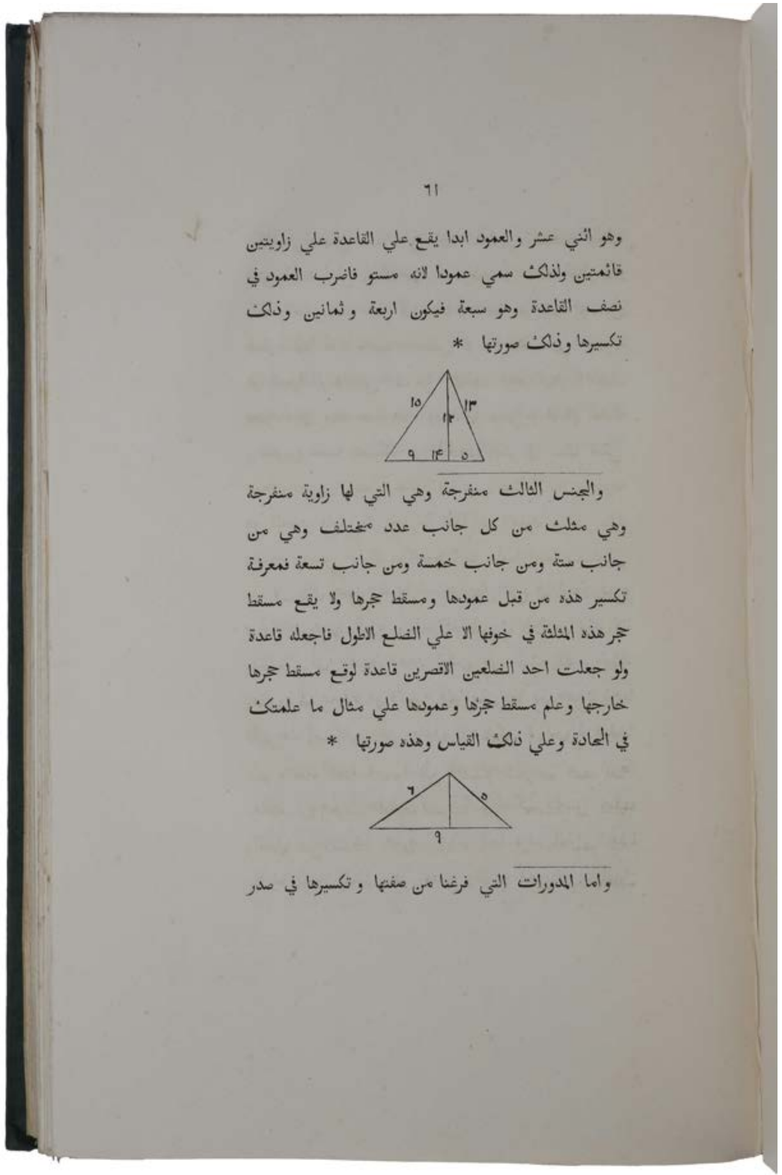
FIRST PRINTING OF AL-KHWĀRIZMĪ'S ALGEBRA IN ANY LANGUAGE

[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. "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


٦١

وهو اثني عشر والعمود ابدا يقع علي القاعدة علي زاويتين
قائمتين ولذلك سمي عمودا لانه مستو فاضرب العمود في
نصف القاعدة وهو سبعة فيكون اربعة و ثمانين وذلك
تكسيرها وذلك صورتها *

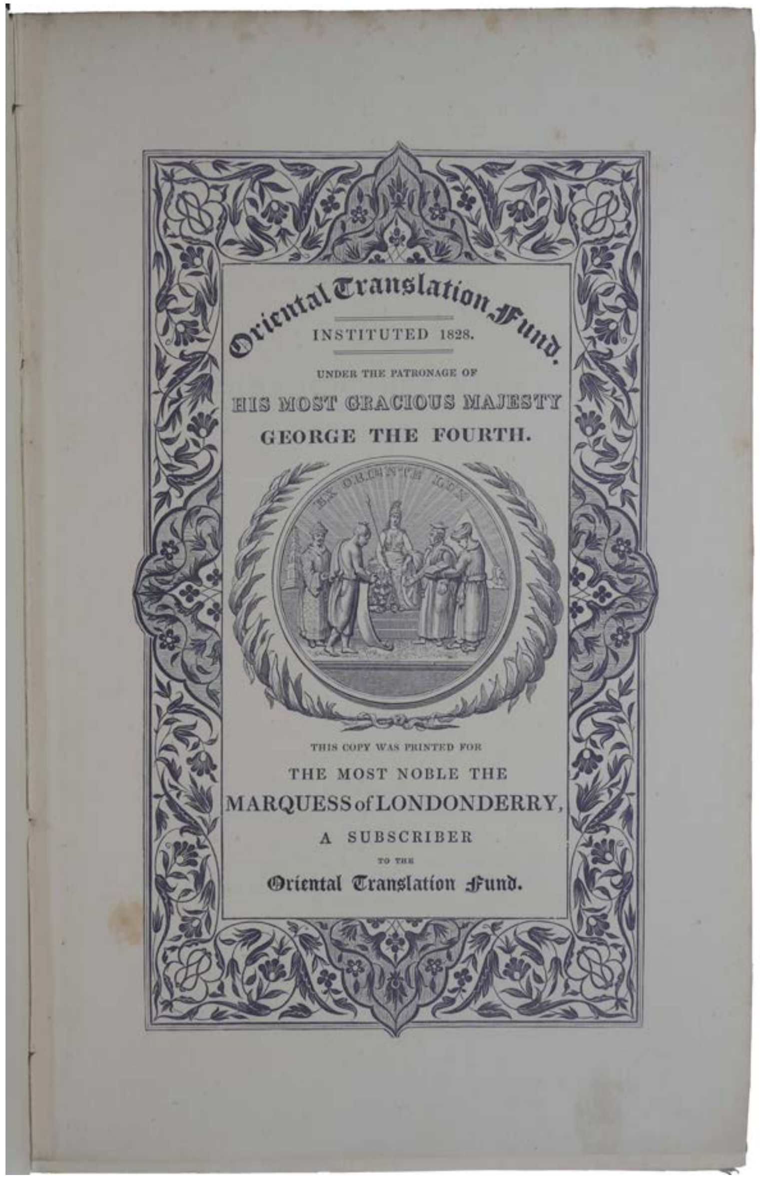
والجنس الثالث منفرجة وهي التي لها زاوية منفرجة
وهي مثلث من كل جانب عدد مختلف وهي من
جانب ستة ومن جانب خمسة ومن جانب تسعة فمعرفة
تكسير هذه من قبل عمودها ومسقط حجرها ولا يقع مسقط
حجر هذه المثلثة في خوفها الا علي الضلع الاطول فاجعله قاعدة
ولو جعلت احد الضلعين الاقصرين قاعدة لوقع مسقط حجرها
خارجها وعلم مسقط حجرها وعمودها علي مثال ما علمتك
في الحادة وعلي ذلك القياس وهذه صورتها *

واما المدورات التي فرغنا من صفتها و تكسيرها في صدر

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


Oriental Translation Fund.
INSTITUTED 1828.
UNDER THE PATRONAGE OF
HIS MOST GRACIOUS MAJESTY
GEORGE THE FOURTH.
THIS COPY WAS PRINTED FOR
THE MOST NOBLE THE
MARQUESS of LONDONDERRY,
A SUBSCRIBER
TO THE
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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) …

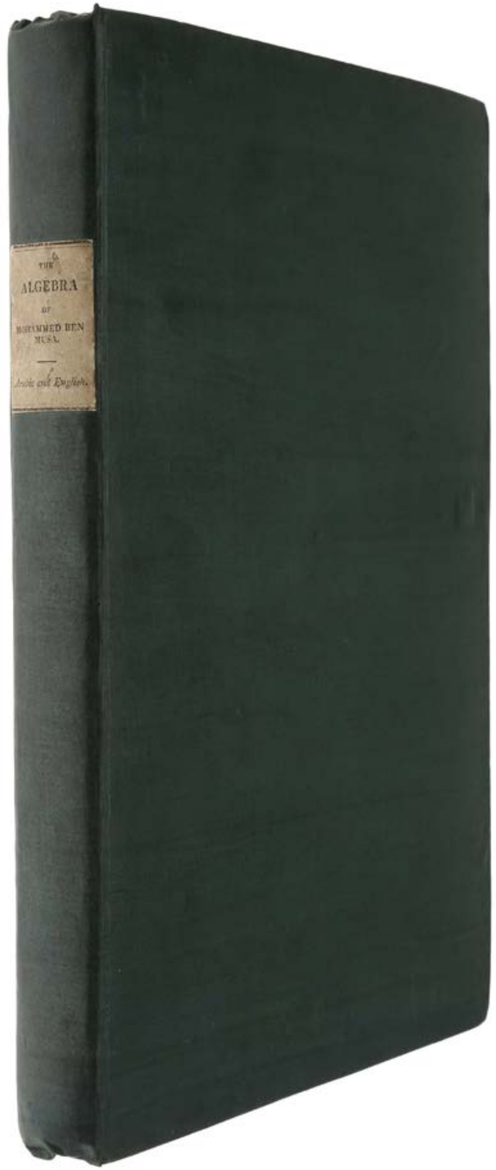
"After illustrating the rules he has expounded for solving problems by a number of worked examples, al-Khwārizmī, in a short section headed 'On Business Transactions,' expounds the 'rule of three,' or how to determine the fourth member in a proportion sum where two quantities and one price, or two price 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-Majūsī,' 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).

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.

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



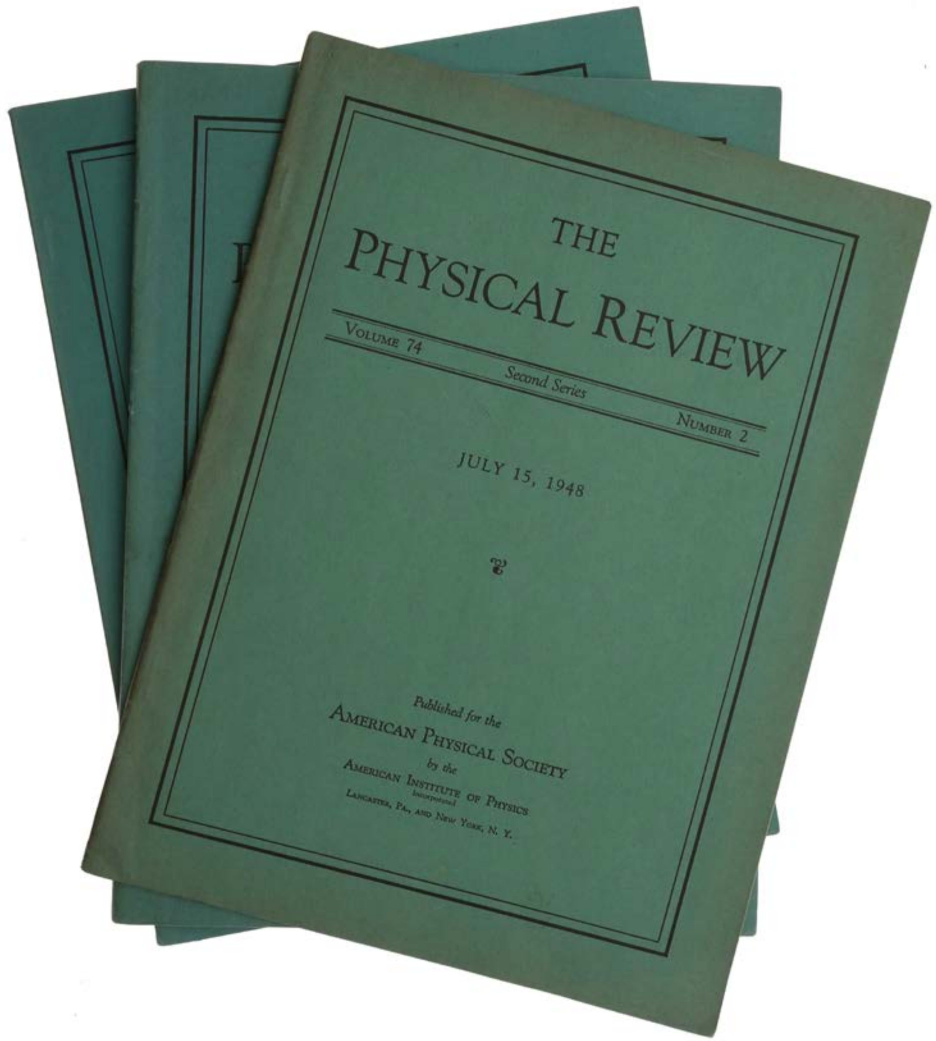
INVENTION OF THE TRANSISTOR

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



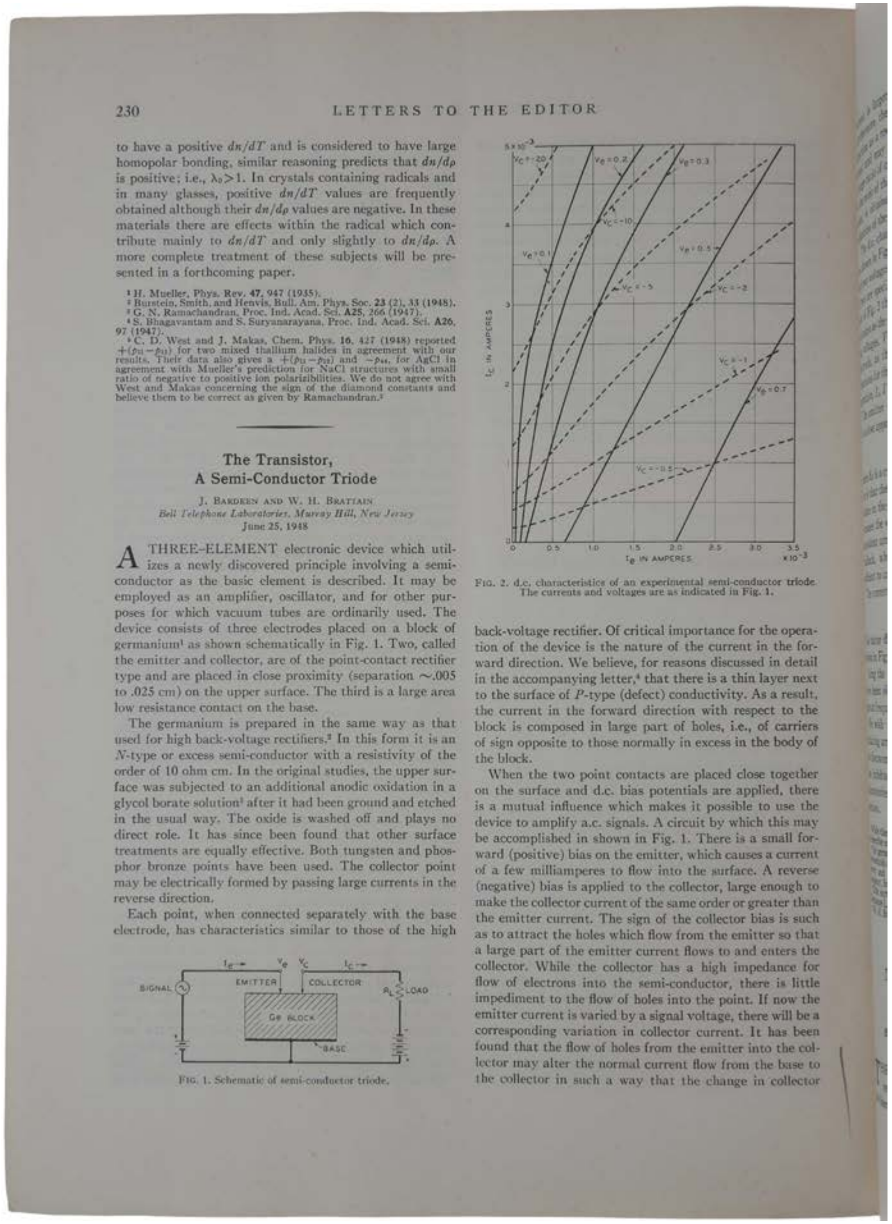
with the Nobel Prize in Physics in 1956 "for their researches on semiconductors and their discovery of the transistor effect".

"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.



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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

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.

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

"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.

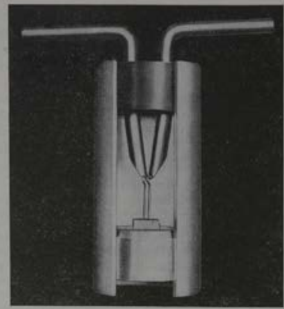


FIG. 2. Microphotograph of a cutaway model of a transistor.

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

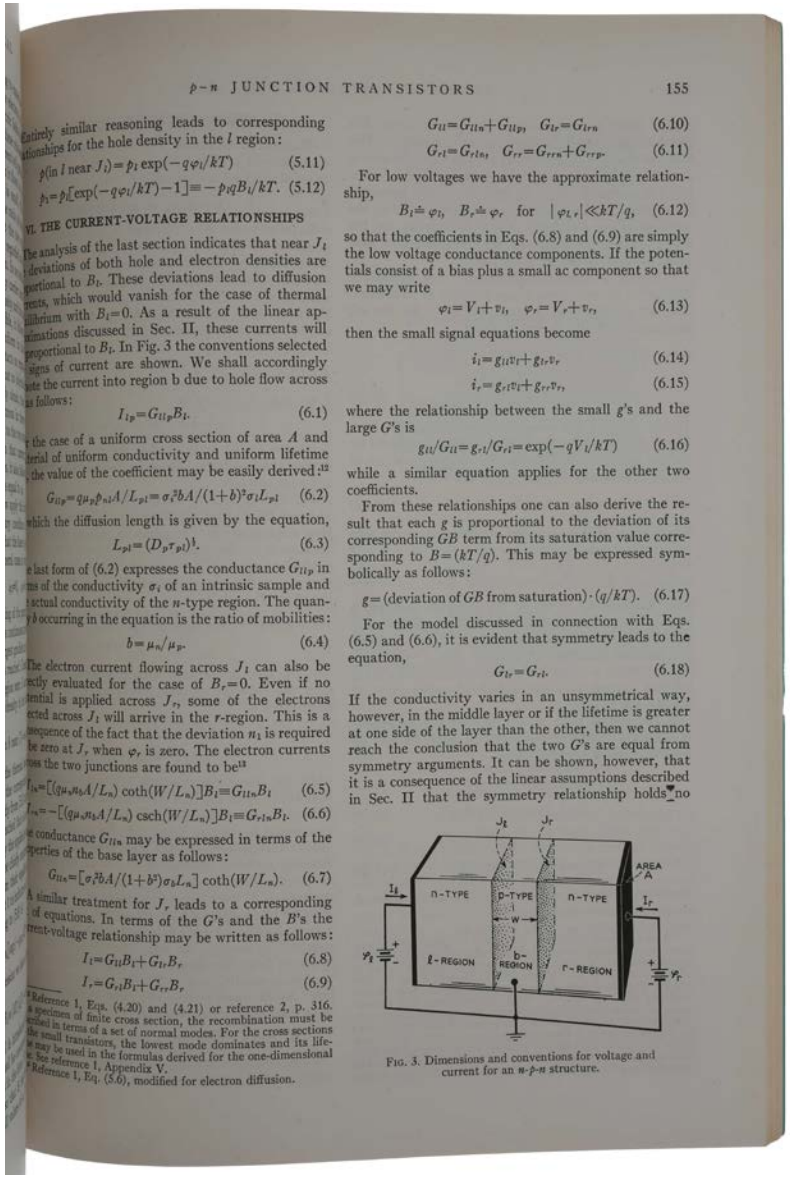
[15] The transistor whose characteristics are given in Fig. 3 is one of an experimented pilot production which is under the general direction of J. A. Morton.

[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

ntirely similar reasoning leads to corresponding ationships for the hole density in the l region:

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

$$p_1=p_l[\exp(-q\varphi_l/kT)-1]\equiv -p_lqB_l/kT. \quad (5.12)$$

VI. THE CURRENT-VOLTAGE RELATIONSHIPS

he 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-ximations 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 ote the current into region b due to hole flow across as follows:

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

r 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-y 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 ectly evaluated for the case of $B_r=0$. Even if no tential is applied across J_r, some of the electrons ected 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]

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

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

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-e may be used in the formulas derived for the one-dimensional e. 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.

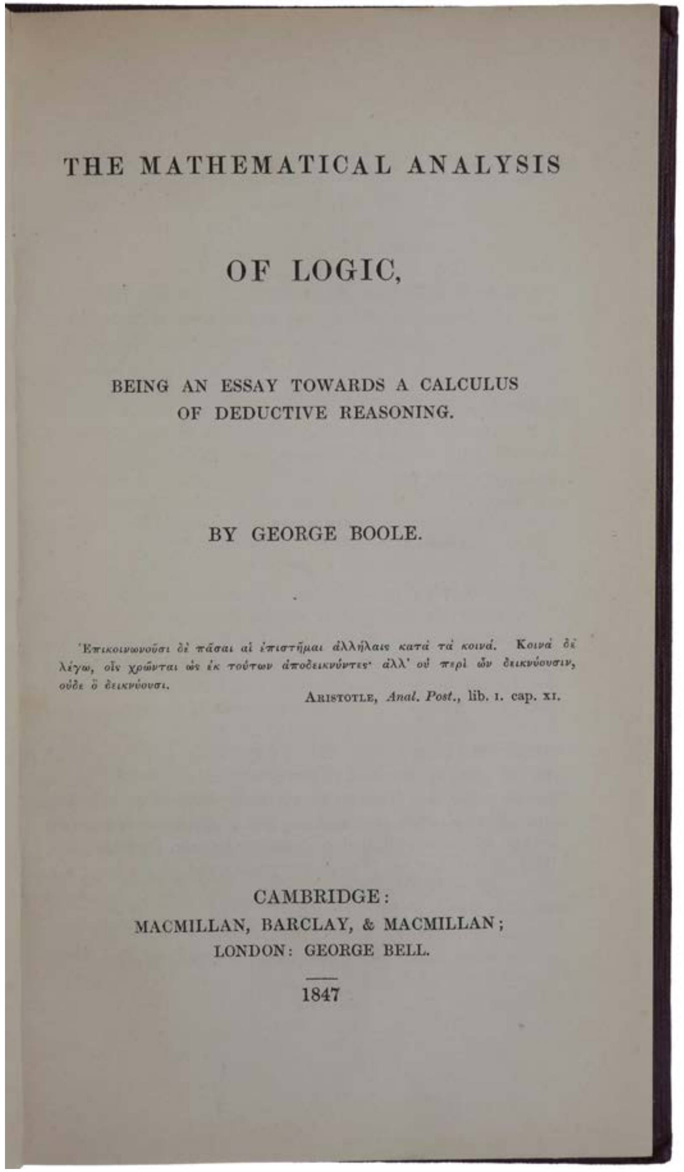
BOOLEAN ALGEBRA - THE BIRTH OF MODERN LOGIC

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

$18,000

8vo (217 x 133 mm), pp. [i:blank] [ii:title], [1-3], 4-82, errata slip tipped in after title. 20th century cloth with gilt spine lettering. Clean and fresh throughout.

First edition, very rare in commerce, of Boole's first book, the birth of modern symbolic logic and the first presentation of 'Boolean algebra'. "Boole's work also contains what Bertrand Russell called the greatest discovery of the nineteenth century: the nature of pure mathematics" (OOC). "Self-taught mathematician George Boole (1815–1864) published a pamphlet in 1847 – *The Mathematical Analysis of Logic* – that launched him into history as one of the nineteenth century's most original thinkers" (Introduction to the CUP reprint). "*The Mathematical Analysis of Logic* marks the beginning of symbolic logic in the modern sense. Boole showed that classical logic was actually a branch of mathematics which gave rise to a hitherto unconsidered type of algebra. Boole's book however went considerably further. It threw a great deal of light on the nature of pure mathematics; it opened up possibilities of an extension of the subject into totally new and unexpected areas – classical mathematics had concentrated on the notions of shape and number and even when symbols were employed, they were generally interpreted in terms of number. Boole had now introduced the notion of interpreting symbols as classes or sets of objects, a concept breathtaking in its



THE MATHEMATICAL ANALYSIS

OF LOGIC,

BEING AN ESSAY TOWARDS A CALCULUS
OF DEDUCTIVE REASONING.

BY GEORGE BOOLE.

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

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

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

1847

scope because it meant that the study of all well-defined sets of objects now came under the realm of mathematics … By enlarging the horizons of mathematics so enormously, Boole unwittingly (but perhaps subconsciously, wittingly) highlighted a topic that has come to influence virtually every aspect of present-day life – the storage and processing of information, which in turn has led to the development of computer science. Not only is Boole's algebra the 'correct' and most economical tool for handling information, but the electronic machines which now do the work actually operate according to principles determined by that self-same algebra. Boole has been called the 'Father of Symbolic Logic' and the 'Founder of Pure Mathematics', but he is just as deserving of the title, 'Father of Computer Science'" (MacHale, p. 82). ABPC/RBH list only two copies since Honeyman: the OOC copy, Christie's 2005, $10,800, and Bonham's 2013, £25,000 = $38,000. Both of these copies were in modern bindings (the OOC copy with the original front wrapper bound in).

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

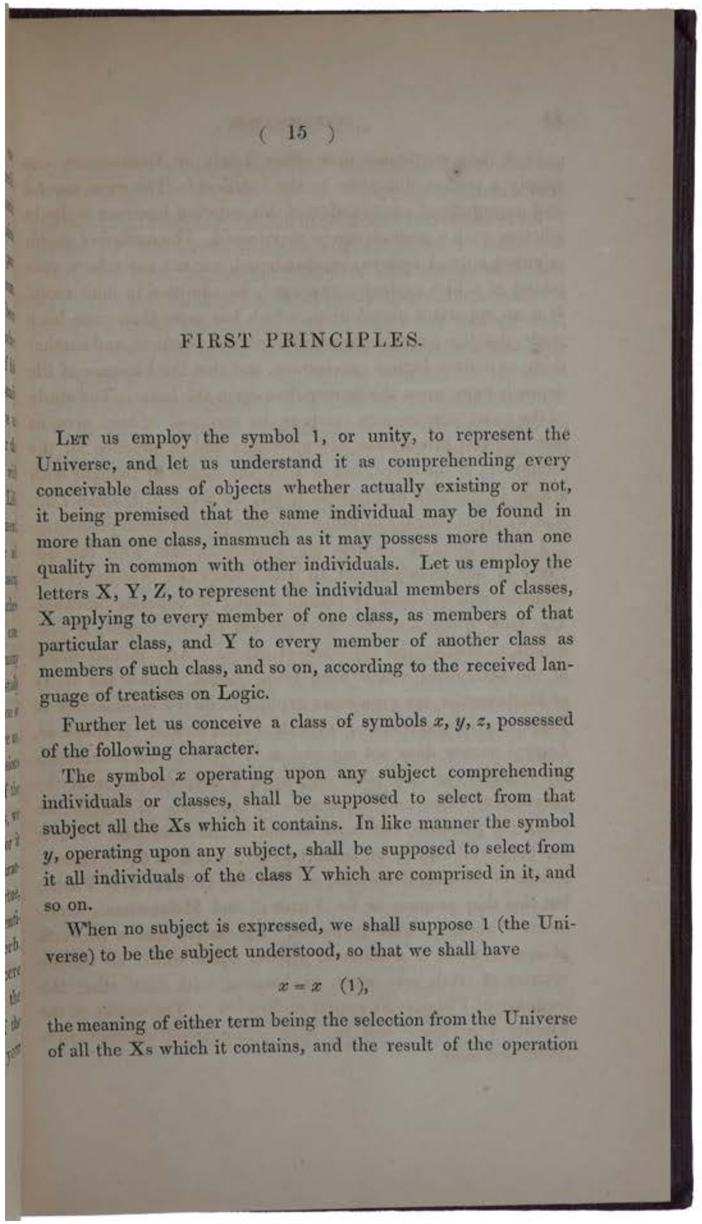
"Early in the spring of 1847, Boole's long-dormant interest in the connections between mathematics and logic was dramatically reawakened. At this time, a furious controversy was raging between the supporters of de Morgan and those of Sir William Hamilton, the Scottish philosopher and metaphysician (not to be confused with Sir William Rowan Hamilton). Hamilton was a logician who distrusted mathematics, but he was an innovator in logic and had, about this time, introduced the notion of 'quantification of the predicate' which was to lead to a widening of the scope of logic. Classical logic had concentrated on the 'four forms' of statement — all A are B, no A are B, some A are B, some A are not B. In Hamilton's approach, the predicate, or second term B, is quantified by considering statements of the type: all A are all B, any A is not some B, and so on. De Morgan too was at this time working on a more mathematical theory of logic which included a notion equivalent to quantification of the predicate. Hamilton at once accused de Morgan of plagiarism, despite the fact that the notion in question was not original to either of them nor, as it transpired, of any great significance *per* se in the development of logic. Hamilton's charges were unjust, even absurd, but controversy raged for many years and attracted a great deal of attention …

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

"The priority dispute triggered Boole to write his first book; but its content was much influenced by his researches on differential operators. Partly drawing upon his work of his friend the Cambridge mathematician Duncan Gregory, he produced a long paper on these methods which he submitted to the Royal Society in 1844. After wondering about rejecting the manuscript they published it ['On a general method in analysis', *Philosophical Transactions*, Vol. 134, pp. 225–282], and then awarded him a Gold Medal for his achievement!

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

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


(15)

FIRST PRINCIPLES.

Let us employ the symbol 1, or unity, to represent the Universe, and let us understand it as comprehending every conceivable class of objects whether actually existing or not, it being premised that the same individual may be found in more than one class, inasmuch as it may possess more than one quality in common with other individuals. Let us employ the letters X, Y, Z, to represent the individual members of classes, X applying to every member of one class, as members of that particular class, and Y to every member of another class as members of such class, and so on, according to the received language of treatises on Logic.

Further let us conceive a class of symbols x, y, z, possessed of the following character.

The symbol x operating upon any subject comprehending individuals or classes, shall be supposed to select from that subject all the Xs which it contains. In like manner the symbol y, operating upon any subject, shall be supposed to select from it all individuals of the class Y which are comprised in it, and so on.

When no subject is expressed, we shall suppose 1 (the Universe) to be the subject understood, so that we shall have

$$x = x \quad (1),$$

the meaning of either term being the selection from the Universe of all the Xs which it contains, and the result of the operation

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

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

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

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

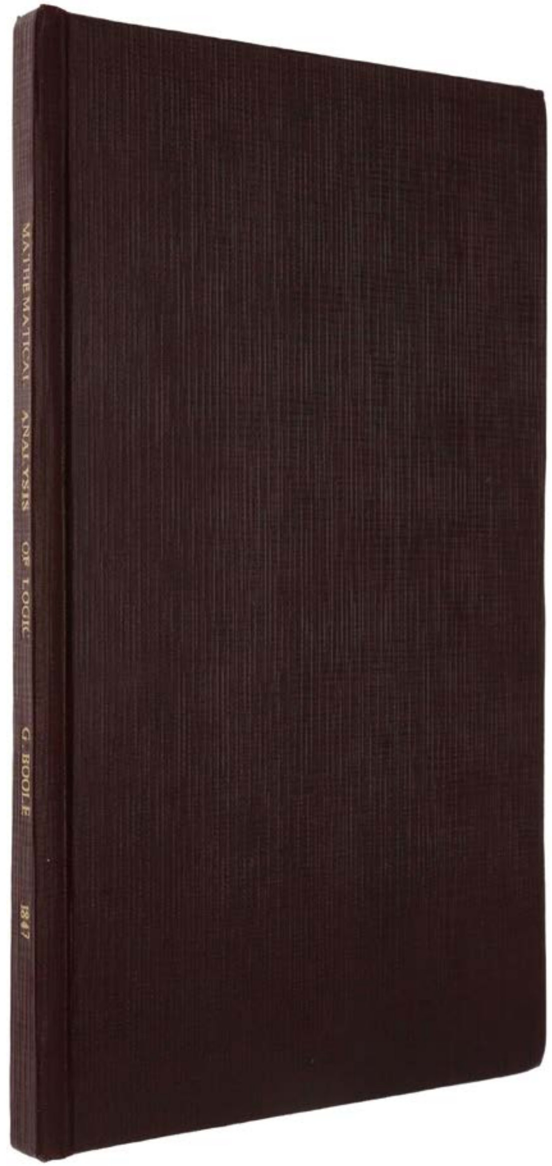
"Beginning with the chapter Properties of Elective Functions, Boole developed general theorems for working with equations in his algebra of logic—the Expansion Theorem and the properties of constituents are discussed in this chapter. Up to this point his sole focus was to show that Aristotelian logic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

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

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

"The range and depth of the achievements of George Boole are especially remarkable when one notes not only the shortness of his life but also the disadvantageous circumstances of his background. He was born to an intelligent tradesman who however was so poor that George had to become the main breadwinner in his 20th year when he opened a school. Nevertheless, he found time to teach himself advanced mathematics, and also Greek, Latin, French and German, especially in order to read important works. His research papers began to appear in the early 1840s, and his principal interest soon turned to an English specialty: the 'calculus of operations', now called 'differential operators', where differentiation was represented by the letter 'D', higher-order differentiation by 'D^2, D^3, . . .', integration by 'D^{-1}', and so on. This tradition had developed under the influence of the algebraised calculus propounded by J. L. Lagrange, initially by some French mathematicians; but from the 1810s this algebra and related topics were prosecuted in England by Charles Babbage and John Herschel as part of the revival of research mathematics there. Boole was to become a major figure in this movement in the next generation; as we have seen, it was to affect his work on logic" (*Landmark Writings*, pp. 470-471).

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



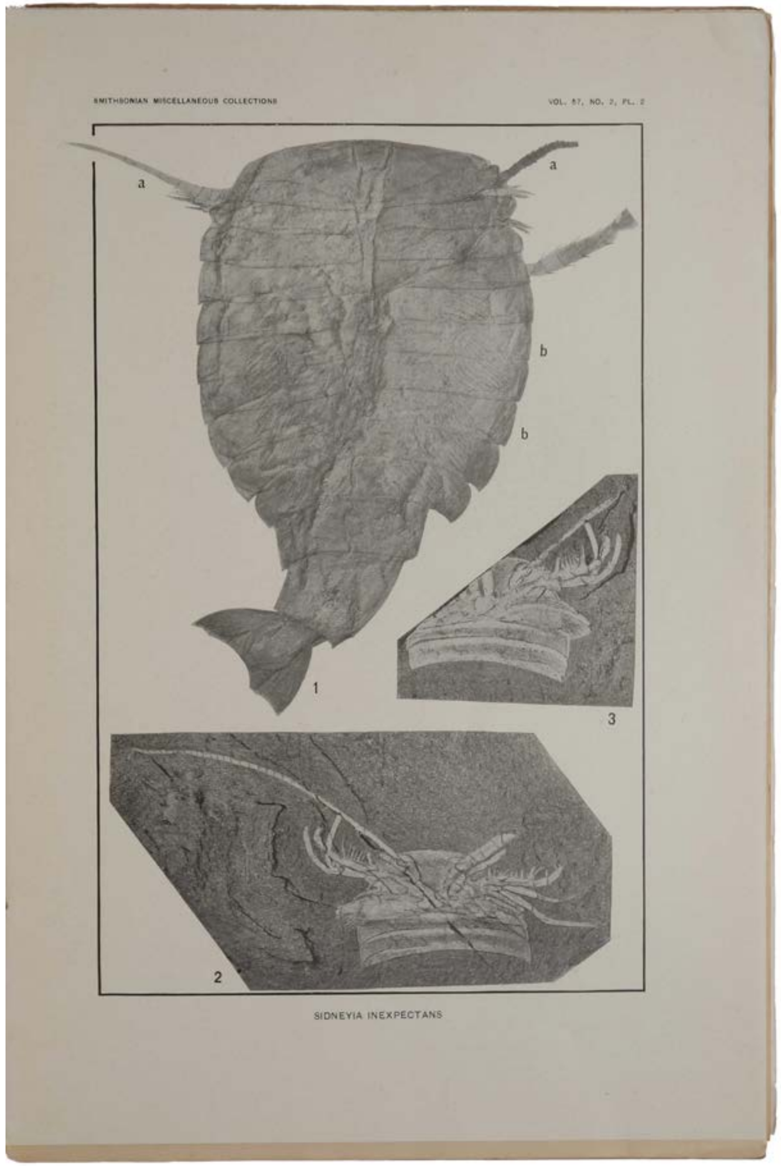
A MAJOR LANDMARK IN 20TH CENTURY GEOLOGY

[BURGESS SHALE] WALCOTT, Charles Doolittle. *Cambrian Geology and Paleontology II, No. 2 – Middle Cambrian Merostomata.* Washington DC: The Smithsonian Institution,1911.

$1,800

Smithsonian Miscellaneous Collections, Vol. II, No. 2. 8vo (245 x 165 mm), pp. 40, with six plates (numbered 2-7, but complete). Stitched as issued in original printed wrappers (spine with tear, some light browning to the text but the plates are in fine condition).

First edition of the first published account of the Burgess Shale fauna, which is now recognized as "one of the most important geologic discoveries of the twentieth century" (Smithsonian). In his *Wonderful Life* (1989), which first described the Burgess Shale fauna for the general reader, Harvard palaeontologist Stephen Jay Gould wrote (pp. 23-4): "Without hesitation or ambiguity, and fully mindful of such paleontological wonders as large dinosaurs and African ape-men, I state that the invertebrates of the Burgess Shale, found high in the Canadian Rockies in Yoho National Park, on the eastern border of British Colombia, are the world's most important fossils. Modern multicellular animals make their first uncontested appearance in the fossil record some 570 million years ago – and with a bang, not a protracted crescendo. This "Cambrian explosion" marks the advent (at least into direct evidence) of virtually all major groups of modern animals … The Burgess Shale is our only extensive, well-documented window upon that most crucial

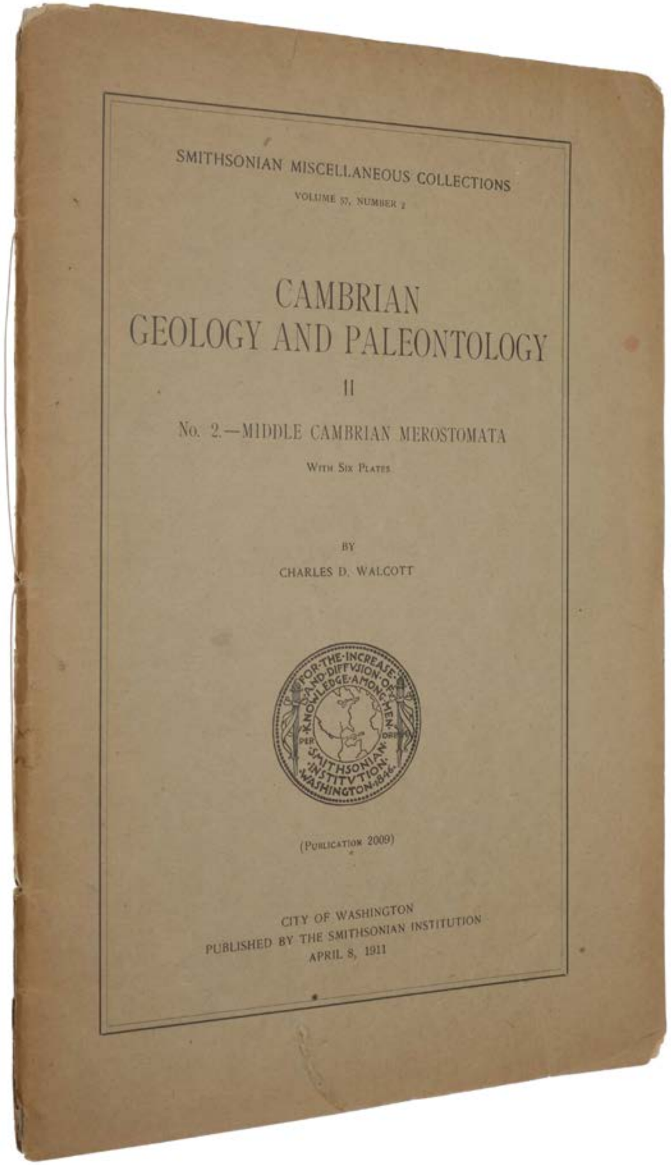


event in the history of animal life, the first flowering of the Cambrian explosion."

Walcott (1850-1927) "was appointed the fourth Secretary of the Smithsonian in January 1907, after serving as director of the U.S. Geological Survey. His research on North American Cambrian fossils took him to sites throughout the United States and Canada. In 1909, while collecting in the Canadian Rockies, he discovered the first fossils of an enormous trove of rare, carbonized, soft-bodied fauna from the Cambrian Period. He dubbed the fossils the Burgess Shale after nearby Mount Burgess. The find was so extensive, and the creatures so foreign and new to science, that Walcott returned to the field for the next five summers straight, and then made additional periodic trips up until 1924 …

"Although the fossils are located high up in the Canadian Rockies today, 505 million years ago the area was covered by the sea. The Burgess Shale animals were preserved in a series of mudslides that instantly buried their thriving late Cambrian reef community. The amazing range of fossilized organisms that Walcott and his colleagues discovered at the Burgess Shale give us one of the best pictures we have of what is known as the Cambrian Explosion – the burst of diversification and proliferation of animals that gave rise to the lineages of life as we know it today. In the Burgess Shale were found the first examples we have of trilobites, brachiopods, echinoderms, and others, including curious oddities that come from extinct lines" (Smithsonian).

"Since its discovery in 1909, the Burgess Shale has become the authoritative picture of life in the Cambrian Period. No longer solely relying on the remnants of hard shells or exoskeletons, we now have a much better and richer picture of early animal communities. The sediment flow fossilization of the Burgess Shale has produced unique dark stained fossils that reveal the countless variety of soft-bodied organisms. Soft-bodied organisms are now known to have existed in



greater number and variety than those Cambrian organisms exhibiting hard parts. Additionally, quarries of the Burgess Shale contain evidence of the existence of our chordate ancestors, with fossils so finely preserved that they display traces of a notochord. Most importantly, the Burgess Shale tells of the Cambrian Explosion, a huge radiation of marine animal life that included sponges, soft-bodied arthropods and those with hard exoskeletons, the first chordates, worms, and trilobites, as well as the strange spiked creatures such as *Wiwaxia*, and the large predator *Anomalocaris*. The Burgess Shale represents a snapshot of the evolution of a marine biota that would come to dominate the world's oceans for the next 300 million years" (ucmp.berkeley.edu/cambrian/burgess.html).

Important as the Burgess Shale fossils are, their interpretation has proved to be controversial. In *Wonderful Life*, Gould accused Walcott of being blind to the obvious strangeness of the Burgess Shale creatures because he was committed to the orthodox view that the cone of evolutionary diversity must expand through time. Gould suggested that the extraordinary diversity of the fossils indicated that life forms at the time were much more disparate in body form than those surviving today, and that many of the unique lineages were evolutionary experiments that became extinct. He also stressed the contingency of the evolutionary process, arguing that, if we could somehow "rewind the tape" of evolution and let it play again, chance would favor a different selection of that original multitude, and the world would be a very different place from the one we see around us. In particular, the appearance of humanity or the human level of awareness was not an inevitable product of evolution.

Gould's interpretation has been challenged, notably by the Cambridge palaeontologist Simon Conway Morris in *The Crucible of Creation: The Burgess Shale and the Rise of Animals* (1998). Conway Morris to some extent rehabilitates Walcott's reputation by showing that the diversity of Cambrian forms was by no means as extensive as Gould claimed. Using cladistic analysis, he argues that the Burgess Shale creatures can all be fitted into known phyla, or show intermediate states that actually throw light on the process by which the known phyla diverged from one another. Modern studies have now shown that all the Burgess Shale arthropods can be accommodated within a scheme that explains their origin in monophyletic terms – from a single common ancestor in which the basic arthropod structure was developed. Conway Morris thus claims that Gould's scenario for the origin of animals is disproved: There was no vast radiation and no winnowing out of many early phyla by extinction. He also takes issue with Gould's rerunning of the tape of evolution, arguing that the combined limitations of the developmental pathways triggered by genetics and the demands of the environment mean that the possible outcomes of the evolutionary process are very limited. We can conceive of all sorts of alien creatures, but they could never exist in the real world – and what can exist is pretty much confined to what we actually see. So rerunning the tape would produce more or less the same results, although the details might be different.

Charles Doolittle Walcott and the Discovery of the Burgess Shale (1909): Celebrating 100 Years, Smithsonian National Museum of Natural History (mnh.si.edu/onehundredyears/expeditions/burgess_shale.html).

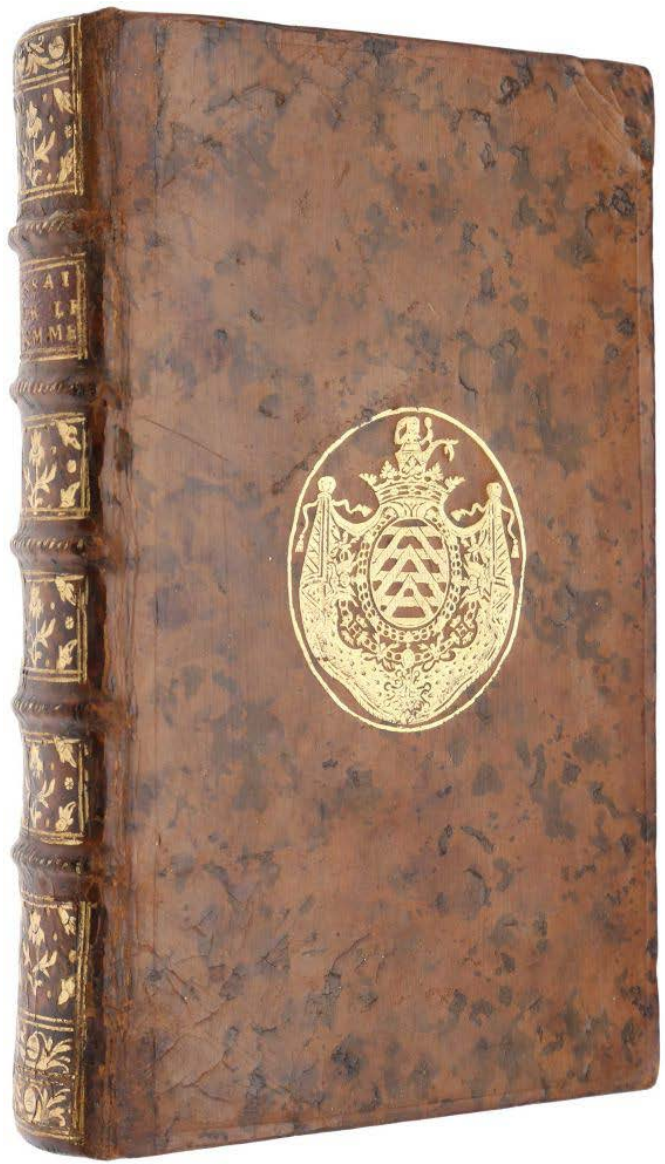
THE LA ROCHEFOUCAULD-LIANCOURT COPY

CANTILLON, Richard. *Essai sur la Nature du Commerce en Général.* London [but probably Paris]: Fletcher Gyles, 1755.

$95,000

12mo (165 x 93mm), pp. [4], [1], 2-430, [6:index]. Contemporary mottled calf, sides with gilt-stamped arms of the duc de Liancourt, gilt spine with raised bands, red morocco label and repeated fleurons, red edges. Exceptionally fresh and crisp. It is hard to imagine a better copy.

First edition, the exceptionally fine La Rochefoucauld-Liancourt copy in untouched armorial binding, of the earliest treatise on modern economics. Cantillon is the "founding father of modern economics" (Rothbard) and the *Essai* has been declared "more emphatically than any other single work, the cradle of political economy" (Jevons), and "the most systematic statement of economic principles before the *Wealth of Nations*" (Roll). It "is notable for its model building, its analysis of market forces and the role of the entrepreneur, its outline of the circular flow of income, and its monetary theory. Cantillon was the first real model builder in economics" (ODNB). Circulating first in manuscript and published posthumously, the *Essai* anticipates Malthus, and was cited by Adam Smith, Condillac, Quesnay, Harris and Postlethwayt. It covers political economy, currency, foreign trade and exchange. Among many new ideas, it contained "an almost complete anticipation of the Malthusian theory of population" (*The New Palgrave,* I, p. 318). "Its treatment of population influenced Mirabeau and Adam Smith and, through the latter, Malthus. It contained a theory of relative wages



which was used by Smith; the famous *Tableau économique* of the Physiocrats was probably inspired by the *Essai*, and the treatment of the theory of money was of pioneering importance. The *Essai* also contains his theories of wages, prices, and interest, the workings of currency circulation, the role of precious metals in the international economy, and other subjects" (Britannica). The *Essai* is divided into three parts: Part I lays out the fundamentals of the analytical structure; Part II is devoted to money, credit, and interest; and Part III deals mainly with foreign trade, but also gives an acute analysis of banks, bank credit, and coinage. The book was chosen among the 400 most influential books ever written in French in the 1990 exhibition at the Bibliothèque Nationale, *En français dans le texte*.

Provenance: A superb copy bound at the time for François-Alexandre-Frédéric, duc de La Rochefoucauld-Liancourt (1747-1827), with his coat of arms gilt on sides and the armorial engraved ex-libris of the Bibliothèque de Liancourt. A philanthropist, traveller, and statesman, the Duke de La Rochefoucauld-Liancourt was one of the leading figures of the end of the 18th century. As a liberal, he participated in the French Revolution from the outset, remaining loyal to the King. He is famous for his answer to Louis XVI who had asked him on 14 July 1789 'Is it a revolt?': 'No, Your Majesty, it is a revolution'. He fled France and found refuge in England before travelling to the United States. He attempted to save the royal family. Back in France after the Revolution, he continued to promote his liberal ideas, helping the poor and creating a school. Two other copies with the coat of arms of the La Rochefoucauld family have appeared on the market in the last twenty years; they both came from the La Roche-Guyon branch of the family. This copy is the only one with the Liancourt ex-libris, stating its provenance.

"Richard Cantillon, acknowledged by many historians as the first great economic 'theorist', is an obscure character. This much is known: he was a British subject, an Irishman, who carved out a career in banking in France during the 1710s

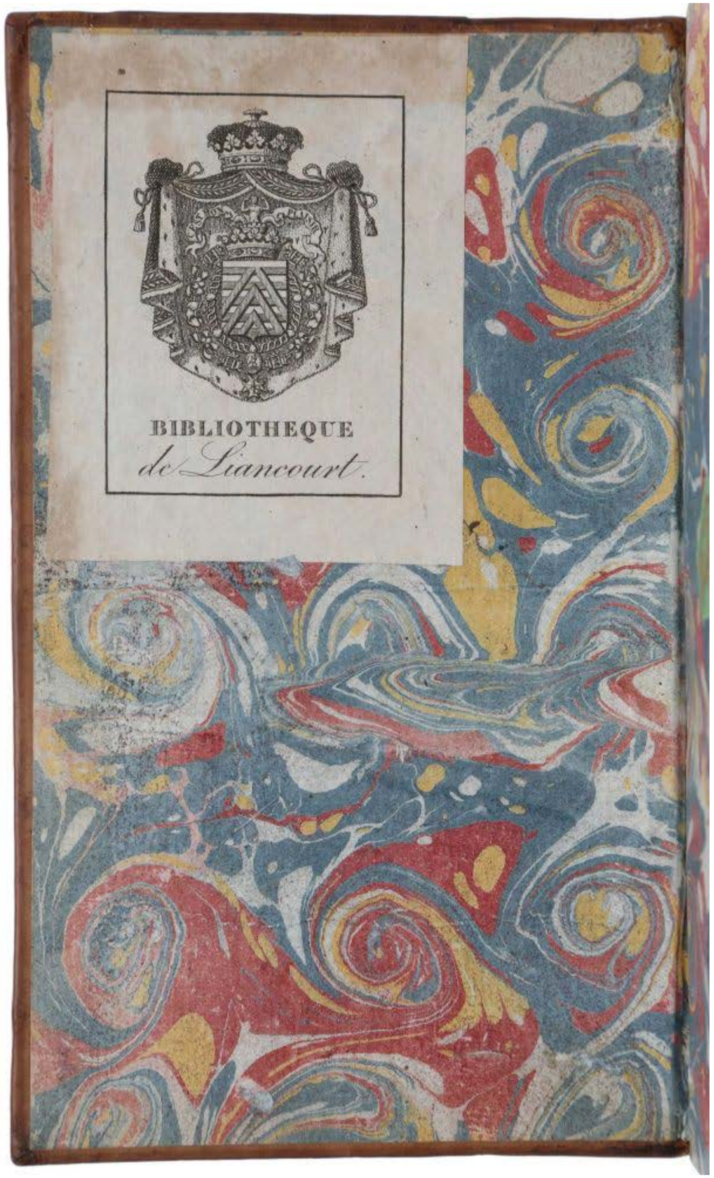
"Although there have been many attempts to reconstruct the story of his background from various sources, it has proven contradictory and elusive. It has been said his family were originally landlords from County Kerry dispossessed of their lands during the confiscations of the 17th C., although they remained well-connected and probably Jacobite. Young Cantillon is said to have taken French nationality in 1709 and in 1711 might been in the service of James Brydges, Paymaster-General, as financial agent to the British forces in Spain. It only becomes more certain that he moved to Paris in 1714 to launch a career as a merchant banker in the house of a relative (also named Richard Cantillon). By 1716, Cantillon had either bought his relative out, or launched his own banking house (possibly with financial help from Brydges). Cantillon got involved early in John Law's schemes, but had little confidence in their success. Cantillon reputedly made a fortune of some twenty million livres by speculating on the collapse of shares in Law's companies in 1720-21, actions which led to multiple lawsuits and even brief arrest by Paris authorities. Cantillon moved to England thereafter, taking up residence in London. Cantillon died in 1734 in a fire in his London home – allegedly set by his discharged cook (although there remains speculation that Cantillon set the fire himself, staging his death to escape his lawsuits, and fled abroad – in at least one account, to Surinam).

"Cantillon's entire reputation rests on his one remarkable treatise, *Essai Sur la Nature du Commerce en Général*. Apparently, the *Essai* was originally written circa 1732, probably in English and containing a statistical supplement, but this version has since been lost. It was translated (probably by Cantillon himself) into French for circulation in manuscript form among his friends. This French version was published anonymously in England [but see below] in 1755, some twenty years after his death (publication was probably undertaken by his second cousin, Philip Cantillon). Two other published versions of this essay exist – a reprint appended to the 1755 French translation of David Hume's *Discours Politiques*, and a smaller pamphlet version published by another press in 1756. Prior to these, excerpts (possibly from the missing English original) were liberally plagiarized by Malachy Postlethwayt in his 1749 *Universal Dictionary of Trade and Commerce*.

"Cantillon's *Essai* was well-known in Enlightenment France, admired by Vincent de Gournay, cited by the Physiocrats (e.g. Mirabeau, Quesnay and Turgot) and one of the few works referenced by Adam Smith. It continued to be read in the 19th C. French school. However, Cantillon's work fell into relative obscurity in the English-speaking world until resurrected and popularized by William Stanley Jevons in the 1880s.

"Cantillon was perhaps the first to define long-run equilibrium as the balance of flows of income, thus setting the foundations both for Physiocracy as well as Classical Political Economy. Cantillon's system was is clear and simple and absolutely path-breaking. He developed a two-sector general equilibrium system from which he obtained a theory of price (determined by costs of production) and a theory of output (determined by factor inputs and technology). He followed up on Petty's conjecture about the par of labor and land, thereby enabling him to reduce labor to the amount of necessities needed to sustain it and thus making both labor supply and output a function of the land absorption necessary to produce the necessities to feed labor and the luxuries to feed landlords. By demonstrating that relative prices are reducible to land-absorption rates, Cantillon can be said to have derived a fully-working 'land theory of value'.

"In determining the natural wage for his model, Cantillon lays out the wage-fertility dynamics ('men multiply like mice in a barn if they have unlimited means



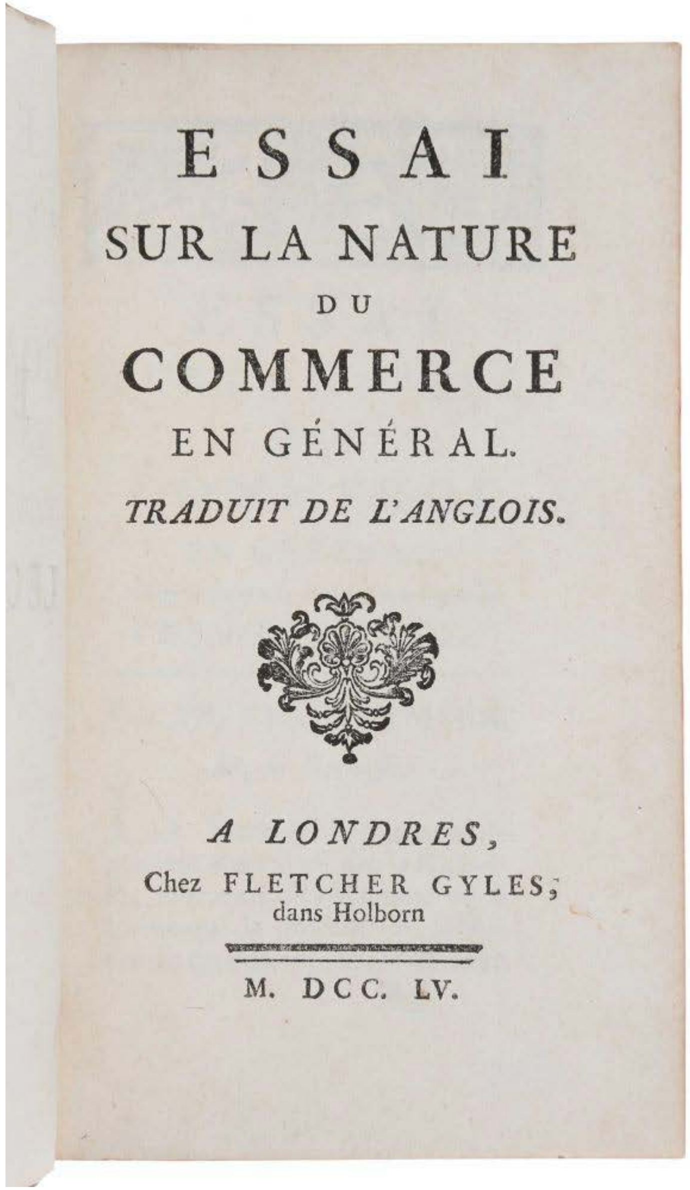
of subsistence'), anticipating the theory developed by Malthus in 1798.

"Cantillon's careful description of a supply-and-demand mechanism for the determination of short-run market price (albeit not long-run natural price) also stand him as a progenitor of the Marginalist Revolution. In particular, his insightful notes on entrepreneurship (as a type of arbitrage) have made him a darling of the modern Austrian School. Cantillon was also one of the first (and among the clearest) articulators of the Quantity Theory of Money and attempted to provide much of the reasoning behind it.

"Finally, one of the consequences of his theory was that he arrived at a quasi-Mercantilist policy conclusion for a favorable balance of trade but with a twist: Cantillon recommended the importation of 'land-based products' and the exporting of 'non-land-based' products as a way of increasing national wealth" (*History of Economic Thought*, hetwebsite.net/het/profiles/cantillon.htm).

Although the work carries a London imprint, according to the recent variorum edition edited by Van den Berg (2015), it was published by the Paris bookseller Pierre-André Guillyn.

Einaudi 846; *En français dans le texte* 159; Goldsmiths' 8989; Higgs, *Bibliography of Economics*, 938; Kress 542. Roll, *History of Economic Thought*, p.121. Schumpeter, *History of Economic Analysis*, 217ff. For a detailed account of the work, see Anthony Brewer's introduction to the 2017 Routledge edition of Henry Higgs' English translation.



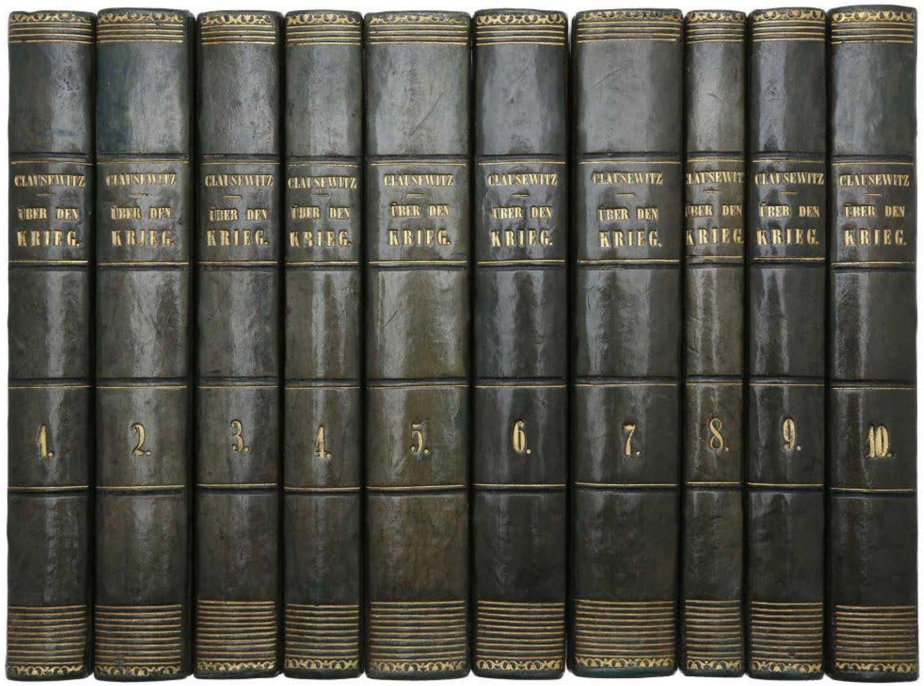
PMM 297 - THE PHILOSOPHY OF WAR

CLAUSEWITZ, Carl von. *Hinterlassene Werke des Generals Carl von Clausewitz über Krieg und Kriegführung.* Berlin: Trowibsch and son for Ferdinand Dümmler, 1832-1837.

$48,500

10 volumes, 8vo (202 x 120 mm). Two folding engraved maps and one folding table. (Some light spotting mainly to the edges, faint dampstain in the margin of vol. 3, short tears at map folds.) Contemporary green quarter calf, flat spines ruled in gilt and blind, lettered and numbered directly in gilt (spines lightly faded). Highly scarce in such fine condition.

First edition, very rare complete set of all 10 volumes, of this classic work, including Vom Kriege (vols. 1-3), "the most profound exposition of the philosophy of war" (PMM). "On War is one of the most important books ever written on the subject of war. Clausewitz, a Prussian officer who fought against the French during the Napoleonic Wars, sought to understand and analyse the phenomenon of war so that future leaders could conduct and win conflicts more effectively. He studied the human and social factors that affect outcomes, as well as the tactical and technological ones. He understood that war was a weapon of government, and that political purpose, chance, and enmity combine to shape its dynamics. On War continues to be read by military strategists, politicians, and others for its timeless insights" (Introduction to abridged English translation, Oxford University Press, 2008). "The book is less a manual of strategy and tactics, although it incorporates the lessons learned from the French revolutionary and Napoleonic wars, than a general inquiry into the interdependence of politics and warfare and the principles governing either or both. War, Clausewitz maintained, must always be



regarded as 'a political instrument'; for war, his most famous aphorism runs, 'is nothing but politics continued by different means'. Consequently, he scorns the notion of 'the harmful influence of politics upon the conduct of war', since blame, or praise, must be attached to politics itself. If the course of politics is sound, political influence on the conduct of war can only be advantageous. 'The French revolutionary victories over twenty years resulted mainly from the faulty politics of the opposing governments.' His basic conception, that military decisions must always be subordinate to political considerations, is buttressed by the emphasis laid on morals and morale as the decisive factors in war. He therefore condemns all rigid blue-prints for campaigns and battles, defines strategy as 'a perpetual alternation and combination of attack and defence,' and implies the then startling proposition that there no bad soldiers but only bad officers" (PMM).

"Clausewitz (1780-1831) enlisted in the Prussian army in 1792, and in 1793–95 he took part (and was commissioned) in the campaigns of the First Coalition against Revolutionary France. In 1801 he gained admission into the Institute for Young Officers in Berlin, an event that proved to be a turning point in his life.

"During his three years at the institute, Clausewitz became the closest protégé of Gerhard Johann David von Scharnhorst, the institute's head. The broad curriculum, coupled with Clausewitz's extensive reading, expanded his horizons dramatically. His basic ideas regarding war and its theory were shaped at that time. After finishing first in his class, Clausewitz was on the road leading to the centre of the political and military events during the French Revolutionary and Napoleonic wars, the reform of the Prussian army that followed Prussia's defeat, and the restoration of European monarchies following the defeat of Napoleon.

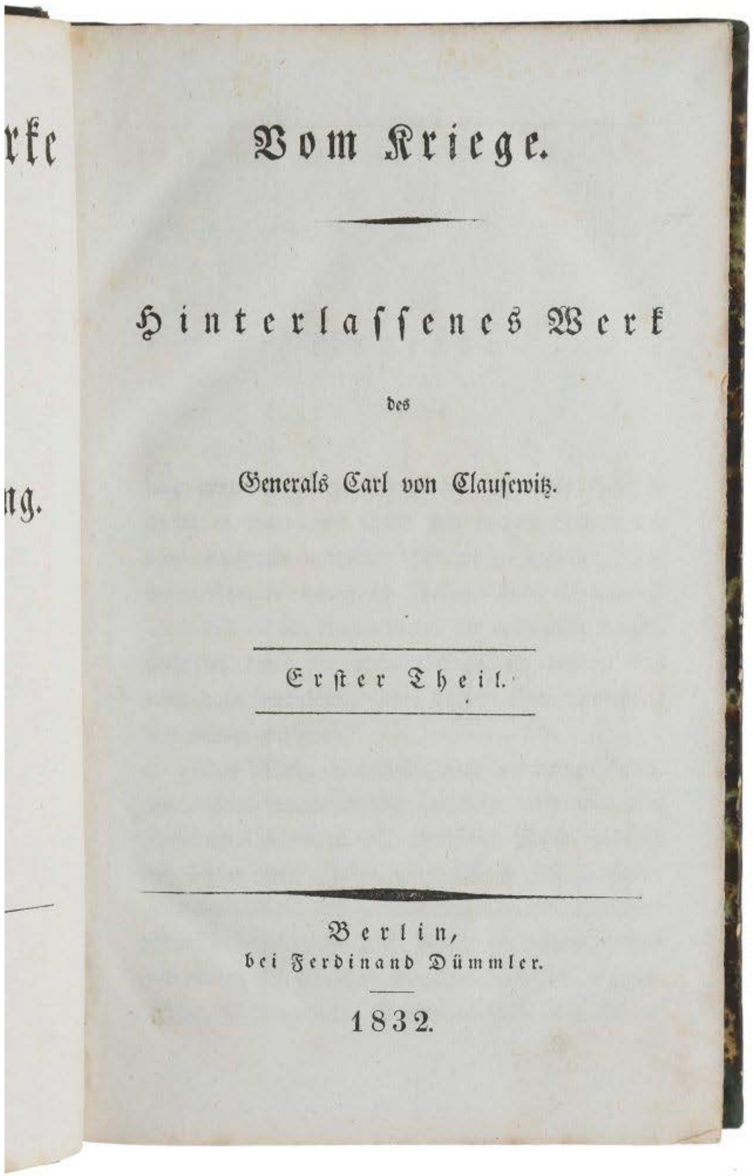
"In 1804 Clausewitz was appointed adjutant to Prince August Ferdinand of Prussia. In this capacity, he took part in the Battle of Jena-Auerstädt (1806). In the wake of Prussia's catastrophic defeat by Napoleon, he and the prince fell into French captivity. With the Prussian army demolished and the prince captured, Prussia was forced to give up half of its territory in the concluding peace treaty. After their release at the end of 1807, Clausewitz joined the group of young and middle-rank officers around Scharnhorst, who struggled to reform the Prussian army. The reformers believed that Prussia's only hope of survival in the age of mass enlistment, as introduced by Revolutionary France, was in adopting similar institutions. However, such a modernization of society, state, and army was widely resisted among the aristocratic elite, which feared an erosion of its status. During these years, Clausewitz married Countess Marie von Bruhl, with whom he formed a very close but childless union. Clausewitz was ill at ease in society and more in his element among a small circle of fellow military reformers.

"In the war ministry that was formed, headed by Scharnhorst, Clausewitz served as his mentor's assistant and was then simultaneously appointed a major in the general staff, instructor at the new Officers' Academy, and military tutor to the Prussian crown prince. Like his friends in the reform circle, he looked for any opportunity to wage a national war of liberation against France, and he was repeatedly frustrated by the king's hesitation to act against the much superior French power. In 1812, when Prussia was forced to join Napoleon's invasion of Russia, Clausewitz, like some of his comrades, resigned his commission and joined the Russian service. He served in various staff posts, and during the catastrophic French retreat he was instrumental in generating the chain of events that ultimately drove Prussia to change sides. Clausewitz took part in the final campaigns that brought down Napoleon in 1813–15. During the Waterloo campaign, he served as chief of staff to one of the four Prussian army corps.

"With the coming of peace and the setting in of the reaction to the terms of the treaty in Prussia, which clouded his career, Clausewitz increasingly concentrated on his intellectual interests. He had been thinking and writing on war and its theory since his days in the Institute for Young Officers. His tenure as head of the Military Academy at Berlin (1818–30) left him plenty of time to work on his major study On War. Appointed chief of staff to the Prussian army that prepared for intervention against the Polish revolt of 1831, Clausewitz died of cholera that year. His unfinished work, together with his historical studies, was posthumously published by his widow.

"Clausewitz's ideas were shaped by the coming together of two revolutions that dominated his life and times. Intellectually, he expressed in the military field the sweeping Romantic reaction against the ideas of the Enlightenment, a reaction that had been brewing in Germany since the late 18th century and that had turned into a tidal wave by the beginning of the 19th century in response to French Revolutionary ideas and imperialism. In the spirit of their time, the military thinkers of the Enlightenment had believed that war ought to come under the domination of reason. A comprehensive theory based on rules and principles ought to be formulated and, wherever possible, given a mathematical form. Against this Clausewitz argued, in line with Romantic critics, that human affairs and war in particular were very different from natural phenomena and the sciences. He ruled out any rigid system of rules and principles for the conduct of war, celebrating instead the free operation of genius, changing historical conditions, moral forces, and the elements of uncertainty and chance. These elements, especially the enemy's counteractions, give war a nonlinear logic. Every simple action encounters 'friction'—in Clausewitz's borrowed metaphor from mechanics—which slows it down and may frustrate it.

"At the same time, Clausewitz believed that a general theory of war was attainable and that it should express war's immutable essence, nature, or concept and guide all military action. Here is the second revolution that dominated his life. His



generation witnessed the collapse of the limited warfare of ancien régimes in the face of the all-out effort and strategy of destruction, or total war, unleashed by the French Revolution and Napoleon. While very conscious of the changing social and political conditions that had brought about this transformation of warfare, Clausewitz, like his contemporaries, held that the new, sweeping way of war making, culminating in the decisive battle and the overthrow of the enemy country, reflected the true nature of war and the correct method of its conduct. He had expressed this view in his writings through 1827, when the first six books of On War (out of an eventual eight) had been completed.

"However, in 1827 Clausewitz began to have serious doubts about whether total war was really the sole legitimate type of war. He came to the conclusion that there were in fact two types of war, total (or absolute) and limited, and that it was, above all, political aims and requirements that imposed themselves on war and dictated its intensity—hence his famous dictum, 'War is a continuation of state policy with the admixture of other means.' In the light of these new ideas, Clausewitz added the last two books of On War and started to revise the first six. He died while working on Book One, however. Thus, the manuscript remained as an incomplete draft—Books Two to Six expressed his old ideas regarding the supremacy of the decisive battle and total war, whereas the beginning and end of On War proclaimed the subservience of war to politics and consequently the legitimacy of limited war. It was in this form that Clausewitz's widow published the manuscript after his death.

"This curious development of Clausewitz's work has had a profound effect on the reception of his ideas. Since later readers have been largely unaware of the reasons for the glaring inconsistency in On War, while being impressed by its sophistication, they have tended to concentrate on those ideas that most accorded with the spirit of their own times. For decades after Clausewitz's death, On War remained a respected but little-known work. However, Prussia's victories in the German Wars of Unification—orchestrated by a self-declared disciple of Clausewitz, Chief of Staff Helmuth von Moltke—made Clausewitz the most celebrated strategic authority by the late 19th century. It was Clausewitz's emphasis on morale, concentration of force, the decisive battle, and the complete overthrow of the enemy that were highlighted in the intellectual climate of that time. However, once disillusionment with total war had set in after the two world wars of the 20th century, and with the advent of nuclear weapons, interpretations completely reversed themselves. Strategic thinkers of the nuclear age now picked up the ideas found in the later stage of Clausewitz's work regarding limited war and the careful political direction of war. A 'Clausewitz renaissance' in academia and the armed forces throughout the West ensued. In the communist camp as well—following Vladimir Lenin's perusal of Clausewitz's work during World War I—commentators praised Clausewitz's understanding of the political context of war, while maintaining that his grasp of the social context did not go far enough and while also criticizing his nationalism" (Britannica).

The first three volumes of this set comprise Vom Kriege itself; the remaining seven contain accounts of various military campaigns: vol. 4, Der Feldzug von 1796 in Italien; vols. 5 & 6, Die Feldzüge von 1799 in Italien und der Schweiz; vol. 7, Der Feldzug von 1812 in Rußland, der Feldzug von 1813 bis zum Waffenstillstand und der Feldzug von 1814 in Frankreich; vol. 8, Der Feldzug von 1815 in Frankreich; vols. 9 & 10, Strategische Beleuchtung meherer Feldzüge von Gustav Adolph, Turenne, Luxemburg.

PMM 297.

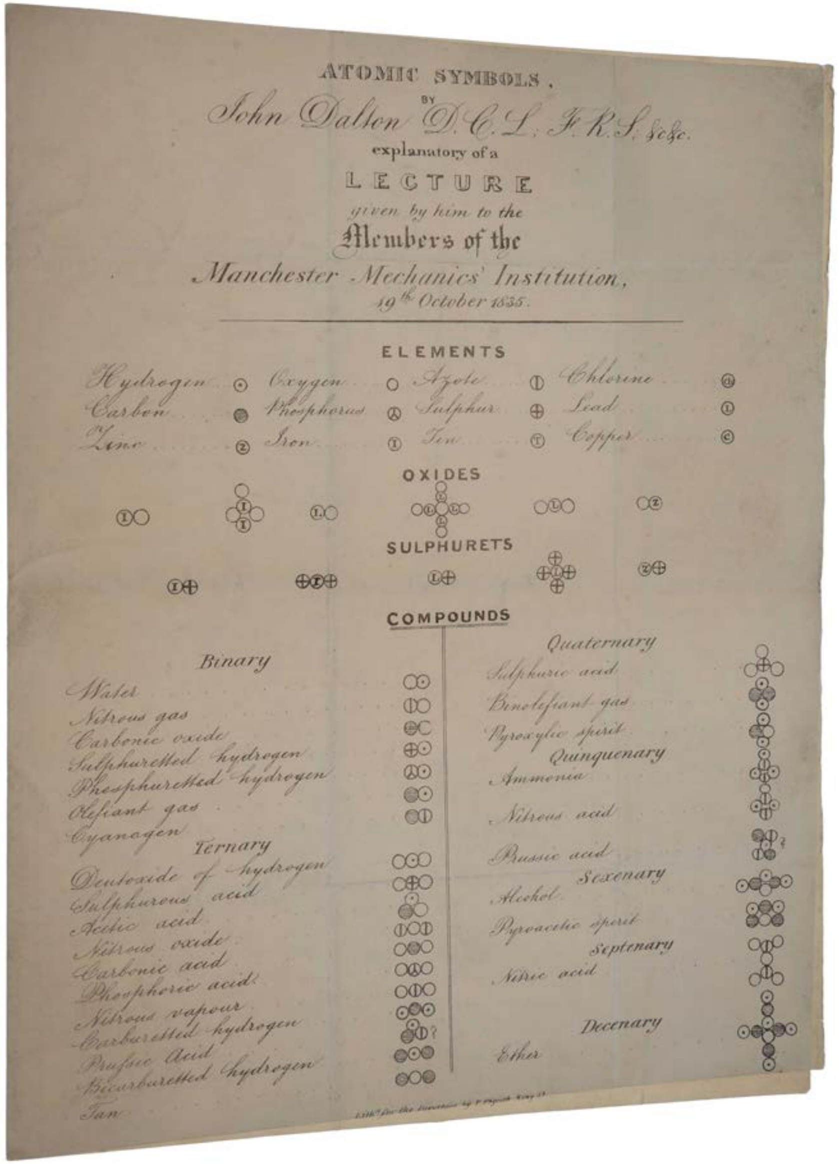
THE EXTREMELY RARE FIRST PRINTING OF HIS ATOMIC BROADSIDE

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].* [Manchester 1835].

$5,000

Single leaf (291 x 223mm) printed on recto only, three horizontal and two vertical creases where folded (small piece torn away from lower outer corner, not affecting printed area).

First edition, extremely rare, of this table of atomic symbols, for both elements and compounds, which was distributed to those attending Dalton's lecture on atomic theory delivered to the Manchester Mechanics' Institution on 19 October, 1835. This is a famous image, which was reprinted in W. C. Henry's *Memoirs of the Life and Scientific Researches of John Dalton* (1854), and is much reproduced even today, but original copies are of the greatest rarity. We know of no other separately printed tables of his atomic symbols. "Dalton made greater use of his symbols in lectures on the atomic theory than in his published works or even in his laboratory note-books … A famous lecture given by Dalton was in October 1835, when he lectured at the Manchester Mechanics' Institution. The subject was the atomic theory and the audience was issued with a lithographed sheet of atomic symbols. The sheet contains examples of compounds containing from two to ten atoms" (Crosland, pp. 261-2). "Dalton's chemical atomic theory was the first to give significance to the relative weights of the ultimate particles of all known



ATOMIC SYMBOLS,
BY
John Dalton D.C.L. F.R.S. &c.
explanatory of a
LECTURE
given by him to the
Members of the
Manchester Mechanics' Institution,
19th October 1835.

ELEMENTS
Hydrogen
Oxygen
Azote
Chlorine
Carbon
Phosphorus
Sulphur
Lead
Zinc
Iron
Tin
Copper

OXIDES

SULPHURETS

COMPOUNDS

Binary
Water
Nitrous gas
Carbonic oxide
Sulphuretted hydrogen
Phosphuretted hydrogen
Olefiant gas
Cyanogen

Ternary
Deutoxide of hydrogen
Sulphurous acid
Acetic acid
Nitrous oxide
Carbonic acid
Phosphoric acid
Nitrous vapour
Carburetted hydrogen
Prussic Acid
Bicarburetted hydrogen
Tan

Quaternary
Sulphuric acid
Binolefiant gas
Pyroxylic spirit

Quinquenary
Ammonia
Nitrous acid
Prussic acid

Sexenary
Alcohol
Pyroacetic spirit

Septenary
Nitric acid

Decenary
Ether

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. OCLC lists two copies (University of Delaware and Chemical Heritage Foundation). We know of only one example having appeared in commerce, tipped in to the Norman copy of Dalton's *New System of Chemical Philosophy*.

"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). Smyth, *John Dalton 1766-1844. A Bibliography of works by him and about him* (1966). 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*).

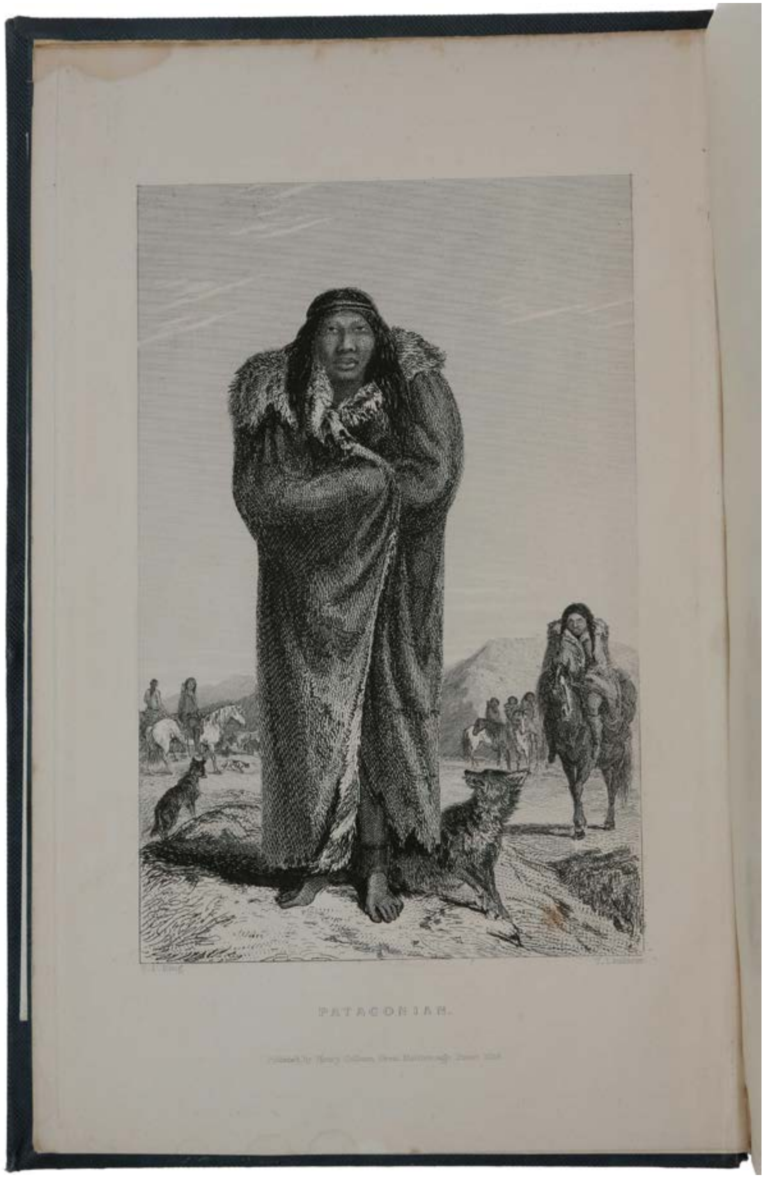
ONE OF THE MOST FAMOUS SCIENTIFIC EXPEDITIONS IN HISTORY

DARWIN, Charles and Robert FITZROY. *Narrative of the Surveying Voyages of His Majesty's Ships Adventure and Beagle, between the Years 1826 and 1836, describing their Examination of the Southern Shores of South America, and the Beagle's Circumnavigation of the Globe.* London: Henry Colburn, 839.

$85,000

Three vols. in four (vol. 2 having a separate Appendix), 8vo (235 x 145 mm), pp. xxviii [iv] 1-559, 556-597 [recte 601]; xiv [ii] 694 [2]; viii 352; xiv 629 [609]–615, with 8 engraved folding maps and charts (loosely inserted in pockets at the front of each volume, as issued, the ribbon for extracting the charts still present in each pocket), 48 plates and charts, and 6 text illustrations. This is an unusually fine copy, with virtually none of the usual fading and wear to the covers, and no cracking of the joints, only some very minor restoration to the top of the spine of vol. 3.

First edition, and a fine copy, almost entirely unopened. "The voyage of the *Beagle* has been by far the most important event in my life, and has determined my whole career" (Charles Darwin, *Life and Letters* I, p. 61). "The five years of the voyage were the most important event in Darwin's intellectual life and in the history of biological science. Darwin sailed with no formal scientific training. He returned a hard-headed man of science, knowing the importance of evidence, almost convinced that species had not always been as they were since the creation but had undergone change … The experiences of his five years in the *Beagle*, how he dealt with them, and what they led to, built up into a process of epoch-making importance in the history of thought" (DSB). The third volume comprises



Darwin's own journal of his voyage in the *Beagle*, which is the first issue of his first published book. It is "is undoubtedly the most often read and stands second only to *On the Origin of Species* as the most often printed" (Freeman, 31). It is "one of the most interesting records of natural history exploration ever written and is one of the most important, for it was on this voyage that Darwin prepared for his lifework, ultimately leading to *The Origin of Species*" (Hill I: 104-105). Volume I of the *Narrative* concerns the initial surveying expedition, 1826–30, under Philip Parker King in the *Adventure*, during which FitzRoy succeeded Pringle Stokes as commander of the accompanying *Beagle*. Volume II describes FitzRoy's continuation and completion of the survey with the *Beagle* alone, ending in 1836. 'The surveys he carried out in South American waters established FitzRoy as a first-rate hydrographer and won for him the gold medal of the Royal Geographical Society (1837). Because his marine surveys were accurate to such a high degree they are still used as the foundation for a number of charts of that area' (DSB).

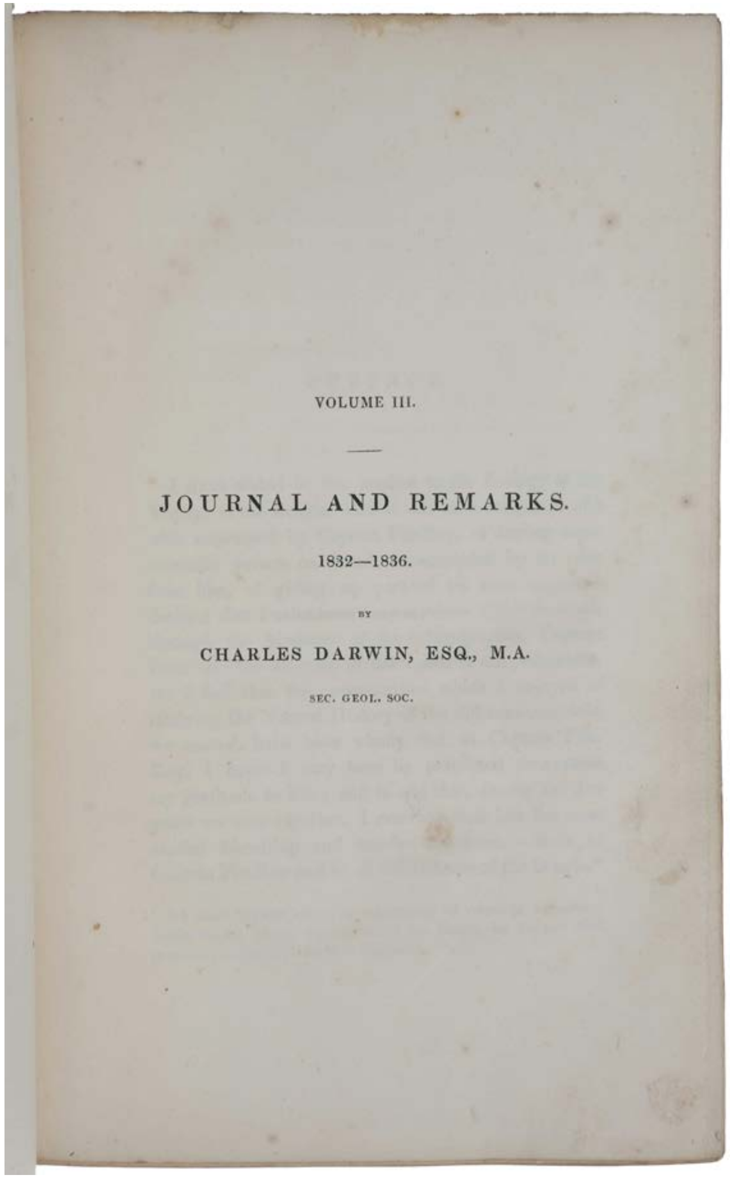
"If it had not been for Robert FitzRoy, the name Charles Darwin would now be remembered, if at all, as that of a country parson with an interest in natural history, perhaps rather in the mould of Gilbert White, of Selborne. The theory of natural selection, which explains the fact of evolution, would be known from the work of Alfred Russel Wallace, who came up with the idea independently of Darwin, and whose work prompted Darwin to go public with his own ideas; we would be as familiar then with the term 'Wallacian evolution' as we are, in the real world where Robert FitzRoy lived, with the term 'Darwinian evolution'. In that real world, FitzRoy is known, so far as he is widely known at all, as Darwin's Captain on the voyage of HMS Beagle during which the young naturalist made the observations which pro- vided the inspiration for the further years of hard work on which his theory would be based. But if Charles Darwin had never lived, the name of Robert FitzRoy might be widely held in higher esteem than it is in our world, where it has remained forever in the shadow of Darwin" (Gribbin, *FitzRoy*, p. 11).

This three-volume narrative, published in 1839, recounts the voyages of His Majesty's ships *Adventure* and *Beagle* between the years 1826 and 1836. Volume I chronicles the ships' adventures while surveying the southern coast of South America from 1826 to 1830. The captain of the *Beagle* during that time, Pringle Stokes (1793-1828), committed suicide, partly due to the stress of battling some of the worst weather on Earth. Phillip Parker King (1791-1856), Captain of the *Adventure*, used Stokes' journal in preparing his official report of the voyage, but glossed over the suicide. Stokes' replacement, Captain Robert FitzRoy, requested the company of a naturalist-scientist as a companion and intellectual peer before undertaking a second voyage:

"Anxious that no opportunity of collecting useful information, during the voyage, should be lost; I proposed to the Hydrographer [i.e., Francis Beaufort, Hydrographer to the British Admiralty] that some well-educated and scientific person should be sought for who would willingly share such accommodations as I had to offer, in order to profit by the opportunity of visiting distant countries yet little known. Captain Beaufort approved of the suggestion, and wrote to Professor Peacock, of Cambridge, who consulted with a friend, Professor Henslow [Darwin's former mentor and teacher], and he named Mr. Charles Darwin, grandson of Dr. Darwin the poet, as a young man of promising ability, extremely fond of geology, and indeed all branches of natural history. In consequence an offer was made to Mr. Darwin to be my guest on board, which he accepted conditionally; permission was obtained for his embarkation, and an order given by the Admiralty that he should be borne on the ship's books for provisions. The conditions asked by Mr. Darwin were, that he should be at liberty to leave the *Beagle* and retire from the Expedition when he thought proper, and that he should pay a fair share of the expenses of my table" (Vol. II, pp. 18-19).

Volume II is FitzRoy's account of the expedition's second voyage during the years 1831-1836, when the *Beagle* explored Tierra del Fuego at the southern tip of South America and the west coast of the continent. The ship then traveled to the Enchanted Islands, better known now as the Galapagos Islands. From there, the *Beagle* sailed to Tahiti, New Zealand, Australia, around the Cape of Good Hope, St. Helena, Ascension Island, Bahia, Cape Verde Islands and the Azores, and then home to England. FitzRoy not only captained the *Beagle,* but also served as the ship's hydrographic surveyor, meteorologist, and amateur naturalist. "FitzRoy, who was more concerned with science than were many naval officers of his day, made it possible for Darwin to visit tropical lands and study their flora, fauna, and geology. The two men shared the same cabin and FitzRoy was attentive to the scientific needs and interests of the young Darwin. FitzRoy's violent temper and his conservative opinions on religion and slavery were responsible for some disagreements between them, but FitzRoy and Darwin remained on friendly terms … While Darwin made his observations in South America and collected his specimens, FitzRoy surveyed the southern coast of that continent. The years of the second *Beagle* voyage marked the beginning of a half-century of supremacy of British hydrography" (DSB). Many years later Darwin reflected in his autobiography that FitzRoy's character "was in several respects one of the most noble which I have ever known."

"FitzRoy's duty in the years immediately following the return of the *Beagle* to England was clear. His first priority was to complete the preparation of the mass of charts, sailing directions, and other technical material resulting from the voyage, and he continued to receive his pay for carrying out this work even after the *Beagle* was paid off … work on this material continued long after the pay for it stopped, and that the last instalment was sent back to Beaufort by FitzRoy on his arrival in New Zealand in September 1844, having presumably been completed on the voyage out from England. But there was also considerable public interest in



the activities of the *Beagle*, and FitzRoy also felt it his duty to write up the material from both voyages into a book. With his predecessor, Pringle Stokes, dead, and with Captain King having retired to Australia, FitzRoy carried the responsibility (as he saw it) for all the material from the first voyage, as well as his own narrative of the second voyage. It soon became clear that with all this, and Darwin's material as well, there was far too much for one volume. We do not have the details of the discussions that must have taken place, but it is clear that Darwin and FitzRoy soon reached an amicable agreement (by January 1837) that FitzRoy would use the material he was responsible for to prepare two volumes, one covering the first voyage (*Beagle* and *Adventure*) and the other the second, while Darwin would write his own account of the second voyage for a third volume to be published with the other two as a single book.

"As the work progressed, FitzRoy found that the only way to cope with the mass of technical material and his own observations on various topics (such as the Fuegian language) that would otherwise break up the flow of the narrative was to relegate the material to a fourth volume of Appendices. Altogether he would produce well over half a million words, all written out by hand" (Gribbin, *FitzRoy*, pp. 175-6).

Publication of Darwin's journal, which contained a history of the voyage and a sketch of observations in natural history and geology, had been urged by FitzRoy during the voyage. It "was ready much earlier than the rest. The manuscript of the main text was finished by June 1837, and it, with the index, was in print early in 1838. The preface was written later and in it he states that 'publication has been unavoidably delayed' … The printing of the preliminaries and the appendix probably took place before January 24 1839. On that day he was elected a Fellow of the Royal Society, but the initials do not appear on the title page of Volume III. Immediately popular, it was reprinted separately later in the same year, and in numerous later editions with different titles, but is widely known today as *The Voyage of the Beagle*. It was one of Darwin's personal favourites, as he writes in his autobiography: "The success of this my first literary child tickles my vanity more than that of any of my other books."

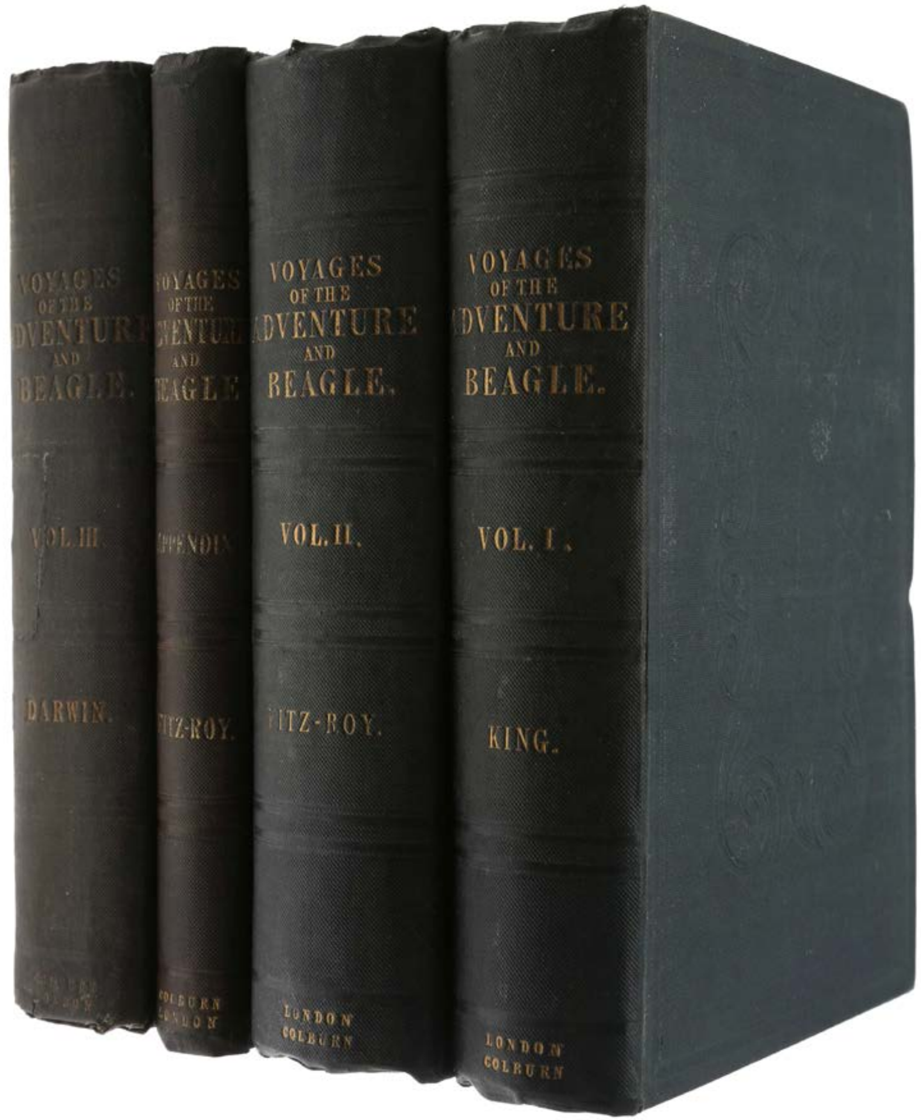
Robert FitzRoy (1805-1865) was appointed to the governorship of New Zealand in 1843, returning to England in 1845; in 1857 he became Rear Admiral and in 1863 Vice Admiral.

"Ever since his *Beagle* days FitzRoy had shown an interest in the study of the weather. Therefore, when the British government created (1855) the Meteorologic Office, instructed to gather weather information for shipping, it was not surprising that the Royal Society should ask FitzRoy be placed in charge of it … While a committee of the Royal Society deliberated about the exact nature of the work to be done by the Meteorologic Office, FitzRoy contacted the ship captains who would make meteorological observations for him. He was not satisfied merely to amass weather information; he wanted to warn sailors and others of approaching weather changes" (DSB).

"FitzRoy was interested in the weather for one reason – to save lives. He knew from direct experience the value of advance warning of storms at sea, and was determined to do something to help his fellow mariners. This was an outstanding example of his sense of duty, a noblesse oblige of the best kind which drove him to spend his own fortune in government service, leaving only debts for his wife and children, to do what he thought right for the common good at all times regardless of the effect on his own reputation, and to work long hours that far exceeded his formal obligations. In the words of one of his obituaries, 'a more high-principled officer, a more amiable man, or a person of more useful general attainments never walked a quarter-deck'.

"It is a sign of FitzRoy's strength of character that even after the setback in New Zealand, back in England he developed the fundamental techniques of weather forecasting, designed a standard barometer and thermometer (a prototype weather station), invented the system of storm warnings and signals which saved countless lives in the ensuing decades, and issued the first daily weather forecasts, published in *The Times* – indeed, he invented the term 'weather forecast' (Gribbin, *FitzRoy*, pp. 10-11).

Freeman 10; Freeman *Companion* p. 213; Norman 584.



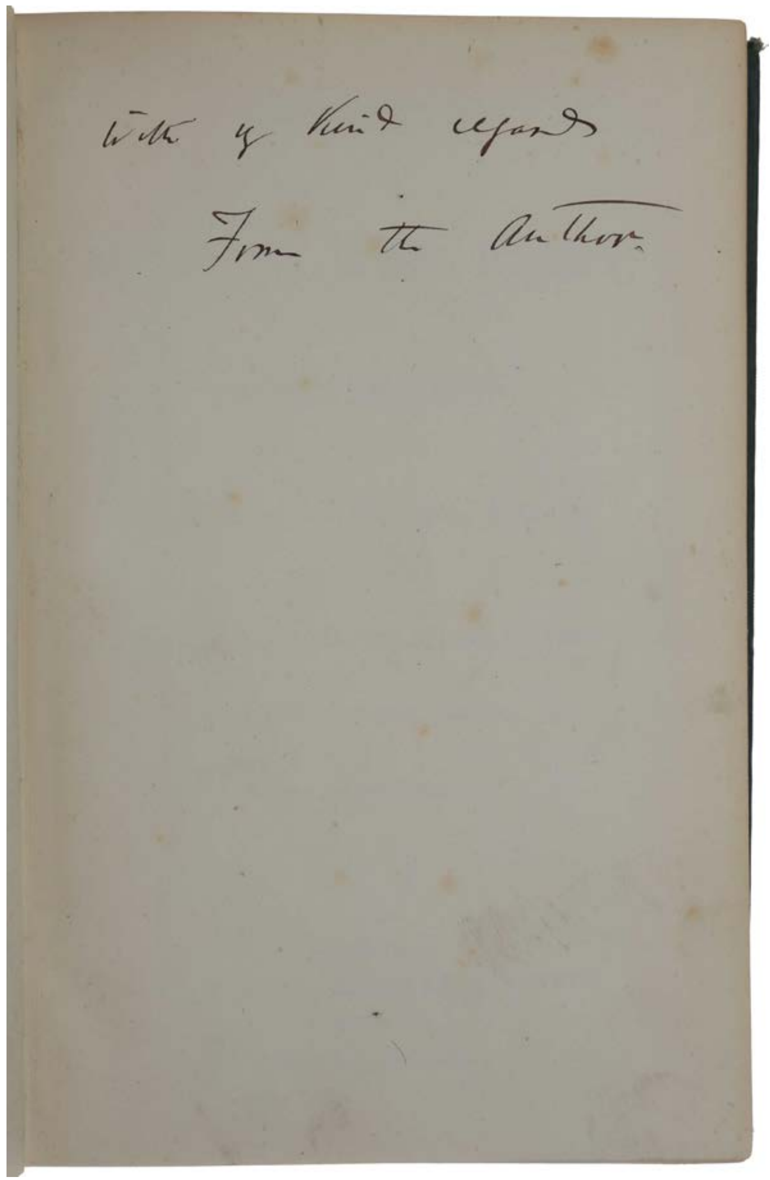
INSCRIBED BY DARWIN

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.

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.

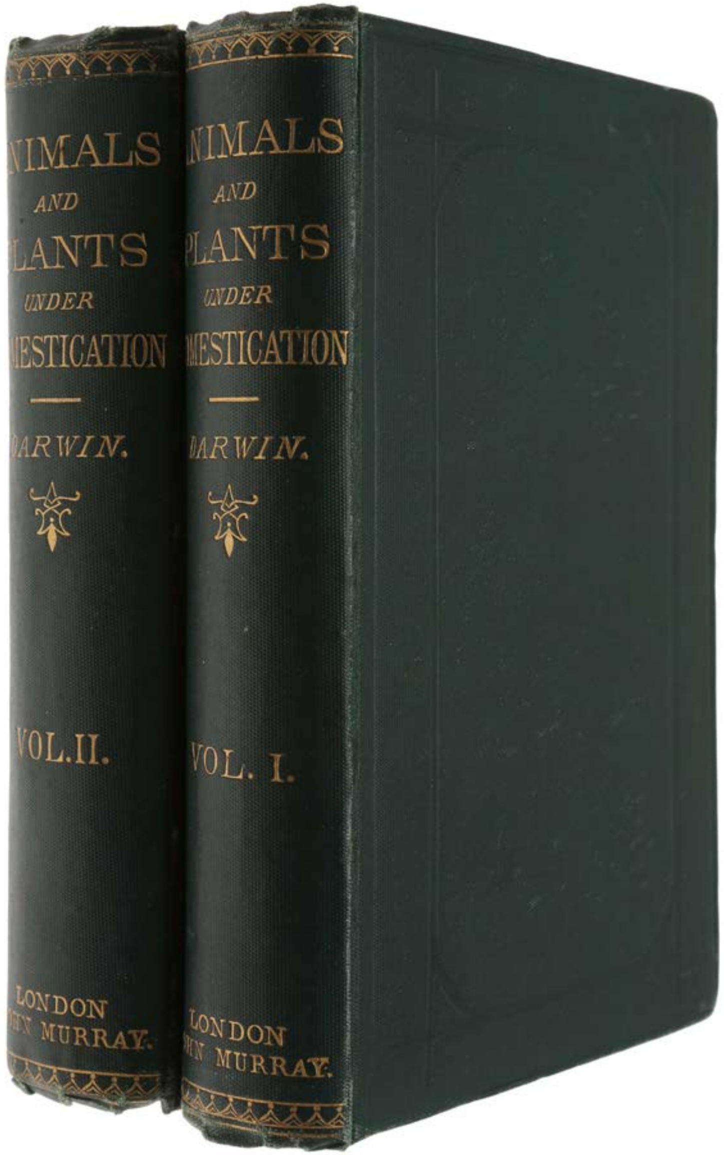
On the Origin of Species was only an abstract of the long manuscript Darwin had begun writing on 14 May 1856 which he originally intended to complete and publish as the formal presentation of his views on evolution. Compared with the *Origin,* this work, which was to be titled *Natural Selection*, has 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.

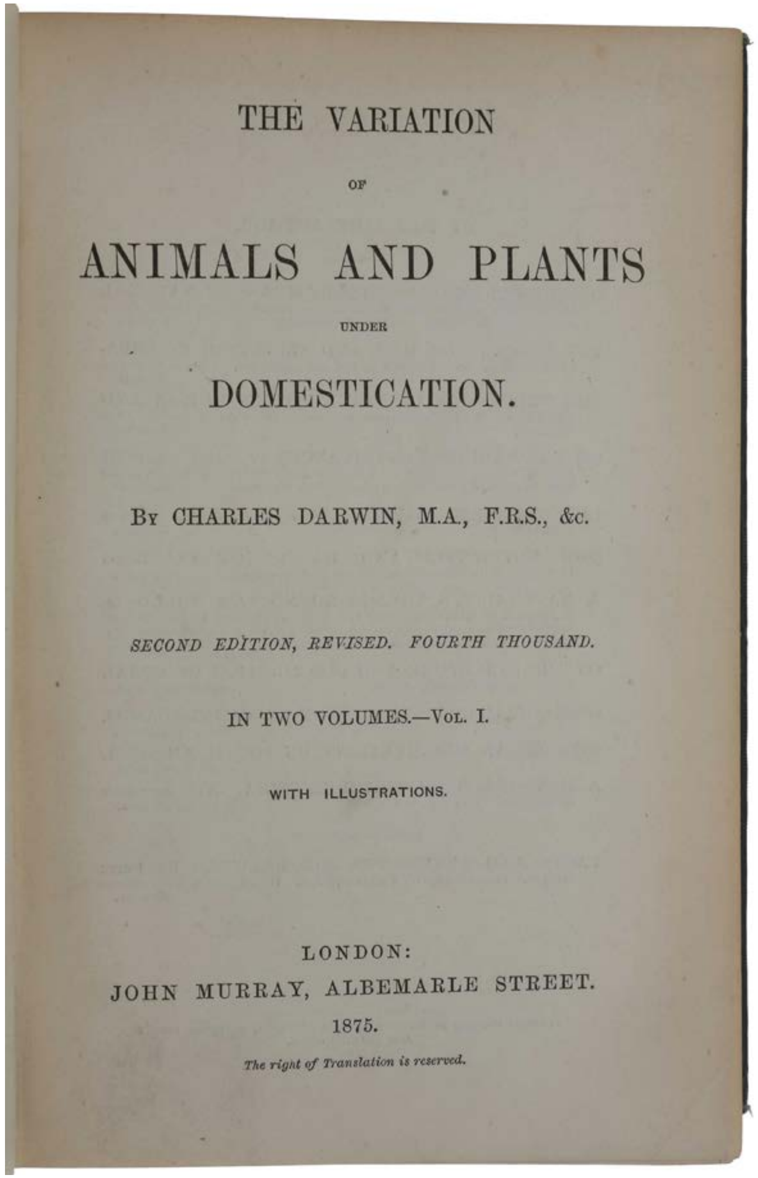
"Notably, in Chapter XXVII Darwin introduced his 'provisional hypothesis' of pangenesis that he had first outlined to Huxley in 1865.He proposed that each part of an organism throws off minute invisible particles which he called gemmules. These were capable of generating a similar part of an organism, 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 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



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.

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).

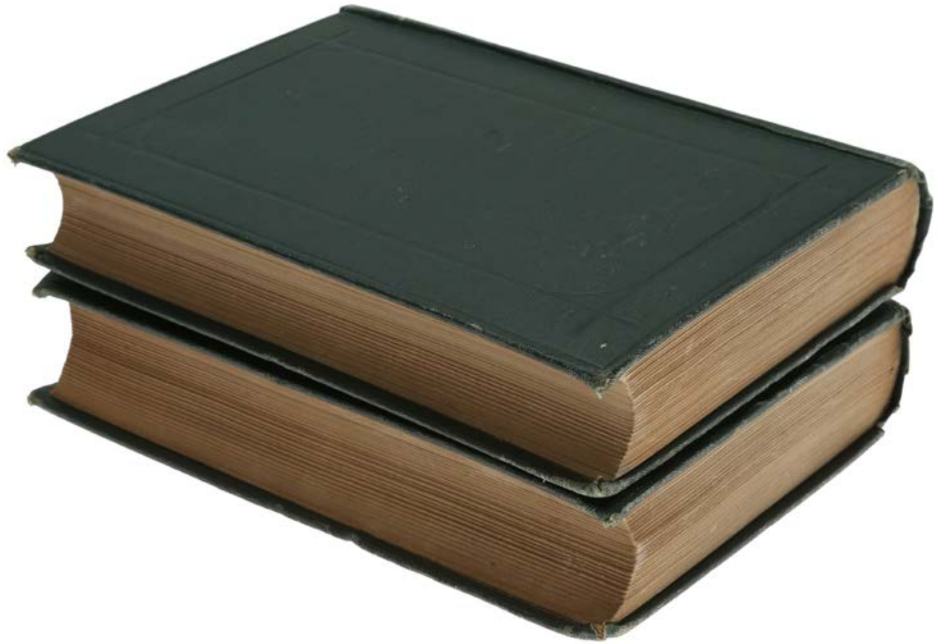
The corrections in this copy have been identified as being in the hand of Charles Darwin's son Francis (1848-1925), who in 1874 began acting as his amanuensis. Charles was ageing and had been unwell for many years, and he now reluctantly accepted his need for a secretary, principally to respond to the 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).

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.

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 ARITHMETIZATION OF ANALYSIS

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

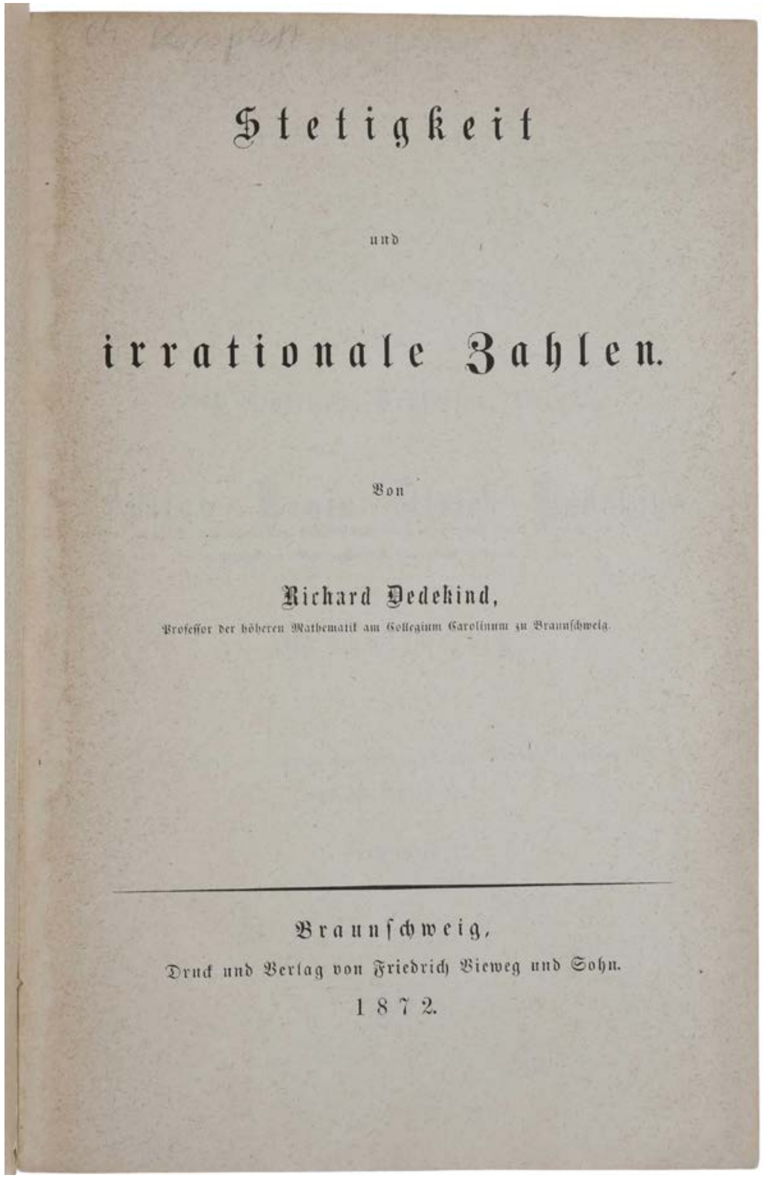
DEDEKIND, Richard. *Stetigkeit und irrationale Zahlen.* Braunschweig: Friedrich Vieweg, 1872.

$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 [*Stetigkeit und irrationale Zahlen*] 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



Stetigkeit

und

irrationale Zahlen.

Von

Richard Dedekind,

Professor der höheren Mathematik am Collegium Carolinum zu Braunschweig.

Braunschweig,

Druck und Verlag von Friedrich Vieweg und Sohn.

1872.

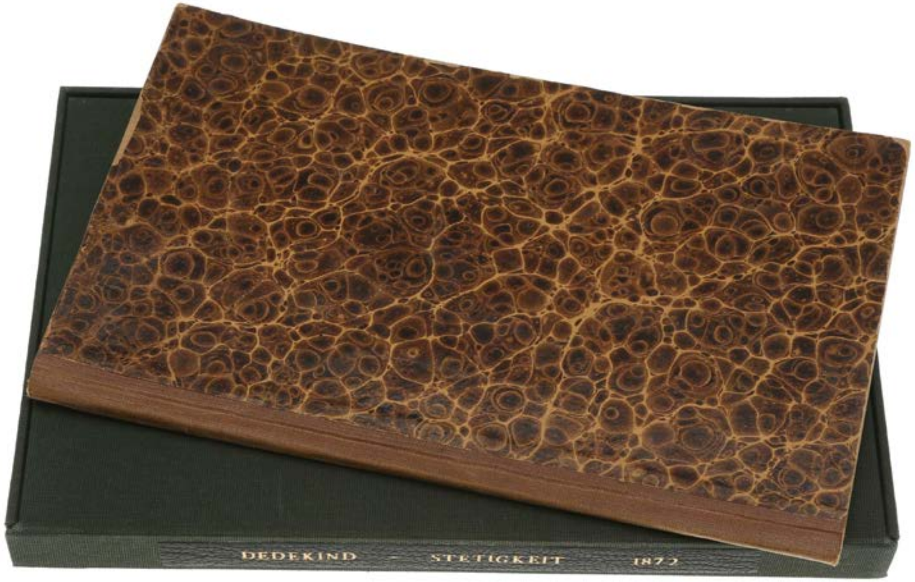
of his considerations, *Stetigkeit und irrationale Zahlen* (Brunswick, 1872, and later editions), and explained the real numbers as "cuts" in the realm of rational numbers. He arrived at concepts of outstanding significance for the analysis of number through the theory of order. The property of the real 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).

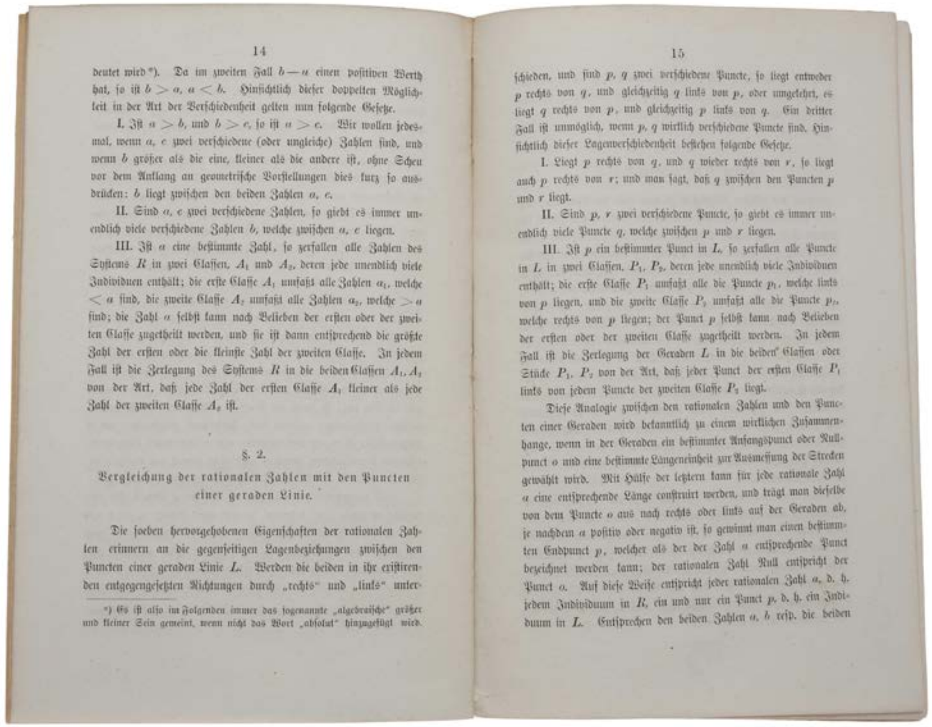
"Occasionally Dedekind has been called a "modern Eudoxus" because an impressive similarity has been pointed out between Dedekind's theory of the irrational number and the definition of proportionality in Eudoxus' theory of proportions (Euclid, *Elements*, bk. V, def. 5). Nevertheless, Oskar Becker correctly 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.





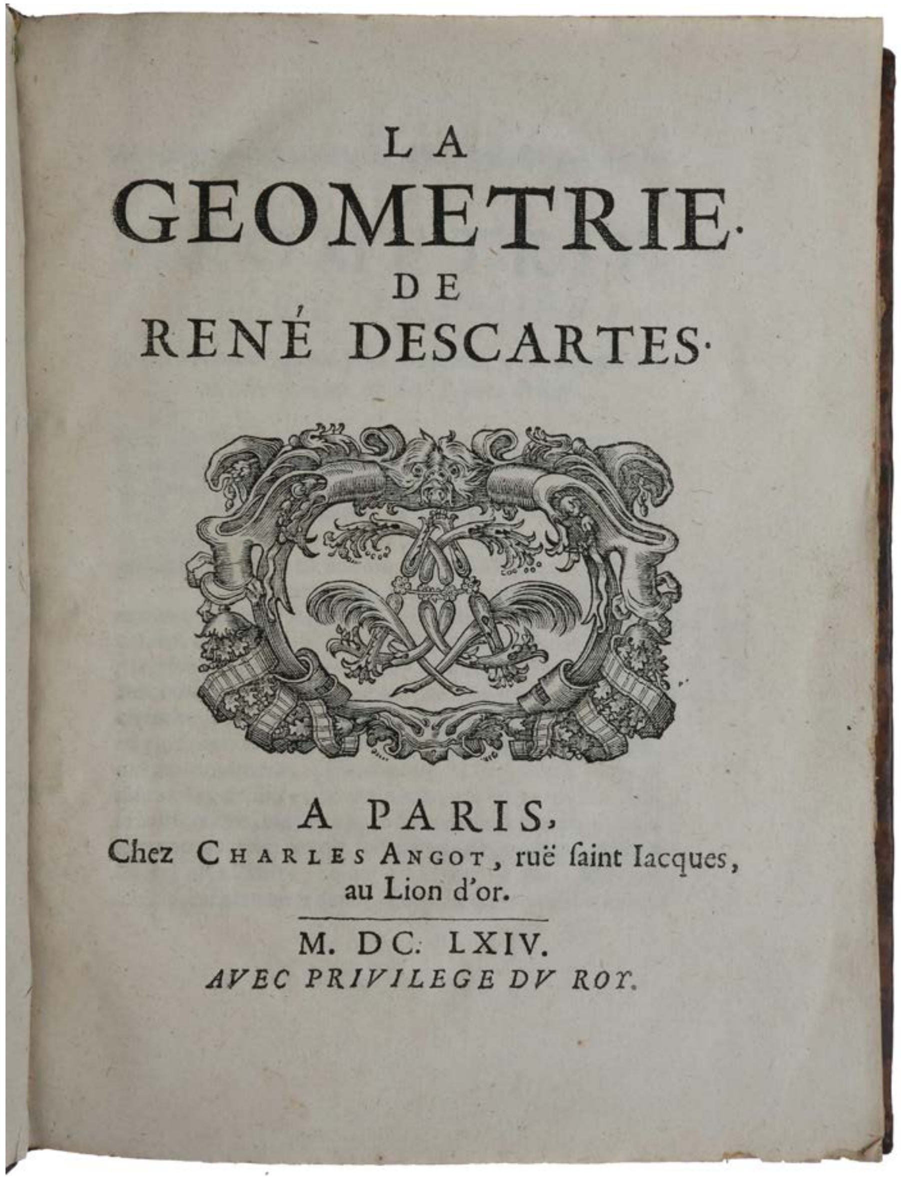
ONE OF THE KEY TEXTS IN THE HISTORY OF MATHEMATICS

DESCARTES, René. *La Géométrie.* Paris: Charles Angot, 1664.

$8,500

4to (210 x 162 mm), pp. 119, [8], [1:blank]. Contemporary calf, spine richly gilt in compartments, lettered directly in gilt in the second compartment (some minor old restoration).

First separate edition in the original French of Descartes's *magnum opus* (DSB), one of the key texts in the history of mathematics. "Inspired by a specific and novel view of the world, Descartes produced his *Géométrie*, a work as exceptional in its contents (analytic geometry) as in its form (symbolic notation), which slowly but surely upset the ancient conceptions of his contemporaries. In the other direction, this treatise is the first in history to be directly accessible to modern-day mathematicians. A cornerstone of our 'modern' mathematical era, the *Géométrie* thus paved the way for Newton and Leibniz … In his *Géométrie*, Descartes organized a mathematical revolution by establishing, in a polished, effective and clear manner, a relation between curves and algebraic calculation, both the continuum of geometry and the discontinuity of number. Today, the use of coordinates in visualizing a curve by means of its equation is an almost automatic process" (*Landmark Writings*, pp. 1 & 18). Descartes' "application of modern algebraic arithmetic to ancient geometry created the analytical geometry which was the basis of the post-Euclidean development of that science" (PMM). "Analytical geometry rendered possible the later achievements of seventeenth-century mathematical physics; without this method the application of mathematics



to science would have been stultified. The problems of physics – especially mechanics – presented themselves in spatial terms, that is, geometrically; by Descartes' discovery they could be subjected to the flexible and solvent attack of algebra" (Hall, *From Galileo to Newton* (1963), pp. 92-3). *La Géométrie* was originally published as the third part of the *Discours de la Méthode* (1637). ABPC/RBH list six copies in the past half-century.

Descartes' (1596-1650) interest in geometry was stimulated when, in 1631, Jacob Golius (1596–1667), a professor of mathematics and oriental languages at Leyden, sent Descartes a geometrical problem, that of 'Pappus on three or four lines'. It had originally been posed and solved shortly before the time of Euclid in a work called *Five books concerning solid loci* by Aristaeus, and was then studied by Apollonius and later by Pappus. But the solution was lost in the 17th century, and the problem became an important test case for Descartes. Claude Hardy, a contemporary at the time of its solution, later reported to Leibniz the difficulties that Descartes had met in solving it (it took him six weeks), which 'disabused him of the small opinion he had held of the analysis of the ancients'. The Pappus problem is a thread running through the entire work.

Book One is entitled 'Problems the construction of which requires only straight lines and circles,' and it is in this opening book that Descartes details his geometrical analysis, that is, how geometrical problems are to be formulated algebraically. It begins with the geometrical interpretation of algebraic operations, which Descartes had already explored in the early period of his mathematical research. However, what we are presented in 1637 is a 'gigantic innovation' (Guicciardini, p. 38) both over Descartes' previous work and the work of his contemporaries. On the one hand, Descartes offers a geometrical interpretation of root extraction and thus treats five arithmetical operations. Crucially, he also uses a new exponential notation (e.g. x^3), which replaces the traditional 'cossic' notation of early modern algebra, and allows Descartes to tighten the connection between algebra and geometry.

Descartes proceeds to describe how one is to give an algebraic interpretation of a geometrical problem:

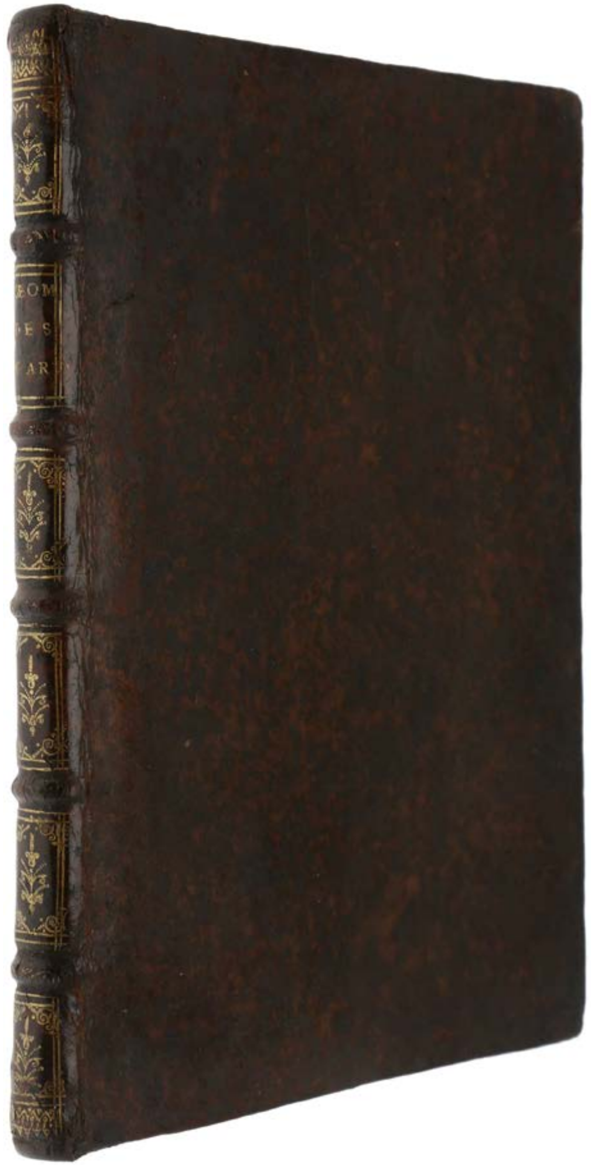
'If, then, we wish to solve any problem, we first suppose the solution already effected, and give names to all the lines that seem needful for its construction, to those that are unknown as well as to those that are known. Then, making no distinction between unknown and unknown lines, we must unravel the difficulty in any way that shows most naturally the relations between these lines, until we find it possible to express a single quantity in two ways. This will constitute an equation, since the terms of one of these two expressions are together equal to the terms of the other.'

Descartes applies his geometrical analysis to solve the four-line case of the Pappus problem, and shows how the analysis can be generalized to apply to the general, *n*-line version of the problem, which had not been solved by the ancients.

Book Two, entitled 'On the Nature of Curved Lines,' commences with Descartes' famous distinction between 'geometric' and 'mechanical' curves. For Pappus, 'plane' curves were those constructible by ruler and compass, 'solid' curves were the conic sections, and 'linear' curves were the rest, such as the conchoids, the spiral, the quadratrix and the cissoid. The linear curves were also called 'mechanical' by the ancient Greeks because instruments were needed to construct them. Following Descartes, the supremacy of algebraic criteria became established: curves were defined by equations with integer degrees. Algebra thus brought to geometry the most natural hierarchies and principles of classification. This was extended by Newton to fractional and irrational exponents, and by Leibniz to 'variable' exponents (*gradus indefinitus*, or transcendental in modern terminology).

Book Three, entitled 'The construction of solid, and higher than solid problems,' is devoted to the theory of equations and the geometrical construction of their roots. "The abundance and variety of results in this section is remarkable. A number of the interesting results presented are not altogether new, some being due to Girolamo Cardano, Thomas Harriot and Albert Girard. The exposition is, however, clear and systematic, and expressed for the first time in history in modern notation. These results were taken up and extended by Newton in *Arithmetica universalis* (1707), in lectures between 1673 and 1683. Descartes is also interested in the number of real roots, and asserts without justification that the *maximum* number of positive or negative roots of an equation is that of the alternances or permanences of the signs '+' and '–' between consecutive coefficients. This is the celebrated 'rule of signs', which earned unfounded criticism for Descartes. Newton took up and extended the matter in the *De limitibus aequationum*, which concludes the *Arithmetica universalis*. The result was proved in the 18th century. Book Three concludes with a discussion of the geometrical construction of roots of equations by means of intersecting curves, particularly cubic and quartic equations which Descartes treats using a circle and a parabola.

"The ease that Descartes displays in handling symbolic notation and the familiarity that we have with it today must not obscure its profound novelty at the time, or the 'shock' of that supposedly 'geometric' work, but with every page covered by calculations, letters and new symbols, provoked among his contemporaries. The use of the exponent, and of a specific sign for equality, just like the systematic literalization of Vieta, were decisive in the advent of the new symbolism. The statement in modern terms of Cardano's formula in one line in Book III proves to the reader the clear superiority of this symbolism over the laborious rhetoric of Cardano. Contemporary mathematicians were not mistaken in using the



Géométrie as a 'Rosetta stone' for deciphering symbolism.

"Conversely, it is the first text in history to be directly accessible to mathematicians of today. By the systematic use of substitutions (his *Art combinatoire*), Leibniz continued the implementation of what was not just a 'change of notation' but a radical modification of modes of mathematical thought. Fractional and literal exponents were added by Newton.

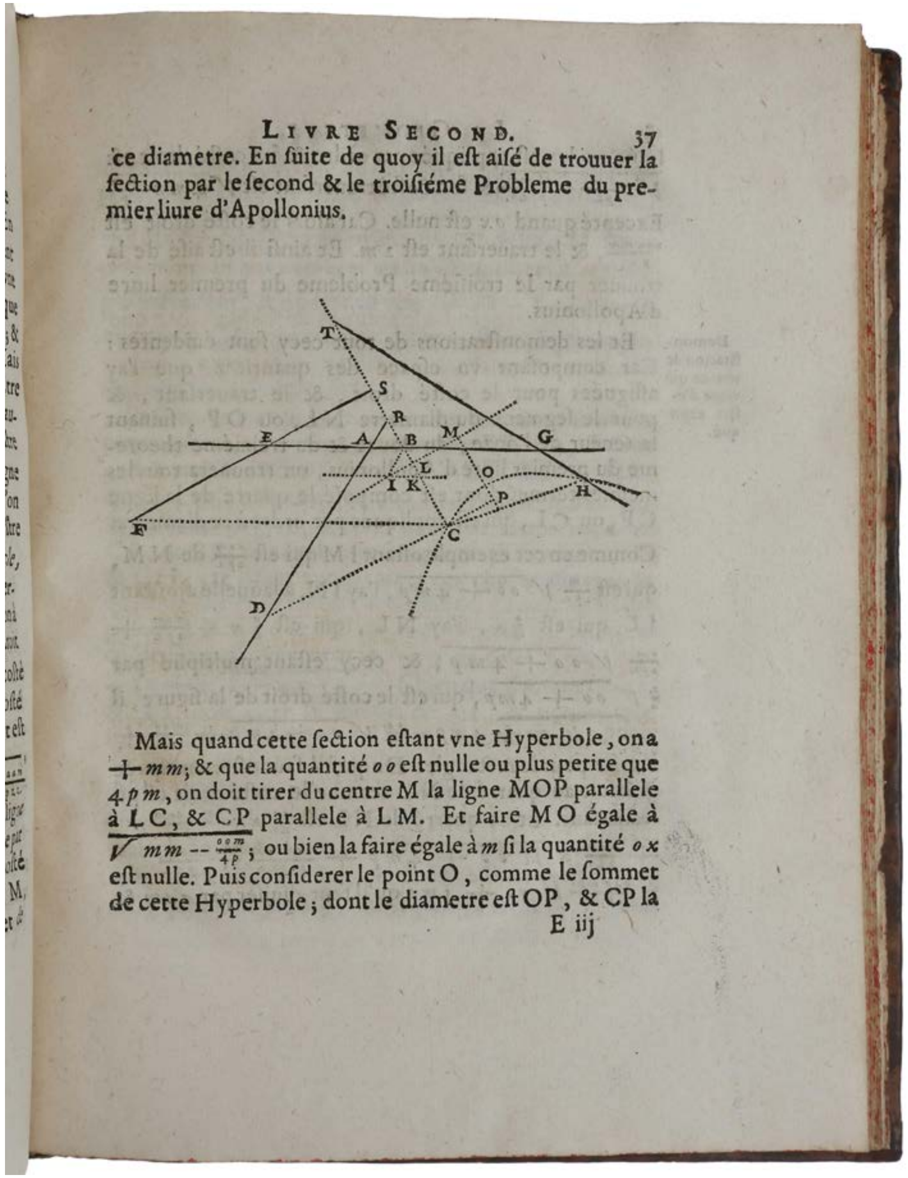
"In 1630, Descartes declared to Mersenne that he was 'tired of mathematics', and he meant it. Leaving for Holland, he thought of the mathematical model of his youth as over and turned to metaphysics. In 1637, however, when faced with the need to find applications of his *Discours*, he returned briefly to mathematics. But while he was proud of the results found, he never intended to continue in mathematics. So incontestably the *Géométrie* represents a culmination in his work and not as an avenue opening towards the future. After 1637, he devoted himself exclusively to philosophy, while occasionally studying with his correspondents certain mathematical problems. Some of these lay in areas previously rejected, such as the question of the divisors of an integer (including, strangely enough, the integers equal to twice the sum of their proper divisors), and the study of non-geometric curves like the 'roulette', or cycloid, or again an 'inverse-tangent' problem posed by de Beaune, the first in the history of differential geometry, of which the solutions are transcendental curves. Descartes's correspondence with Mersenne after 1637 shows that he was conscious of having produced an exceptional mathematical work that few of his contemporaries seemed able to understand. Admittedly, he had simply sought to introduce, through his analytical geometry, a method into geometry, the algebra being merely a tool; but in fact he had achieved more, and with his customary pride, he gave a good account of himself. Henceforth, he said to Mersenne at the end of 1638, he had no need to go any further in mathematics" (*Landmark Writings*, p. 20).

"Descartes was aware of the originality of the project: 'Finally, in the *Géométrie* I try to give a general method for solving all the problems that have never been solved'. He was aware of the difficulty of the work, and asked the reader to 'follow all the calculations, which may seem difficult at first, with pen in hand', whereupon he will get used to them 'after a few days'. He also advised passing 'from the first book to the third before reading the second'" (*Landmark Writings*, p. 4).

La Géométrie was completed just before the publication of the *Discours* on 8 June, 1637. It was not included in the second (1658) or third (1668) editions of the *Discours*, nor in its Latin translation *Specimina philosophiae* (1644, 1650, 1656, 1664). A Latin translation was published in 1649 with the commentary of Frans van Schooten (1615-60).

The offered work is the first separate printing of the original text of *La Géométrie*, except for an offprint from the *Discours* of which six copies were printed; the only known survivor is held by the Bodleian Library (Savile V.2). The offprint retains the pagination in the *Discours* but the first page 297 is preceded in the offprint by a leaf not present in the *Discours* with 'La Géométrie' printed on the recto and on the verso the following four lines: 'On n'a imprimé que six exemplaires de cette géométrie en cette form; Et ils sont pour les six premiers qui auront fait connoistre à l'autheur qu'ils l'entendent. C'est pouquoy cetui-cy appartient à:' In the Bodleian copy this is followed by the words 'Monsieur Chauveau' in manuscript (probably the French painter and engraver François Chauveau (1613-76)). Guibert (p. 27) believed that this inscription is in Descartes' hand. A further difference is that the last leaf Fff4 of the offprint is blank, whereas in the *Discours* this leaf comprises the first two pages of the *Tables*. Finally, the offprint is printed on large paper; no large paper copies of the *Discours* are known. The fate of the other five copies of the offprint, and the names of their recipients, are unknown.

Guibert, p. 31, no. 4 ('fort rare'); Honeyman 860; Macclesfield 622 (contemporary calf, \$5806); Norman 629 (contemporary calf, spine defective, \$4560). Cajori, *A History of Mathematics,* p. 174 ("Of epoch-making importance"); DSB IV, pp. 55-58; I. Grattan-Guinness (ed.), *Landmark Writings in Western Mathematics 1640-1940* (2005), Ch. 1; Guibert 27; N. Guicciardini, *Isaac Newton on Mathematical Certainty and Method* (2009); PMM 129 (for the 1637 edition); Roller & Goodman I p. 314.



LIVRE SECOND. 37

ce diametre. En ſuite de quoy il eſt aiſé de trouuer la ſection par le ſecond & le troiſiéme Probleme du premier liure d'Apollonius.

Mais quand cette ſection eſtant vne Hyperbole, on a $+ mm$; & que la quantité oo eſt nulle ou plus petite que $4pm$, on doit tirer du centre M la ligne MOP parallele à LC, & CP parallele à LM. Et faire MO égale à $\sqrt{mm - \frac{oom}{4p}}$; ou bien la faire égale à m ſi la quantité ox eſt nulle. Puis conſiderer le point O, comme le ſommet de cette Hyperbole; dont le diametre eſt OP, & CP la

E iij

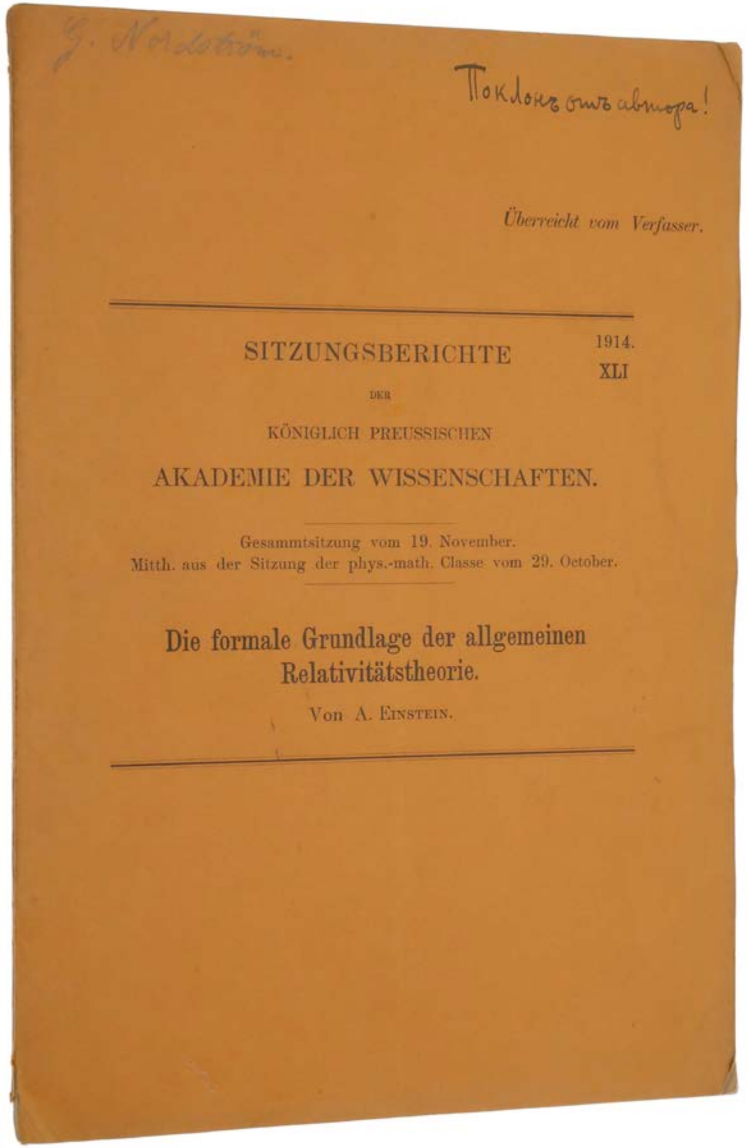
PRESENTATION OFFPRINT, THE COPY OF GUNNAR NORDSTRÖM

EINSTEIN, Albert. *Die formale Grundlage der allgemeinen Relativitätstheorie. Author's presentation offprint from Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften, XLI, 19 November 1914.* Berlin: Königlichen Akademie der Wissenschaften, 1914.

$5,000

8vo (254 x 176 mm), pp. 1030-1085. Original printed wrappers, light wear to upper and lower part of spine, very light vertical crease from having been folded (for post?), small ink stain to rear wrapper, outer margin of all text leaves and wrapper have been unevenly cut - this might have been done by an early owner as a way of opening all the text leaves at once, instead of having to cut open each of the closed pages one at a time.

First edition of this extremely rare offprint, a remarkable association copy inscribed by the theoretical physicist Gunnar Nordström, often designated by modern writers as 'The Einstein of Finland'. Einstein had an extended correspondence with Nordström on the subject of Nordström's own competing theory of gravitation, which at the time was considered a serious competitor to Einstein's, and which he completed in the same year as the present paper. A few years later Nordström also assisted Einstein in his work on gravitational waves. The present paper was the crucial step between Einstein's *Entwurf* theory of 1913 and the final form of general relativity which Einstein completed in November 1915: it develops the mathematical techniques necessary for the final formulation, namely the 'absolute differential calculus' of Tullio Levi-Civita, as well as the expression of the field



equations in terms of a variational principle, which later proved to be of great importance. This author's presentation offprint, with "Überreicht vom Verfasser" printed on upper wrapper, must not to be confused with the much more common trade offprint which lacks this printed statement (see below). We have located only one copy of this author's presentation offprint at auction, in the collection belonging to Einstein's son Hans Albert sold at Christie's in 2006 (there was no copy in Einstein's own collection of his offprints sold by Christie's in 2008).
Provenance: Gunnar Nordström (1881-1923) ('G. Nordström' written in pencil on upper wrapper in Nordström's hand). Mathematical annotations in pencil to margin of p. 1077 (in Nordström's hand?). Later inscription in Russian on upper wrapper.

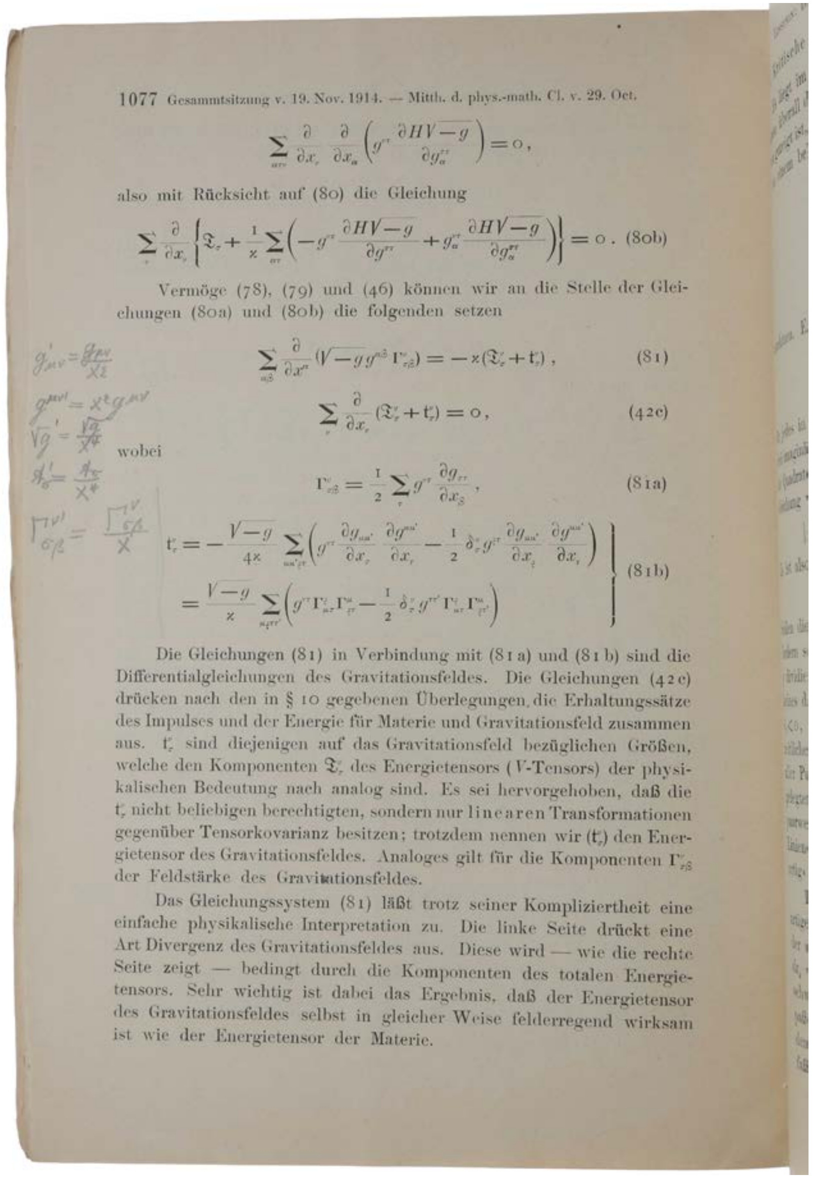
"In summer 1914, Einstein felt that the new theory should be presented in a comprehensive review. He also felt that a mathematical derivation of the field equations that would determine them uniquely was still missing. Both tasks are addressed in a long paper, presented in October 1914 to the Prussian Academy for publication in its *Sitzungsberichte*. It is entitled 'The formal foundation of the general theory of relativity'; here, for the first time, Einstein gave the new theory of relativity the epithet 'general' in lieu of the more cautious 'generalized' that he had used for the *Entwurf*' (*Landmark Writings in Western Mathematics 1640-1940*). "In the year that he was called to Berlin, on October 29, 1914, Einstein was able to present his work "Die formale Grundlage der allgemeinen Relativitätstheorie" … The "formal foundation" of the general theory of relativity was the tensor calculus. Without the tensor calculus, the general theory of relativity could not have been formulated … By October 1914, Einstein was finally able to present his results in mathematical form, and indeed in a manner that became the basis of his general theory of relativity of 1916. He introduced general covariants, contravariants, and also—what was new—mixed tensors, in order to represent the individual arithmetic operations, above all, the various types of multiplication. Thus the mathematical calculus necessary for the general theory of relativity was at the ready in 1914" (Reich). "The principal novelty [in the present paper] lies in the mathematical formulation of the theory. Drawing on earlier work with [Marcel] Grossman, Einstein formulated his gravitational field equations using a variation principle" (Calaprice, 47).

The first important stage in the development of Einstein's theory of gravitation was accomplished, with his friend and classmate the mathematician Marcel Grossmann, in their 1913 work *Entwurf einer verallgemeinerten Relativitätstheorie und einer Theorie der Gravitation*. "In this book, Einstein and Grossman investigated curved space and curved time as they relate to a theory of gravity. They presented virtually all the elements of the general theory of relativity with the exception of one striking omission: gravitational field equations that were not generally covariant. Einstein soon reconciled himself to this lack of general covariance through the 'hole argument,' which sought to establish that generally covariant gravitational field equations would be physically uninteresting" (Calaprice 40). Einstein's 'hole argument', he believed, implied that general covariance was incompatible with the requirement that the distribution of mass-energy should determine the gravitational field *uniquely*. He believed, therefore, that the field equations should only be valid in certain coordinate systems, which he called 'adapted', and that only coordinate transformations from one adapted system to another adapted system should be allowed – he called these 'justified coordinate transformations'.

"Einstein's move to Berlin in April 1914 marked the end of his collaboration with Grossmann. Fortunately, by this time Einstein no longer seems to have needed Grossmann's mathematical guidance. By October 1914, he had completed a lengthy summary article [offered here] on his new theory, whose form and detailed nature suggest that Einstein felt his theory had reached its final form. The article contained a review of the methods of tensor calculus used in the theory and, flexing his newfound mathematical muscles, Einstein could even promise to give new and simpler derivations of the basic laws of the 'absolute differential calculus'. Of great importance was the fact that Einstein had taken the new mathematical techniques of his last paper with Grossmann, generalized them and found in them a quite new derivation of the field equations" (Norton, p. 293).

This new derivation made use, for the first time in Einstein's work on the theory of gravity, of an action principle (or variational principle). Einstein worked initially with an action that was an arbitrary function of the metric tensor and its first derivatives, and then showed that with a particular choice of the action he could recover the *Entwurf* field equations. He further believed that he had found a simple general covariance condition which forced the action to take the *Entwurf* form. "Einstein had good reason to be pleased with this result. For it seemed to show that his theory was not just a theory of gravitation, but a generalized theory of relativity, in so far as it was concerned with establishing the widest covariance possible in its equations. His original derivation of the field equations [in *Entwurf*] had been based squarely on considerations in gravitation theory ... The new derivation, however, focused on covariance considerations. He had found a simple way of formulating field equations that would have exactly the maximum covariance allowed by the 'hole argument', and they led him almost directly to his original *Entwurf* field equations. As a result he could promise to "recover the equations of the gravitational field in a purely-covariant-theoretical way" and claim to "have arrived at quite definite field equations in a purely formal way, i.e., without directly drawing on our physical knowledge of gravitation" …

"Einstein appears to have remained satisfied with the theory he developed in 1914 through the first half of 1915. In March, April and early May he defended the theory



1077 Gesammtsitzung v. 19. Nov. 1914. — Mitth. d. phys.-math. Cl. v. 29. Oct.

$$\sum_{\alpha\tau\nu} \frac{\partial}{\partial x_\nu} \frac{\partial}{\partial x_\alpha}\left(g^{\tau\nu} \frac{\partial H\sqrt{-g}}{\partial g^{\tau\nu}_\alpha}\right) = 0,$$

also mit Rücksicht auf (80) die Gleichung

$$\sum_\nu \frac{\partial}{\partial x_\nu}\left\{\mathfrak{T}_\nu + \frac{1}{\varkappa}\sum_{\alpha\tau}\left(-g^{\nu\tau}\frac{\partial H\sqrt{-g}}{\partial g^{\sigma\tau}} + g^{\nu\tau}_\alpha \frac{\partial H\sqrt{-g}}{\partial g^{\sigma\tau}_\alpha}\right)\right\} = 0. \quad (80b)$$

Vermöge (78), (79) und (46) können wir an die Stelle der Gleichungen (80a) und (80b) die folgenden setzen

$g'_{\mu\nu} = \frac{g_{\mu\nu}}{X^2}$

$$\sum_{\alpha\beta} \frac{\partial}{\partial x^\alpha}\left(\sqrt{-g}\, g^{\alpha\beta}\, \Gamma^\nu_{\sigma\beta}\right) = -\varkappa(\mathfrak{T}^\nu_\sigma + \mathfrak{t}^\nu_\sigma), \quad (81)$$

$g^{\mu\nu'} = X^2 g^{\mu\nu}$

$$\sum_\nu \frac{\partial}{\partial x_\nu}(\mathfrak{T}^\nu_\sigma + \mathfrak{t}^\nu_\sigma) = 0, \quad (42c)$$

$\sqrt{g'} = \frac{\sqrt{g}}{X^4}$

wobei

$\mathfrak{T}'_\sigma = \frac{\mathfrak{T}_\sigma}{X^4}$

$$\Gamma^\nu_{\sigma\beta} = \frac{1}{2}\sum_\tau g^{\nu\tau}\frac{\partial g_{\sigma\tau}}{\partial x_\beta}, \quad (81a)$$

$\Gamma^{\nu\prime}_{\sigma\beta} = \frac{\Gamma^\nu_{\sigma\beta}}{X}$

$$\left.\begin{aligned} \mathfrak{t}^\nu_\sigma &= -\frac{\sqrt{-g}}{4\varkappa}\sum_{\mu\mu'\rho\tau}\left(g^{\nu\tau}\frac{\partial g_{\mu\mu'}}{\partial x_\sigma}\frac{\partial g^{\mu\mu'}}{\partial x_\tau} - \frac{1}{2}\delta^\nu_\sigma g^{\rho\tau}\frac{\partial g_{\mu\mu'}}{\partial x_\rho}\frac{\partial g^{\mu\mu'}}{\partial x_\tau}\right) \\ &= \frac{\sqrt{-g}}{\varkappa}\sum_{\mu\rho\tau\tau'}\left(g^{\nu\tau}\Gamma^\rho_{\mu\sigma}\Gamma^\mu_{\rho\tau} - \frac{1}{2}\delta^\nu_\sigma g^{\tau\tau'}\Gamma^\rho_{\mu\tau}\Gamma^\mu_{\rho\tau'}\right) \end{aligned}\right\} \quad (81b)$$

Die Gleichungen (81) in Verbindung mit (81a) und (81b) sind die Differentialgleichungen des Gravitationsfeldes. Die Gleichungen (42c) drücken nach den in § 10 gegebenen Überlegungen die Erhaltungssätze des Impulses und der Energie für Materie und Gravitationsfeld zusammen aus. $\mathfrak{t}^\nu_\sigma$ sind diejenigen auf das Gravitationsfeld bezüglichen Größen, welche den Komponenten $\mathfrak{T}^\nu_\sigma$ des Energietensors (V-Tensors) der physikalischen Bedeutung nach analog sind. Es sei hervorgehoben, daß die $\mathfrak{t}^\nu_\sigma$ nicht beliebigen berechtigten, sondern nur linearen Transformationen gegenüber Tensorkovarianz besitzen; trotzdem nennen wir ($\mathfrak{t}^\nu_\sigma$) den Energietensor des Gravitationsfeldes. Analoges gilt für die Komponenten $\Gamma^\nu_{\sigma\beta}$ der Feldstärke des Gravitationsfeldes.

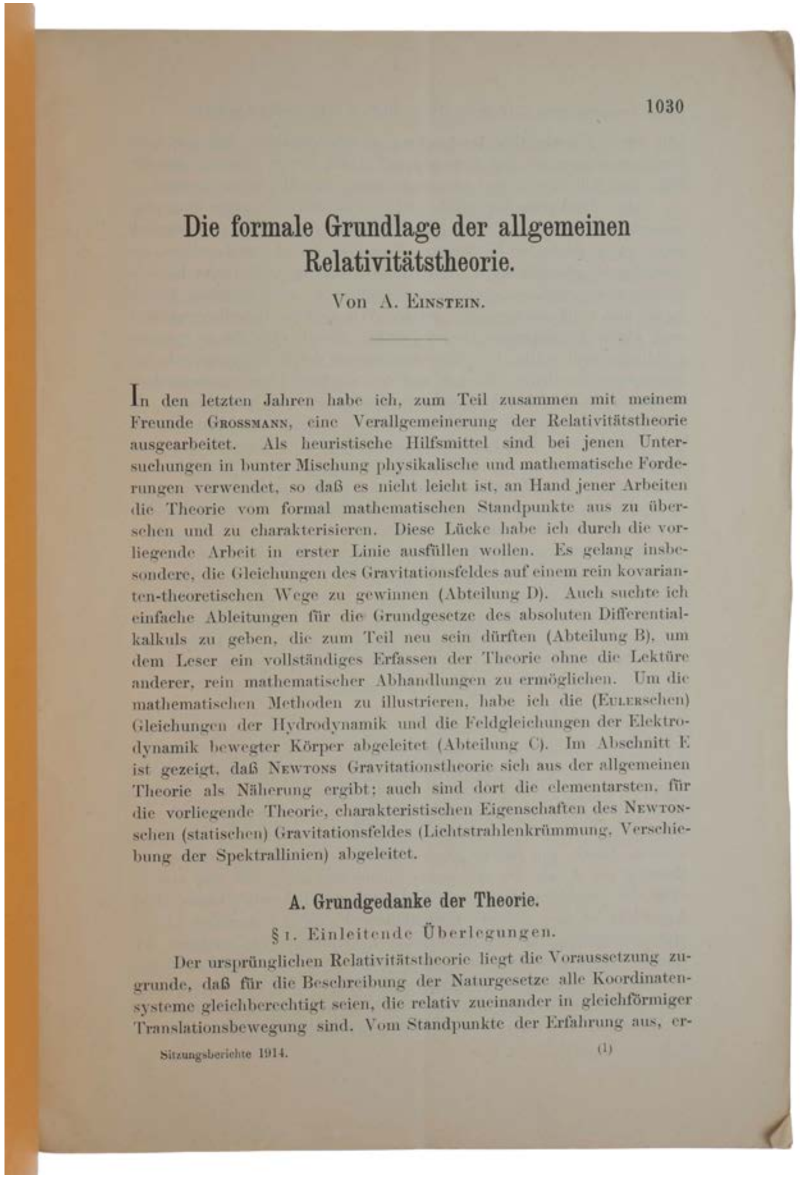
Das Gleichungssystem (81) läßt trotz seiner Kompliziertheit eine einfache physikalische Interpretation zu. Die linke Seite drückt eine Art Divergenz des Gravitationsfeldes aus. Diese wird — wie die rechte Seite zeigt — bedingt durch die Komponenten des totalen Energietensors. Sehr wichtig ist dabei das Ergebnis, daß der Energietensor des Gravitationsfeldes selbst in gleicher Weise felderregend wirksam ist wie der Energietensor der Materie.

wholeheartedly in an intense correspondence with Levi-Cività, who challenged Einstein's derivation of the covariance properties of his gravitation tensor. But it seems that by mid-July he was less certain … By mid-October Einstein's points of dissatisfaction with his theory had grown in number and intensity. They soon culminated in some of the most agitated and strenuous works of his life, in which generally covariant field equations were discovered … Einstein's work [in the present paper] had brought him both temporally and conceptually closer than ever before to a generally covariant theory … It is hard to imagine that Einstein was unprepared for the ease with which his formalism of 1914 could be applied to his final generally covariant theory" (Norton, pp. 296-303).

After publishing the generally covariant theory in November 1915, Einstein gave a further treatment of the variational formulation (Hamiltonsches Prinzip und allgemeine Relativitätstheorie, *Sitzungsberichte* (1916), pp. 1111-1116). By this time, the great German mathematician David Hilbert had published his own account of general relativity in terms of a variational principle [Die Grundlagen der Physik (Erste Mitteilung), *Nachrichten derKönigliche Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse,* November 1915, 395-407]. This led to some controversy over who had been the first to publish the final version of general relativity (although Hilbert himself never claimed priority). "Hilbert, through his important paper of November 1915, is generally thought of as introducing the comprehensive use of these action principles to the theory. My analysis shows that although Einstein might have drawn some of his work of 1916 in this area from Hilbert's, his basic mathematical apparatus and even the notation itself had its ancestry in his own work earlier in 1914 and 1915" (Norton, p. 303).

Gunnar Nordström first studied at the University of Helsinki (1903-7), and then spent a year at Göttingen, where he became a convert to the theory of relativity in its Minkowskian formulation. His remaining published work was focused almost exclusively on relativity, the most important being his theory of gravitation, developed between 1912 and 1914. "After an initial enchantment and subsequent disillusionment with [Max] Abraham's theory of gravitation, Einstein found himself greatly impressed by a Lorentz covariant gravitation theory due to the Finnish physicist Gunnar Nordström. In fact, by late 1913, Einstein had nominated [in a lecture at the 85th Congress of the German Natural Scientists and Physicians] Nordström's theory as the only viable competitor to his own emerging theory of relativity. This selection came, however, only after a series of exchanges between Einstein and Nordström that led Nordström to significant modifications of his theory … under continued pressure from Einstein, Nordström made his theory compatible with the equality of inertial and gravitational mass by assuming that rods altered their length and clocks their rate upon falling into a gravitational field so that the background Minkowskian space-time had become inaccessible to direct measurement. As Einstein and Fokker showed in 1914, the space-time actually revealed by direct clock and rod measurement had become curved, much like the space-times of Einstein's own theory. Moreover, Nordström's gravitational field equation was equivalent to a geometrical equation in which the Riemann-Christoffel curvature tensor played the central role. In it, the curvature scalar is set proportional to the trace of the stress-energy tensor. What is remarkable about this field equation is that it comes almost two years *before* Einstein recognized the importance of the curvature tensor in constructing field equations for his own general theory of relativity! In this regard, the conservative approach actually anticipated Einstein's more daring approach" (Norton in Earman et al, pp. 4-5). As late as 1917, more than a year after Einstein published his final version of general relativity, Max von Laue published an exposition of Nordström's theory: it was still considered by some a potential competitor to Einstein's. This finally changed with the confirmation of the bending of light rays during the solar eclipse of 1919 as predicted by general relativity: Nordström's theory predicted no such bending. Nordström is remembered today for two other contributions. In 1914 he introduced an additional space dimension to his theory, which provided coupling to electromagnetism. This was the first of the extra-dimensional theories, which later came to be known as Kaluza-Klein theories, although Kaluza and Klein did not publish their work until the 1920s. Today extra-dimensional theories are widely researched. Then in 1918 Nordström obtained the solution of Einstein's field equations for a spherically symmetric charged body, used today in the description of charged black holes (this is now known as the 'Reissner-Nordström solution,' as Hans Reissner (1874-1902) had in 1916 given the solution for a charged point mass).

Nordström also assisted Einstein in his work on gravitational waves. In Einstein's first published paper on gravitational waves (1916), "he made use of a somewhat controversial mathematical construct known as a pseudotensor to describe the energy in the gravitational field. He made a mistake in doing so, however, which was only discovered when Nordström attempted to use the pseudotensor from Einstein's linearized approximation paper to calculate the energy in the gravitational field of an isolated mass. After some to and fro between himself and Nordström, Einstein realized the nature of his mistake, which had given rise to an incorrect formula for the energy transmitted in a gravitational wave. He presented a new paper in 1918" (*Cambridge Companion to Einstein*, pp. 272-3). This author's presentation offprint is of extreme rarity, and must be distinguished from other so-called 'offprints' of papers from the Berlin *Sitzungsberichte*, many of which are commonly available on the market. The celebrated bookseller Ernst Weil (1919-1981), in the introduction to his Einstein bibliography, wrote: "I have often been asked about the number of those offprints. It seems to be



1030

Die formale Grundlage der allgemeinen Relativitätstheorie.

Von A. Einstein.

In den letzten Jahren habe ich, zum Teil zusammen mit meinem Freunde Grossmann, eine Verallgemeinerung der Relativitätstheorie ausgearbeitet. Als heuristische Hilfsmittel sind bei jenen Untersuchungen in bunter Mischung physikalische und mathematische Forderungen verwendet, so daß es nicht leicht ist, an Hand jener Arbeiten die Theorie vom formal mathematischen Standpunkte aus zu übersehen und zu charakterisieren. Diese Lücke habe ich durch die vorliegende Arbeit in erster Linie ausfüllen wollen. Es gelang insbesondere, die Gleichungen des Gravitationsfeldes auf einem rein kovarianten-theoretischen Wege zu gewinnen (Abteilung D). Auch suchte ich einfache Ableitungen für die Grundgesetze des absoluten Differentialkalkuls zu geben, die zum Teil neu sein dürften (Abteilung B), um dem Leser ein vollständiges Erfassen der Theorie ohne die Lektüre anderer, rein mathematischer Abhandlungen zu ermöglichen. Um die mathematischen Methoden zu illustrieren, habe ich die (Eulerschen) Gleichungen der Hydrodynamik und die Feldgleichungen der Elektrodynamik bewegter Körper abgeleitet (Abteilung C). Im Abschnitt E ist gezeigt, daß Newtons Gravitationstheorie sich aus der allgemeinen Theorie als Näherung ergibt; auch sind dort die elementarsten, für die vorliegende Theorie, charakteristischen Eigenschaften des Newtonschen (statischen) Gravitationsfeldes (Lichtstrahlenkrümmung, Verschiebung der Spektrallinien) abgeleitet.

A. Grundgedanke der Theorie.

§ 1. Einleitende Überlegungen.

Der ursprünglichen Relativitätstheorie liegt die Voraussetzung zugrunde, daß für die Beschreibung der Naturgesetze alle Koordinatensysteme gleichberechtigt seien, die relativ zueinander in gleichförmiger Translationsbewegung sind. Vom Standpunkte der Erfahrung aus, er-

Sitzungsberichte 1914. (1)

certain that there were few before 1914. They were given only to the author, and mostly 'Überreicht vom Verfasser' (Presented by the Author) is printed on the wrapper. Later on, I have no doubt, many more offprints were made, and also sold as such, especially by the Berlin Academy." If the term 'offprint' means, as we believe it should, a separate printing of a journal article given (only) to the author for distribution to colleagues, then 'offprints' were not commercially available. Although there is certainly some truth in Weil's remark, in our view it requires clarification and explanation.

Until about 1916, most of Einstein's papers were published in *Annalen der Physik*; from 1916 until he left Germany for the United States in 1933, most were published in the Berlin *Sitzungsberichte*. The *Sitzungsberichte* differed from other journals in which Einstein published in that it made separate printings of its papers commercially available. These separate printings have 'Sonderabdruck' printed on the front wrapper, the usual German term for offprint, but they are not offprints according to our definition. They were available to anyone; indeed a price list of these 'trade offprints' is printed on the rear wrapper. True author's presentation offprints can be distinguished from these trade offprints by the presence of 'Überreicht vom Verfasser' on the front wrapper.

In the period 1916 to 1919 or 1920, the *Sitzungsberichte* trade offprints are themselves rare: for example, ABPC/RBH list only three 'offprints' of Einstein's famous 1917 *Sitzungsberichte* paper 'Kosmologische Betrachtungen zur allgemeinen Relativitätstheorie' (the auction records do not distinguish between trade and author's presentation offprints). After 1919 or 1920, however, the trade offprints become much more common, although the author's presentation offprints are still very rare. The reason for this change is that it was only in 1919 that Einstein became famous among the general public.

It might seem obvious that Einstein's fame dates from 1905, his 'annus mirabilis', in which he published his epoch-making papers on special relativity and the light quantum. However, these works did not make him immediately well known even in the physics community – many physicists did not understand or accept his work, and it was two or three years before his genius was fully accepted even by his colleagues. He secured his first academic position, at the University of Bern, in 1908. Among the general public, Einstein became well known only in late 1919, following the success of Eddington's expedition to observe the bending of light by the Sun, which confirmed Einstein's general theory of relativity. This was front-page news, and made Einstein universally famous. (See Chapter 16, 'The suddenly famous Doctor Einstein', in Pais, *Subtle is the Lord*, for an account of these events). Before 1919 the trade offprints of Einstein's papers would probably only have been purchased by professional physicists; after 1919 everyone wanted a memento of the famous Dr. Einstein, whether or not they understood anything of theoretical physics, and the trade offprints of his papers were printed and sold in far greater numbers than before to meet the demand. It is telling that when these post-1919 trade offprints appear on the market, they are often in mint condition – they were never read simply because their owners were unable to understand them.
In our view, Einstein's author's presentation offprints are rare, from any journal and any period, though of course some are rarer than others. Before 1919 or 1920, the *Sitzungsberichte* trade offprints are also quite rare, although not nearly as rare as the author's presentation offprints; after 1919 or 1920, the trade offprints are much more common.

Boni 65; Weil 68. Calaprice, *The Einstein Almanac;* Norton, 'How Einstein Found His Field Equations: 1912-1915', *Historical Studies in the Physical Sciences* 14 (1984), pp. 253-316; Norton, 'Einstein and Nordström: some lesser-known thought

experiments in gravitation,' pp. 3-30 in *The Attraction of Gravitation*: *New Studies in the History of General Relativity*, edited by John Earman, Michel Janssen, John D. Norton, 1993; Reich, Einstein's "Formal Foundations of the General Theory of Relativity" (1914) (http://mathineurope.eu/images/information_pic/hist_phil_pic/calendar_pic/2014einstein/Einstein_English.pdf). For the history of tensor calculus, including Einstein's application of it to general relativity, see Reich, *Die Entwicklung des Tensorkalküls*, Birkhäuser 2012.

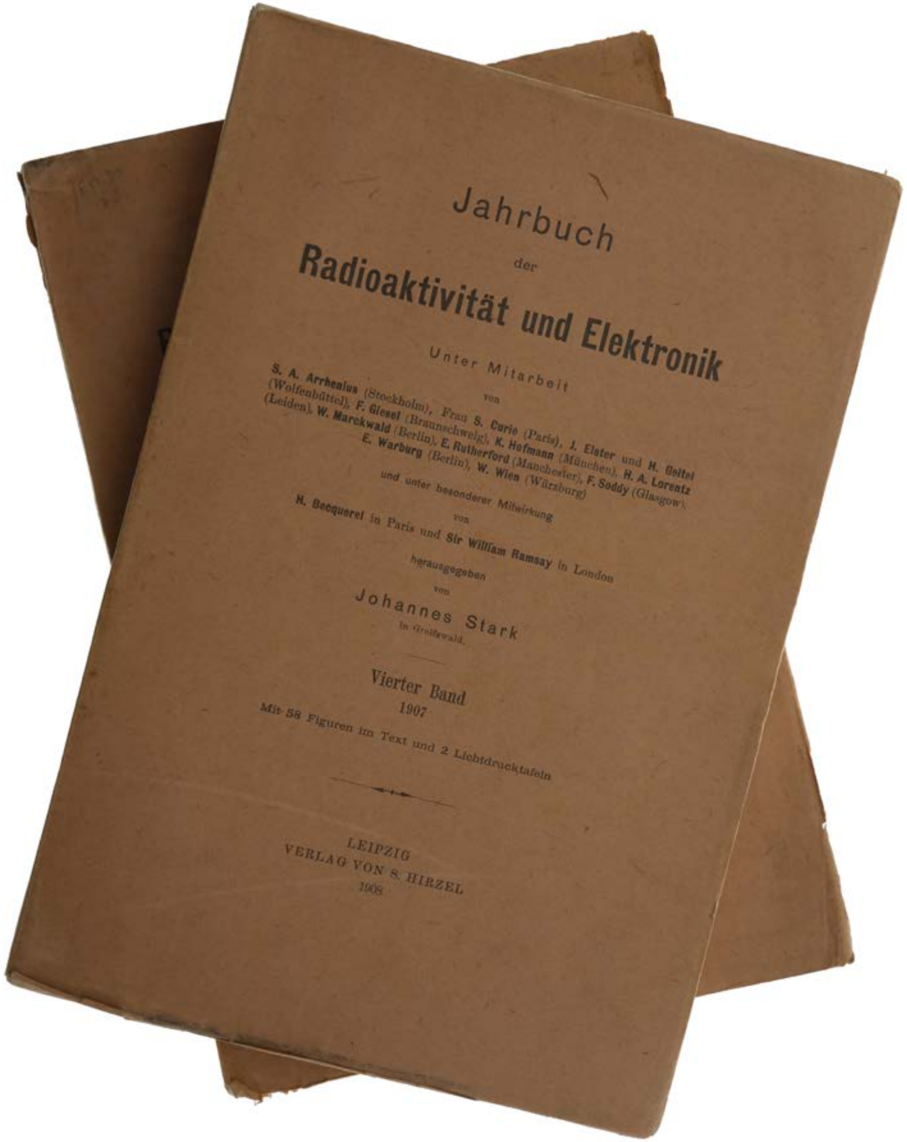
THE MASS-ENERGY EQUIVALENCE

EINSTEIN, Albert. *Über das Relativitätprinzip und die aus demselben gezogenen Folgerungen. [with:] Berichtigungen zu der arbeit "Über das Relativitätprinzip..."* Leipzig: S. Hirzel, 1907;1908.

$7,500

In: Jahrbuch der Radioaktivität und Elektronik, Bd. 4, pp. 411-62 and Bd. 5, pp. 98-99 [Berichtigungen]. Two vols. 8vo (242 x 163 mm), the complete volumes offered here in their original printed wrappers, manuscript lettering to spines, a fine set.

First edition, journal issues in original printed wrappers, of this crucially important transitional paper, in which Einstein introduced the principle of equivalence for uniformly accelerated motion, which launched him on his path to general relativity. "On p. 443 are probably the first explicit statements both of the equivalence of inertial and gravitational mass and of the equation for mass in terms of energy [$E = mc^2$] now regarded as the theoretical basis for the release of atomic energy" (Weil). In 1905, "Einstein said that all energy of whatever sort has mass. It took even him two years more to come to the stupendous realization that the reverse must also hold: that all mass, of whatever sort, must have energy. ... With mass and energy thus wholly equivalent, Einstein was able in 1907, in a long and mainly expository paper published in the *Jahrbuch der Radioactivität* [the offered paper], to write his famous equation $E = mc^2$ ... In presenting his equation in 1907 Einstein spoke of it as the most important consequence of his theory of relativity" (Hoffmann, *Albert Einstein*, p. 81). "Of greatest importance is the last part of the paper which generalizes the principle of relativity from uniformly moving systems to uniformly 'accelerated' systems. ... He introduces the principle of equivalence which claims that the problem of a uniform and



stationary gravitational field on the one hand, and the system moving with a constant acceleration without any gravitation on the other hand, are physically indistinguishable situations. This principle put him in a position to find out what effect gravitation has on an arbitrary physical phenomenon, because all he had to do was to observe that phenomenon from an accelerated reference system. He thus obtains the speeding up of clocks in a field of increased gravitational potential, which must lead to a universal red shift of the spectral lines coming from the Sun, and likewise to a bending of light rays near to the limb of the Sun. Furthermore, this hypothesis at once makes it clear why inertial mass and gravitational mass must be, under all circumstances, strictly proportional to one another. ... Hence the principle of the energy value of inertial mass must be extended to the gravitational mass" (Lanczos, *The Einstein Decade*, p. 153). Later Einstein wrote that when he was working on this paper, "There occurred to me the happiest thought of my life, in the following form. The gravitational field has only a relative existence in a way similar to the electric field generated by magnetoelectric induction. *Because for an observer falling freely from the roof of a house there exists* – at least in his immediate surroundings – *no gravitational field*" [Einstein's emphasis] (Pais, *Subtle is the Lord*, p. 178).

"His first important paper on relativity theory after 1905 is the 1907 review [the first offered paper]. This article was written at the request of {Johannes] Stark, the editor of the *Jahrbuch*. On September 25, 1907, Einstein had accepted this invitation. On November 1, Einstein further wrote to Stark: 'I am now ready with the first part of the work for your *Jahrbuch*. I am working zealously on the second [part] in my unfortunately scarce spare time.' Since this second part contains the remarks on gravitation, it seems probable that Einstein's 'happiest thought' came to him sometime in November 1907. We certainly know where he was when he had this idea. In his Kyoto lecture he told the story: 'I was sitting in a chair in the patent office at Bern when all of a sudden a thought occurred to me. 'If a person falls freely he will not feel his own weight!' I was startled. This simple thought made a deep impression on me. It impelled me toward a theory of gravitation' …

"Three main issues are raised in Section V of the *Jahrbuch* article.

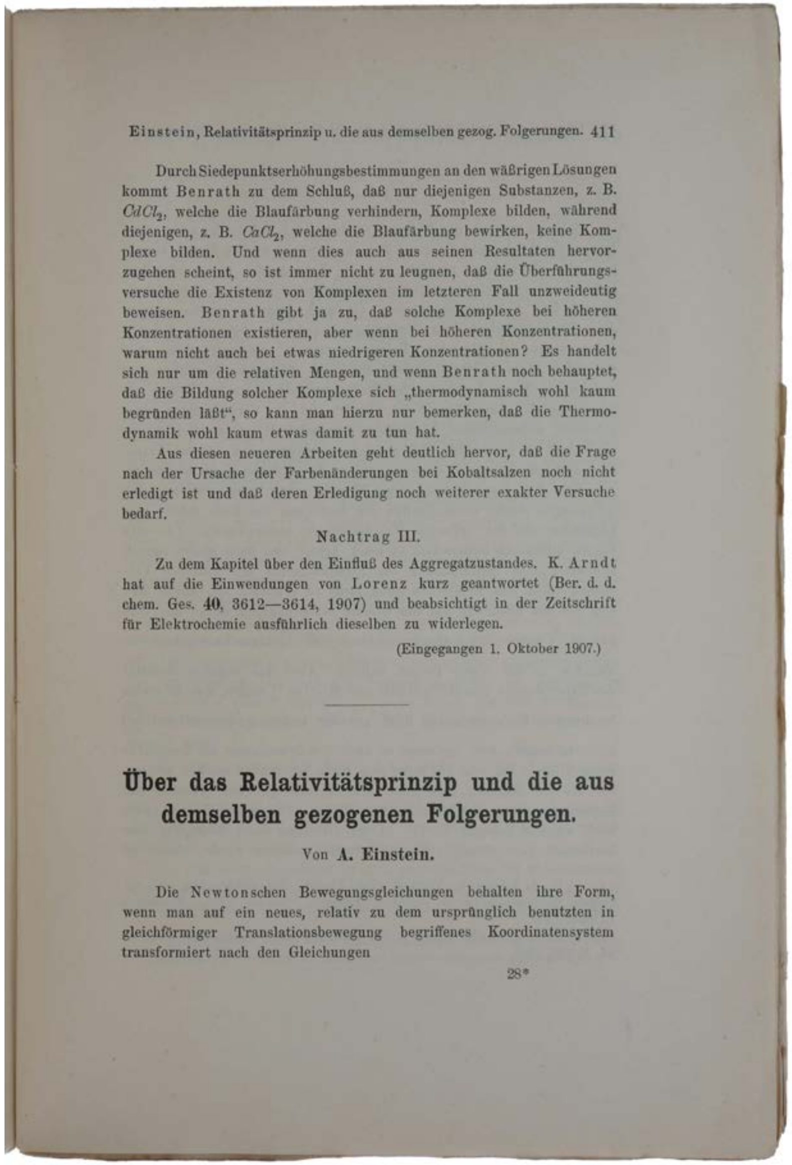
The Equivalence Principle. 'Is it conceivable that the principle of relativity also holds for systems which are accelerated relative to each other?' That is Einstein's starting question. Then he gives the standard argument. A reference frame Σ_1 is accelerated in the x direction with a constant acceleration γ. A second frame Σ_2 is at rest in a homogeneous gravitational field which imparts an acceleration $-\gamma$ in the x direction to all objects. 'In the present state of experience, we have no reason to assume that … Σ_1 and Σ_2 are distinct in any respect, and in what follows we shall therefore assume the complete physical equivalence of a gravitational field and the corresponding acceleration of the reference frame. This assumption extends the principle of relativity to the case of uniformly accelerated motion of the reference frame … he began by applying his new postulate to the Maxwell equations, always for uniform acceleration. He did not raise the question of the further extension to nonuniform acceleration until 1912, the year he first referred to his hypothesis as the 'equivalence principle.'

The Gravitational Red Shift. Many textbooks on relativity ascribe to Einstein the method of calculating the red shift by means of the Doppler effect of light falling from the top to the bottom of an upwardly accelerating elevator. That is indeed the derivation he gave in 1911. However, he was already aware of the red shift in 1907. The derivation he gave at that time is less general, more tortured, and yet, oddly, more sophisticated. It deserves particular mention because it contains the germ of two ideas that were to become cornerstones of his final theory: the existence of local Lorenz frames and the constancy of the velocity of light for infinitesimally small paths …

Maxwell's Equations; *Bending of Light*; *Gravitational Energy* = mc^2. Indomitably Einstein goes on. He tackles the Maxwell equations next. [He concludes that Maxwell's equations have the same form in a uniformly accelerated reference frame as in a non-accelerated frame but with a modified velocity of light.] 'It follows that the light rays … are bent by the gravitational field.' Second, he examines the energy conservation law in [the accelerated frame] and finds 'a very notable result … In a gravitational field, one must associate with every energy E an additional position-dependent energy which equals the position-dependent energy of a 'ponderable' mass of magnitude E/mc^2. The law [$E = mc^2$] therefore holds not only for inertial but also for gravitational mass' …

"This review does not have the perfection of the 1905 paper on special relativity. The approximations are clumsy and mask the generality of the conclusions. Einstein was the first to say so, in 1911. The conclusion about the bending of light is qualitatively correct, quantitatively wrong – though, in 1907, not yet logically wrong. Einstein was the first to realize this, in 1915. Despite all this I admire this article at least as much as the perfect relativity paper on 1905, not as much for its details as for its courage" (Pais, pp. 179-182).

Stanitz 94; Weil *21; Boni 20; Plotnick 77.



Einstein, Relativitätsprinzip u. die aus demselben gezog. Folgerungen. 411

Durch Siedepunktserhöhungsbestimmungen an den wäßrigen Lösungen kommt Benrath zu dem Schluß, daß nur diejenigen Substanzen, z. B. $CdCl_2$, welche die Blaufärbung verhindern, Komplexe bilden, während diejenigen, z. B. $CaCl_2$, welche die Blaufärbung bewirken, keine Komplexe bilden. Und wenn dies auch aus seinen Resultaten hervorzugehen scheint, so ist immer nicht zu leugnen, daß die Überführungsversuche die Existenz von Komplexen im letzteren Fall unzweideutig beweisen. Benrath gibt ja zu, daß solche Komplexe bei höheren Konzentrationen existieren, aber wenn bei höheren Konzentrationen, warum nicht auch bei etwas niedrigeren Konzentrationen? Es handelt sich nur um die relativen Mengen, und wenn Benrath noch behauptet, daß die Bildung solcher Komplexe sich „thermodynamisch wohl kaum begründen läßt", so kann man hierzu nur bemerken, daß die Thermodynamik wohl kaum etwas damit zu tun hat.

Aus diesen neueren Arbeiten geht deutlich hervor, daß die Frage nach der Ursache der Farbenänderungen bei Kobaltsalzen noch nicht erledigt ist und daß deren Erledigung noch weiterer exakter Versuche bedarf.

Nachtrag III.

Zu dem Kapitel über den Einfluß des Aggregatzustandes. K. Arndt hat auf die Einwendungen von Lorenz kurz geantwortet (Ber. d. d. chem. Ges. **40**, 3612—3614, 1907) und beabsichtigt in der Zeitschrift für Elektrochemie ausführlich dieselben zu widerlegen.

(Eingegangen 1. Oktober 1907.)

Über das Relativitätsprinzip und die aus demselben gezogenen Folgerungen.

Von **A. Einstein.**

Die Newtonschen Bewegungsgleichungen behalten ihre Form, wenn man auf ein neues, relativ zu dem ursprünglich benutzten in gleichförmiger Translationsbewegung begriffenes Koordinatensystem transformiert nach den Gleichungen

28*

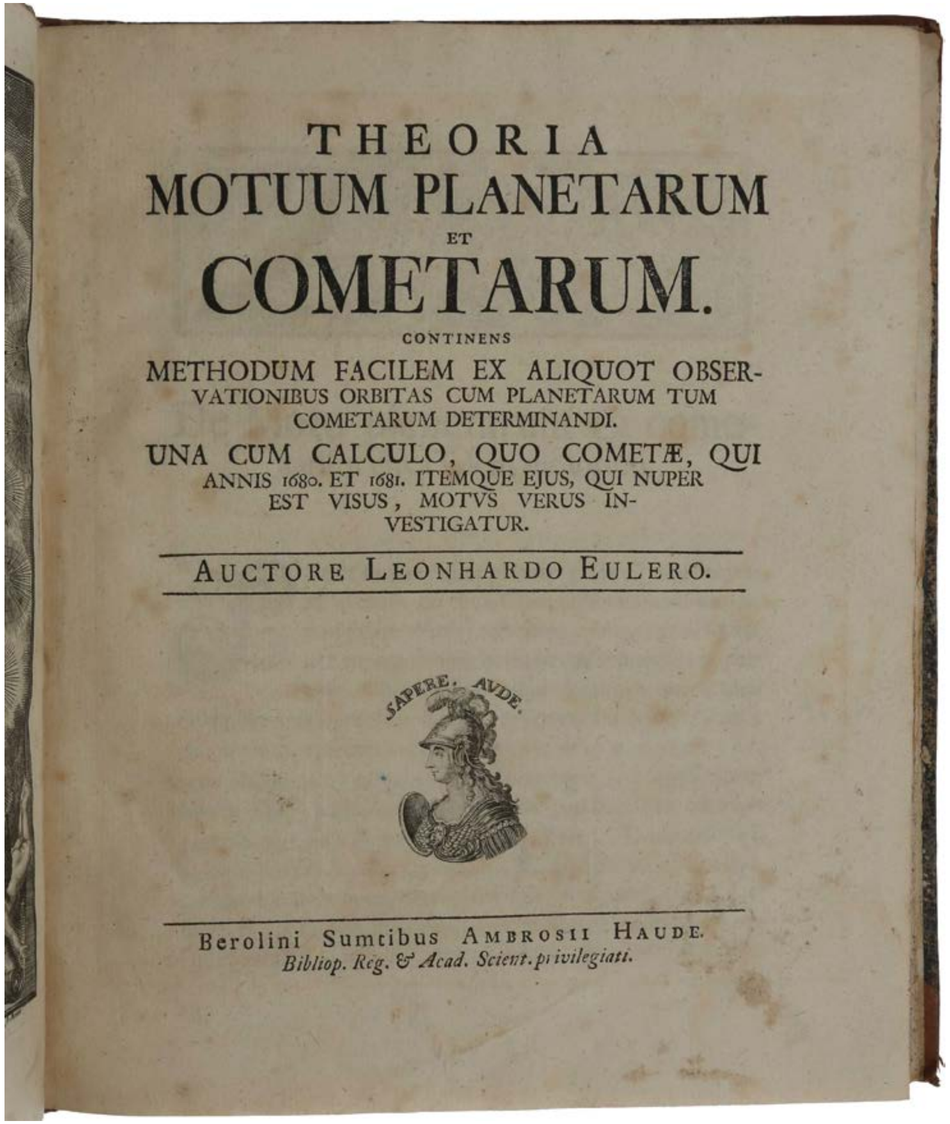
HIS FIRST TREATISE ON ASTRONOMY

EULER, Leonhard. *Theoria motuum planetarum et cometarum. Continens methodum facilem ex aliquot observationibus orbitas cum planetarum tum cometarum determinandi. Una cum calculo, quo cometae, qui annis 1680 et 1681. Itemque ejus, qui nuper est visus, motus verus investigatur.* Cambridge: University Press, 1913.

$5,000

4to (223 x 180 mm), pp. [3], 4-6, 9-187 (i.e., 188: last page mispaginated 187), with engraved frontispiece and four folding engraved plates of diagrams. Woodcut vignette on title, woodcut initials and head- and tail-pieces. In this, as in all copies we have seen, the frontispiece, which was printed on A4, has been cut out and bound facing the title. Pages 7 & 8 are therefore omitted, but the text is continuous and the volume is absolutely complete. Contemporary half calf, text with some light spotting.

First edition of Euler's first treatise on astronomy, "a fundamental work on calculation of orbits" (DSB). Stimulated by the appearance of two great comets in 1742 and 1744 (now designated C/1742 C1 and C/1743 X1), Euler developed new methods to determine the (elliptic) orbits of planets and the (elliptic and parabolic) orbits of comets. His first major contribution in the present work was to the 'two-body problem,' the problem of determining the motion of two spherical bodies under their mutual gravitational attraction (such as the Sun and a planet). Newton had attacked the two-body problem using geometrical methods in *Principia*, and preliminary analytical results had been presented in 1734 by Daniel Bernoulli, but it was Euler in the present work who gave the first complete analytical solution. The second major contribution of the present work



was the introduction of new techniques of perturbation theory – the method of successive approximations that Euler used to determine parabolic orbits is still known as 'Euler's method.' Euler used these new techniques, together with observational data supplied by Alexis-Claude Clairaut in Paris, to calculate the orbits of the comets of 1742 and 1744, and his success stimulated others to use his methods to predict the next return of Halley's comet, which Edmond Halley had first observed in 1682. "When Euler reported back on his successful calculation of an orbit from their data, the Parisian astronomers, even die-hard Cartesians like Jacques Cassini, had to accede to the power of Newtonian theory. In fact the French adopted it with such enthusiasm that they virtually took over the work on Halley's comet at its forthcoming apparition, Clairaut foremost among them" (Broughton, p. 126). As mentioned in the title of the present work, Euler also applied his methods to check the orbit of the great comet of 1680/81 – Newton had initially believed that the observations of 1680 and 1681 were of two different comets, but eventually agreed with Flamsteed that they were of a single comet that initially approached, and then receded from the Sun. The treatment of the full three-body problem had to wait another decade, until the appearance of Euler's first theory of the motion of the Moon, presented in his *Theoria motus lunae* (1753).

"The year 1744 saw the publication of Euler's second Berlin book, *Theoria motuum planetarum et cometarum* (Theory of the motions of planets and comets), his first on astronomy. This smaller, 187-page treatise was printed anonymously after another comet was sighted on 18 January, the fourth sighting since 13 December …

"The *Theoria motuum* begins with the orbit of Mercury and makes a table of interpolations from the radius vector to compute eccentricities more closely. Euler calculated the interpolations with logarithms, trigonometric functions, and fourth-degree roots. He developed the first differential equations for computing every point in the orbits of Earth and Mars. Thereby he illustrated new methods for examining planetary perturbations. Using the latest telescopic observations, Euler differentiated comets from fixed stars and described the research of the time on both of them. While the mass of comets was still unknown, he found their orbits to be nearly parabolic ellipses and sought to devise functions for computing each course element, including anomalies. A much-elongated ellipse was assumed if comets were permanent. But Euler found their path closer to a parabola. Comparing accurate observations of the comet of 1742, constructing variations of similar ellipses and parabolas, and employing records concerning comets in 1743 and 1744, Euler revised and improved upon Jacques Cassini's method for computing the time at which a comet reaches perihelion, its nearest point to the sun. After gathering the latest observations, Euler concentrated on formulating new differential equations that better traced the paths of comets. The application of his equations and formulas provided the most accurate computation of most points in the orbits of planets and comets to that time. But the *Theoria motuum* also cites unresolved problems in constructing a precise, theoretical account for comets' entire orbits; Euler recommended caution toward the belief that comets foretell the wrath of God, and rejected the notion – even though they could come close enough to do so – that they would ever destroy Earth, citing the Bible as denying that this could happen.

"This work was formulated entirely as a two-body problem and not the classical three-body problem that had drawn Euler's interest as early as 1730. Until the nineteenth century, the book was fundamental for calculating the orbits of planets and comets. It posed severe difficulties for the publisher Ambrose Haude in Berlin. A small but difficult work, it contains errors in the printing of computations and of some formulas; through footnotes, Euler attempted to correct many of these" (Calinger, p. 229).

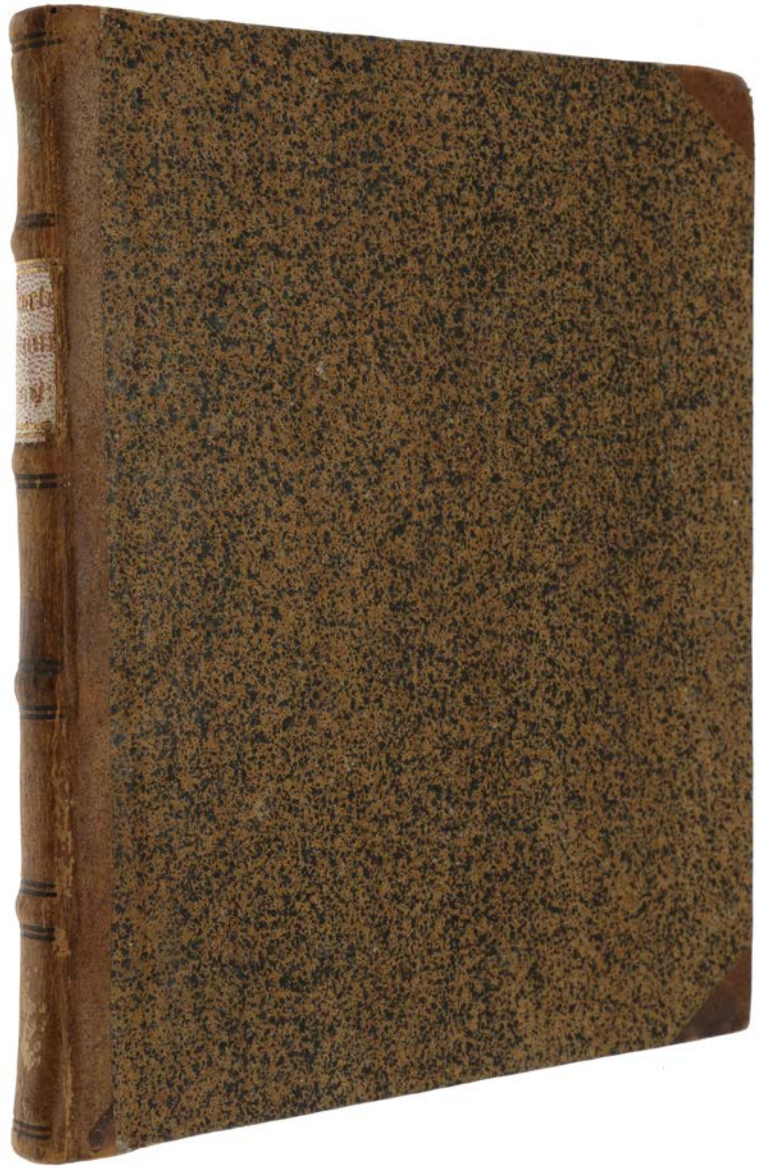
Using the techniques developed in the present work, Euler went on to develop his theory of the Moon's motion which Tobias Mayer used to constructed his important lunar tables. These were bequeathed to the English Board of Longitude on Mayer's death in 1762; their great accuracy allowed longitude to be found within a few nautical miles and also permitted the position of the Moon to be calculated several years in advance. For this Mayer's widow in 1770 received £5000 from the English parliament, and in recognition of Euler's theoretical contributions a sum of £300 was also voted as an honorarium to him.

The remarkable engraved frontispiece, by Berol after F. H. Fritsch, depicts the solar system with the Sun as one among many other stars in a plurality of worlds.

Leonhard Euler (1707-83) was the most prolific mathematician of all time and one of the most influential. "He made large bounds forward in the study of modern analytic geometry and trigonometry where he was the first to consider sin, cos, etc. as functions rather than as chords as Ptolemy had done. He made decisive and formative contributions to geometry, calculus and number theory. He integrated Leibniz's differential calculus and Newton's method of fluxions into mathematical analysis. He introduced beta and gamma functions, and integrating factors for differential equations. He studied continuum mechanics, lunar theory with Clairaut, the three-body problem, elasticity, acoustics, the wave theory of light, hydraulics, and music. He laid the foundation of analytical mechanics" (Mactutor). "Euler's studies in astronomy embraced a great variety of problems: determination of the orbits of comets and planets by a few observations, methods of calculation of the parallax of the sun, the theory of refraction, considerations on the physical nature of comets, and the problem of retardation of planetary motions under the action of cosmic ether. His most outstanding works, for which he won many prizes from the Paris Académie des Sciences, are concerned with celestial mechanics, which especially attracted scientists at that time" (DSB). The

great French mathematician Pierre-Simon Laplace advised to "read Euler, as he is a master of us all".

Eneström 66; Honeyman 1063; Houzeau & Lancaster 11948. Broughton, 'The first predicted return of Halley's comet,' *Journal for the History of Astronomy* 16 (1985), pp. 123-133. Calinger, *Leonhard Euler. Mathematical Genius of the Enlightenment*, 2016.



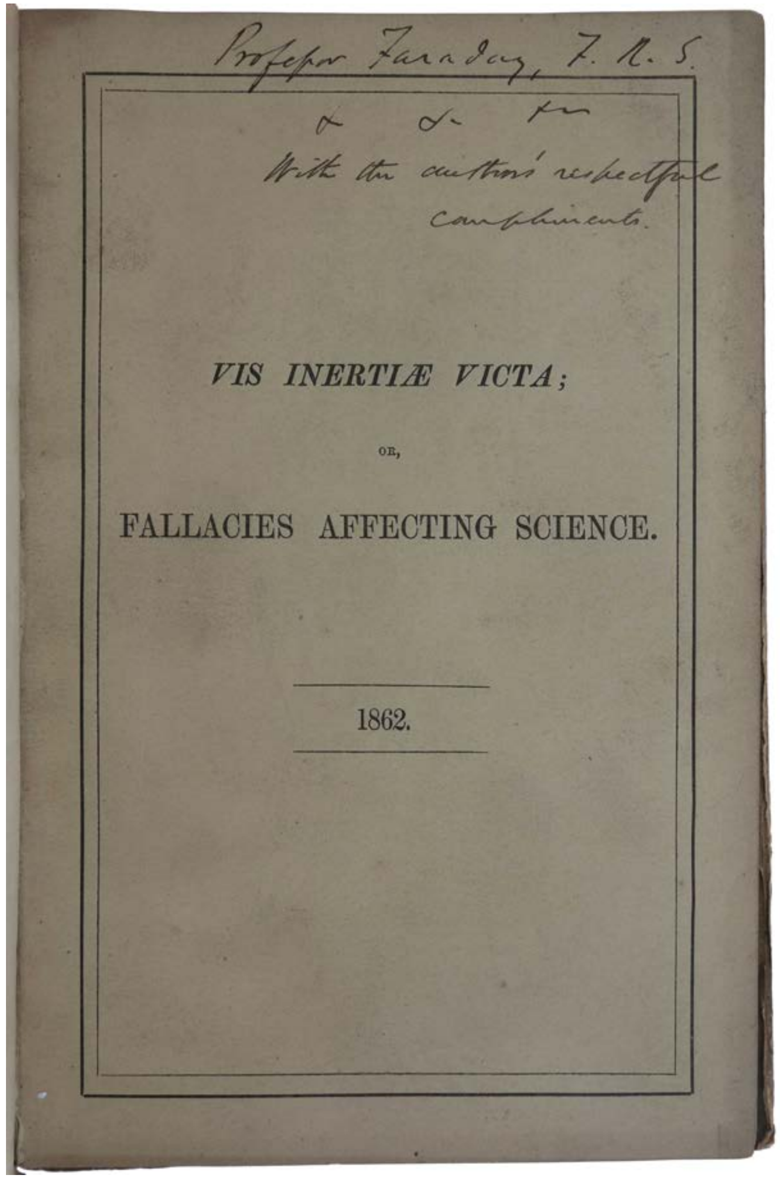
FROM THE LIBRARY OF FARADAY

[FARADAY, Michael]. *A fascinating sammelband, almost certainly from the personal library of the great English chemist, natural philosopher, and founder of electromagnetism Michael Faraday (1791-1867), and very possibly bound by him.*

$6,500

Contemporary quarter roan, with decorated boards, sewn on four binding supports (rubbed, with slight loss at head of spine).

The present volume contains two works **inscribed to Faraday**, three other scientific works, and four examination papers for the Society of Arts for the Encouragement of Manufactures, one of the institutions in which Faraday was most involved – he had chaired its chemistry committee between 1826 and 1838, and was awarded its highest honour, the Albert Medal, in 1866, for his 'discoveries in Chemistry, Electricity, and other branches of Physical Science'. This is surely corroboration beyond all reasonable doubt that this volume once formed part of Faraday's library. It is also possible that the binding itself, which is competently, if not professionally, executed, was done by Faraday: before becoming Humphry Davy's (1778-1829) assistant in 1813, the appointment which ignited his great scientific career, Faraday had been an apprentice bookbinder. His early interest and knowledge of science was gleaned from some of the books on which he practiced his skills. It is known that he bound his laboratory notebooks, and some of his other books, himself (many are still held by The Royal Institution in London). A volume of pamphlets such as ours would have been a prime candidate on which Faraday could exercise his old skills. Books from Faraday's library are very rarely seen on the market.



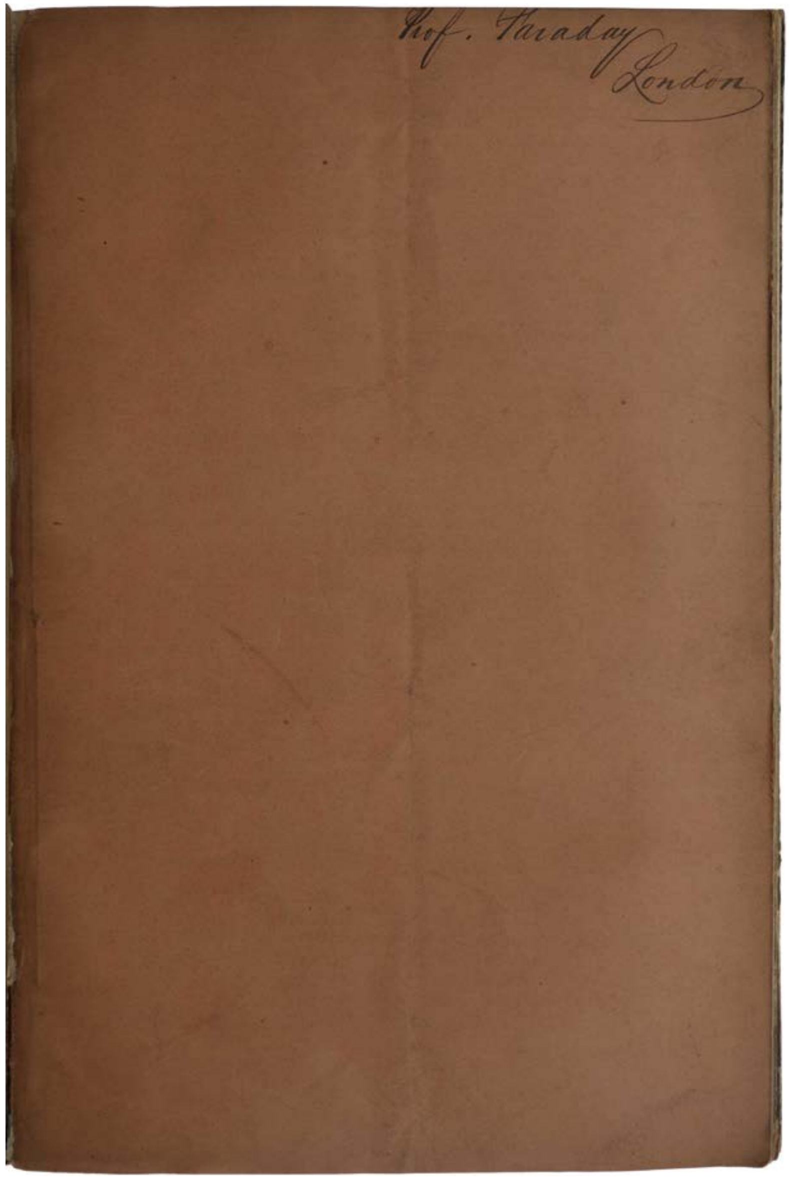
The contents of the volume are as follows (in the order in which they are bound; all items except the last are 8vo):

REDDIE, James. Vis inertiae victa, or fallacies affecting science: an essay towards increasing our knowledge of some physical laws, and a review of certain mathematical principles of natural philosophy. London: Bradbury & Evans, 1862. Pp. xii, 65, [1]. Original upper wrapper bound in. Presentation copy, inscribed 'Professor Faraday, F.R.S. With the author's respectful compliments'.

First edition of this rare 'paradoxical' tract. "An attack on the Newtonian mechanics; revolution by gravitation demonstrably impossible; much to be said for the earth being the immovable centre. A good analysis of contents at the beginning, a thing seldom found. The author has followed up his attack in a paper submitted to the British Association, but which it appears the Association declined to consider. It is entitled *Victoria Toto Coelo*; *or, Modern Astronomy recast* (London, 1863)" (De Morgan, *Budget of Paradoxes*). "Mr. Reddie attaches much force to Berkeley's old arguments against the doctrine of fluxions, and advances objections to Newton's second section" (*ibid*., Supplement No. XII). No copies on ABPC/RBH.

KIRKMAN, Thomas Penyngton. 'A Paper was communicated on July 1st, entitled, 'The Complete Theory of Groups, being the Solution of the Mathematical Prize Question of the French Academy for 1860 ...'' [Drop-head title]. [N.p., n.d.] Extract from: *Memoirs of the Manchester Literary and Philosophical Society*, No. 2, Session, 1863-4. Pp. 133-152. Browned and marked, with damp-staining to margins and horizontal crease.

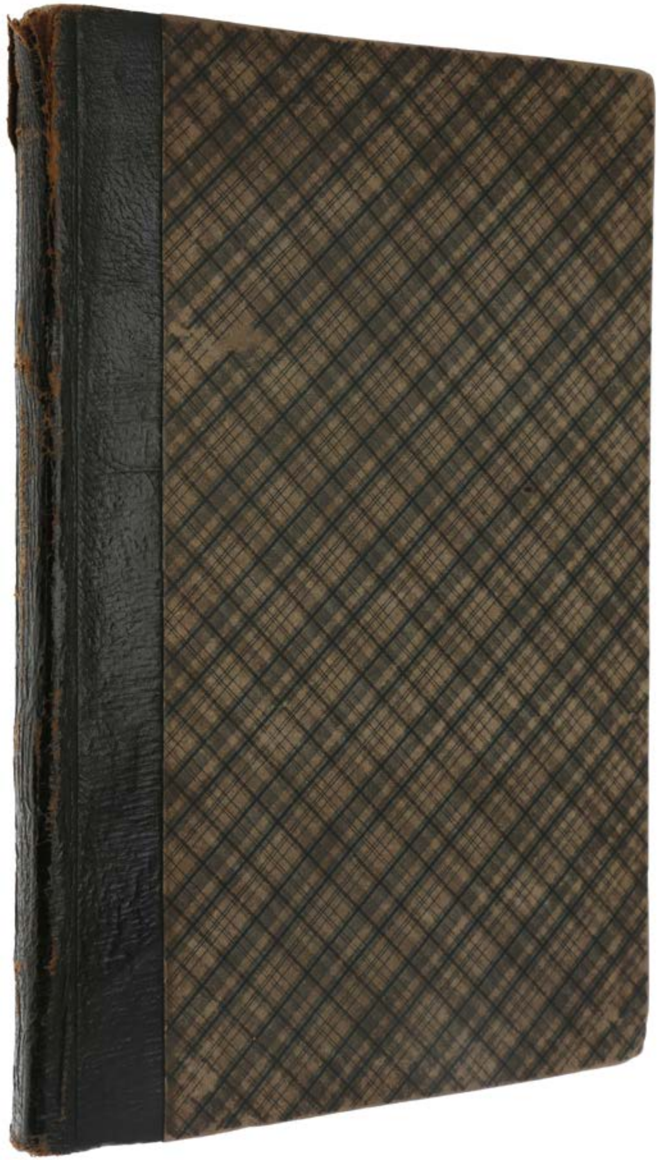
An important early contribution to group theory. "By the late 1850s, groups had also come to interest one of [Arthur] Cayley's mathematical acquaintances, the Anglican clergyman Thomas Kirkman (1806-95). Kirkman had noticed with



interest the announcement in 1858 of a prize question on the theory of groups of substitutions in the *Comptes Rendus* of the Paris Academy of Sciences. The Grand Prix would be awarded in 1860, and Kirkman spent the years from 1858 to 1860 working on his submission. Although the Academy ultimately found none of the three submissions – by Camille Jordan, Émile Mathieu, and Kirkman – suitably original to justify the awarding of the prize, Kirkman made a brief announcement of his ideas at the 1860 meeting of the British Association for the Advancement of Science held in Oxford, and then submitted them for publication to the *Memoirs of the Manchester Literary and Philosophical Society* in 1860 [the offered paper]. There, drawing from the work of [Augustin-Louis] Cauchy, he gave a widely ranging account of permutation groups. In addition to laying out the basic definitions, Kirkman treated (to use modern terminology) such fundamental concepts as left and right cosets, the normalizer of a group, extensions of a group, and the direct product of groups. He also gave a new construction of the projective groups of degree $q + 1$, where q is a power of a prime. In particular, he constructed the projective group of degree 9 and order 1512 (the first of what are now called the *Ree groups*) some thirty years before Frank Nelson Cole claimed to have discovered this new finite group" (Flood, Rice & Wilson (eds.), *Mathematics in Victorian Britain* (2011), p. 349).

MARSH, Benjamin V. The aurora, viewed as an electric discharge between the magnetic poles of the earth, modified by the earth's magnetism. Offprint from: *American Journal of Science and Arts*, vol. XXXI, May 1861. [N.p.], 1861. Pp. 8, with two coloured lithographic plates. With upper plain wrapper bound in, inscribed 'Prof. Faraday London'.

MARSH, Benjamin V. Remarks on the aurora of August 28, 1859. Offprint from the *Journal of the Franklin Institute*. [N.p., n.d.]. Pp. 5, [3], with terminal advertisement leaf.



Extremely rare author's presentation offprints (no copies on COPAC or in auction records). "Eventually it became quite evident that the aurora borealis, or northern lights, had a tendency to disrupt compass readings, a situation which had become something of a threat to navigation. In 1740, Anders Celsius, the inventor of the centigrade scale named after him, had already interpreted the aurora as an electromagnetic phenomenon when he, too, repeatedly noticed that a big compass needle on his desk changed its orientation every time an aurora appeared in the sky above Uppsala, Sweden. So did his brother-in-law, Olaf Peter Hjorter, who spent the entire year between 1741 and 1742 observing compass needles going awry at each appearance of the lights. In 1861, Benjamin Marsh also 'endeavored to show that an auroral streamer is a current of electricity which, originating in the upper portions of [the] atmosphere and following upward the magnetic curve which passes through its base' reaches 'far beyond the supposed limits of the atmosphere'" (thunderbolts.info/tpod/2009/arch09/090907aurorae.htm). It is now understood that the aurora are indeed electrical in origin, being the result of radiation emitted by charged particles in the solar wind being accelerated by the Earth's magnetic field.

Wheeler Gift 3242 (for the 1861 paper).

Four 'Society of Arts Examinations' papers, three of which are folded folio broadsides dating from 1858 (Arithmetic, Mensuration, Algebra), the fourth, a two-page 8vo, from 1863 (Arithmetic). Tears to folds.

"Faraday was elected a member of the Society of Arts for the Encouragement of Manufactures (now dignified by the prefix Royal) in 1819. In subsequent years he was joined by a number of other members of the City Philosophical Society as that society disbanded. Although he remained a member of the Society of Arts throughout his life he was most active in the 1820s and 1830s when he served on several occasions as chairman of the Chemistry Committee. In this position he was called to consider the merits, or otherwise, of new chemical processes and their application to industry" (Cantor, Gooding & Fames, *Faraday* (1991), p. 38). The Society awarded prizes, called 'premiums', for inventions of particular merit. This practice laid the foundations for developing a more comprehensive system of examinations, particularly in association with the Mechanics' Institutions movement. The first examinations for artisans were held in 1855. Candidates had to sit at least three subjects and a preliminary/qualifying examination in handwriting and spelling. In subsequent years the examinations were extended from London to 40 centres around the UK. The Board of Examiners, which included a number of influential individuals such as Thomas Huxley, was replaced in 1857 by the Council of the Society of Arts, which then established a system of paid examiners. The Society offered the first national public examinations in 1882. This led to the formation of the RSA Examinations Board, which now forms part of the Oxford, Cambridge and RSA Examinations Board (OCR).COPAC lists copies of these examinations at BL and V&A only; no copies in auction records.
On Kirkman, see Biggs, 'T. P. Kirkman, Mathematician,' *Bulletin of the London Mathematical Society*, vol. 13 (1981), pp. 97-120.

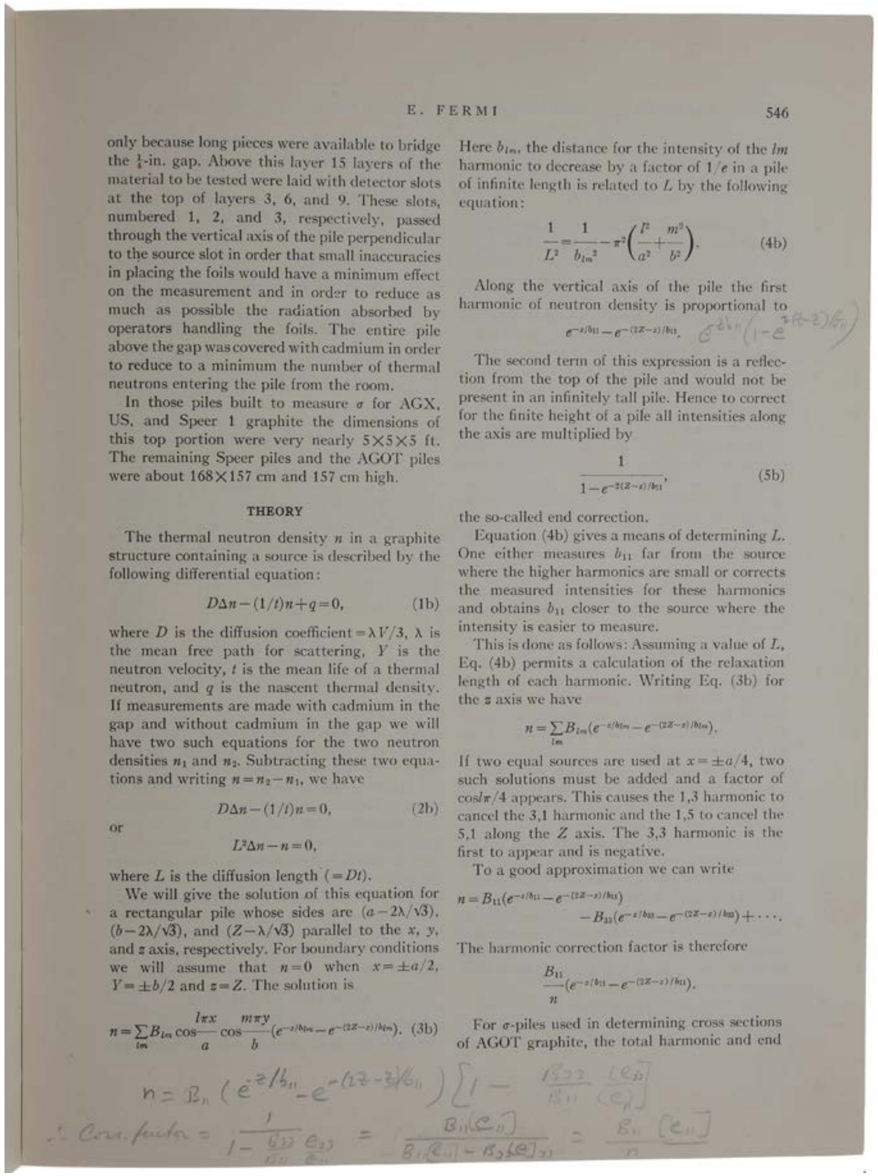
ANNOTATED BY FERMI'S ASSISTANT

FERMI, Enrico. *'Experimental Production of a Divergent Chain Reaction.' Offprint from: American Journal of Physics, Vol. 20, No. 9.* Lancaster, PA: Lancaster Press, 1952.

$9,500

Quarto (267 x 197mm). Illustrations in text. Original printed wrappers. Small closed tear to first text leaf, otherwise in very fine condition.

First published edition, very rare author's presentation offprint, and an important association copy, of Fermi's report describing the first controlled nuclear chain reaction. "Within weeks of his arrival [at Columbia University], news that uranium could fission astounded the physics community … The implications were both exciting and ominous, and they were recognized widely. When uranium fissioned, some mass was converted to energy, according to Albert Einstein's famous formula $E = mc^2$. Uranium also emitted a few neutrons in addition to the larger fragments. If these neutrons could be slowed to maximize their efficiency, they could participate in a controlled chain reaction to produce energy; that is, a nuclear reactor could be built. The same neutrons traveling at their initial high speed could also participate in an uncontrolled chain reaction, liberating an enormous amount of energy through many generations of fission events, all within a fraction of a second; that is, an atomic bomb could be built … Fermi had built a series of 'piles,' as he called them, at Columbia. Now he moved to the University of Chicago, where he continued to construct piles in a space under the stands of the football field. The final structure, a flattened sphere about 7.5 metres (25 feet) in diameter, contained 380 tons of graphite blocks as the moderator and 6 tons of uranium metal and 40 tons of uranium oxide as



E. FERMI 546

only because long pieces were available to bridge the $\frac{1}{4}$-in. gap. Above this layer 15 layers of the material to be tested were laid with detector slots at the top of layers 3, 6, and 9. These slots, numbered 1, 2, and 3, respectively, passed through the vertical axis of the pile perpendicular to the source slot in order that small inaccuracies in placing the foils would have a minimum effect on the measurement and in order to reduce as much as possible the radiation absorbed by operators handling the foils. The entire pile above the gap was covered with cadmium in order to reduce to a minimum the number of thermal neutrons entering the pile from the room.

In those piles built to measure σ for AGX, US, and Speer 1 graphite the dimensions of this top portion were very nearly 5×5×5 ft. The remaining Speer piles and the AGOT piles were about 168×157 cm and 157 cm high.

THEORY

The thermal neutron density n in a graphite structure containing a source is described by the following differential equation:

$$D\Delta n-(1/t)n+q=0, \qquad (1b)$$

where D is the diffusion coefficient $=\lambda V/3$, λ is the mean free path for scattering, V is the neutron velocity, t is the mean life of a thermal neutron, and q is the nascent thermal density. If measurements are made with cadmium in the gap and without cadmium in the gap we will have two such equations for the two neutron densities n_1 and n_2. Subtracting these two equations and writing $n=n_2-n_1$, we have

$$D\Delta n-(1/t)n=0, \qquad (2b)$$

or

$$L^2\Delta n-n=0,$$

where L is the diffusion length $(=Dt)$.

We will give the solution of this equation for a rectangular pile whose sides are $(a-2\lambda/\sqrt{3})$, $(b-2\lambda/\sqrt{3})$, and $(Z-\lambda/\sqrt{3})$ parallel to the x, y, and z axis, respectively. For boundary conditions we will assume that $n=0$ when $x=\pm a/2$, $Y=\pm b/2$ and $z=Z$. The solution is

$$n=\sum_{lm} B_{lm}\cos\frac{l\pi x}{a}\cos\frac{m\pi y}{b}(e^{-z/b_{lm}}-e^{-(2Z-z)/b_{lm}}). \qquad (3b)$$

Here b_{lm}, the distance for the intensity of the lm harmonic to decrease by a factor of $1/e$ in a pile of infinite length is related to L by the following equation:

$$\frac{1}{L^2}=\frac{1}{b_{lm}^2}-\pi^2\left(\frac{l^2}{a^2}+\frac{m^2}{b^2}\right). \qquad (4b)$$

Along the vertical axis of the pile the first harmonic of neutron density is proportional to

$$e^{-z/b_{11}}-e^{-(2Z-z)/b_{11}}.$$

The second term of this expression is a reflection from the top of the pile and would not be present in an infinitely tall pile. Hence to correct for the finite height of a pile all intensities along the axis are multiplied by

$$\frac{1}{1-e^{-2(Z-z)/b_{11}}}, \qquad (5b)$$

the so-called end correction.

Equation (4b) gives a means of determining L. One either measures b_{11} far from the source where the higher harmonics are small or corrects the measured intensities for these harmonics and obtains b_{11} closer to the source where the intensity is easier to measure.

This is done as follows: Assuming a value of L, Eq. (4b) permits a calculation of the relaxation length of each harmonic. Writing Eq. (3b) for the z axis we have

$$n=\sum_{lm} B_{lm}(e^{-z/b_{lm}}-e^{-(2Z-z)/b_{lm}}).$$

If two equal sources are used at $x=\pm a/4$, two such solutions must be added and a factor of $\cos l\pi/4$ appears. This causes the 1,3 harmonic to cancel the 3,1 harmonic and the 1,5 to cancel the 5,1 along the Z axis. The 3,3 harmonic is the first to appear and is negative.

To a good approximation we can write

$$n=B_{11}(e^{-z/b_{11}}-e^{-(2Z-z)/b_{11}})-B_{33}(e^{-z/b_{33}}-e^{-(2Z-z)/b_{33}})+\cdots.$$

The harmonic correction factor is therefore

$$\frac{B_{11}}{n}(e^{-z/b_{11}}-e^{-(2Z-z)/b_{11}}).$$

For σ-piles used in determining cross sections of AGOT graphite, the total harmonic and end the fuel, distributed in a careful pattern. The pile went 'critical' on Dec. 2, 1942, proving that a nuclear reaction could be initiated, controlled, and stopped. Chicago Pile-1, as it was called, was the first prototype for several large nuclear reactors constructed at Hanford, Wash., where plutonium, a man-made element heavier than uranium, was produced. Plutonium also could fission and thus was another route to the atomic bomb" (Britannica). The original site at the University of Chicago was designated a national historic landmark on October 15, 1966, and a city landmark in 1971. The plaque inscription reads, "On December 2, 1942, man achieved here the first self-sustaining chain reaction and thereby initiated the controlled release of nuclear energy." It was based upon this work that Fermi was sent to Los Alamos to work under the direction of J. Robert Oppenheimer to assemble the atomic bomb. Fermi's original classified report for the Metallurgical Laboratory of the University of Chicago had been written almost ten years earlier. Fermi "was the only physicist in the twentieth century who excelled in both theory and experiment, and he was one of the most versatile" (DSB). No other copy in auction records.

Provenance: 1. George Leon Weil (1907-95), Fermi's assistant (signature on front wrapper, a few pencil annotations in text). "On December 2, 1942, he [i.e., Weil] removed the control rod from the Chicago Pile-1 nuclear reactor, initiating the first man-made, self-sustaining nuclear chain reaction" (Wikipedia), while Fermi monitored the neutron activity. 2. Harvey Plotnick, publisher (sale of his library, Christie's, New York, 4 October 2002, lot 113, $1016).

Fermi has described the experiment and its origins in his autobiography *Fermi's Own Story*: "The year was 1939. A world war was about to start. The new possibilities appeared likely to be important, not only for peace, but also for war. A group of physicists in the United States—including Leo Szilard, Walter Zinn, now director of Argonne National Laboratory, Herbert Anderson, and myself—agreed privately to delay further publications of findings in this field.

"We were afraid these findings might help the Nazis. Our action, of course, represented a break with scientific tradition and was not taken lightly. Subsequently, when the government became interested in the atom bomb project, secrecy became compulsory. Here it may be well to define what is meant by the 'chain reaction,' which was to constitute our next objective in the search for a method of utilizing atomic energy.

"An atomic chain reaction may be compared to the burning of a rubbish pile from spontaneous combustion. In such a fire, minute parts of the pile start to burn, and in turn ignite other tiny fragments. When sufficient numbers of these fractional parts are heated to the kindling points, the entire heap bursts into flames. A similar process takes place in an atomic pile, such as was constructed under the West Stands of Stagg Field at the University of Chicago in 1942.

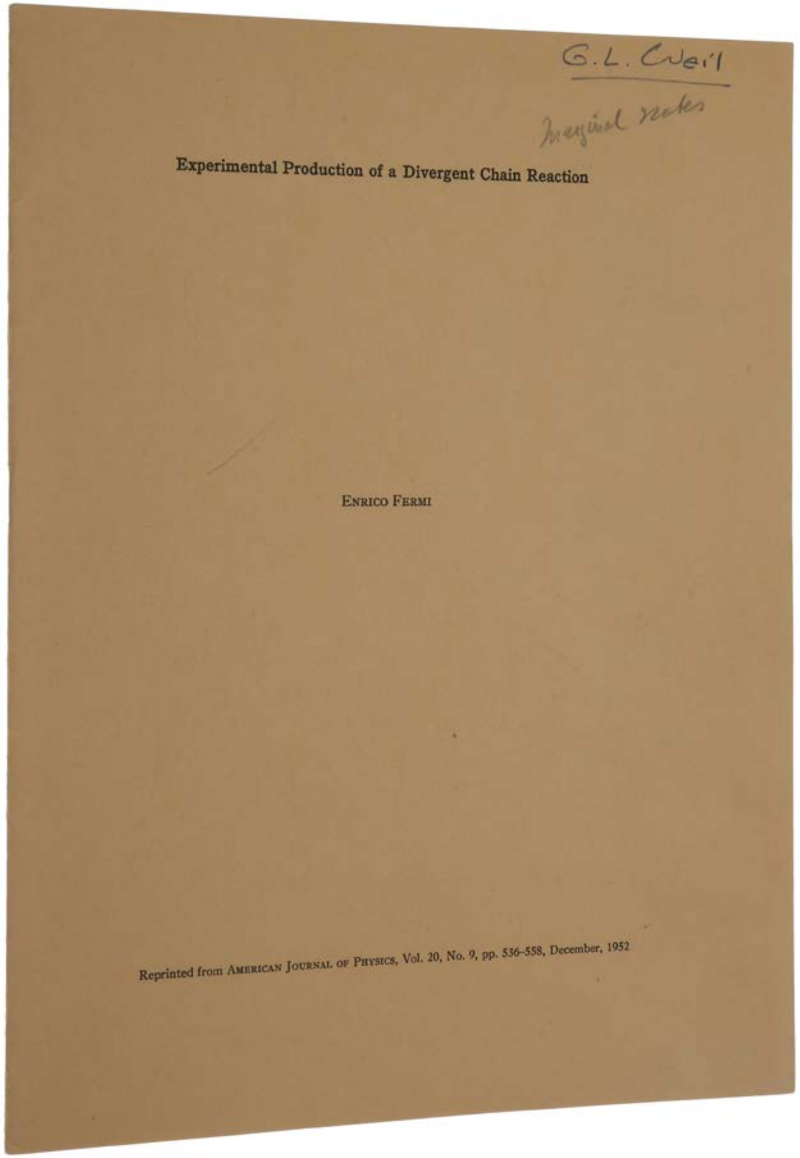
"The pile itself was constructed of uranium, a material that is embedded in a matrix of graphite. With sufficient uranium in the pile, the few neutrons emitted in a single fission that may accidentally occur strike neighboring atoms, which in turn undergo fission and produce more neutrons. These bombard other atoms and so on at an increasing rate until the atomic 'fire' is going full blast. The atomic pile is controlled and prevented from burning itself to complete destruction by cadmium rods, which absorb neutrons and stop the bombardment process. The same effect might be achieved by running a pipe of cold water through a rubbish heap; by keeping the temperature low, the pipe would prevent the spontaneous burning.

"The first atomic chain reaction experiment was designed to proceed at a slow rate. In this sense, it differed from the atomic bomb, which was designed to proceed at as fast a rate as was possible. Otherwise, the basic process is similar to that of the atomic bomb. The atomic chain reaction was the result of hard work by many hands and many heads. Arthur H. Compton, Walter Zinn, Herbert Anderson, Leo Szilard, Eugene Wigner, and many others worked directly on the problems at the University of Chicago. Very many experiments and calculations had to be performed. Finally, a plan was decided upon.

"Thirty 'piles' of less than the size necessary to establish a chain reaction were built and tested. Then the plans were made for the final test of a full-sized pile. The scene of this test at the University of Chicago would have been confusing to an outsider—if he could have eluded the security guards and gained admittance. He would have seen only what appeared to be a crude pile of black bricks and wooden timbers. All but one side of the pile was obscured by a balloon cloth envelope.

"As the pile grew toward its final shape during the days of preparation, the measurement performed many times a day indicated everything was going, if anything, a little bit better than predicted by calculations. Finally, the day came when we were ready to run the experiment. We gathered on a balcony about 10 feet above the floor of the large room in which the structure had been erected. Beneath us was a young scientist, George Weil, whose duty it was to handle the last control rod that was holding the reaction in check.

"Every precaution had been taken against an accident. There were three sets of control rods in the pile. One set was automatic. Another consisted of a heavily weighted emergency safety held by a rope. Walter Zinn was holding the rope ready to release it at the least sign of trouble. The last rod left in the pile, which acted as starter, accelerator, and brake for the reaction, was the one handled by Weil. Since the experiment had never been tried before, a 'liquid control squad' stood ready to flood the pile with cadmium salt solution in case the control rods



failed. Before we began, we rehearsed the safety precautions carefully.

"Finally, it was time to remove the control rods. Slowly, Weil started to withdraw the main control rod. On the balcony, we watched the indicators which measured the neutron count and told us how rapidly the disintegration of the uranium atoms under their neutron bombardment was proceeding. At 11:35 a.m., the counters were clicking rapidly. Then, with a loud clap, the automatic control rods slammed home. The safety point had been set too low.

"It seemed a good time to eat lunch. During lunch everyone was thinking about the experiment but nobody talked much about it. At 2:30, Weil pulled out the control rod in a series of measured adjustments. Shortly after, the intensity shown by the indicators began to rise at a slow but ever-increasing rate. At this moment we knew that the self-sustaining reaction was under way. The event was not spectacular, no fuses burned, no lights flashed. But to us it meant that release of atomic energy on a large scale would be only a matter of time. The further development of atomic energy during the next three years of the war was, of course, focused on the main objective of producing an effective weapon.

"At the same time we all hoped that with the end of the war emphasis would be shifted decidedly from the weapon to the peaceful aspects of atomic energy. We hoped that perhaps the building of power plants, production of radioactive elements for science and medicine would become the paramount objectives. Unfortunately, the end of the war did not bring brotherly love among nations. The fabrication of weapons still is and must be the primary concern of the Atomic Energy Commission.

"Secrecy that we thought was an unwelcome necessity of the war still appears to be an unwelcome necessity. The peaceful objectives must come second, although very considerable progress has been made also along those lines. The problems posed by this world situation are not for the scientist alone but for all people to resolve. Perhaps a time will come when all scientific and technical progress will be hailed for the advantages that it may bring to man, and never feared on account of its destructive possibilities."

Enrico Fermi (1901-54) was born in Rome. "Fermi's first important scientific contribution was a 1922 paper in an Italian journal where he first introduced the concept of 'Fermi coordinates'– coincidentally the same year he graduated from Scuola Normale Superiore. He went on to study at the University of Göttingen and the University of Florence before becoming a professor at the University of Italy [i.e., the Sapienza University of Rome] at the tender age of 24.

"The year before, Fermi had been writing an appendix for the Italian translation of A. Kopff's *The Mathematical Theory of Relativity*. He realized that Albert Einstein's most famous equation ($E = mc^2$) implied a very large amount of potential nuclear energy that might conceivably be made available under the right experimental conditions. He pursued that avenue of research enthusiastically while in Rome with a small group of colleagues that included Emilio Segrè. They earned the moniker 'the Via Panisperna boys' after the street on which the labs were located. Among their many seminal contributions was the discovery of so-called slow neutrons and their effect on various elements.

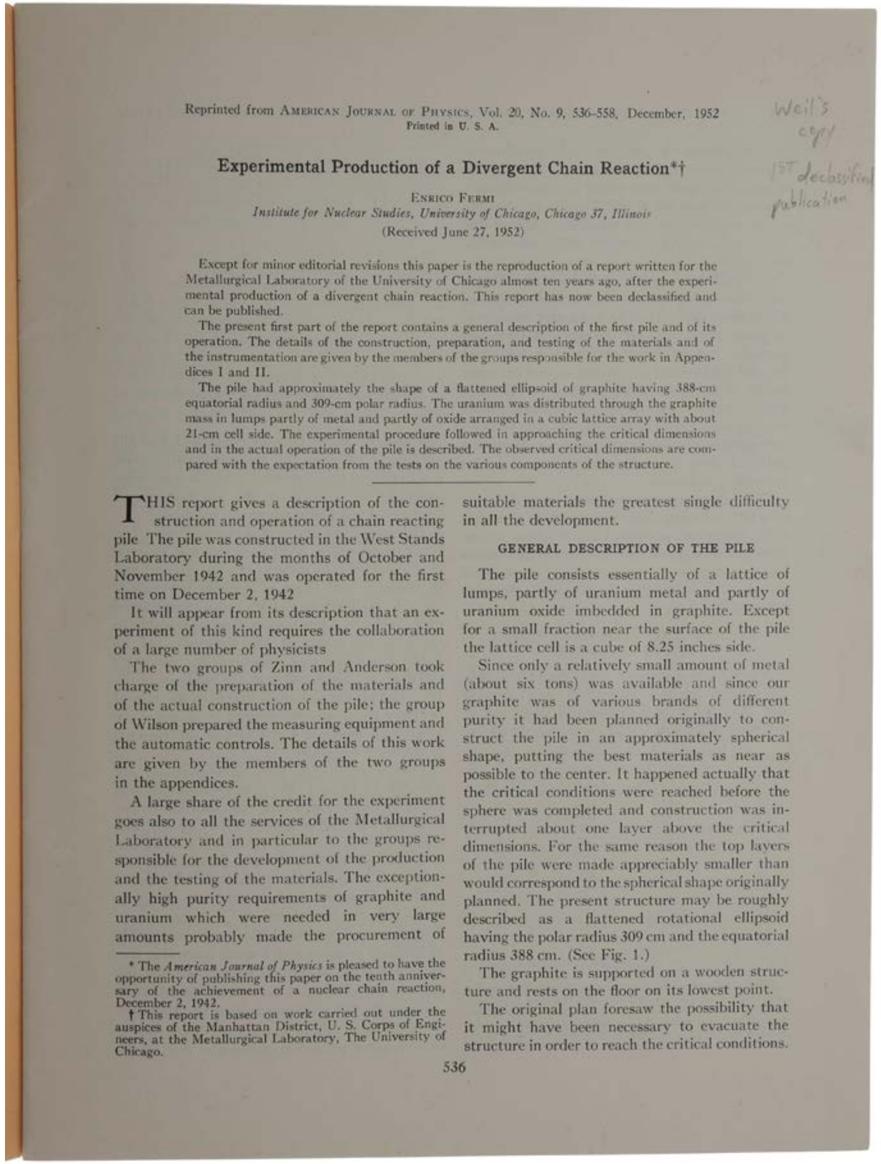
"Ultimately, Fermi won the 1938 Nobel Prize in Physics for his 'demonstrations of the existence of new radioactive elements produced by neutron irradiation, and for his related discovery of nuclear reactions brought about by slow neutrons.' Fermi took advantage of the award ceremony in Stockholm to emigrate to the United States, concerned about the safety of his Jewish wife, Laura, under Mussolini's Fascist regime–specifically the newly instituted Manifesto of Race"

('December 2, 1942: First self-sustained nuclear chain reaction,' *APS News*, vol. 20, no. 11, December 2011).

"Soon after his arrival in the United States, Fermi became instrumental in sparking the U. S. government's interest in developing atomic energy for military purposes. Working with other physicists at the Metallurgical Laboratory in Chicago, which had been established for precisely this reason, he led a team to design and construct an exponential pile (sometimes referred to as Chicago Pile 1) in an empty room (formerly a squash court) under one of the university's athletics fields. The first-ever controlled self-sustaining nuclear chain reaction took place in that pile on December 2, 1942. During the subsequent two years, Fermi conducted various experiments using the reactor, and assisted in the development of a larger reactor at the nearby Argonne Laboratory.

"Following a brief period during which he worked primarily with the DuPont Company developing plutonium on an industrial scale, Fermi moved his family in August 1944 to Los Alamos, New Mexico, where the War Department's top-secret Manhattan Project to develop the atomic bomb was already well underway. As Associate Director of the project, he oversaw all experimental physics projects already in progress, conducted a few experiments on neutron cross section measurements, and observed the first [Trinity] test of the bomb at Alamogordo, New Mexico. Fermi then served as one of four scientific consultants to President Truman's advisory Interim Committee, which, after lengthy deliberation, recommended that the bomb be used for military purposes" (fermi.lib.uchicago.edu/fermibiog.htm).

George Weil was born in New York City. He entered Harvard College, from which he graduated in 1939 and then Columbia University, where he earned his master's degree, and later his doctorate. At Columbia, Weil became involved in Fermi's



Reprinted from American Journal of Physics, Vol. 20, No. 9, 536-558, December, 1952
Printed in U. S. A.

Weil's copy

1st declassified publication

Experimental Production of a Divergent Chain Reaction*†

Enrico Fermi
Institute for Nuclear Studies, University of Chicago, Chicago 37, Illinois
(Received June 27, 1952)

Except for minor editorial revisions this paper is the reproduction of a report written for the Metallurgical Laboratory of the University of Chicago almost ten years ago, after the experimental production of a divergent chain reaction. This report has now been declassified and can be published.

The present first part of the report contains a general description of the first pile and of its operation. The details of the construction, preparation, and testing of the materials and of the instrumentation are given by the members of the groups responsible for the work in Appendices I and II.

The pile had approximately the shape of a flattened ellipsoid of graphite having 388-cm equatorial radius and 309-cm polar radius. The uranium was distributed through the graphite mass in lumps partly of metal and partly of oxide arranged in a cubic lattice array with about 21-cm cell side. The experimental procedure followed in approaching the critical dimensions and in the actual operation of the pile is described. The observed critical dimensions are compared with the expectation from the tests on the various components of the structure.

THIS report gives a description of the construction and operation of a chain reacting pile The pile was constructed in the West Stands Laboratory during the months of October and November 1942 and was operated for the first time on December 2, 1942

It will appear from its description that an experiment of this kind requires the collaboration of a large number of physicists

The two groups of Zinn and Anderson took charge of the preparation of the materials and of the actual construction of the pile; the group of Wilson prepared the measuring equipment and the automatic controls. The details of this work are given by the members of the two groups in the appendices.

A large share of the credit for the experiment goes also to all the services of the Metallurgical Laboratory and in particular to the groups responsible for the development of the production and the testing of the materials. The exceptionally high purity requirements of graphite and uranium which were needed in very large amounts probably made the procurement of suitable materials the greatest single difficulty in all the development.

GENERAL DESCRIPTION OF THE PILE

The pile consists essentially of a lattice of lumps, partly of uranium metal and partly of uranium oxide imbedded in graphite. Except for a small fraction near the surface of the pile the lattice cell is a cube of 8.25 inches side.

Since only a relatively small amount of metal (about six tons) was available and since our graphite was of various brands of different purity it had been planned originally to construct the pile in an approximately spherical shape, putting the best materials as near as possible to the center. It happened actually that the critical conditions were reached before the sphere was completed and construction was interrupted about one layer above the critical dimensions. For the same reason the top layers of the pile were made appreciably smaller than would correspond to the spherical shape originally planned. The present structure may be roughly described as a flattened rotational ellipsoid having the polar radius 309 cm and the equatorial radius 388 cm. (See Fig. 1.)

The graphite is supported on a wooden structure and rests on the floor on its lowest point.

The original plan foresaw the possibility that it might have been necessary to evacuate the structure in order to reach the critical conditions.

* The *American Journal of Physics* is pleased to have the opportunity of publishing this paper on the tenth anniversary of the achievement of a nuclear chain reaction, December 2, 1942.

† This report is based on work carried out under the auspices of the Manhattan District, U. S. Corps of Engineers, at the Metallurgical Laboratory, The University of Chicago.

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efforts to build a nuclear reactor. Construction commenced in November 1942 and was completed on December 2, 1942. Weil worked the final control rod, while Fermi monitored the neutron activity. Weil continued to work in reactor development and in April 1945, went to the Los Alamos laboratory, where he worked on the Trinity nuclear test. After the war, Weil worked for General Electric on reactor design. He then joined the Atomic Energy Commission (AEC), where he became the assistant director of the Reactor Development Division. "In 1952, Dr Weil resigned from the AEC, to become a consultant. But he grew more and more alarmed at the rush to turn nuclear technology over to private industry before the technology was fully understood, and he found himself increasingly lined up in opposition to his old organisation" ('George Weil – from activator to activist,' *New Scientist*, 30 November 1972).

bricks were machined to a cross section of $4\frac{1}{8} \times 4\frac{1}{8}$ in. and were cut to a length of $16\frac{1}{2}$ in. The structure was planned as a sphere of maximum radius of 13 ft; the choice of a sphere being necessary because of the fact that probably not sufficient material would be available for any other shape which would be chain reacting. The decision to build a sphere necessitated two important additions to the structure. The first of these was a wooden framework in which the sphere was inscribed, and secondly, a graphite pier which supports the side of the sphere through which the control rods pass. It was believed to be entirely possible that after the structure was erected that the wood might warp or shrink and cause some displacement of the graphite above it. Since this would be undesirable from the point of view of passing control rods into the pile, that part of the pile through which the rods pass is entirely supported by this graphite pier. Originally it had been intended to evacuate the pile and, therefore, considerable pains were taken to see that the wooden framework fitted the graphite securely and that it presented a smooth continuous surface to the surrounding balloon cloth envelope. It turned out, however, that evacuation was not necessary and, therefore, these details are unimportant.

The cube in which the sphere is inscribed has a side of 24 ft 2 in. From this it follows that a part of the 26-ft diameter sphere is cut off on the sides; these parts represent a rather small percentage of the total volume of the sphere. As planned the sphere was to have a shell 1 in. thick on the outside made up of graphite without uranium or so-called dead graphite. The graphite-uranium lattice was expected to occupy a sphere of 12-ft radius and have a total volume of 7200 cu ft and hence about 22,300 cells were expected to be included in the structure.

As has been indicated earlier not all of the material available was of uniform quality and in order to use this material most efficiently that of highest quality was placed at the center with the less reactive types arranged in concentric shells; the quality decreasing outward from the center. The 26-ft diameter sphere would have required that about 75 layers of the $4\frac{1}{8}$-in. thick bricks would be piled up; however, the chain-reacting condition was reached at the 57th layer.

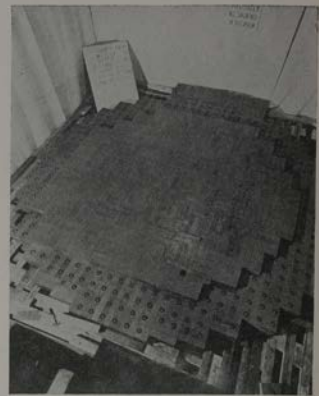


Fig. 5. A layer of the pile.

The actual amount of graphite in the pile is indicated in Table VIII in which also the amounts of each brand are given.

The US and AGX brands had dimensions somewhat different than the majority of the graphite and since they are of lower quality they were mostly used in the outer shell of dead graphite.

In Table IX the details of the uranium lumps are given. Column 1 gives the geometrical form of the lump.

The designation $3\frac{1}{4}$-in. pseudosphere indicates pressings which were cylinders of $3\frac{1}{4}$ in. diameter and $3\frac{1}{4}$ in. height, but which had the edges cut off at 45° so that they were roughly spherical. The designation 3-in. cylinder means a cylinder of height and diameter of 3 in. Since five varieties of uranium lumps and four brands of graphite

Table VIII. Graphite in pile.

Source	Brand	Lbs
National Carbon Co.	AGOT	510,000
Speer Graphite Co.		145,000
U. S. Graphite Co.	U. S.	32,000
National Carbon Co.	AGX	60,000
AGX+Speer (Pier only)		24,000
		771,000 = 385.5 tons

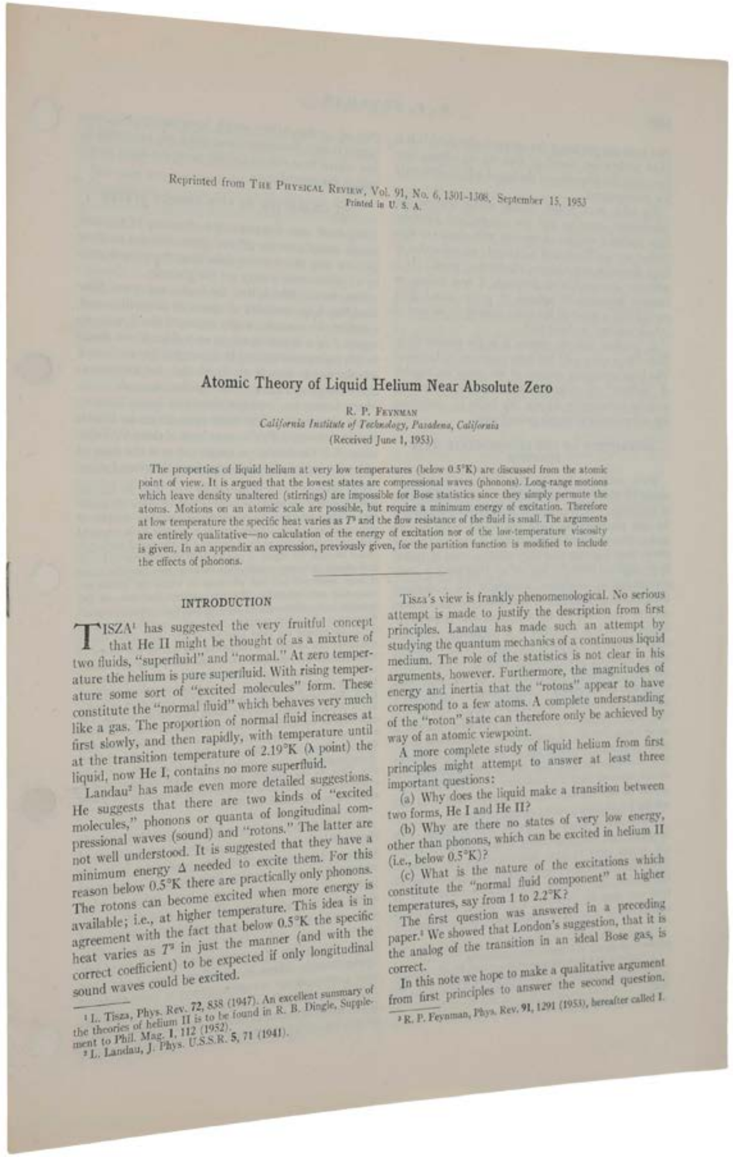
FEYNMAN EXPLAINS SUPERFLUIDITY

FEYNMAN, Richard Phillips. *Atomic theory of liquid helium near absolute zero. Offprint from Physical Review, Vol. 91, No. 6, September 15, 1953.* [Lancaster, PA and New York, NY: American Institute of Physics, 1953.

$6,500

4to (266 x 200 mm), pp. 1301-1308. Self-wrappers, stapled as issued (punch holes in inner margin filled, not affecting text).

First edition, extremely rare offprint, of the first of Feynman's important papers which provided a quantum mechanical explanation of the superfluidity of liquid helium at temperatures below the 'lambda-point' of 2.18K. "The dramatic announcement of superfluidity of liquid He^4 in 1938 is one of the defining moments in modern physics" (Griffin, p. 1). In 1938, Pyotr Kapitza in Moscow and John Allen and Donald Misener in Cambridge (UK) discovered independently that, at sufficiently low temperatures, liquid He^4 has zero viscosity. Phenomenological theories of this property of superfluidity were developed by Fritz London and Laszlo Tisza before World War II, and by Lev Landau in the 1940s. Successful as these theories were, they lacked an atomistic foundation. "Between 1953 and 1958, Feynman published a seminal series of papers on the atomic theory of superfluid helium … A significant part of Feynman's central contribution was the demonstration that these phenomenological concepts arose directly from the fundamental quantum mechanics of interacting bosonic atoms with strong repulsive cores. One of his earliest helium papers [offered here] showed in detail how the symmetric character of the many-body wave function severely restricts the allowed class of low-lying excited states" (*Selected Papers of Richard Feynman*



Reprinted from The Physical Review, Vol. 91, No. 6, 1301-1308, September 15, 1953
Printed in U. S. A.

Atomic Theory of Liquid Helium Near Absolute Zero

R. P. Feynman
California Institute of Technology, Pasadena, California
(Received June 1, 1953)

The properties of liquid helium at very low temperatures (below 0.5°K) are discussed from the atomic point of view. It is argued that the lowest states are compressional waves (phonons). Long-range motions which leave density unaltered (stirrings) are impossible for Bose statistics since they simply permute the atoms. Motions on an atomic scale are possible, but require a minimum energy of excitation. Therefore at low temperature the specific heat varies as T^3 and the flow resistance of the fluid is small. The arguments are entirely qualitative—no calculation of the energy of excitation nor of the low-temperature viscosity is given. In an appendix an expression, previously given, for the partition function is modified to include the effects of phonons.

INTRODUCTION

Tisza[1] has suggested the very fruitful concept that He II might be thought of as a mixture of two fluids, "superfluid" and "normal." At zero temperature the helium is pure superfluid. With rising temperature some sort of "excited molecules" form. These constitute the "normal fluid" which behaves very much like a gas. The proportion of normal fluid increases at first slowly, and then rapidly, with temperature until at the transition temperature of 2.19°K (λ point) the liquid, now He I, contains no more superfluid.

Landau[2] has made even more detailed suggestions. He suggests that there are two kinds of "excited molecules," phonons or quanta of longitudinal compressional waves (sound) and "rotons." The latter are not well understood. It is suggested that they have a minimum energy Δ needed to excite them. For this reason below 0.5°K there are practically only phonons. The rotons can become excited when more energy is available; i.e., at higher temperature. This idea is in agreement with the fact that below 0.5°K the specific heat varies as T^3 in just the manner (and with the correct coefficient) to be expected if only longitudinal sound waves could be excited.

Tisza's view is frankly phenomenological. No serious attempt is made to justify the description from first principles. Landau has made such an attempt by studying the quantum mechanics of a continuous liquid medium. The role of the statistics is not clear in his arguments, however. Furthermore, the magnitudes of energy and inertia that the "rotons" appear to have correspond to a few atoms. A complete understanding of the "roton" state can therefore only be achieved by way of an atomic viewpoint.

A more complete study of liquid helium from first principles might attempt to answer at least three important questions:

(a) Why does the liquid make a transition between two forms, He I and He II?

(b) Why are there no states of very low energy, other than phonons, which can be excited in helium II (i.e., below 0.5°K)?

(c) What is the nature of the excitations which constitute the "normal fluid component" at higher temperatures, say from 1 to 2.2°K?

The first question was answered in a preceding paper.[3] We showed that London's suggestion, that it is the analog of the transition in an ideal Bose gas, is correct.

In this note we hope to make a qualitative argument from first principles to answer the second question.

[1] L. Tisza, Phys. Rev. **72**, 838 (1947). An excellent summary of the theories of helium II is to be found in R. B. Dingle, Supplement to Phil. Mag. **1**, 112 (1952).
[2] L. Landau, J. Phys. U.S.S.R. **5**, 71 (1941).
[3] R. P. Feynman, Phys. Rev. **91**, 1291 (1953), hereafter called I.

(2000), p. 313). 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." No copies in auction records, or on OCLC.

Helium was first liquefied by the Dutch physicist Heike Kamerlingh Onnes (1853–1926) in 1908, who is best known for his discovery three years later of superconductivity in mercury at very low temperatures. "Kamerlingh Onnes also observed the transition to the related phenomenon of superfluidity in liquid helium in an experiment performed in 1908, without recognizing it. The nominal discovery of superfluidity came in 1937 when Pyotr Kapitza in the Soviet Union and John Allen and Donald Misener at Cambridge independently discovered it. Three papers were published, one after the other, in *Nature* on January 8, 1938. Appallingly, when Kapitsa was awarded the Nobel Prize in 1978, no mention was made of Allen's simultaneous discovery, probably because of the dominance of Kapitsa's group after the war" (Purrington, p. 337). (See Griffin for a detailed analysis of the relation between the work of Kapitsa and Allen & Misener.)

"The most spectacular signature of the transition of liquid ^{4}He into the superfluid phase is the sudden onset of the ability to flow without apparent friction through capillaries so small that any ordinary liquid (including ^{4}He itself above the lambda transition) would be clamped by its viscosity; thus, a vessel that was 'helium-tight' in the so-called normal phase (i.e., above the lambda temperature) might suddenly spring leaks below it. Related phenomena observed in the superfluid phase include the ability to sustain persistent currents in a ring-shaped container; the phenomenon of film creep, in which the liquid flows without apparent friction up and over the side of a bucket containing it; and a thermal conductivity that is millions of times its value in the normal phase and greater than that of the best metallic conductors. Another property is less spectacular but is extremely significant for an understanding of the superfluid phase: if the liquid is cooled through the lambda transition in a bucket that is slowly rotating, then, as the temperature decreases toward absolute zero, the liquid appears gradually to come to rest with respect to the laboratory even though the bucket continues to rotate. This non-rotation effect is completely reversible; the apparent velocity of rotation depends only on the temperature and not on the history of the system" (Britannica).

The papers of Kapitsa and Allen & Misener "stimulated feverish activity in the period leading up to World War II, and in the 1950s developed into a major research area called 'quantum fluids.' The phenomenon of flow without any measurable viscosity suggested that liquid He^4 below the transition temperature of 2.18K was some strange new phase of matter. Within a few weeks after the discovery, Fritz London [and Laszlo Tisza] suggested that this new phase might have some connection with the phenomenon of Bose-Einstein condensation (BEC). This was originally predicted by Einstein to occur in an ideal gas of atoms in a 1925 paper, but this had been largely discounted as wrong over the next decade" (Griffin, p. 1). Landau rejected the description of He^4 below the lambda-point as an ideal Bose-Einstein gas, and proposed instead to derive the properties of the superfluid from a consistent quantum-mechanical approach to a fluid. His phenomenological 'two-fluid' model of superfluidity, published in 1941, led to his award of the Nobel Prize for Physics in 1962.

Then, in the spring of 1953, "Richard Feynman entered the scene. He set himself the task of providing a theoretical understanding of the problem of liquid helium on an atomic basis, which could only be done if one approached the problem from first principles. While he greatly admired Landau's contributions to and successes in the field, Feynman pointed out several weaknesses in Landau's theory. Notably, Landau's quantum hydrodynamical approach treated Helium II [i.e., He^4 below the lambda-point] as a continuous medium which right from the beginning sacrificed the atomic structure of the liquid and thus forestalled the possibility of calculating the various characteristics of the system, such as the various parameters, on an atomic basis … By writing 'the exact partition function as an integral over trajectories, using the space-time approach to quantum mechanics', Feynman could indeed derive a Landau-type energy spectrum [in the present paper]" (Mehra & Rechenberg, *The Historical Development of Quantum Theory*, Vol. 6, p. 1160).

In the abstract of the present paper, Feynman writes: "The properties of liquid helium at very low temperatures (below 0.5°K) are discussed from the atomic point of view. It is argued that the lowest states are compressional waves (phonons). Long-range motions which leave density unaltered (stirrings) are impossible for Bose statistics since they simply permute the atoms. Motions on an atomic scale are possible, but require a minimum energy of excitation. Therefore at low temperature the specific heat varies as T^3 and the flow resistance of the fluid is small."

Feynman's only earlier paper on superfluid helium ['Atomic theory of the lambda transition in liquid helium,' *Physical Review* 91, pp. 1291-1301] dealt with the *transition* at the lambda-point "which signals the formation of a new phase Helium II (and the onset of superfluidity); this paper is necessarily quite different from those focussing on the low temperature behaviour" (*Selected Papers*, p. 314). Thus, this earlier paper was devoted to an explanation of the transition itself, rather than of the superfluid behaviour below the lambda-point.

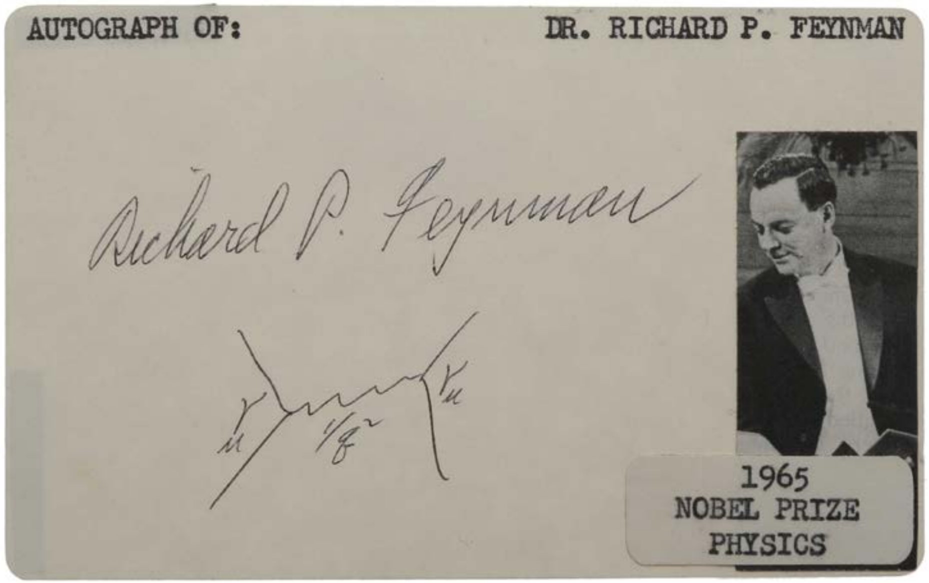
"Today, it is generally accepted that the phenomenon of Bose-condensation underlies all the unusual properties of superfluid He^4, superconductivity in metals, and superfluidity of liquid He^3. More recently, the achievement of BEC in an ultra-cold trapped gas of Bose atoms has opened up a whole new field of research into the superfluid properties and the coherent matter waves which arise in atomic gases. All this work, lying at the core of modern condensed matter and atomic physics, has put renewed emphasis on the historical importance of the dramatic and unexpected discovery of the superfluid behaviour in liquid He^4 in the last months of 1937" (Griffin, p. 1).

The only other offprints of Feynman's *Physical Review* papers we have ever seen on the market are two (on a different subject) we ourselves handled a few years ago. The present offprint derives from the estate of an officer of the Press Office of the Physics Department at Caltech, where Feynman spent most of his career.
Griffin, The discovery of superfluidity: a chronology of events in 1935-1938 (http://www.pi5.uni-stuttgart.de/lectures/89/Superfluidity%20article%202.pdf).

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).

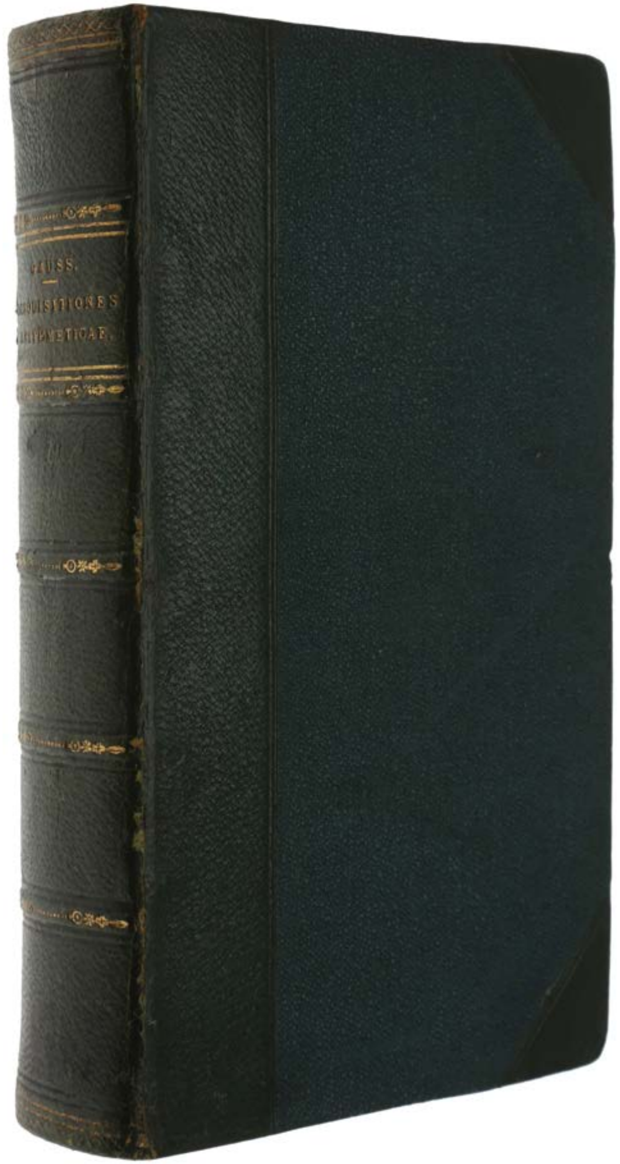
PMM 257 - 'THE PRINCE OF MATHEMATICS'

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.

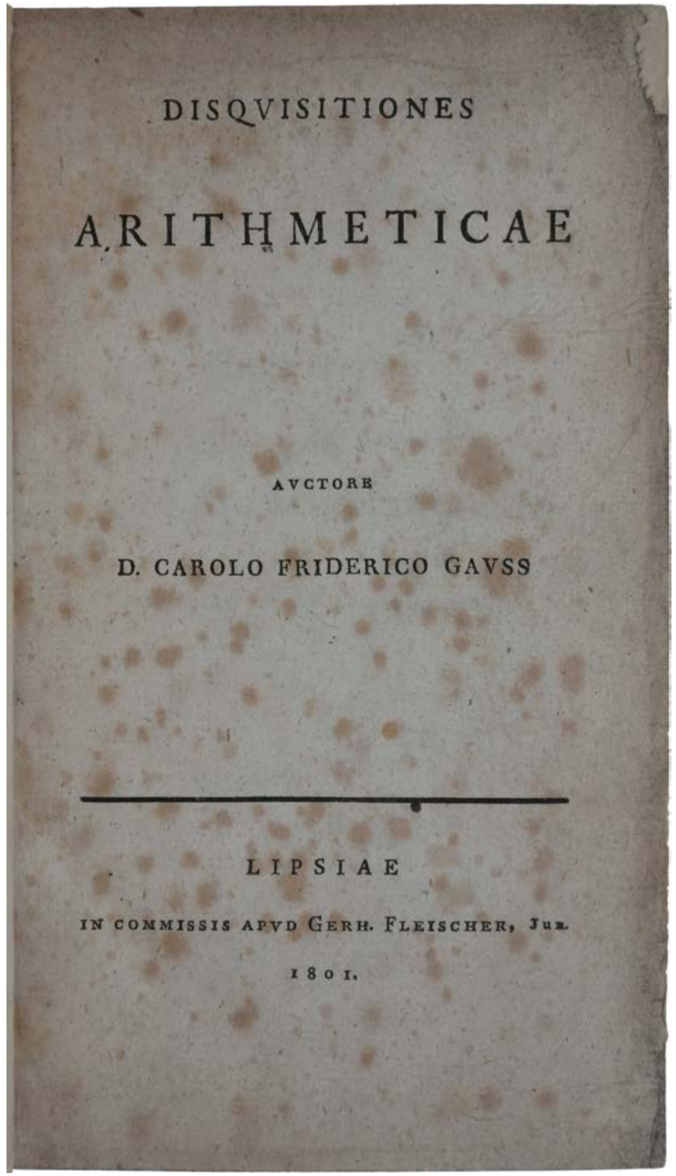
"The 665 pages and 355 articles of the main text are divided unevenly into seven sections. The first and smallest one (7 pp., 12 arts.) establishes a new notion and notation which, despite its elementary nature, modified the practice of number theory:

'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



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) …

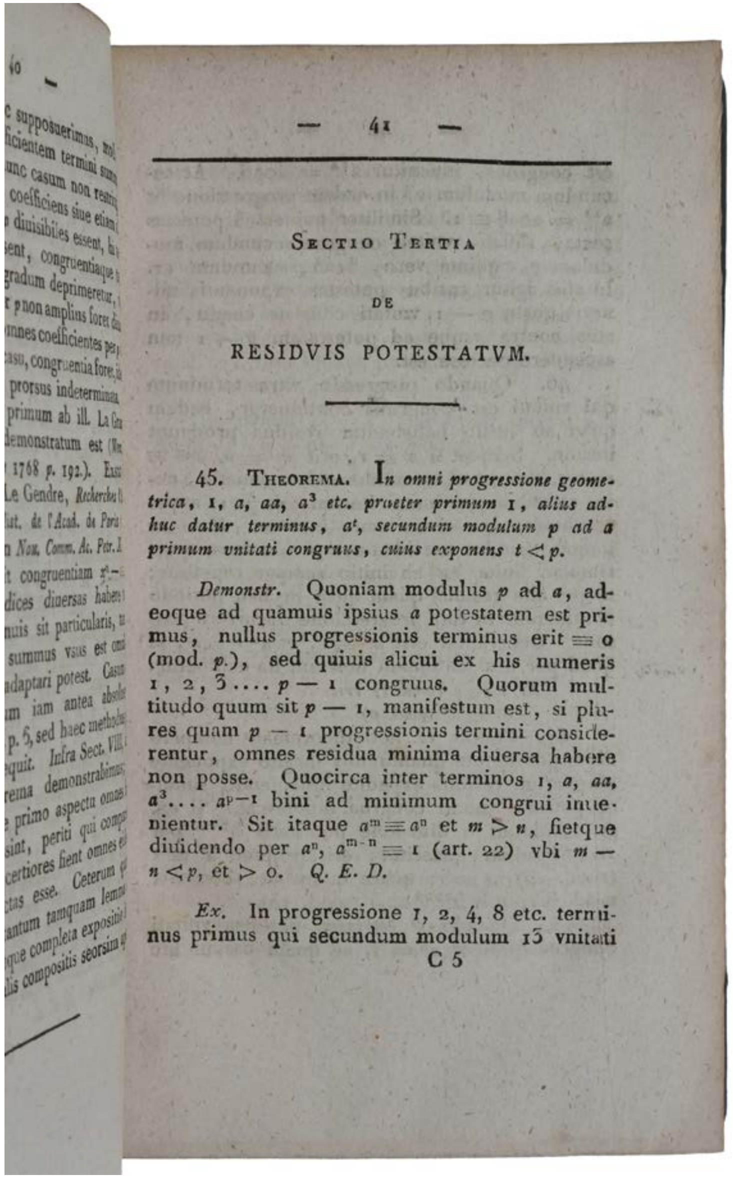
"The classification of forms also ushers the reader into the second half of sec. 5, entitled 'further investigations on forms' … In art. 226, certain classes are grouped together into an 'order' according to the divisibility properties of their coefficients. There follows (arts. 229–233) a finer grouping 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 $1, 2, 3 \ldots p-1$ 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 $1, a, aa, a^3 \ldots a^{p-1}$ 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.

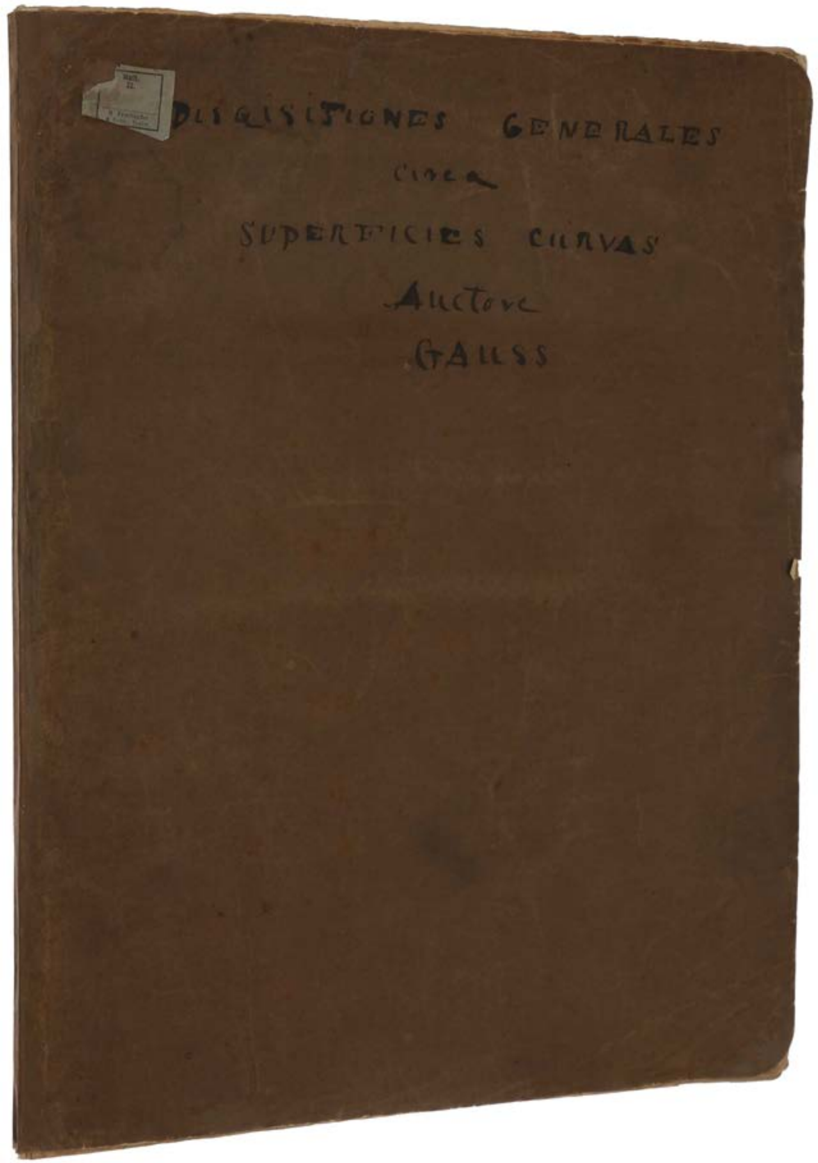
THE FOUNDING PAPER OF MODERN DIFFERENTIAL GEOMETRY

GAUSS, Carl Friedrich. *Disquisitiones generales circa superficies curvas.* Göttingen: Dieterich, 1828.

$9,500

4to (258 x 209 mm), pp 50, uncut in the original brown plain wrappers, spine strip renewed, early manuscript lettering to front wrapper, some spotting to first and final leaves.

First edition, the very rare separately-paginated offprint from *Commentationes Societatis Regiae Scientiarum Göttingensis* (Vol. VI, 1828, pp. 99-146), of this "masterpiece of the mathematical literature" (Zeidler, p. 16). "... the crowning contribution of the period, and his last great breakthrough in a major new direction of mathematical research, was *Disquisitiones generales circa superficies curvas* (1828), which grew out of his geodesic meditations of three decades and was the seed of more than a century of work on differential geometry" (DSB). "A decisive influence on the entire course of development of differential geometry was exerted by the publication of a remarkable paper of Gauss, 'Disquisitiones generales circa superficies curvas' (Göttingen, 1828), written in Latin, as was the custom in the seventeenth and eighteenth centuries. It was this paper, carefully polished and containing a wealth of new ideas, that gave this area of geometry more or less its present form and opened a large circle of new and important problems whose development provided work for geometers for many decades" (Kolmogorov & Yushkevitch, p. 7). Gauss's *Disquisitiones* was, in particular, the basis for Riemann's famous 1854 Habilitationsschrift 'Uber die Hypothesen welche die Geometrie zu Grunde liegen' (see below). ABPC/RBH list only four



copies sold in the last 40 years (Gedeon, Honeyman, Norman, and Stanitz).

"The surface theory of Gauss was strongly influenced by Gauss' work as a surveyor. Under great physical pains, Gauss worked from 1821 to 1825 as a land surveyor in the kingdom of Hannover … In 1822 he submitted his prize memoir "General solution of the problem of mapping parts of a given surface onto another surface in such a way that image and pre-image become similar in their smallest parts" to the Royal Society of Sciences in Copenhagen … When writing his prize memoir, Gauss had apparently already worked on a more general surface theory, because he added the following Latin saying to his title page: *Ab his via sterniture ad maiora* (From here the path to something more important is prepared). The development of the general surface theory, however, was difficult, though the basic ideas were known to Gauss since 1816. On February 19, 1826, he wrote to Olbers: 'I hardly know any period in my life, where I earned so little real gain for truly exhausting work, as during this winter. I found many, many beautiful things, but my work on other things has been unsuccessful for months.' Finally on October 8, 1827 Gauss presented the general surface theory. The title of the paper was "Disquisitiones generales circa superficies curvas" (Investigations about curved surfaces). The most important result of this masterpiece in the mathematical literature is the *Theorema egregium*" (Zeidler, p. 15).

"Gauss made the parametric representation of a surface and the corresponding expression for its element of length [the 'metric'] into the foundations of the *Disquisitiones*. He was the first to formulate clearly and explicitly the concept of intrinsic geometry of a surface, and he proved that the curvature could be measured by a quantity (the Gaussian curvature) that belongs to intrinsic geometry, i.e., does not vary when the surface is deformed. He further developed the theory of geodesic lines [shortest paths on a surface], which also belong to intrinsic geometry …

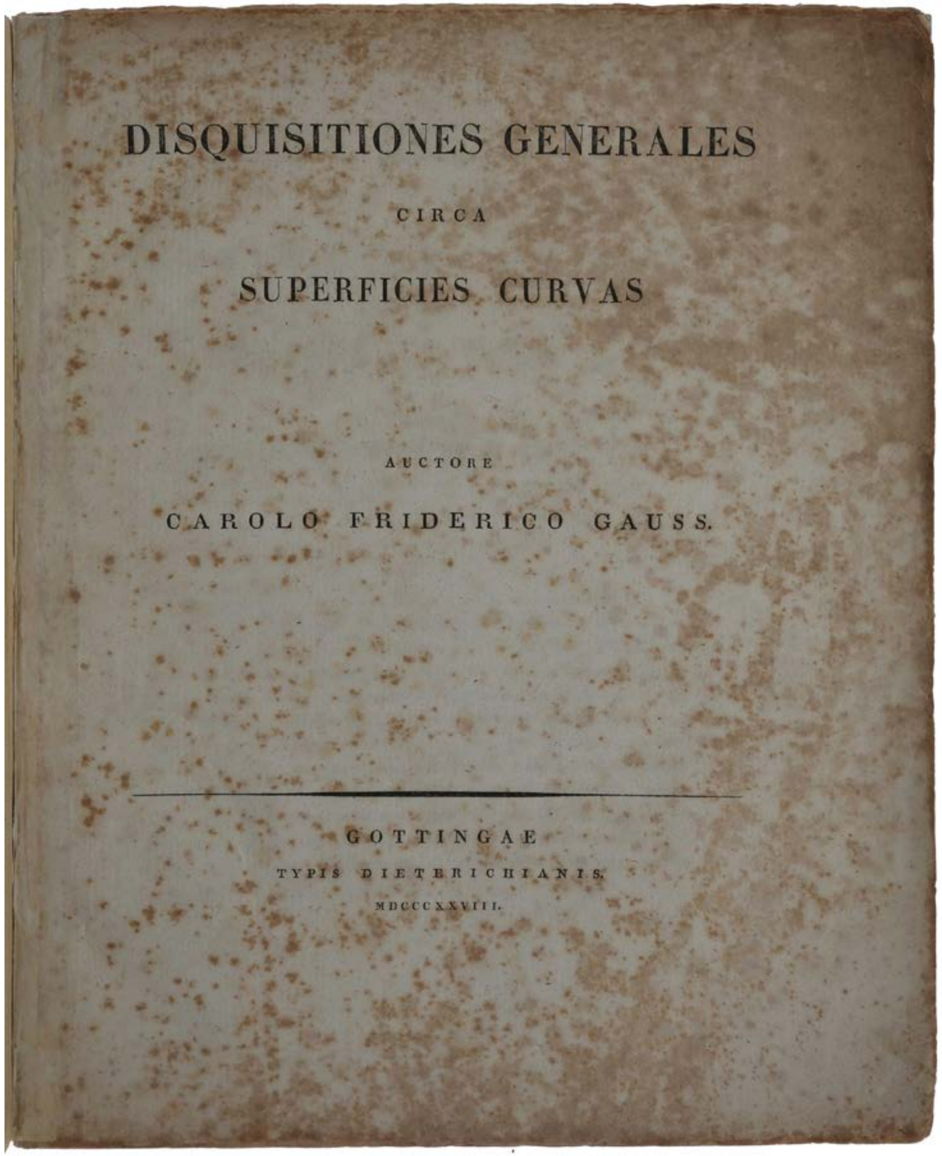
"Another useful innovation due to Gauss was the use of spherical mapping in geometry, which was usually applied in astronomy. Every oriented line is assigned the point on the unit sphere having radius vector parallel to the line. Thus a region of the surface is mapped to a region on the unit sphere using the normal. Relying on this mapping Gauss introduced the concept of a measure on the curvature K (the Gaussian curvature of the surface at the given point) as the ratio of the areas of the corresponding infinitesimal regions on the sphere and on the surface" (Kolmogorov & Yushkevitch, pp. 7-9).

Gauss established the new and unexpected fact that the curvature K could be expressed entirely in terms of the metric of the surface, and therefore belongs to the intrinsic geometry of the surface. This, "as Gauss pointed out, leads to his 'great theorem' (*Theorema Egregium*): *If a curved surface is developed on any other surface, the measure of the curvature at each point remains invariant* (*ibid*., p. 10). "Gauss' *Theorema egregium* had an enormous impact on the development of modern differential geometry and modern physics culminating in the principle 'force equals curvature'. This principle is basic for both Einstein's theory of general relativity on gravitation and the Standard Model in elementary particle physics" (Zeidler, p. 16).

"The concept of a geodesic, i.e., a shortest line, also belongs to the intrinsic geometry, since geodesic lines remain geodesics under deformation. For that reason Gauss, in studying problems of intrinsic geometry, found the equations of the geodesic lines in curvilinear coordinates and studied their behavior further. He introduced the notion of a geodesic circle, i.e., the geometric locus of the endpoints of geodesic radii of constant length emanating from a single point, and he showed that it was orthogonal to its radii" (Kolmogorov & Yushkevitch, p. 11).

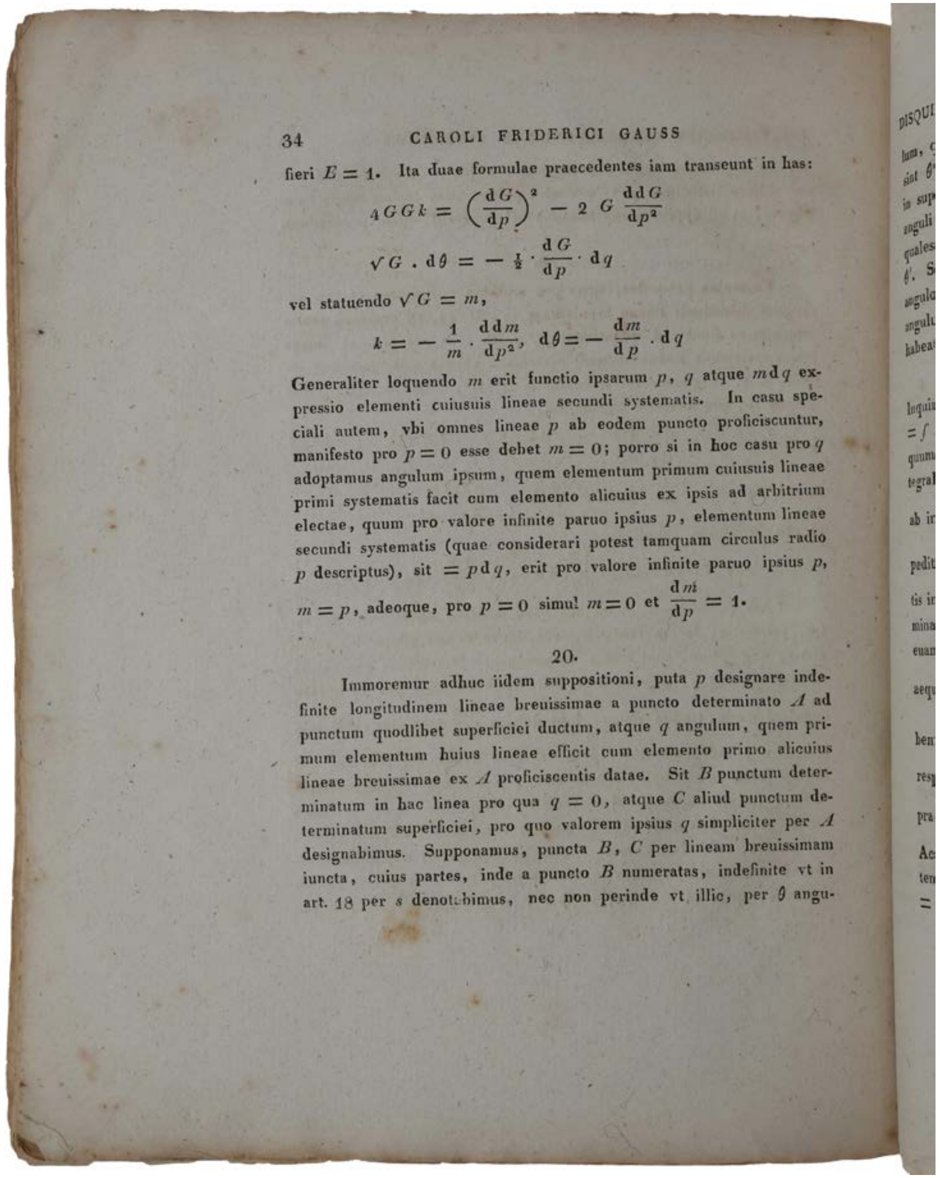
A second major result contained in the present paper, which perhaps had even greater ramifications in mathematics than the *Theorema egregium*, is a version of what is now known as the Gauss-Bonnet theorem. "The remarkable formula found by Gauss for the sum of the angles of a geodesic triangle amounts to the statement that the excess over 180° of the sum of the angles of such a triangle in the case of a surface of positive curvature, or the deficiency in the case of a surface of negative curvature, equals the area of the spherical image of the triangle, called by Gauss the *total curvature* (curvature integra) of the triangle. This formula has a direct connection with Gauss' reflections and computations on non-Euclidean geometry, which he kept secret during his lifetime" (*ibid*., p. 12). This result was generalized by Pierre-Ossian Bonnet in 1848 and by many later mathematicians, and served as the prototype of results linking the local geometry of a space (e.g. its curvature) with its 'topology' (its overall shape), a theme which pervades much of 20th century mathematics. Remarkably, Gauss describes at the end of the present work some measurements he has made to verify the Gauss-Bonnet theorem. He tells us that he has measured the angles of a triangle, the greatest side of which was more than 15 miles, and found that the sum of the angles of the triangle is greater than two right-angles by almost 15 seconds of arc, in agreement with the theorem as it applies to the surface of the Earth.

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



associated finite surface inside the manifold, which is generated by all geodesic lines starting in the surface element" (*Companion Encyclopedia*, p. 928).

Norman 880. I. Grattan-Guinness (ed.), *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences*, 1994; A. N. Kolmogorov & A. P. Yushkevitch, *Mathematics of the 19th century: Geometry. Analytic Function Theory*, 1996; E. Zeidler, *Quantum Field theory III. Gauge Theory,* 2013, pp. 15-19. For a detailed account of the content and historical antecedents of the work, see P. Dombrowski, '150 years after Gauss' 'Disquisitiones generales circa superficies curvas'', *Astérisque* 62 (1979), pp. 97-153.


34 CAROLI FRIDERICI GAUSS

fieri $E = 1$. Ita duae formulae praecedentes iam transeunt in has:

$$4GGk = \left(\frac{\mathrm{d}G}{\mathrm{d}p}\right)^2 - 2G\frac{\mathrm{dd}G}{\mathrm{d}p^2}$$

$$\sqrt{G}\,.\,\mathrm{d}\theta = -\tfrac{1}{2}\cdot\frac{\mathrm{d}G}{\mathrm{d}p}\cdot\mathrm{d}q$$

vel statuendo $\sqrt{G} = m$,

$$k = -\frac{1}{m}\cdot\frac{\mathrm{dd}m}{\mathrm{d}p^2},\quad \mathrm{d}\theta = -\frac{\mathrm{d}m}{\mathrm{d}p}\,.\,\mathrm{d}q$$

Generaliter loquendo m erit functio ipsarum p, q atque $m\mathrm{d}q$ expressio elementi cuiusuis lineae secundi systematis. In casu speciali autem, vbi omnes lineae p ab eodem puncto proficiscuntur, manifesto pro $p = 0$ esse debet $m = 0$; porro si in hoc casu pro q adoptamus angulum ipsum, quem elementum primum cuiusuis lineae primi systematis facit cum elemento alicuius ex ipsis ad arbitrium electae, quum pro valore infinite paruo ipsius p, elementum lineae secundi systematis (quae considerari potest tamquam circulus radio p descriptus), sit $= p\mathrm{d}q$, erit pro valore infinite paruo ipsius p, $m = p$, adeoque, pro $p = 0$ simul $m = 0$ et $\frac{\mathrm{d}m}{\mathrm{d}p} = 1$.

20.

Immoremur adhuc iidem suppositioni, puta p designare indefinite longitudinem lineae breuissimae a puncto determinato A ad punctum quodlibet superficiei ductum, atque q angulum, quem primum elementum huius lineae efficit cum elemento primo alicuius lineae breuissimae ex A proficiscentis datae. Sit B punctum determinatum in hac linea pro qua $q = 0$, atque C aliud punctum determinatum superficiei, pro quo valorem ipsius q simpliciter per A designabimus. Supponamus, puncta B, C per lineam breuissimam iuncta, cuius partes, inde a puncto B numeratas, indefinite vt in art. 18 per s denotabimus, nec non perinde vt illic, per θ angu-

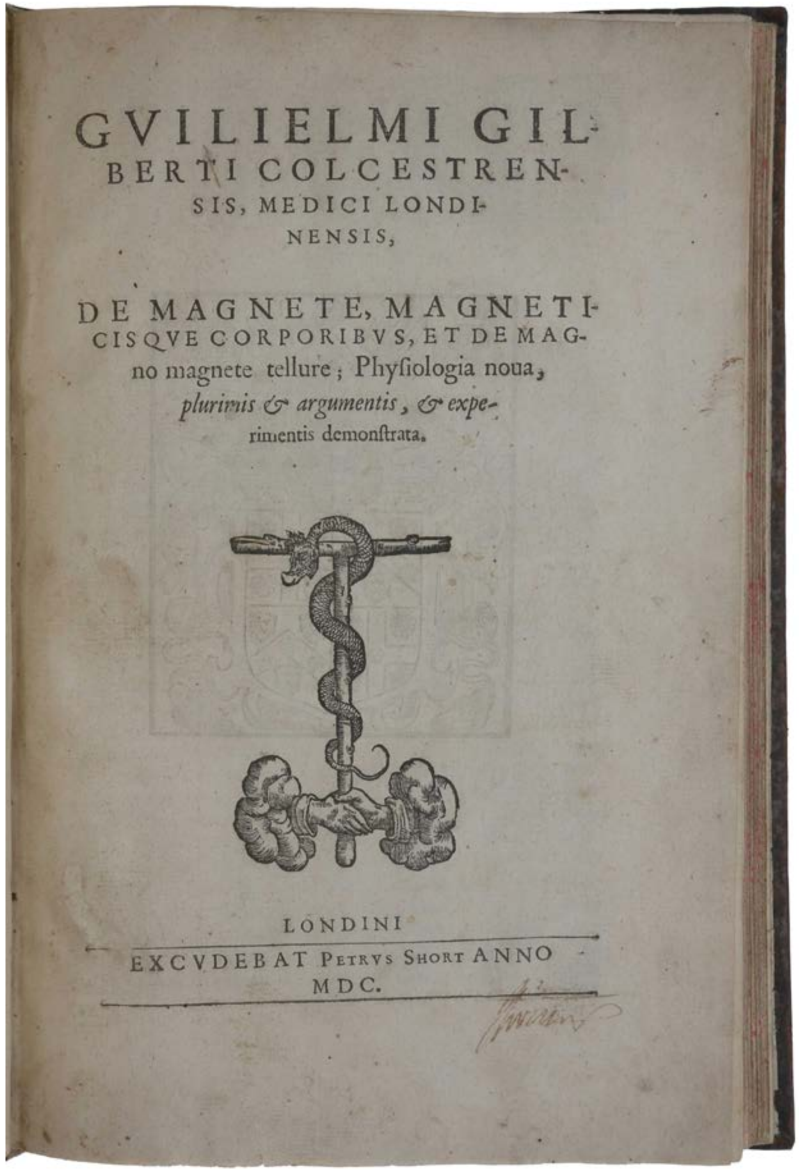
PMM 107 - THE FOUNDER OF ELECTRICAL SCIENCE

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 GILBERTI COLCESTRENSIS, MEDICI LONDINENSIS,

DE MAGNETE, MAGNETICISQVE CORPORIBVS, ET DE MAGNO MAGNETE TELLURE; Physiologia noua, plurimis & argumentis, & experimentis 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.

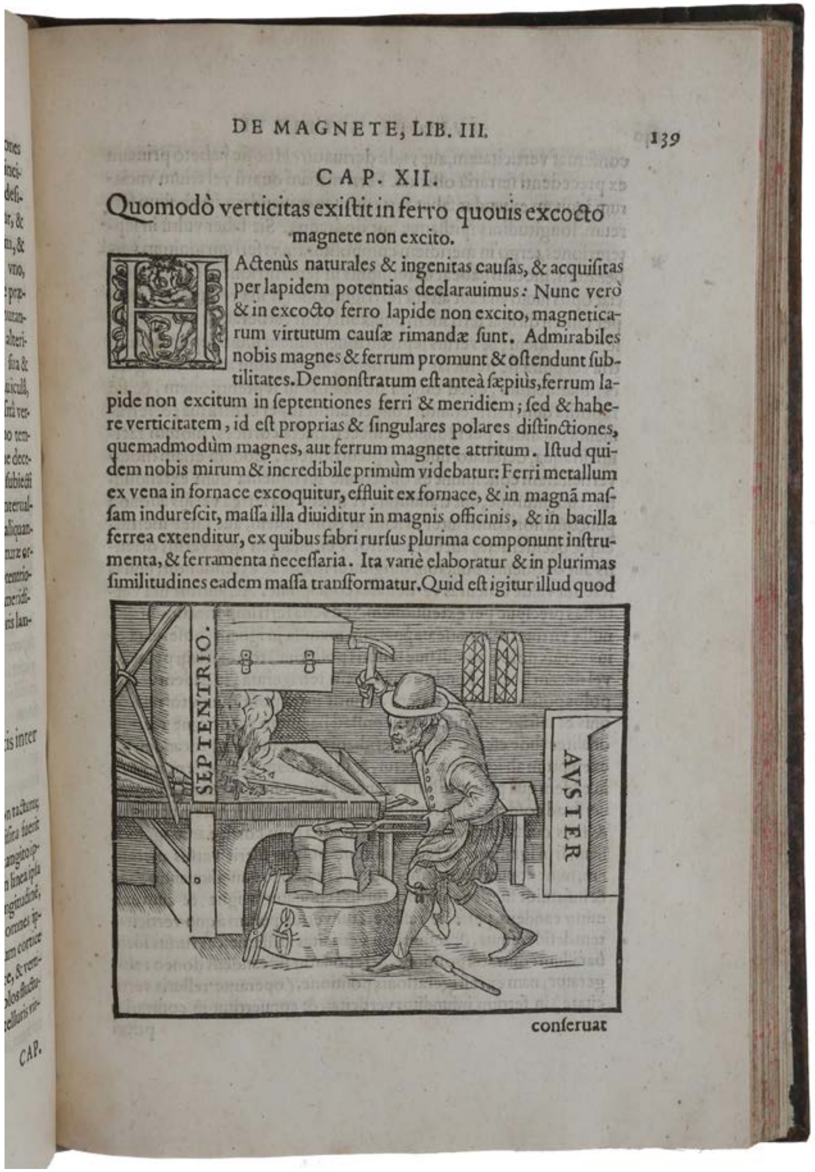
“After disposing of the amber effect, Gilbert returned to his study of the magnetic phenomena. In discussing these, Gilbert relied for his explanations on several assumptions: (1) the earth is a giant lodestone and has the magnetic property; (2) the magnetic property is due to the form of the substance; (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



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

the terrella maintained a horizontal position only when placed on the equator. 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.

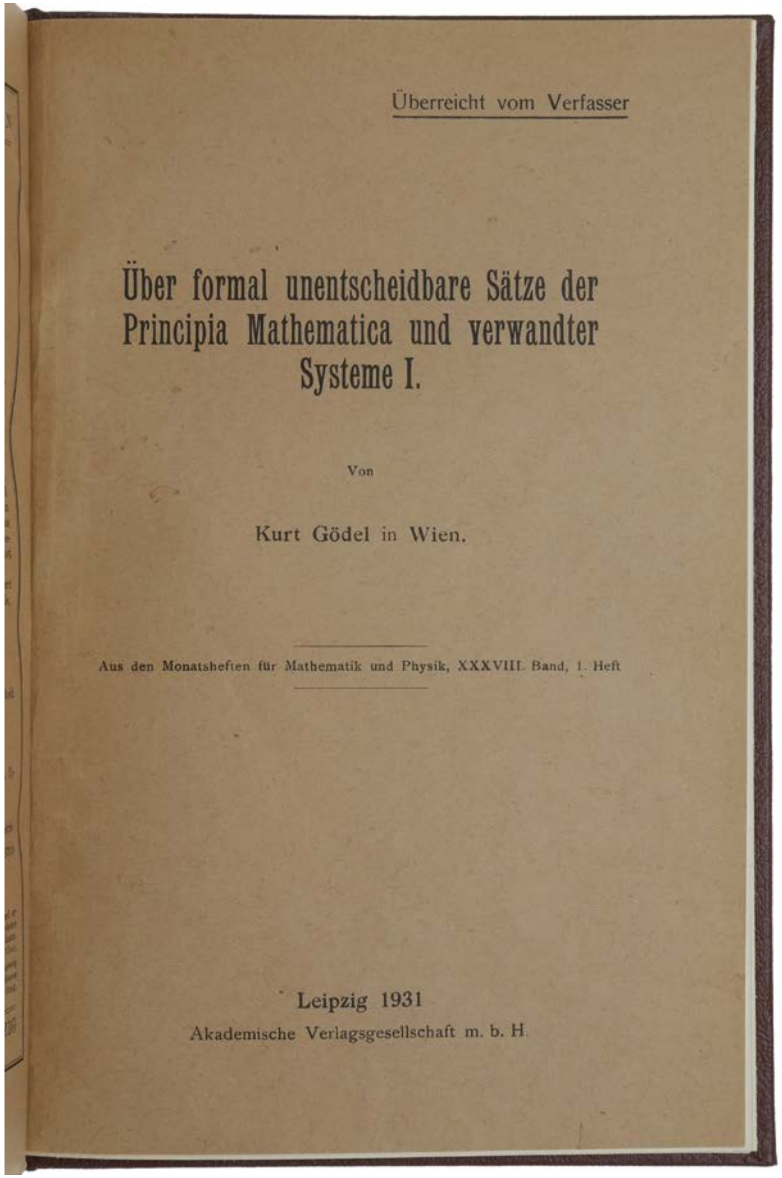
THE INCOMPLETENESS THEOREM

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.

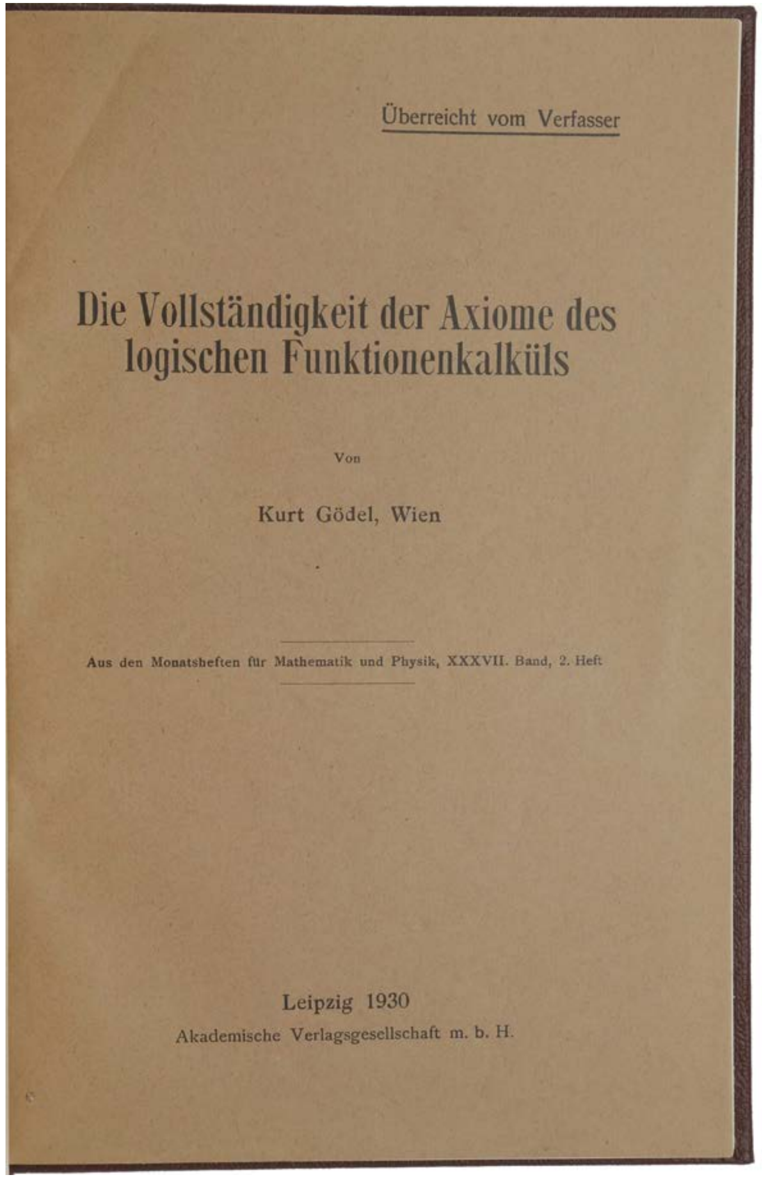
After Kaila's death, the offprints were acquired by Georg Henrik von Wright (1916-2003), the famous Finnish philosopher (partly of Scottish ancestry) who had studied under Kaila at the University of Helsinki. Von Wright was also a relative of Kaila: his mother was the cousin of Kaila's wife Anna. Von 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).

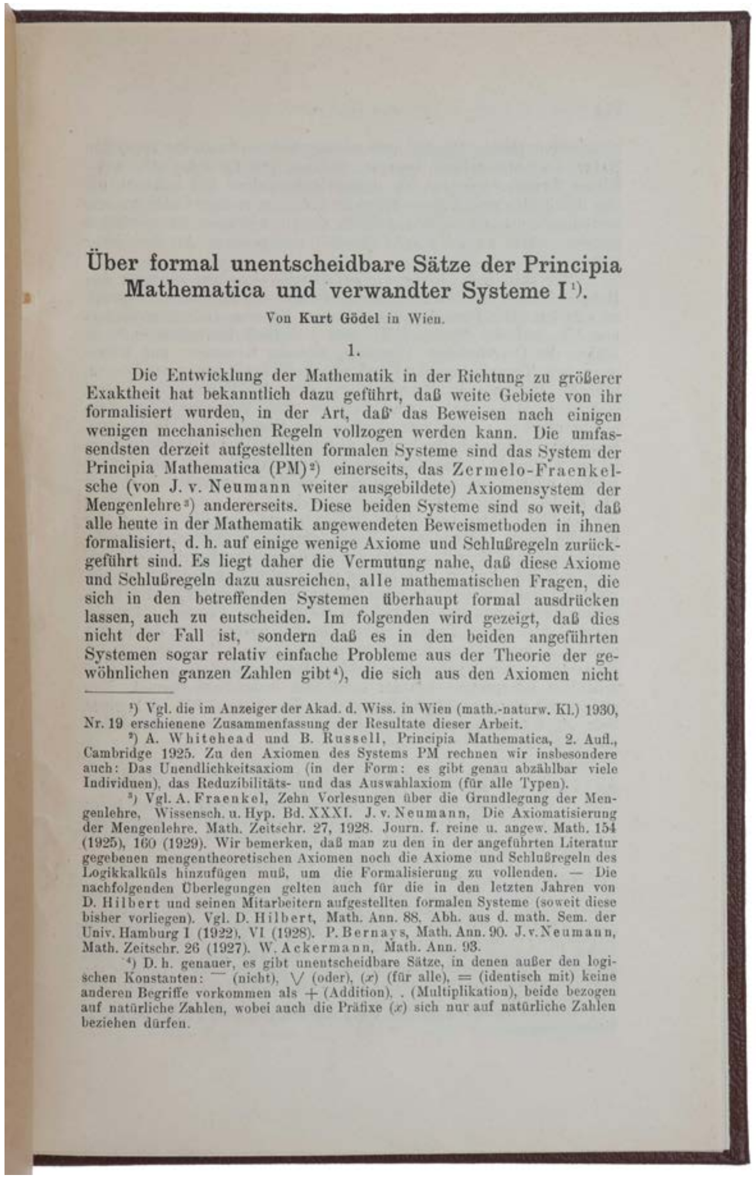
"The immediate effect of Gödel's theorem was that the assumptions of Hilbert's program were challenged. Hilbert assumed quite explicitly that arithmetic was complete in the sense that it would settle all questions that could be formulated in its language—it was an open problem he was confident could 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

[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.

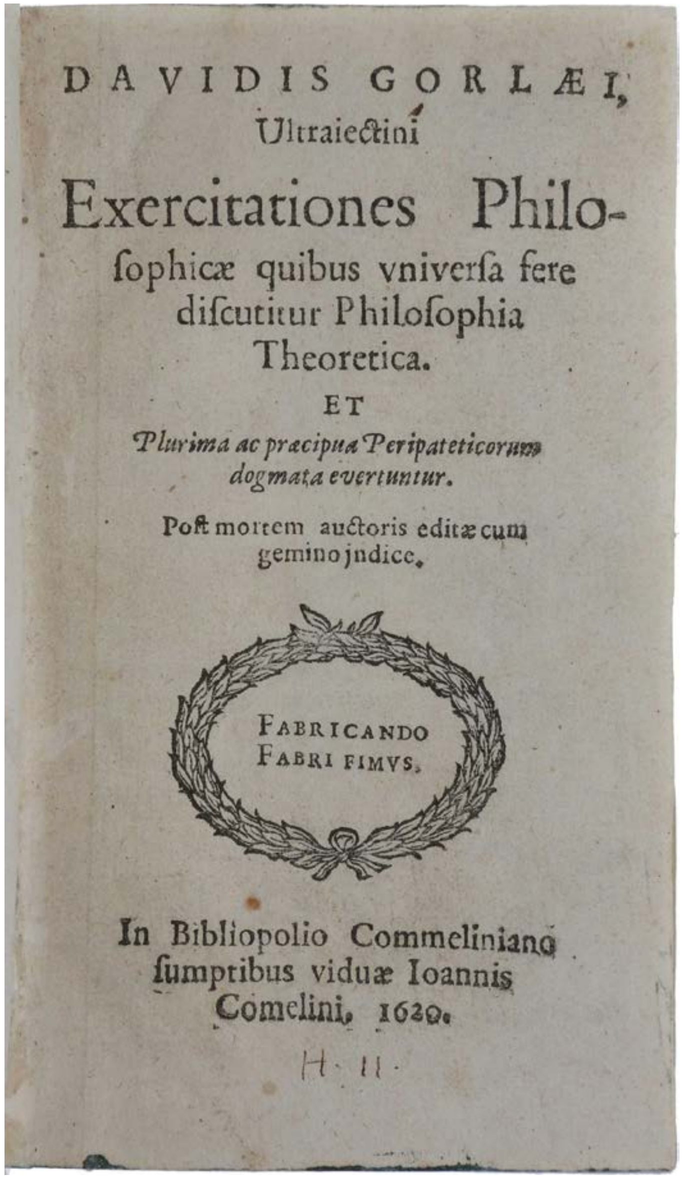
A FOUNDING WORK OF MODERN ATOMISM

GORLAEUS (VAN GOORLE, VAN GOOIRLE), David. *Exercitationes philosophicæ quibus universa fere discutitur philosophia theoretica, et plurima ac præcipua Peripateticorum dogmata evertuntur … cum gemino indice.* [Leyden]: in bibliopolio Commeliniano sumptibus viduae Joannis Comelini, 1620.

$45,000

8vo (154 x 89 mm), pp. [xxviii], 352. Contemporary half vellum, manuscript title on spine, some worming to lower and upper parts of the spine, internally fine and clean.

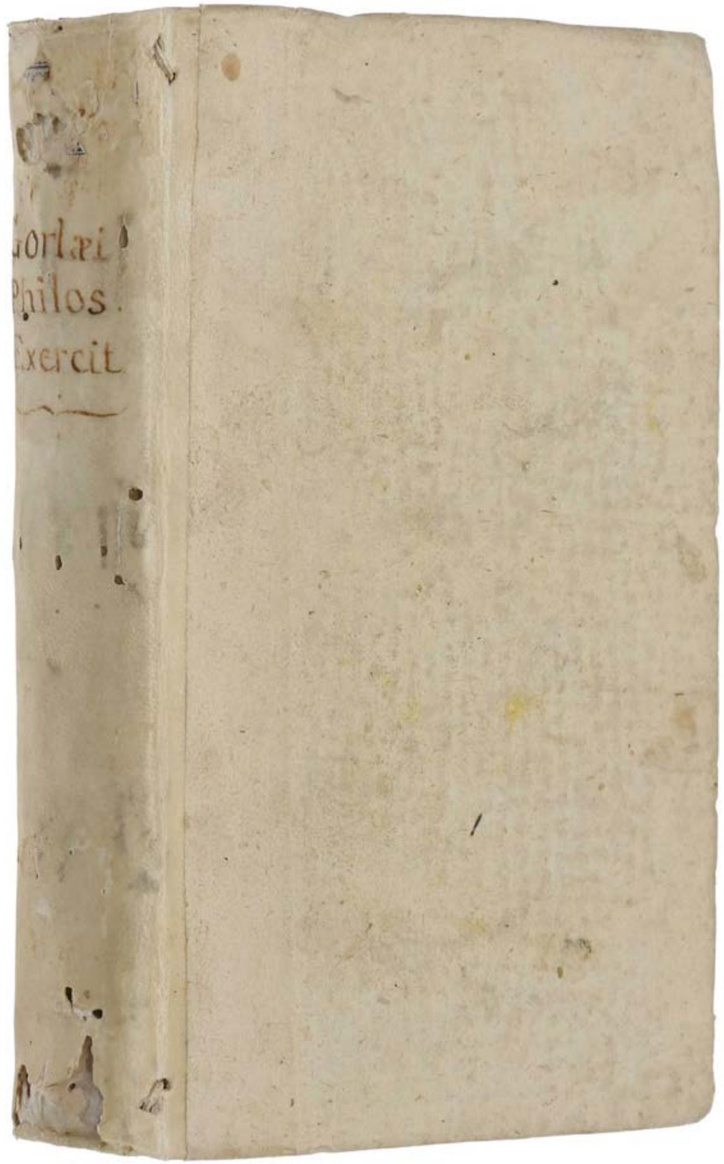
First edition, extremely rare, of one of the earliest modern works on atomism. "Gorlaeus is counted among the founders of modern atomism, which he proposed as an alternative to Aristotelian matter theory. Because of his notion of atomic compounds, he is also regarded as a contributor to the evolution of chemistry" (DSB). "When David Gorlaeus (1591-1612) passed away at 21 years of age, he left behind two highly innovative manuscripts. Once they were published [as the present work, and as *Idea physicae* (1610), his work had a remarkable impact on the evolution of seventeenth-century thought. However, as his identity was unknown, divergent interpretations of their meaning quickly sprang up. Seventeenth-century readers understood him as an anti-Aristotelian thinker and as a precursor of Descartes. Twentieth-century historians depicted him as an atomist, natural scientist and even as a chemist. And yet, when Gorlaeus died, he was a beginning student in theology. His thought must in fact be placed at the intersection between philosophy, the nascent natural sciences, and theology" (Lüthy). This is a very rare book. In his review of Lüthy's book in 2012, Henri



Krop wrote: “until now Gorlaeus’s life and ideas have remained basically unknown because both his elaborate *Exercitationes philosophicae* and his *Idea physicae* are extremely rare and copies were unavailable in Dutch public libraries. (However since 1986 the libraries of both Leiden and Leeuwarden have acquired copies of the former.)” We have been unable to locate any copies in auction records.

“Gorlaeus’s atomism, which took center stage in his *Idea physicae*, is however more fully embedded in the *Exercitationes philosophicae*. There, philosophy is defined as “the naked knowledge of entities” and thus identified with ontology. Each discipline, wrote Gorlaeus, tackles one type of entity, whereby physics deals with natural entities. His ontology distinguishes between self-subsisting entities (*entia per se*), which are defined as numerically unique, fully existing, unchanging, and indivisible, and the accidental compositions (*entia per accidens*) that are brought about when several *entia per se* gather. This view of reality is essentially atomistic, although primarily in a metaphysical sense. By denying universals and allowing only for individuals, it is also heavily indebted to medieval nominalism. The only self-subsisting entities are God, angels, souls, and physical atoms, whereas all other entities, including humans, are transitory composites. Gorlaeus’s definition of man as an “accidental being,” which he took from Taurellus, was to be used in a 1641 university disputation by René Descartes’s friend Henricus Regius and triggered the first conflict between Descartes and the Aristotelian university establishment. Since that episode Gorlaeus has, somewhat misleadingly, been seen as a forerunner of Cartesianism.

“Although his atomism is primarily metaphysical, Gorlaeus spent much time and effort to apply it to the realms of physics and chemistry. Rejecting Aristotle’s concept of place, he maintained that atoms move in an absolute space, which does not necessarily have to be filled. Possessing quantity, atoms are furthermore extended, and they come in two types, namely dry (as earth atoms) and wet (as



water atoms). All natural bodies can be resolved into these two types of atoms. Fire is explained in terms of the friction of closely packed atoms, while air is defined as a real, but non-elementary substance, which fills all voids and which is capable of transmitting celestial heat, but not of combining into compounds. When bodies rarefy, this is due to the entrance of air between the atoms; air itself cannot be rarefied or condensed. The emergent physical and chemical properties of higher-level compounds are due to the mixing of the elementary qualities of wet and dry with the so-called "real accidents" of warm and cold, which are communicated to the elementary atoms from the ambient air. Within this framework, Gorlaeus explained the most common physical and chemical properties of substances as a "temperament" created by the interacting atoms under the influence of ambient heat or cold. Although he demonstrated a certain ingenuity in this enterprise, he was yet forced to introduce additional elements such as heaviness, which is a divinely "impressed downward force." Divine providence is also responsible for the aggregation of atoms into the more complex bodies" (DSB).

Gorlaeus' contributions are the more remarkable given his short life (1591-1612). When he died, at age 21, he was a student of theology. "From the correspondence of his father, David Gorlaeus Sr., it appears that alchemical interests were being cultivated in his family. Still, the interests of the younger Gorlaeus in natural philosophy had above all philosophical and theological sources. As for philosophy, as an undergraduate student at Franeker between 1606 and 1610, Gorlaeus followed the comparatively innovative natural science course of Henricus de Veno, who incorporated recent developments in the fields of astronomy, meteorology, and natural philosophy into his otherwise Aristotelian framework. By 1610, Gorlaeus had furthermore come under the influence of Julius Caesar Scaliger's *Exercitationes exotericae* (1557), which explained a range of natural phenomena in terms of corpuscles and interstitial voids. Scaliger figures as the only recent author in Gorlaeus's two books and is quoted with great frequency.

"Whereas Scaliger depicted himself as an Aristotelian and anti-atomist, Gorlaeus's strongly anti-Aristotelian physics relied fully on the interaction of atoms. In order to understand why a theology student should have ended up developing such a system, one must consider his circumstances in 1611. A crisis pitting two currents of Calvinism against each other was just then reaching its acme at Leiden University's theological faculty, where Gorlaeus had recently enrolled. The point of departure for the conflict had been the non-orthodox view of one of the professors, Jacob Arminius, that the election of the faithful to heaven was not predestined by God since eternity, a view that was combated by his colleague Franciscus Gomarus. The so-called Arminian conflict, which quickly turned into a national and indeed international affair, had strong political overtones, but conceptually revolved around such philosophical concepts as the nature of divine and human causality, time and eternity, place and ubiquity, and determinism and free will. When Arminius died in 1609, Conrad Vorstius was chosen to succeed him, but upon his arrival at Leiden in 1611 was expelled speedily on charges of heresy. Some of the alleged heresies, which King James I of England stooped to rebut in person, were said to reside in his physicalist understanding of God, an understanding that had been inspired, it was charged, by the metaphysics of the German professor of medicine Nicolaus Taurellus. The point is that Gorlaeus, who was a partisan of the Arminian cause, quickly acquainted himself with the writings of both Vorstius and Taurellus, and his atomism can be understood as a radicalization of the ontology he had found particularly in Taurellus's metaphysics" (*ibid*.).

Christoph Lüthy, *David Gorlaeus (1591-1612): An Enigmatic Figure in the History of Philosophy and Science*, Amsterdam, 2012.

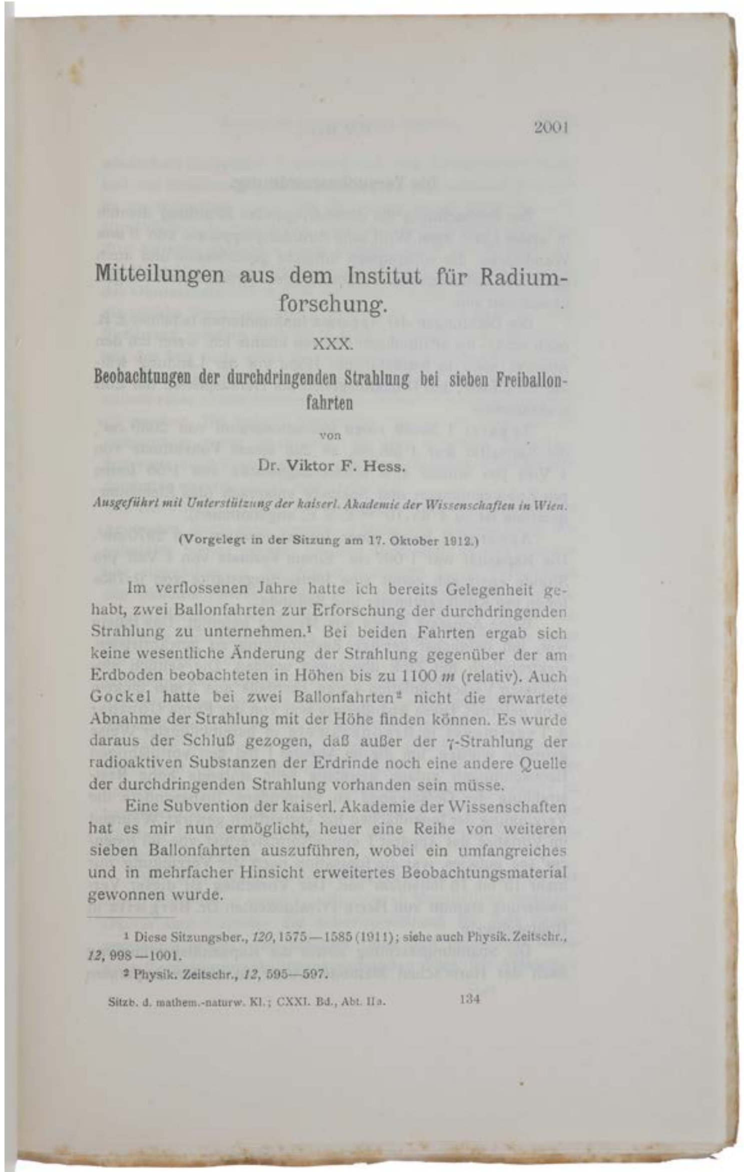
DISCOVERY OF COSMIC RADIATION

HESS, Victor Franz. *Beobachtungen der durchdringenden Strahlung bei sieben Ballonfahrten.* Vienna: Aus der Kaiserlich-Königlichen Hof- und Staatsdruckerei, 1912.

$2,800

Pp. 2001-2032 in: Sitzungsberichte der Königlichen Akademie der Wissenschaften in Wien, Mathematisch-naturwissenschaftliche Klasse, Abtheilung IIa. CXXI. Band, IX. Heft, November 1912. 8vo (244 x 160 mm), pp. [2], 1763-2078, 5. Original printed wrappers, uncut and mostly unopened, mild spotting to margins, a very fine copy, completely untouched in its original state.

First edition, journal issue in the original printed wrappers, of the discovery of cosmic radiation. "The 1912 discovery of cosmic rays by physicist Victor Franz Hess showed that radiation of extraterrestrial origin permeates the Earth's atmosphere. After determining that ground-based radiation would fade to negligible amounts of measurable ionization at about 500 feet above the Earth's surface, Hess conducted his experiments by ascending into the sky tethered to a helium balloon" (NNDB). "From April to August 1912 [Hess] had the opportunity of undertaking seven balloon flights up to 5,200m above ground, about which he reported in a detailed paper communicated to the Vienna academy meeting of 17 October 1912 [the offered work] … As a result of his measurements Hess stated: '(i) Immediately above ground the total radiation decreases a little … (ii) At altitudes of 1,000 to 2,000 m there occurs again a noticeable growth of penetrating radiation. (iii) The increase reaches, at altitudes of 3,000 to 4,000 m, already 50 per cent of the total radiation observed on the ground. (iv) At 4,000 to 5,200 m the radiation is stronger by [producing] 15 to 18 [more] ions [that is, more than


2001

Mitteilungen aus dem Institut für Radium-forschung.

XXX.

Beobachtungen der durchdringenden Strahlung bei sieben Freiballon-fahrten

von

Dr. Viktor F. Hess.

Ausgeführt mit Unterstützung der kaiserl. Akademie der Wissenschaften in Wien.

(Vorgelegt in der Sitzung am 17. Oktober 1912.)

Im verflossenen Jahre hatte ich bereits Gelegenheit gehabt, zwei Ballonfahrten zur Erforschung der durchdringenden Strahlung zu unternehmen.[1] Bei beiden Fahrten ergab sich keine wesentliche Änderung der Strahlung gegenüber der am Erdboden beobachteten in Höhen bis zu 1100 *m* (relativ). Auch Gockel hatte bei zwei Ballonfahrten[2] nicht die erwartete Abnahme der Strahlung mit der Höhe finden können. Es wurde daraus der Schluß gezogen, daß außer der γ-Strahlung der radioaktiven Substanzen der Erdrinde noch eine andere Quelle der durchdringenden Strahlung vorhanden sein müsse.

Eine Subvention der kaiserl. Akademie der Wissenschaften hat es mir nun ermöglicht, heuer eine Reihe von weiteren sieben Ballonfahrten auszuführen, wobei ein umfangreiches und in mehrfacher Hinsicht erweitertes Beobachtungsmaterial gewonnen wurde.

[1] Diese Sitzungsber., *120*, 1575—1585 (1911); siehe auch Physik. Zeitschr., *12*, 998—1001.
[2] Physik. Zeitschr., *12*, 595—597.

Sitzb. d. mathem.-naturw. Kl.; CXXI. Bd., Abt. IIa. 134

100 per cent] than on the ground' (pp. 2026-7). These results seemed to him 'most easy to explain by the assumption that radiation of very high penetrating power enters the atmosphere from above and creates, even in the lowest layers [of the atmosphere], a part of the ionization observed in closed vessels' (p. 2025)" (Mehra & Rechenberg, *The Historical Development of Quantum Theory*, Vol. 5 (1987), pp. 160-161). "Most experts in the field scoffed at his findings until after World War I, when additional research backed Hess's conclusions. In a 1925 paper written by Robert A. Millikan, the radiation Hess had discovered was given its present name, cosmic rays" (NNDB). "As it turned out much later, [cosmic radiation] consists of protons and heavier atomic nuclei with a wide range of energies. Some have an energy very much larger than can be achieved by modern particle accelerators. Because of that, many important discoveries could be made with cosmic rays … In 1932, [Carl D.] Anderson discovered the positron in a cloud chamber exposed to cosmic radiation on a mountain" (Brandt, *The Harvest of a Century* (2009), pp. pp. 77-79). The 1936 Nobel Prize in Physics was awarded jointly to Hess "for his discovery of cosmic radiation" and Anderson "for his discovery of the positron". No copies located in auction records.

"Today we take it for granted that Earth's atmosphere is constantly bombarded by high-energy cosmic rays originating far outside our solar system. But such was not always the case. It was a 29-year-old Austrian physicist named Victor Hess who officially "discovered" cosmic rays, and went on to devote an illustrious scientific career to studying the effects of radiation on the human body.

"Born in Austria in June 1883, Hess was the son of the chief forester for the estate of Prince Oettingen-Wallerstein. He attended the University of Graz in 1901 and earned his PhD at 23. Hess initially planned to study optics under famed physicist Paul Drude, the man who gave physics the symbol c for the speed of light. Tragically, Drude committed suicide weeks before Hess was due to arrive.

"The young Victor wound up accepting a position at the University of Vienna instead, studying under Franz Exner, an early pioneer in the study of radiation. Under Exner's tutelage, Hess began studying radioactivity and atmospheric electricity. It was during his work as an assistant at the Institute for Radium Research at the Austrian Academy of Sciences that Hess became intrigued by frequent reports of electrical charges being detected inside electroscopes–no matter how well those containers were insulated. Most scientists at the time believed the source of the ionization to be terrestrial in nature–radioactivity from ground minerals–and postulated that the ionization measured in the atmosphere therefore would decrease the further one got from the ground.

"Prior experiments with electroscopes gave rough estimates of ionization levels in the atmosphere, but those results seemed to indicate that the levels might actually increase beyond a certain altitude. For instance, in 1910, Theodore Wulf measured ionization at both the bottom and top of the Eiffel Tower in Paris, and found that there was far more ionization at 300 feet (the top) than one would expect if this effect were solely attributable to ground radiation. Other scientists mounted their instruments on balloons to record ionization at higher levels, but their results were inconclusive due to instrumentation defects.

"Speculating that perhaps the main source of the ionization could be in the sky rather than the ground, Hess tackled the problem first by designing instruments that could withstand the temperature and pressure changes at higher altitudes. He also determined that ground radiation would no longer produce ionization at 500 meters.

"Hess then mounted his instrumentation on a balloon and made ten separate ascents over the course of three years (1911-1913), measuring ionization levels. He found that initially ionization fell off with height, and then began to rise rapidly. At a height of several miles, the ionization was several times greater than that at Earth's surface. Hess concluded that "a radiation of very high penetrating power enters our atmosphere from above."

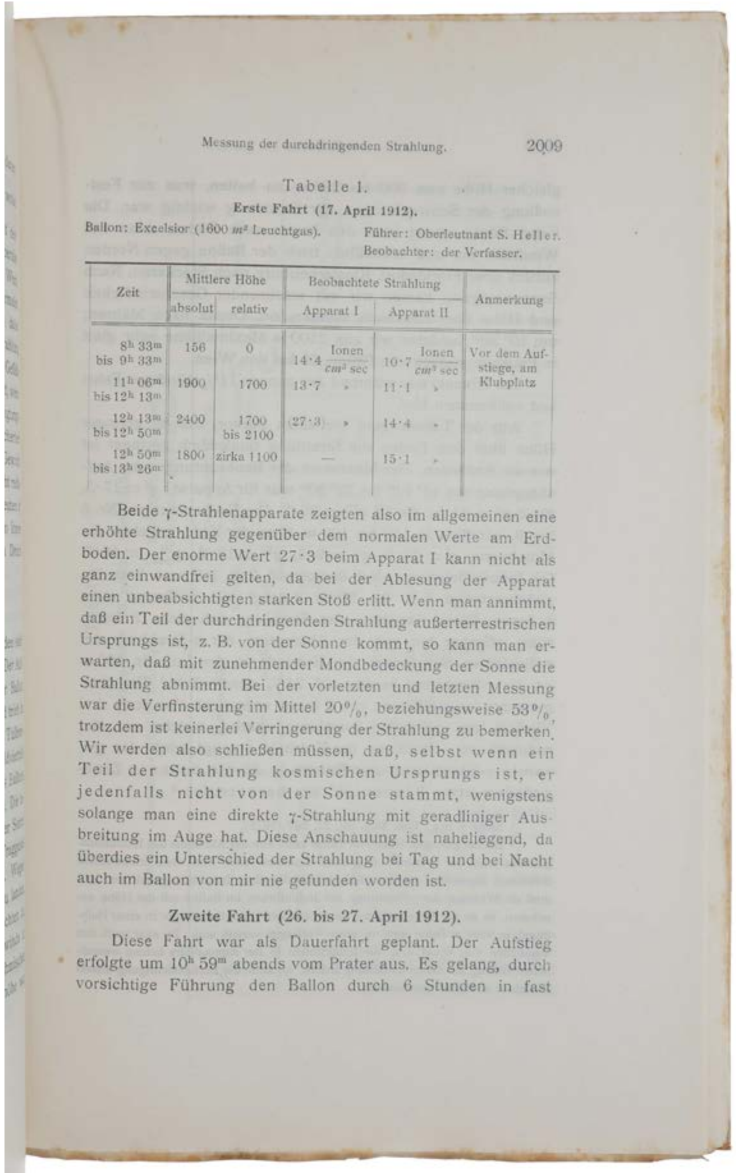
"Another clinching piece of evidence came during Hess's ascent on April 17, 1912, during a near-total eclipse of the sun. Since the ionization did not decrease during the eclipse, Hess concluded that the source of the radiation could not be the sun itself; it had to be coming from further out in space …

"Two years after Hess received the Nobel Prize, the Nazis invaded Austria and Hess was abruptly dismissed from his post as professor of physics at the University of Graz, in part because his wife was Jewish, and in part because he had been a scientific representative in the independent government of Chancellor Kurt von Schuschnigg. Warned by a sympathetic Gestapo officer that he and his wife would be sent to a concentration camp if they stayed in Austria, the couple fled to Switzerland.

"Hess immigrated to the US to become a professor at Fordham University. He participated in the first tests for radioactive fallout less than a year after the atomic bomb was dropped on Hiroshima, many conducted from the 87th floor of the Empire State Building in New York City. The following year found Hess in the bowels of Manhattan, measuring the radioactivity of granite in the 190th Street subway station near Fort Tryon.

"Along with William T. McNiff, Hess developed "an integrating gamma ray method" for detecting minute traces of radium in the human body, thereby making it possible to determine if someone was suffering from radium poisoning before it became critical.

"Even after retiring from Fordham, Hess continued to do research. He was



Messung der durchdringenden Strahlung. 2009

Tabelle 1.

Erste Fahrt (17. April 1912).

Ballon: Excelsior (1600 m^3 Leuchtgas). Führer: Oberleutnant S. Heller.
Beobachter: der Verfasser.

Zeit	Mittlere Höhe		Beobachtete Strahlung		Anmerkung
	absolut	relativ	Apparat I	Apparat II	
$8^h\ 33^m$ bis $9^h\ 33^m$	156	0	14·4 $\frac{\text{Ionen}}{cm^3\ sec}$	10·7 $\frac{\text{Ionen}}{cm^3\ sec}$	Vor dem Aufstiege, am Klubplatz
$11^h\ 06^m$ bis $12^h\ 13^m$	1900	1700	13·7 »	11·1 »	
$12^h\ 13^m$ bis $12^h\ 50^m$	2400	1700 bis 2100	(27·3) »	14·4 »	
$12^h\ 50^m$ bis $13^h\ 26^m$	1800	zirka 1100	—	15·1 »	

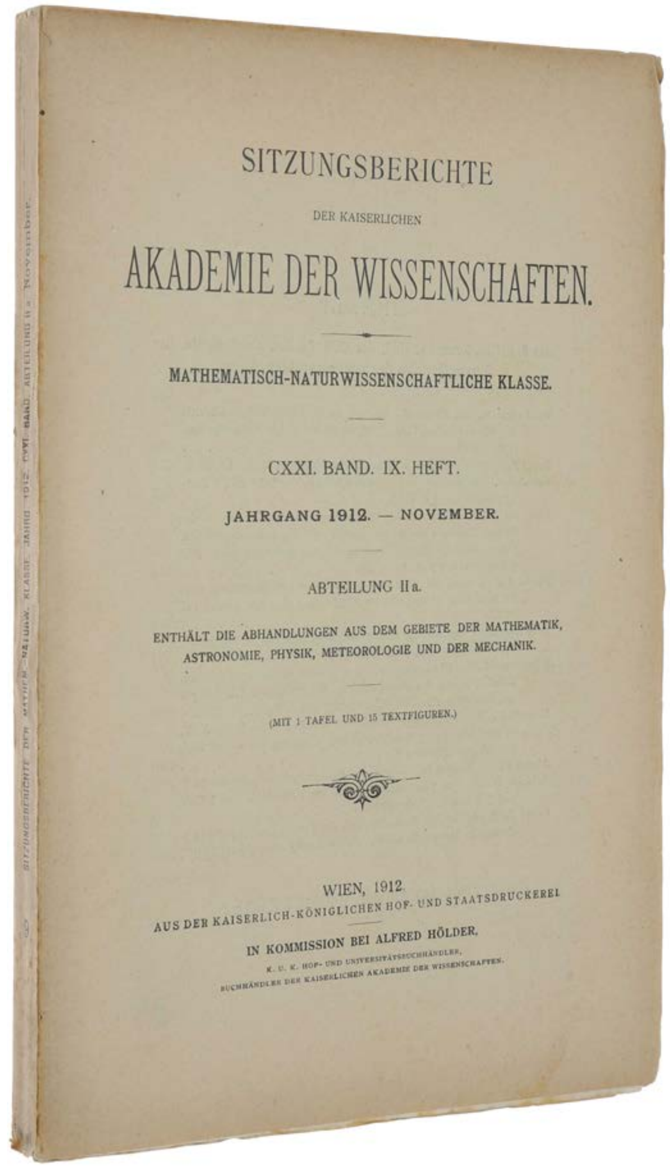
Beide γ-Strahlenapparate zeigten also im allgemeinen eine erhöhte Strahlung gegenüber dem normalen Werte am Erdboden. Der enorme Wert 27·3 beim Apparat I kann nicht als ganz einwandfrei gelten, da bei der Ablesung der Apparat einen unbeabsichtigten starken Stoß erlitt. Wenn man annimmt, daß ein Teil der durchdringenden Strahlung außerterrestrischen Ursprungs ist, z. B. von der Sonne kommt, so kann man erwarten, daß mit zunehmender Mondbedeckung der Sonne die Strahlung abnimmt. Bei der vorletzten und letzten Messung war die Verfinsterung im Mittel 20%, beziehungsweise 53%, trotzdem ist keinerlei Verringerung der Strahlung zu bemerken. Wir werden also schließen müssen, daß, selbst wenn ein Teil der Strahlung kosmischen Ursprungs ist, er jedenfalls nicht von der Sonne stammt, wenigstens solange man eine direkte γ-Strahlung mit geradliniger Ausbreitung im Auge hat. Diese Anschauung ist naheliegend, da überdies ein Unterschied der Strahlung bei Tag und bei Nacht auch im Ballon von mir nie gefunden worden ist.

Zweite Fahrt (26. bis 27. April 1912).

Diese Fahrt war als Dauerfahrt geplant. Der Aufstieg erfolgte um $10^h\ 59^m$ abends vom Prater aus. Es gelang, durch vorsichtige Führung den Ballon durch 6 Stunden in fast keenly interested in creating a more accurate scale of how much radioactivity the human body could tolerate–a difficult thing to determine, since individuals could tolerate different levels, and because the effects were often cumulative, taking as along as 50 years to fully present. He strongly opposed nuclear testing, claiming, "We know too little about radioactivity at this time to state definitely that testing underground or above the atmosphere will have no effect on the human body."

"Hess died on December 17, 1964, but his legacy lives on. In 2004, an observatory opened in the deserts of Namibia to detect gamma rays from cosmic sources. It was named the High Energy Stereoscopic System (HESS) telescope, in homage to the man who discovered cosmic rays" (This Month in Physics History. April 17, 1912: Victor Hess's balloon flight during total eclipse to measure cosmic rays. *APS News*, Vol. 19, No. 4, April 2010 - aps.org/publications/apsnews/201004/physicshistory.cfm).

An abbreviated announcement of Hess's discovery was published simultaneously, under almost the same title, in *Physikalische Zeitschrift*, Band 13 ('Über Beobachtungen der durchdringenden Strahlung bei sieben Ballonfahrten,' pp. 1084-1091) – both papers appeared in the November 1912 issues of their respective journals. The *PZ* version is digitized here: http://inspirehep.net/record/1623161/files/HessArticle.pdf



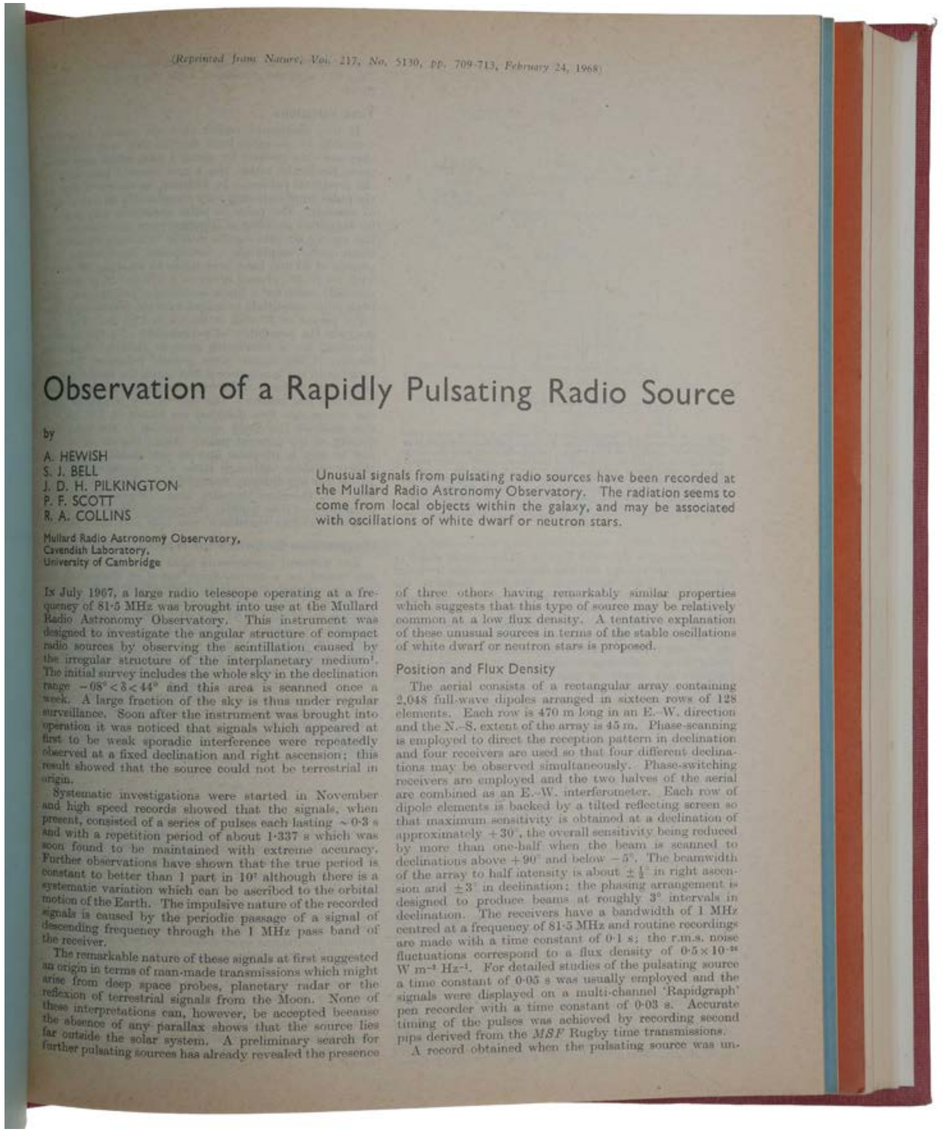
1974 NOBEL PRIZE IN PHYSICS - THE DISCOVERY OF PULARS

HEWISH, A., BELL, S. J., PILKINGTON, J. D. H., SCOTT, P. F. & COLLINS, R. A. *Observations of a rapidly pulsating radio source. Offprint from: Nature, vol. 217, no. 5130, February 24, 1968, pp. 709-713.* London: Macmillan, 1968. *Bound with 17 other offprints and separate publications relating to pulsars and radio astronomy (listed below).*

$4,500

4to (259 x 210 mm), contemporary red cloth, spine lettered in gilt 'Radio Astronomy XVIII'. Two-page typed index loosely inserted.

First edition, the extremely rare offprint, of the discovery of pulsars, here bound in a volume with 17 other offprints and separate works from the library of the Mullard Radio Astronomy Observatory, where the research leading to this discovery was carried out. While pursuing her PhD at Cambridge University, Jocelyn Bell's (b. 1943) advisor was the radio astronomer Anthony Hewish (b. 1924). Hewish and his graduate students in 1967 completed a radio telescope specially designed to observe the scintillation (twinkling) of stars, particularly quasars. That summer, they observed an unusual signal at a wavelength of 3.7m – unusual in that it corresponded to a sharp burst of radio energy at a regular interval of about one second. These were not like signals from other known sources such as stars, galaxies, or solar wind. Bell realized that the unusual reading was regular (every 1.3373011 seconds) and synchronized with sidereal (star) time and not Earth time. That insight, plus the ruling out of various earthbound sources such as pirate radio and police transmissions, suggested that the signal was extraterrestrial. Bell and



Hewish announced the discovery in the offered paper, although they still had not determined the nature of the source. Explanations were soon offered by several leading physicists, but most opinion soon settled on neutron stars as the best solution. Predicted in the 1930s, a neutron star is an incredibly dense, spinning body that is formed when a massive star dies. Bell and Hewish's discovery was the first known evidence for neutron stars, and the pulsing signal source became known as a pulsar. Many more pulsars have since been found. They are now believed to be rapidly rotating neutron stars with intense electromagnetic fields, which emit radio waves from north and south poles. From far off, the spinning pulsar's radio emission is perceived in pulses, much as one perceives a light signal from the rotating lantern of a lighthouse. The discovery of pulsars was a first step in verifying the existence of black holes. In 1968 Bell earned her PhD – pulsars appeared in the appendix of her dissertation. In 1974 Hewish received the Nobel Prize in Physics, jointly with his Cambridge colleague Martin Ryle, but without the inclusion of Bell as a co-recipient. The Royal Swedish Academy of Sciences, in their press release announcing the 1974 Nobel Prize in Physics, cited Ryle and Hewish for their pioneering work in radio-astrophysics, with particular mention of Ryle's work on the aperture-synthesis technique, and Hewish's decisive role in the discovery of pulsars. Many prominent astronomers criticised Bell's omission, including Sir Fred Hoyle. No other copies of this offprint listed on ABPC/RBH. We have located only one other copy, at the Royal Society.

Provenance: Mullard Radio Astronomy Observatory, Cambridge (stamps on several of the works), where the discovery of pulsars was made.

"In 1967, when Jocelyn Bell, then a graduate student in astronomy, noticed a strange 'bit of scruff' in the data coming from her radio telescope, she and her advisor Anthony Hewish initially thought they might have detected a signal from an extraterrestrial civilization. It turned out not be aliens, but it was still quite exciting: they had discovered the first pulsar. They announced their discovery in February 1968.

"Bell, who was born in Ireland in 1943, was inspired by her high school physics teacher to study science, and went to Cambridge to pursue her Ph.D. in astronomy. Bell's project, with advisor Anthony Hewish, involved using a new technique, interplanetary scintillation, to observe quasars. Because quasars scintillate more than other objects, Hewish thought the technique would be a good way to study them, and he designed a radio telescope to do so.

"Working at the Mullard Radio Astronomy Observatory, near Cambridge, starting in 1965 Bell spent about two years building the new telescope, with the help of several other students. Together they hammered over 1000 posts, strung over 2000 dipole antennas between them, and connected it all up with 120 miles of wire and cable. The finished telescope covered an area of about four and a half acres.

"They started operating the telescope in July 1967, while construction was still going on. Bell had responsibility for operating the telescope and analyzing the data — nearly 100 feet of paper every day–by hand. She soon learned to recognize scintillating sources and interference.

"Within a few weeks Bell noticed something odd in the data, what she called a bit of 'scruff.' The signal didn't look quite like a scintillating source or like manmade interference. She soon realized it was a regular signal, consistently coming from the same patch of sky.

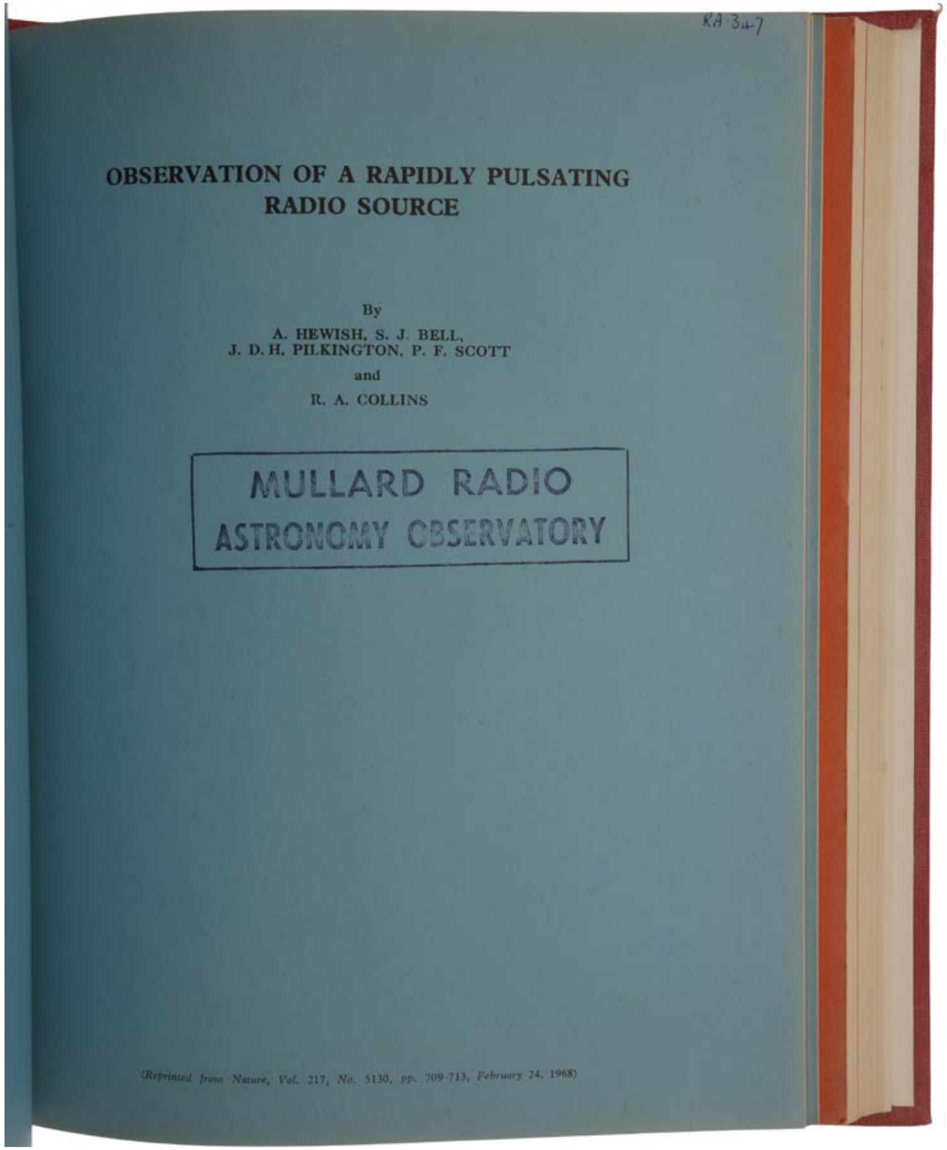
"No known natural sources would produce such a signal. Bell and Hewish began to rule out various sources of human interference, including other radio astronomers, radar reflected off the moon, television signals, orbiting satellites, and even possible effects from a large corrugated metal building near the telescope. None of those could explain the strange signal.

"The signal, a series of sharp pulses that came every 1.3 seconds, seemed too fast to be coming from anything like a star. Bell and Hewish jokingly called the new source LGM-1, for 'Little Green Men.' (It was later renamed.)

"But soon they managed to rule out extraterrestrial life as the source of the signal, when Bell noticed another similar signal, this time a series of pulses arriving 1.2 seconds apart, coming from an entirely different area of the sky. It seemed quite unlikely that two separate groups of aliens were trying to communicate with them at the same time, from completely different locations. Over Christmas 1967, Bell noticed two more such bits of scruff, bringing the total to four.

"By the end of January, Bell and Hewish submitted a paper to Nature describing the first pulsar. In February, a few days before the paper was published, Hewish gave a seminar in Cambridge to announce the discovery, though they still had not determined the nature of the source.

"The announcement caused quite a stir. The press jumped on the story–the possible finding of extraterrestrial life was too hard to resist. They became even more excited when they learned that a woman was involved in the discovery. Bell later recalled the media attention in a speech about the discovery: 'I had my photograph taken standing on a bank, sitting on a bank, standing on a bank examining bogus records, sitting on a bank examining bogus records. Meanwhile the journalists were asking relevant questions like was I taller than or not quite as tall as Princess Margaret, and how many boyfriends did I have at a time?'


OBSERVATION OF A RAPIDLY PULSATING RADIO SOURCE

By

A. HEWISH, S. J. BELL,
J. D. H. PILKINGTON, P. F. SCOTT
and
R. A. COLLINS

MULLARD RADIO ASTRONOMY OBSERVATORY

(Reprinted from Nature, Vol. 217, No. 5130, pp. 709-713, February 24, 1968)

"Other astronomers were also energized by the finding, and joined in a race to discover more pulsars and to figure out what these strange sources were. By the end of 1968, dozens of pulsars had been detected. Soon Thomas Gold showed that pulsars are actually rapidly rotating neutron stars. Neutron stars were predicted in 1933, but not detected until the discovery of pulsars. These extremely dense stars, which form from the collapsed remnants of massive stars after a supernova, have strong magnetic fields that are not aligned with the star's rotation axis. The strong field and rapid rotation produces a beam of radiation that sweeps around as the star spins. On Earth, we see this as a series of pulses as the neutron star rotates, like a beam of light from a lighthouse" (*APS News*).

"Neutron stars were first predicted by Walter Baade and Fritz Zwicky in 1934 but the idea of a rotating neutron star with a strong magnetic field only arose in 1967, championed by Franco Pacini, and independently in 1968 by Thomas Gold, who even made the connection between such stars and the discovery reported by Hewish and Bell in the same year. The importance of this discovery for astrophysics and physics in general was perhaps made crystal clear by the discovery by Joseph Taylor and Russell Hulse in 1975 of a pulsar in a binary system with another neutron star. The orbit of the pulsar was observed to shrink in the exact way that Einstein's general theory of relativity predicted if the system was emitting gravitational waves (and brought in another Nobel Prize for the pulsar field). Today, we are not only detecting pulsars and neutron stars from space, but a network of pulsars is poised to become the largest gravitational-wave detector at our disposal. Gravitational-wave detection aside, pulsars are now being used to probe the most fundamental properties of spacetime and of condensed matter" (*Nature Astronomy*).

"Pulsars are incredible objects: dead stars the size of a city, with more mass than the Sun and magnetic fields as much as 20 trillion times that of Earth, which spin at speeds of up to 70,000 kilometres a second. But astronomers immediately saw beyond pulsars' status as objects of curiosity to their potential as cosmic probes.

"The flashes of light they emit are as regular as a ticking clock. And the timing, polarization and shape of incoming signals give clues to the environment they were born in, as well as the journey they've been on. Since the 1960s, precision studies of pulsar light have allowed astronomers to study everything from the Sun's corona, or outer atmosphere, to the density of matter in the interstellar medium.

"Pulsars also provided a way to study gravity in extreme situations, when, in 1974, astronomers found one orbiting a fellow neutron star in a binary system. This celestial dance also yielded the first evidence of gravitational waves, when the rate at which the stars slowed in their orbit was found to match predictions from Einstein's general theory of relativity about how such rapid, heavy objects should lose energy as they emit ripples in space-time. In 1992, precise measurements of bleeping radio waves from pulsar PSR1257+12 even revealed the first exoplanet.

"Today, astronomers have seen more than 2,000 pulsars, and the flow of ideas for how to use them has not slowed. Members of the Pulsar Timing Array collaboration hope to be able to use pulsars to detect gravitational waves directly, from the way in which the stretching and contracting of space-time subtly shifts the arrival time of pulses from sources across the sky. Studies of pulsars using NASA's Neutron Star Interior Composition Explorer (NICER) should reveal how nuclear forces behave in extreme environments (see Nature 546 (2017), p. 18), and the same mission will test whether pulsars can be used to triangulate position in a navigation system that needs no contact with Earth" ('Pulsar's still dazzle after 50 years,' Nature 547 (2017), pp. 5-6).

Postscript: In September 2018 Bell (now Professor Dame Jocelyn Bell Burnell) won the $3 million Breakthrough Prize both for her discovery of pulsars and for her 'inspiring leadership' over the past five decades. The Breakthrough Prizes were launched in 2012 and are funded by entrepreneurs including Google co-founder Sergey Brin and Facebook chief Mark Zuckerberg. Awarded in fundamental physics, life sciences and mathematics, they are usually handed out in December, based on selections made after an open nomination process. But the selection committee can decide to make special awards, bypassing the standard nomination procedure, to those they deem particularly deserving. "'Jocelyn Bell Burnell's discovery of pulsars will always stand as one of the great surprises in the history of astronomy,' said Edward Witten in a statement. Witten, a physicist at the Institute for Advanced Study, is also chair of the 26-member Selection Committee for Breakthrough's physics prizes. 'Until that moment, no one had any real idea how neutron stars could be observed, if indeed they existed. Suddenly it turned out that nature has provided an incredibly precise way to observe these objects, something that has led to many later advances'" (*Scientific American*).

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'This month in Physics History: February 1968: The Discovery of Pulsars Announced,' *APS News*, vol. 15, no. 2, February 2006. 'On the pulse of discovery,' *Nature Astronomy*, vol. 1 (2017), p. 807.

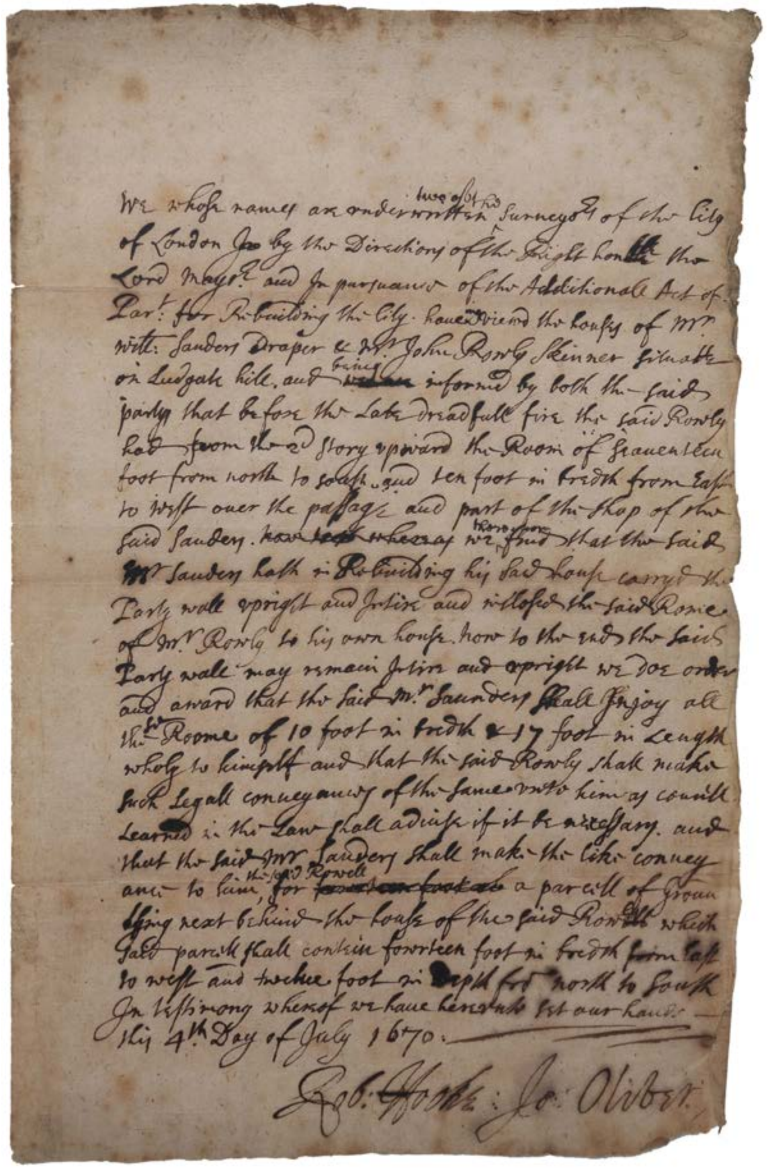
EXTREMELY RARE AUTOGRAPH DOCUMENT BY HOOKE

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


We whose names are underwritten two of the Surveyors of the City
of London by the Directions of the Right honble the
Lord Mayor and in pursuance of the Additionall Act of
Parlt for Rebuilding the City haue viewed the houses of Mr
Will: Sanders Draper & Mr John Rowly Skinner scituate
on Ludgate hill, and being informed by both the said
partys that before the Late dreadfull fire the said Rowly
had from the 2d story upward the Room of seauenteen
foot from north to south and ten foot in bredth from East
to West ouer the passage and part of the shop of the
said Sanders. whereas we therefore find that the said
Mr Sanders hath in Rebuilding his said house carryd the
Party wall upright and intire and inclosed the said Roome
of Mr Rowly to his own house. now to the end the said
Party wall may remain intire and upright we doe order
and award that the said Mr Saunders shall Enjoy all
the sd Roome of 10 foot in bredth & 17 foot in Length
wholy to himself and that the said Rowly shall make
such Legall conueyances of the same unto him as counsill
Learned in the Law shall aduise if it be necessary. and
that the said Mr Sanders shall make the like conuey-
ance to him the said Rowell for a parcill of Groun
lying next behind the house of the said Rowell which
said parcill shall contein fowrteen foot in bredth from East
to west and twelue foot in depth fro north to South
In testimony whereof we haue hereunto set our hands
this 4th Day of July 1670.
Rob: Hooke : Jo: Oliver

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).

AN EXCEPTIONALLY FINE COPY

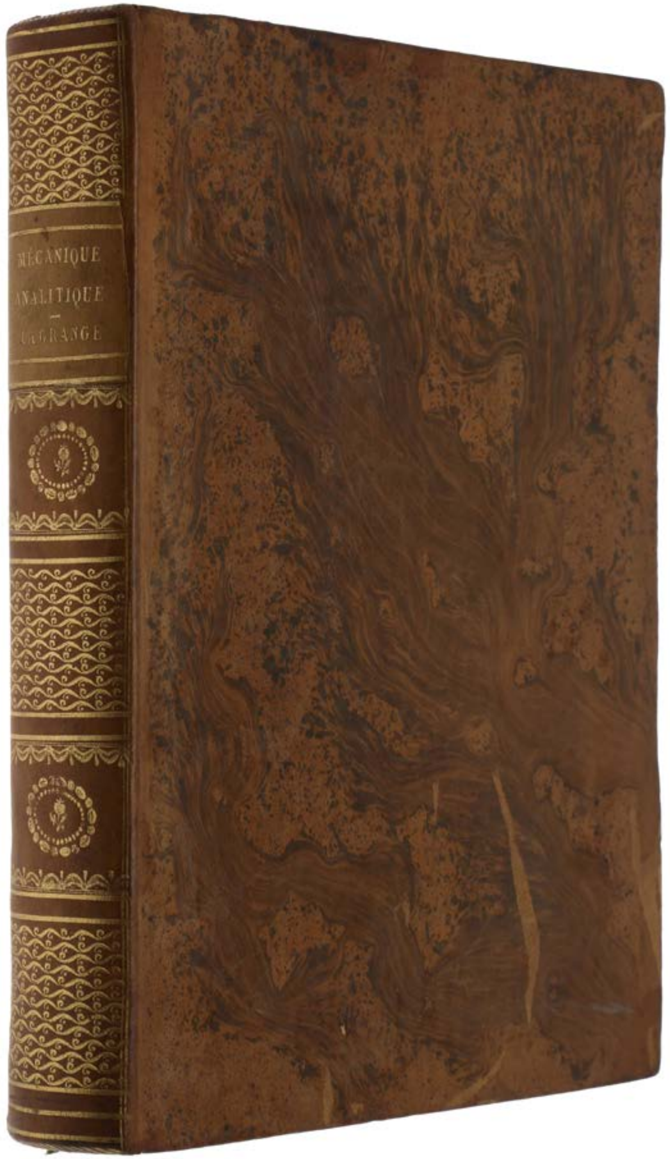
LAGRANGE, Joseph Louis de. *Méchanique analitique.* Paris: Veuve Desaint, 1788.

$12,500

4to (250 x 195 mm), pp. [i-v] vi-xii, [1-] 2-512, contemporary calf, spine lettered in gilt and with elaborate gilt decoration in a floral design (lightly rubbed, one corner worn), internally clean and fresh. A fine, untouched copy.

First edition, and an unusually fine copy, of "perhaps the most beautiful mathematical treatise in existence. It contains the discovery of the general equations of motion, the first epochal contribution to theoretical dynamics after Newton's *Principia*" (Evans). "Lagrange's masterpiece, the *Méchanique Analitique* (Paris, 1788), laid the foundations of modern mechanics, and occupies a place in the history of the subject second only to that of Newton's *Principia*" (Wolf). "With the appearance of the *Mécanique Analytique* in 1788, Lagrange proposed to reduce the theory of mechanics and the art of solving problems in that field to general formulas, the mere development of which would yield all the equations necessary for the solution of every problem ... [it] united and presented from a single point of view the various principles of mechanics, demonstrated their connection and mutual dependence, and made it possible to judge their validity and scope" (DSB). "In the preface of the book La Grange proudly points to the complete absence of diagrams, so lucid is his presentation. He regarded mechanics (statics and dynamics) as a geometry of four dimensions and in this book set down the principle of virtual velocities as applied to mechanics" (Dibner).

"In [*Méchanique Analitique*] he lays down the law of virtual work, and from that



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

"Lagrange introduces the principle of virtual velocities in the first edition as 'a kind of axiom for mechanics' (p. 12) for *statics*, where it 'has all the simplicity one might desire in a fundamental principle' (p. 10). By statics he means the 'science of equilibrium of forces' (p. 1), as he says right at the beginning. If one now considers a system of mass-points in a static equilibrium acted on at any given time by forces P , Q, R, . . . and gives it a small perturbation, then the individual masses experience 'virtual' displacements, that is, displacements compatible with any connections that may exist between the masses. Let δp, δq, δr, … be their projections on the forces P, Q, R, ..., with the sense of direction of the projection indicated by a suitable choice of sign. Lagrange labels these displacements as 'virtual velocities' by appealing to a fixed time element dt. The *principle of virtual velocities* (or *displacements*) now asserts that a system is in equilibrium if the sum of the 'moments of force' vanishes (p. 15):

$$P\delta p + Q\delta q + R\delta r + \cdots = 0.$$

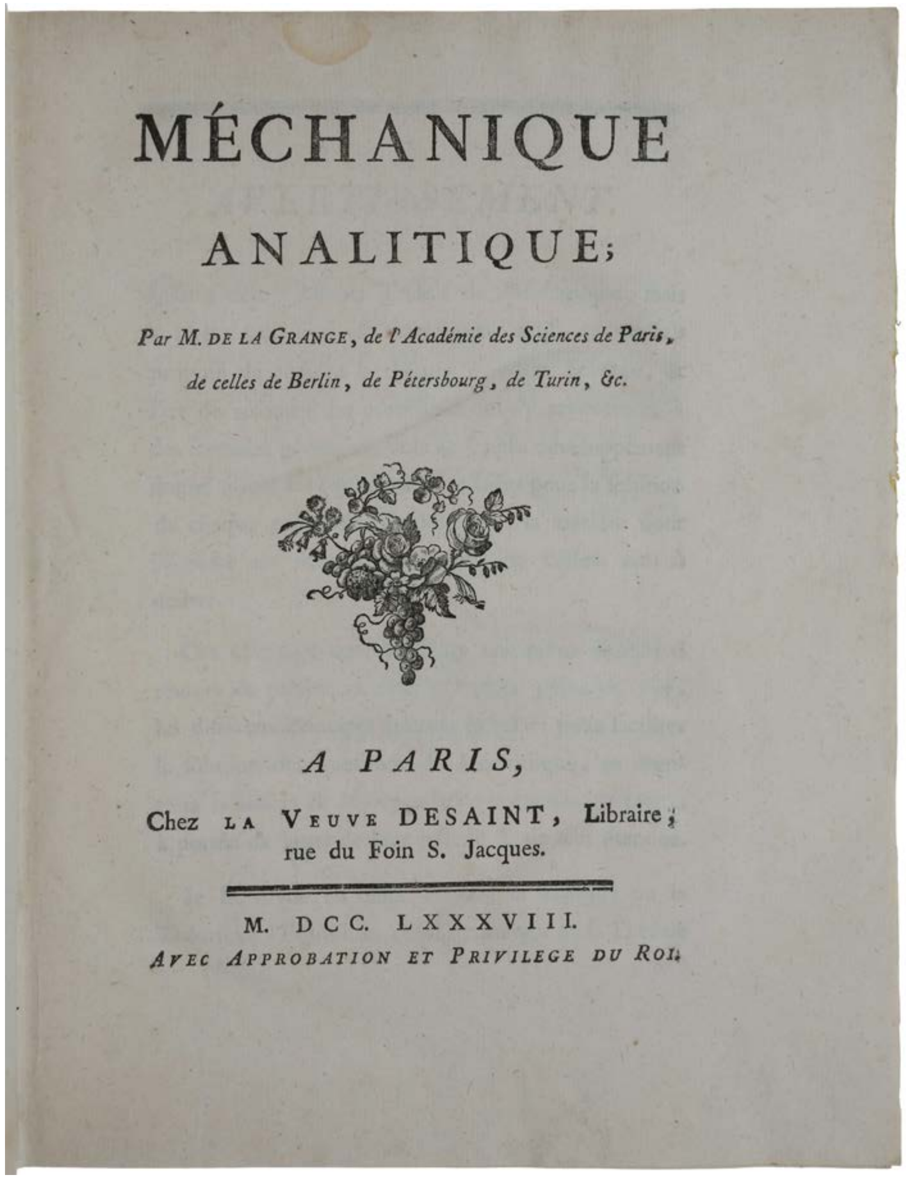
He then applies this relation, from 'Section III' of the *Méchanique analitique*, in the treatment of general properties of the equilibrium of point systems (Section III), methods for solving the resulting equations (Section IV), special problems in statics (Section V), hydrostatics (Section VI), problems of equilibrium of incompressible fluids (Section VII) and problems of equilibrium of compressible and elastic fluids (Section VIII).

"Lagrange constructs *dynamics* in an entirely analogous way. He first extends the principle of virtual velocities to problems of motion in that, as well as the external forces P, Q, R, ..., he also takes into account on the individual point masses their accelerations, which must be compatible with the connections within the system. Multiplication by the instantaneous masses yields the forces that the same accelerations would produce in free masses. His claim is then that under a virtual displacement the 'moments of the forces' P, Q, R, … must be equal to the moments of these forces of acceleration" (Pulte, p. 213). Using the method of 'Lagrange multipliers', Lagrange deduces from this equality the 'Lagrange equations' of motion, which reduce all dynamical problems to the determination of the two functions kinetic and potential energy. This has proved to be enormously influential right up to the present day, when modern quantum field theories are presented in terms of their 'Lagrangian.'

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

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

Grolier/Horblit 61; Evans 10; Dibner 112; Sparrow 120; Norman 1257; Wolf II, 69. Pulte, 'Joseph Louis Lagrange, *Méchanique analitique* (1788),' Chapter 16 in *Landmark Writings in Western Mathematics 1640–1940*, Grattan-Guinness (ed.).



MÉCHANIQUE

ANALITIQUE;

Par M. DE LA GRANGE, de l'Académie des Sciences de Paris, de celles de Berlin, de Pétersbourg, de Turin, &c.

A PARIS,

Chez LA VEUVE DESAINT, Libraire, rue du Foin S. Jacques.

M. DCC. LXXXVIII.

AVEC APPROBATION ET PRIVILEGE DU ROI.


MÉCANIQUE
ANALITIQUE
I
LAGRANGE

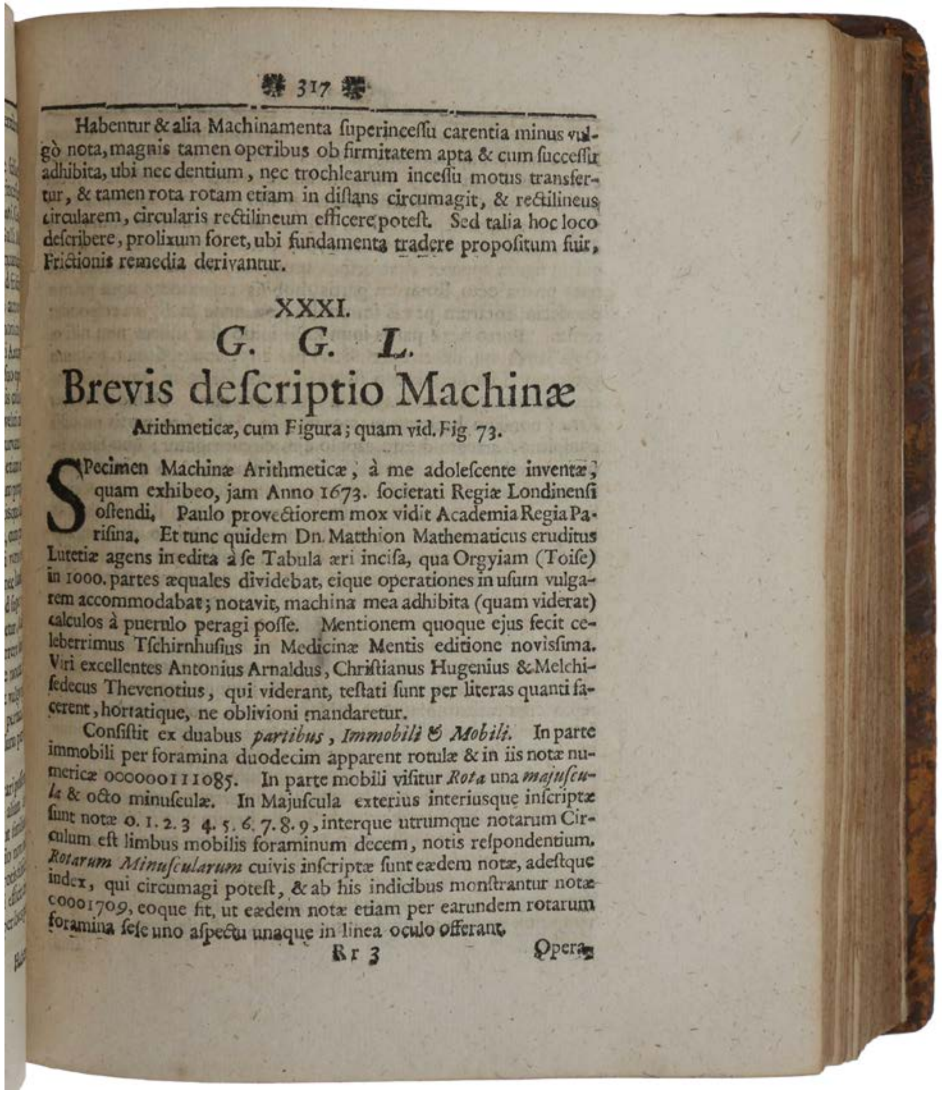
THE LEIBNIZ STEP RECKONER - THE FIRST FOUR-FUNCTION CALCULATOR

LEIBNIZ, Gottfried Wilhelm. *Brevis descriptio machinae arithmeticae, cum figura.* Berlin: Johann Christian Papen, 1710.

$35,000

Pp. 317-319 and one folding plate in Miscellanea Berolinensia ad incrementum scientiarum... Two complete journal volumes bound together, 4to (205 x 168 mm), [1710:] pp. [xxiv, including engraved allegorical frontispiece], 394, with 31 folding engraved plates; [1713:] pp. [xiv, including engraved allegorical frontispiece], 188, with 8 folding engraved plates. Contemporary calf, gilt spine, edges with some rubbing, hinges a little worn and corners slightly bumped, otherwise very good and clean without any restoration.

First edition, rare, of this milestone in computer history, Leibniz's description of his famous calculating machine, the first stepped-drum calculator, and the first machine that could perform multiplication and division. "Leibniz studied Morland's and Pascal's various designs and set himself the task of constructing a more perfect and efficient machine. To begin with, he improved Pascal's device by adding a stepped-cylinder to represent the digits 1 through 9 … In 1694, Leibniz built his calculating machine, which was far superior to Pascal's and was the first general purpose calculating device able to meet the major needs of mathematicians and bookkeepers" (Rosenberg, *The Computer Prophets*, p 48). "[He] invented a device now known as the Leibniz wheel and still in use in some machines . The mechanism enabled him to build a machine which surpassed Pascal's in that it could do not only addition and subtraction fully automatically



317

Habentur & alia Machinamenta superincessu carentia minus vulgò nota, magnis tamen operibus ob firmitatem apta & cum successu adhibita, ubi nec dentium, nec trochlearum incessu motus transfertur, & tamen rota rotam etiam in distans circumagit, & rectilineus circularem, circularis rectilineum efficere potest. Sed talia hoc loco describere, prolixum foret, ubi fundamenta tradere propositum fuit, Frictionis remedia derivantur.

XXXI.

G. G. L.

Brevis descriptio Machinæ

Arithmeticæ, cum Figura; quam vid. Fig 73.

Specimen Machinæ Arithmeticæ, à me adolescente inventæ, quam exhibeo, jam Anno 1673. societati Regiæ Londinensi ostendi, Paulo provectiorem mox vidit Academia Regia Parisina. Et tunc quidem Dn. Matthion Mathematicus eruditus Lutetiæ agens inedita à se Tabula æri incisa, qua Orgyiam (Toise) in 1000. partes æquales dividebat, eique operationes in usum vulgarem accommodabat; notavit, machina mea adhibita (quam viderat) calculos à puerulo peragi posse. Mentionem quoque ejus fecit celeberrimus Tschirnhusius in Medicinæ Mentis editione novissima. Viri excellentes Antonius Arnaldus, Christianus Hugenius & Melchisedecus Thevenotius, qui viderant, testati sunt per literas quanti facerent, hortatique, ne oblivioni mandaretur.

Consistit ex duabus *partibus*, *Immobili & Mobili*. In parte immobili per foramina duodecim apparent rotulæ & in iis notæ numericæ 000000111085. In parte mobili visitur *Rota* una *majuscula* & octo minusculæ. In Majuscula exterius interiusque inscriptæ sunt notæ 0. 1. 2. 3 4. 5. 6. 7. 8. 9, interque utrumque notarum Circulum est limbus mobilis foraminum decem, notis respondentium. *Rotarum Minuscularum* cuivis inscriptæ sunt eædem notæ, adestque index, qui circumagi potest, & ab his indicibus monstrantur notæ 00001709, eoque fit, ut eædem notæ etiam per earundem rotarum foramina sese uno aspectu unaque in linea oculo offerant.

Rr 3 Opera

but also multiplication and division. Leibniz's device enabled his machine to perform the operation of multiplication automatically by repeated additions. His idea was apparently re-invented in 1820 by Charles Xavier de Colmar" (Goldstine, *The Computer from Pascal to Neumann*, p. 7). Although Leibniz demonstrated his machine before the Royal Society and elsewhere, no description of it appeared in print until in the present form. It is contained in the first volume of the journal of the Berlin Academy of Science, which Leibniz founded. Although the volume is naturally present in some institutional holdings, it is absent from many, and is very rare on the market. It contains several other important papers by Leibniz on mathematics and physics (see Ravier for a full list).

"In 1673 German mathematician and philosopher Gottfried Wilhelm Leibniz made a drawing of his calculating machine mechanism. Using a stepped drum, the Leibniz 'Stepped Reckoner', mechanized multiplication as well as addition by performing repetitive additions. The stepped-drum gear, or 'Leibniz Wheel', was the only workable solution to certain calculating machine problems until about 1875. The technology remained in use through the early 1970s in the Curta hand-held calculator. Leibniz first published a brief illustrated description of his machine in 'Brevis descriptio machinae arithmeticae, cum figura. . . ,' *Miscellanea Berolensia ad incrementum scientiarum* (1710), 317-19. The lower portion of the frontispiece of the journal volume also shows a tiny model of Leibniz's calculator. Because Leibniz had only a wooden model and two working metal examples of the machine made, one of which was lost, his invention of the stepped reckoner was primarily known through the 1710 paper and other publications. Nevertheless, the machine became well-enough known to have great influence.

"Leibniz conceived the idea of a calculating machine in the early 1670s with the aim of improving upon Blaise Pascal's calculator, the Pascaline. He concentrated on expanding Pascal's mechanism so it could multiply and divide. The first recorded indirect reference is in a letter from the French mathematician Pierre de Carcavy dated June 20, 1671 in which Pascal's machine is referred to as "la machine du temps passé." Leibniz demonstrated a wooden model of his calculator at the Royal Society of London on February 1, 1673, though the machine could not yet perform multiplication and division automatically. In a letter of March 26, 1673 to Johann Friedrich, where he mentioned the presentation in London, Leibniz described the purpose of the 'arithmetic machine' as making calculations "leicht, geschwind, gewiß" [sic], i.e., easy, fast, and reliable. Leibniz also added that theoretically the numbers calculated might be as large as desired, if the size of the machine was adjusted: "eine zahl von einer ganzen Reihe Ziphern, sie sey so lang sie wolle (nach proportion der größe der Maschine)" ("a number consisting of a series of figures, as long as it may be in proportion to the size of the machine").

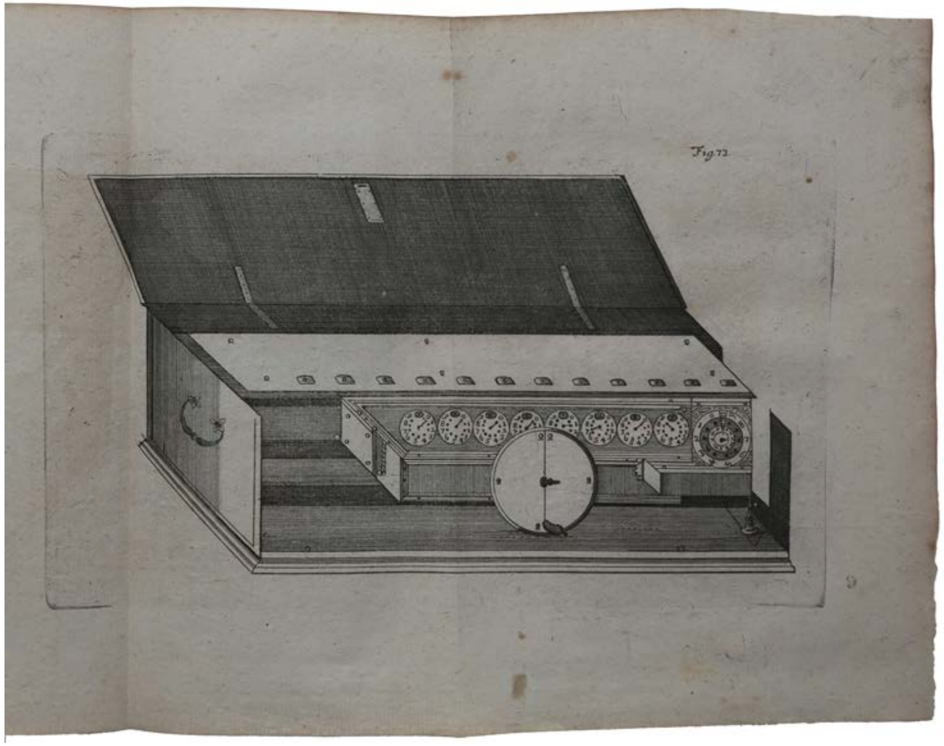
"On July 14, 1674, Leibniz informed Henry Oldenburg, secretary of the Royal Society, that a new model had "at last been successfully completed" and was able to "produce a multiplication by making a few turns of a particular wheel, without any effort." The letter also refers to his good fortune in being able to entrust the work to the Parisian craftsman and clockmaker Olivier (or Ollivier: his first name does not seem to be known), 'a man who preferred fame to fortune' (quoted in M.R. Antognazzi. *Leibniz: an intellectual biography* [2009]). Leibniz showed off an improved version of the calculating machine at the Académie Royale des Sciences in Paris on January 9, 1675, and on his final departure from Paris on October 4, 1676 took a further improved model to show Oldenburg in London.

"After Leibniz's departure, work on the calculating machine continued under the supervision of his Danish friend Friedrich Adolf Hansen (1652-1711), and Leibniz continued to correspond with Olivier. The Leibniz archive includes three letters from Olivier, dated March 24 and July 29, 1677 and November 15, 1678; indeed Leibniz seems to have had some effort made to have Olivier called to

Hanover to continue his work. After about 1678 work on the machine seems to have lapsed until Leibniz began to develop a new prototype in the early 1690s. At some point Leibniz's wooden model and his first metal machine were lost. The second machine, which was built from 1690 to 1720, is preserved in the Niedersächsische Landesbibliothek, Hanover.

"On May 21, 2014 Christie's in London auctioned Leibniz's autograph draft contract between Leibniz's friend Adolf Hansen, acting on Leibniz's behalf and the clockmaker Olivier in Paris, for the construction of Leibniz's calculating machine. The 3.5 page contract written by Leibniz in French consisted of 20 numbered articles with some details of payments left blank. The contract was undated but Christie's assigned to it the date of circa 1677" (historyofinformation.com).

"The contract comprises 20 meticulously detailed clauses, describing in detail the machine and the financial and practical arrangements for its construction: it is to produce numbers up to three figures; it is to be capable of multiplication and division, as well as addition and subtraction, with the mechanism (consisting of a system of fixed and mobile pieces, and equal and unequal cogs) described in detail, first for multiplication and division, then for addition and subtraction, noting that the operations should be effected immediately 'et non pas comme dans la machine du temps passé après un delay ou intervalle'; the machine is to be perfectly finished, made of iron or steel, and enclosed in 'une petite boëtte propre, à fin qu'il ne paroisse que ce qu'il faut pour l'opération'; the operation of the machine is then specified. The contract goes on to note that Olivier had previously agreed to construct such a machine in one or two months for a payment of 'cent écus blancs ou trois cens francs', part of which has been advanced, but that he had failed (in part because of illness) to give satisfaction; he now engages to complete the work in three months, with his goods as surety; and he is to show the progress of his work to Hansen, and inform Leibniz by letter, each week"



(Catalogue of Valuable Manuscripts and Printed Books, Sale 1550, Christie's, King Street, London, 21 May 2014).

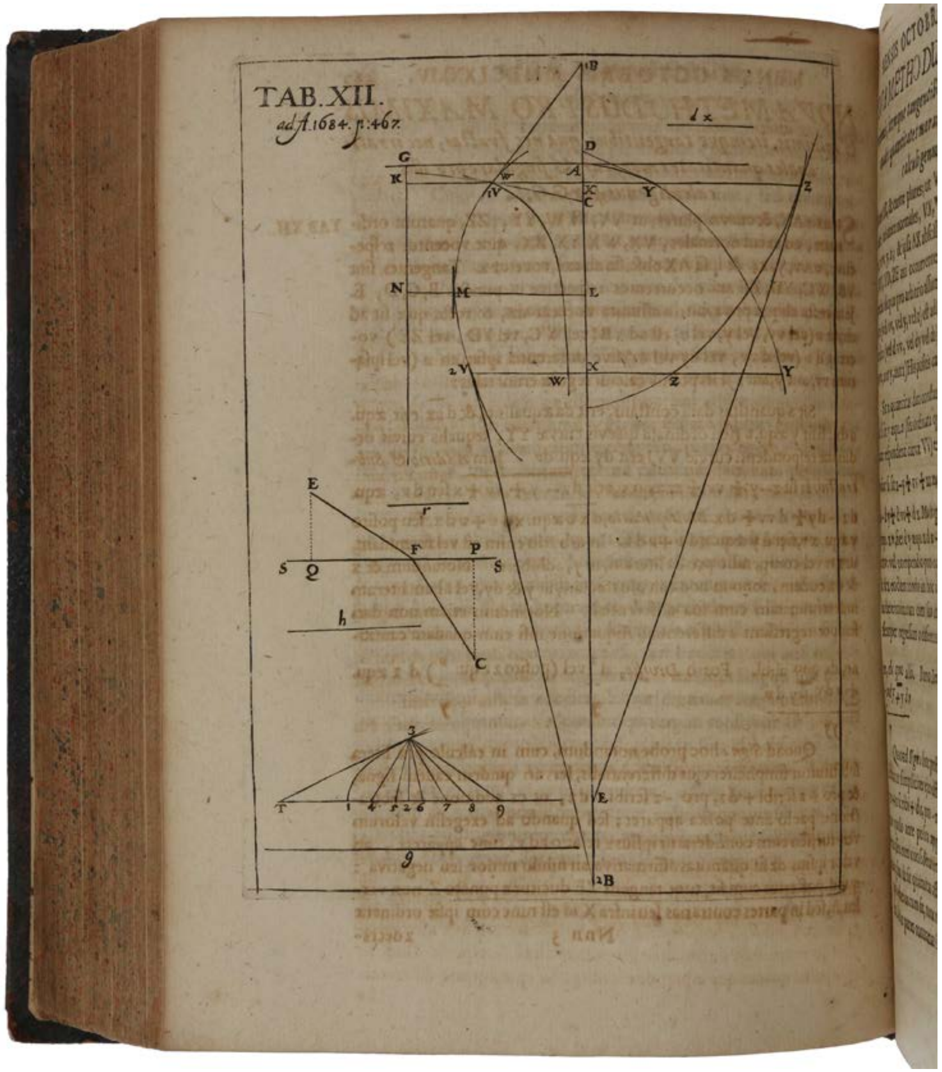
Ravier 305; Parkinson, *Breakthroughs*, p. 113.

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.

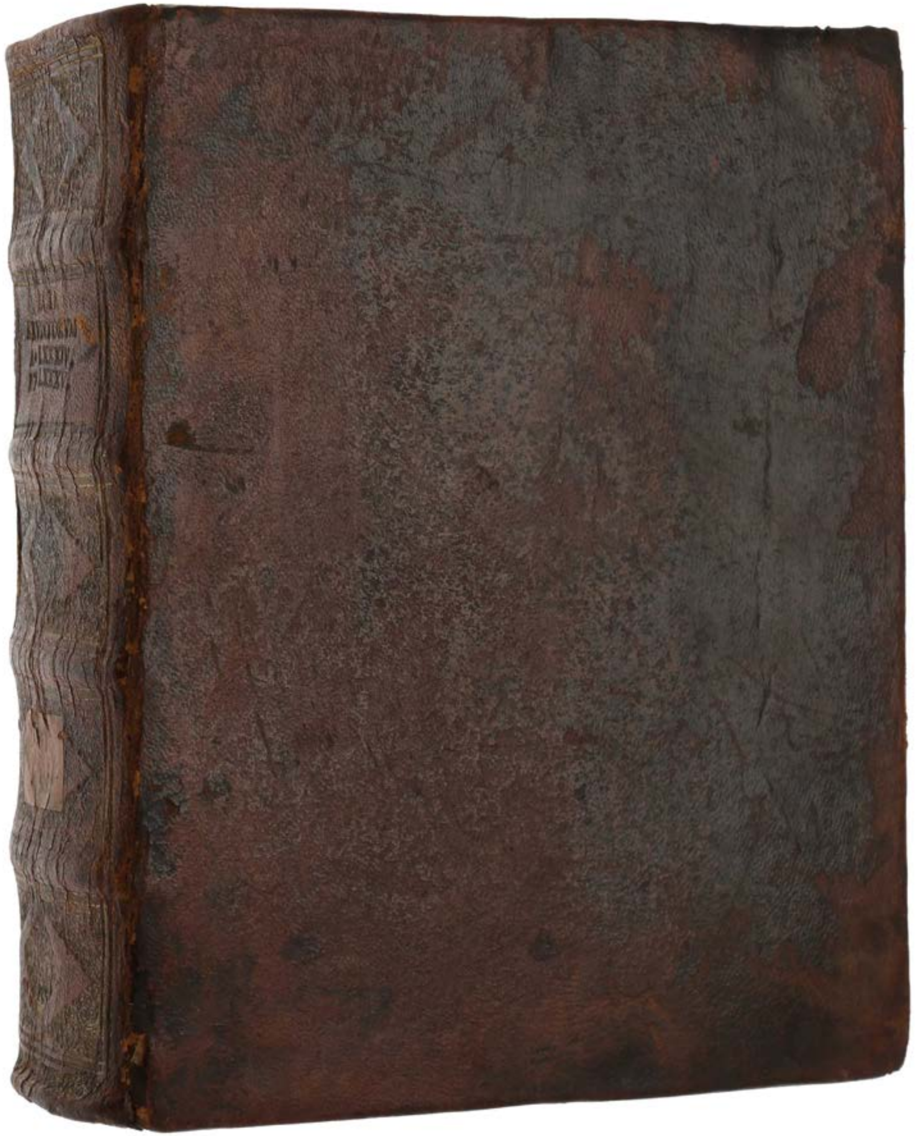
"In Leibniz's recollections of the origin of his differential calculus he relates that reflecting on the arithmetical triangle of Pascal he formed his own harmonic triangle in which each number sequence is the sum-series of the series following it and the difference-series of the series that precedes 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.

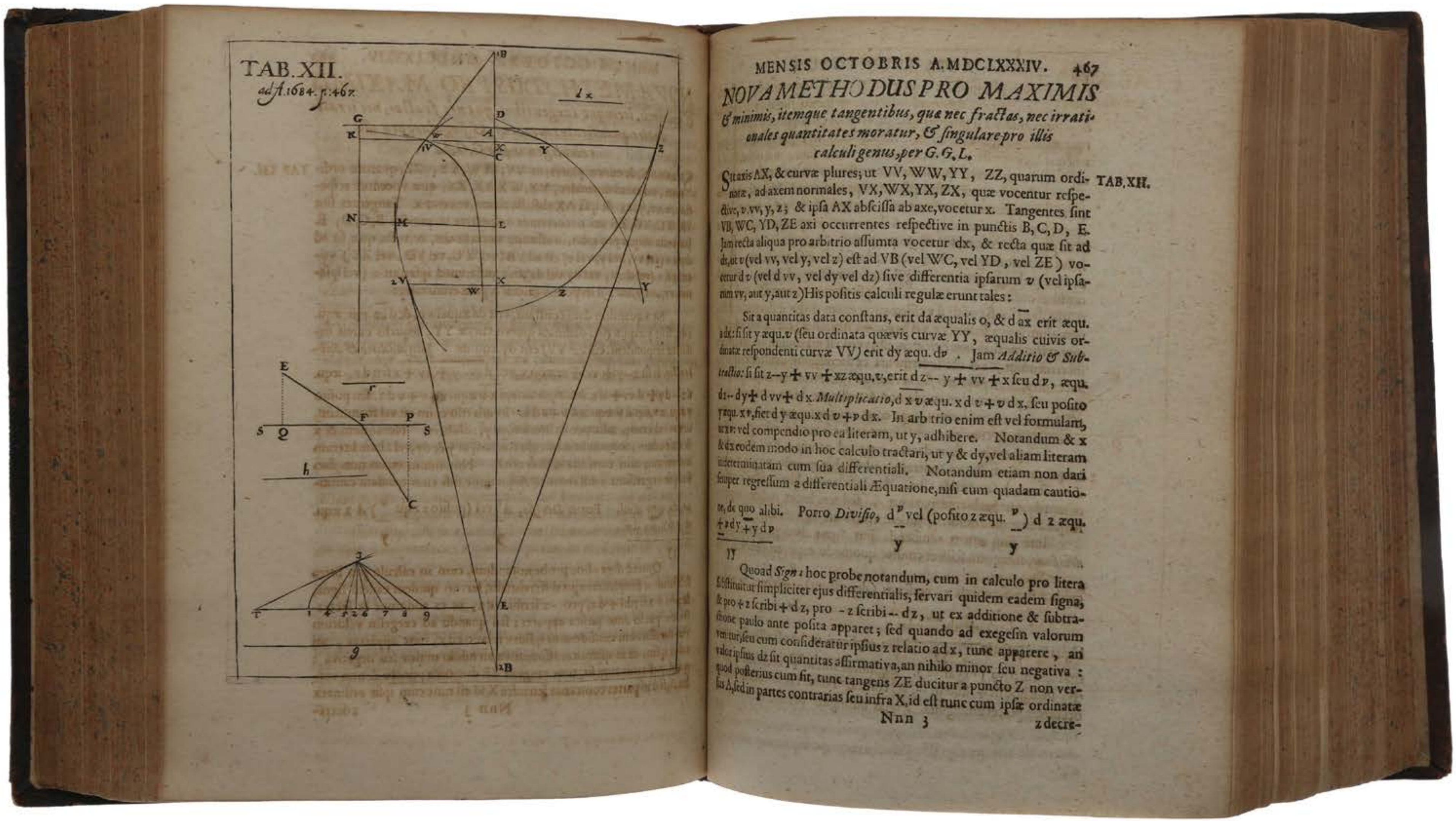
"Leibniz then set out the differentiation rules for powers, roots and composite functions. In the latter case, he chose to connect a generic curve to the cycloid because he wanted to demonstrate that his calculus was easily also applied to transcendent curves, possibility that Descartes wanted to exclude 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.
Nnn 3

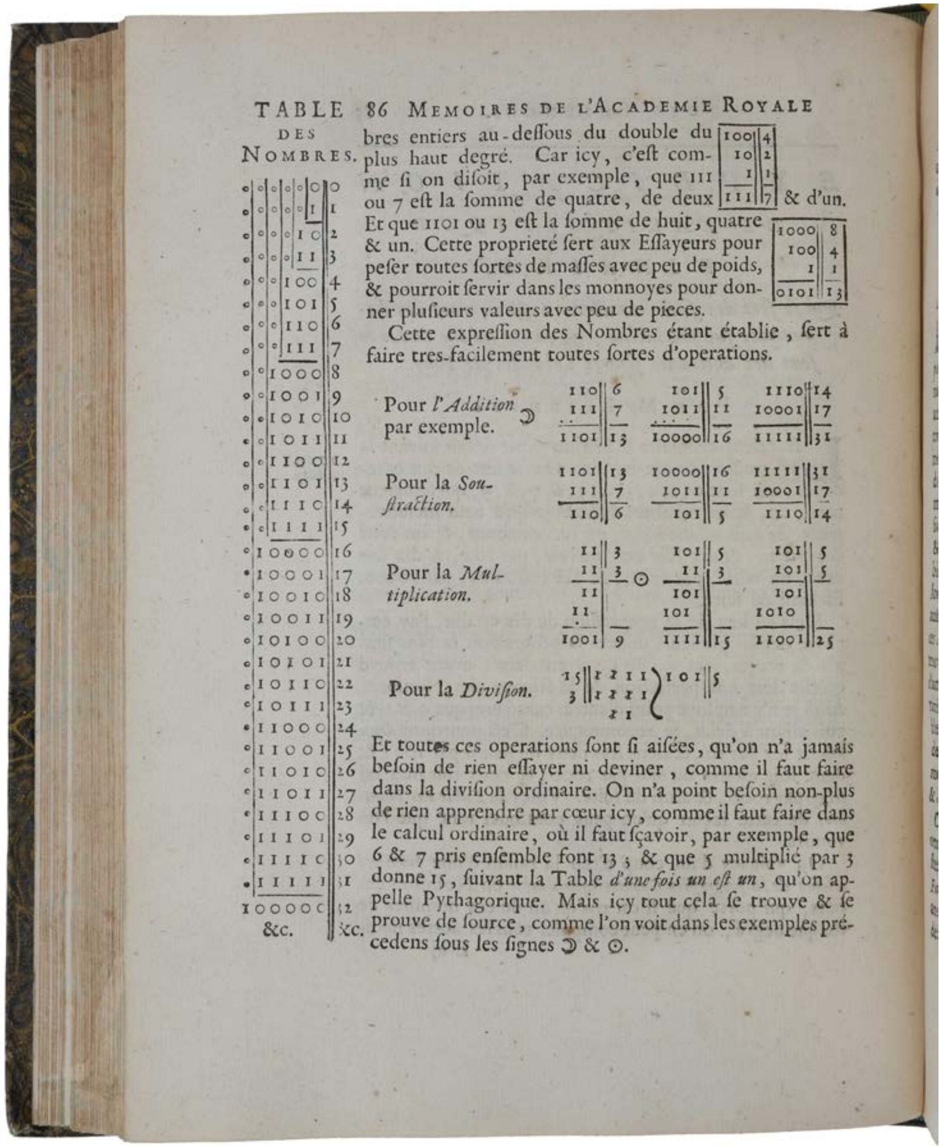
THE FOUNDATION OF THE ELECTRONIC COMPUTER INDUSTRY

LEIBNIZ, Gottfried Wilhelm. *'Novelle Arithmetique binaire'*. [With:] *'Explication de l'Arithmetique binaire, qui se sert des seuls caracteres 0 & 1; avec des remarques sur son utilité, & sue ce qu'elle donne le sens des anciens figues Chinoises de Fohy'*. Pages 58-63 (*Histoires*) and 85-9 (*Mémoires*) in *Histoire de l'Académie Royale des Sciences Année MDCCIII. Avec les Mémoires de Mathématiques & de Physique, pour la même Année*. Paris: Jean Boudot, 1705.

$17,500

4to (242 x 184 mm), in: Histoire de l'Académie Royale des Sciences, année 1703 (printed 1705), pp. 85-89. The complete volume offered here in a fine mid-19th-century half morocco binding with richly gilt spine and five raised bands. A very fine, clean, and completely unmarked copy of the rare original Paris edition (a later Amsterdam reprint was also issued). Fully complete with frontispiece, (10), 148, 467, (1:blank), (1:errta), (1:blank) pp. and 12 engraved plates.

First edition, first issue, of Leibniz's invention of binary arithmetic, the foundation of the electronic computer industry. This is the second of Leibniz's great trilogy of works on mathematics and computation, following *Nova methodus pro maximis et minimis* (1684), his independent invention of calculus, and preceding *Brevis descriptio machinae arithmeticae* (1710), his (decimal) mechanical calculating machine. "A dated manuscript by Gottfried Wilhelm Leibniz, preserved in the Niedersachsische Landesbibliothek, Hannover, 'includes a brief discussion of the possibility of designing a mechanical binary calculator which would use moving balls to represent binary digits.' Though Leibniz thought of the application of



binary arithmetic to computing in 1679, the machine he outlined was never built, and he published nothing on the subject until [the offered work]" (Norman). Leibniz viewed binary arithmetic less as a computational tool than as a means of discovering mathematical, philosophical and even theological truths. It was a candidate for the characteristica generalis, his long sought-for alphabet of human thought. With base 2 numeration Leibniz witnessed a confluence of several intellectual strands in his world view, including theological and mystical ideas of order, harmony and creation. ABPC/RBH list only one copy of this first issue (Zisska & Schauer, May 4, 2011, lot 461, €5,616). The copy of the extracted leaves sold at the Hans Merkle sale (Reiss, Auktion 85, October 15, 2002, lot 696) realised €6500.

"About this time [1679] Leibniz also outlined a design for a calculating machine to operate the four rules in binary arithmetic, though he recognised that the development of such a machine would not be easy. Owing to the great number of wheels needed, the problems related to friction and smooth movement already encountered with the ordinary calculating machine would be more serious, while the greatest difficulty would be the mechanical conversion of ordinary numbers into binary and the binary answers into ordinary numbers. Perhaps it was on account of these seemingly insuperable obstacles that Leibniz failed to mention the binary calculating machine in his correspondence. Concerning the 'binary progression' itself, he remarked to Tschirnhaus in 1682 that he anticipated from its use discoveries in number theory that other progressions could not reveal" (Aiton, p. 104).

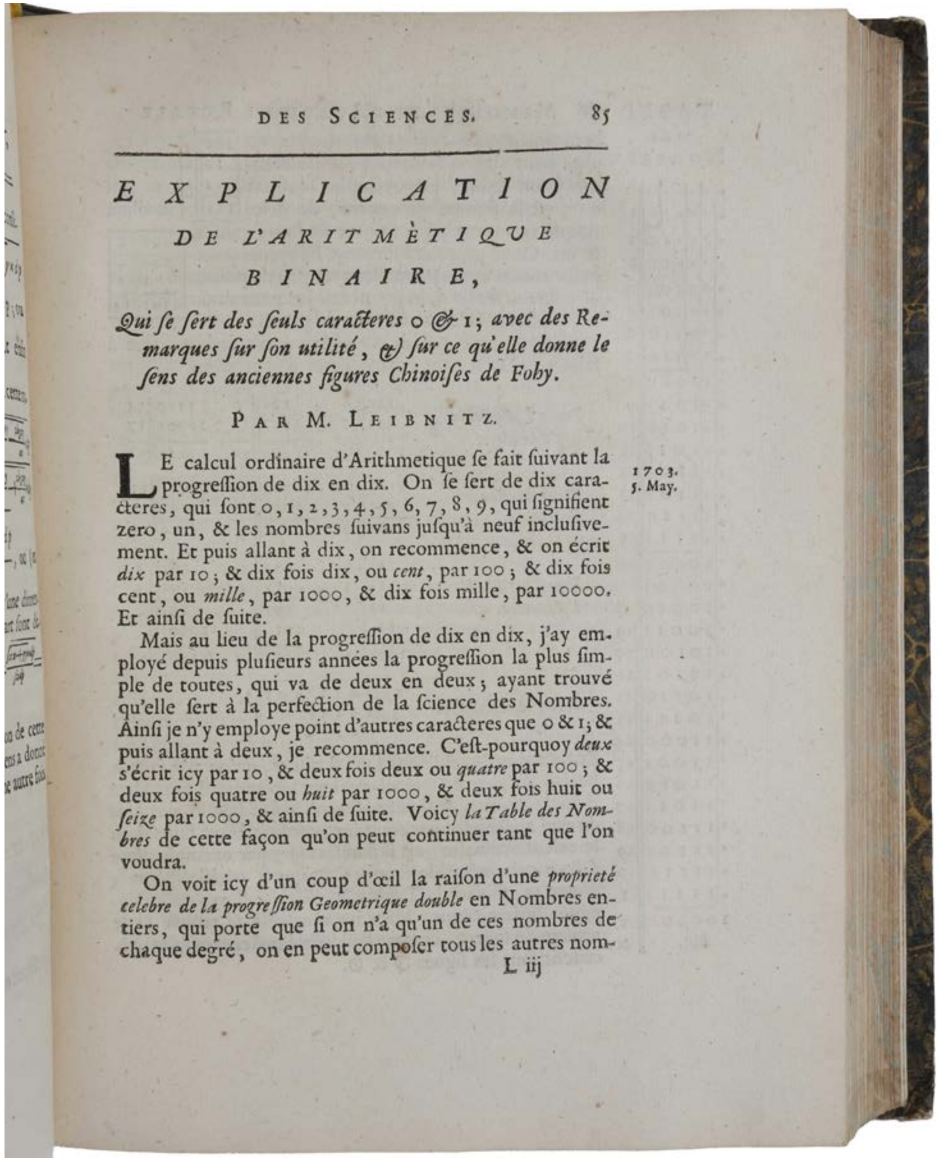
"… in April [1697] he [Leibniz] edited a collection of letters and essays by members of the Jesuit mission in China, entitled Novissima Sinica … One of the copies of the Novissima Sinica that Leibniz sent to to Verjus [Antoine Verjus, the leader of the mission] came into the hands of Joachim Bouvet, a member of the Mission who had just returned home to Paris on leave. Bouvet wrote to Leibniz on 18 October 1697 expressing his commendation of the Novissima Sinica and giving him more recent news from China … In the years that followed , the correspondence with Bouvet proved to be of great importance in relation to the dissemination of Leibniz's binary arithmetic" (ibid., pp. 213-4).

"In his reply of 2 December 1697 to Bouvet's first letter, Leibniz described the nature of his own researches, in which he had shown by mathematics that the Cartesians did not have the true laws of nature. To arrive at these, he explained, it was necessary to suppose in nature not only matter but also force, and the forms or entelechies of the ancients were nothing other than forces. Bouvet, in his letter of 28 February 1698, written before his return to Peking, expressed the view that the ancient Chinese philosophy did not differ from that of Leibniz, for it supposed in nature only matter and movement, which was the same as form, or what Leibniz called force. The ancient Chinese philosophy, he added, was embodied in the hexagrams of the I ching, of which he had found the true meaning. In his view they represented in a very simple and natural manner the principles of all the sciences, or rather a complete system of a perfect metaphysics, of which the Chinese had lost the knowledge a long time before Confucius. It is in the 'Great appendix' of the I ching that the words 'yin' and 'yang' make their first appearance in philosophical terms, used to describe the fundamental forces of the universe, symbolising the broken and full lines of the trigrams and hexagrams" (ibid., p. 245).

"Early in 1700 Leibniz was elected a foreign member of the reconstituted Royal Academy of Sciences in Paris … In return for his election to the Academy, he contributed papers on the binary system of arithmetic" (ibid., p. 218).

"During his visit to Berlin in the summer of 1700, Leibniz evidently sought the collaboration of the Court mathematician Philippe Naudé in further researches on the binary system. For on his return from the conversations on Church reunion with Buchhaim in Vienna, he received a letter from Naudé containing tables of series of numbers in binary notation, including the natural numbers up to 1023. Thanking Naudé for the pains he had taken to compile these tables, Leibniz explained his intention to investigate the periods in the columns of the various series of numbers. For it was remarkable that series – such as the natural numbers, triples, squares, and figurate numbers generally – not only have periods in the columns but that in every case the intervals are the same, namely 2 in the units column, 4 in the twos column, 8 in the fours column, and so on. In the case of the triples, for example, the periods in the last three columns were 01, 0110 and 00101101. Already he had noticed a good theorem: that the periods consisted of two halves in which the 0s and 1s were interchanged; but the general rule for the periods in successive columns had thus far eluded him …

“The possession of Naudé’s tables enabled Leibniz to compose his Essay d’une nouvelle science des nombres, which he sent from Wolfenbüttel on 26 February 1701 to the Paris Academy of Sciences to mark his election as a foreign member. In his essay, and also in his letter to Fontenelle, he explained that the new system of arithmetic was not intended for practical calculation but rather for the development of number theory. To Fontenelle he remarked that, before publication, there was perhaps a need to add something more profound and he hoped that some young scholar might be stimulated to collaborate with him to this end … Concerning his decision to communicate his binary system, although the applications had not been achieved, Leibniz explained [in a latter to L’Hospital] that, in view of his many commitments that prevented him from bringing his researches to completion, he feared that his continued silence might lead to the loss of an idea which seemed worthy of conservation.



DES SCIENCES. 85

EXPLICATION

DE L'ARITMÈTIQUE

BINAIRE,

Qui se sert des seuls caractères 0 & 1; avec des Remarques sur son utilité, & sur ce qu'elle donne le sens des anciennes figures Chinoises de Fohy.

PAR M. LEIBNITZ.

LE calcul ordinaire d'Arithmetique se fait suivant la progression de dix en dix. On se sert de dix caractères, qui sont 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, qui signifient zero, un, & les nombres suivans jusqu'à neuf inclusivement. Et puis allant à dix, on recommence, & on écrit *dix* par 10; & dix fois dix, ou *cent*, par 100; & dix fois cent, ou *mille*, par 1000, & dix fois mille, par 10000. Et ainsi de suite. 1703. 5. May.

Mais au lieu de la progression de dix en dix, j'ay employé depuis plusieurs années la progression la plus simple de toutes, qui va de deux en deux; ayant trouvé qu'elle sert à la perfection de la science des Nombres. Ainsi je n'y employe point d'autres caracteres que 0 & 1; & puis allant à deux, je recommence. C'est-pourquoy *deux* s'écrit icy par 10, & deux fois deux ou *quatre* par 100; & deux fois quatre ou *huit* par 1000, & deux fois huit ou *seize* par 1000, & ainsi de suite. Voicy *la Table des Nombres* de cette façon qu'on peut continuer tant que l'on voudra.

On voit icy d'un coup d'œil la raison d'une *proprieté celebre de la progression Geometrique double* en Nombres entiers, qui porte que si on n'a qu'un de ces nombres de chaque degré, on en peut composer tous les autres nom-

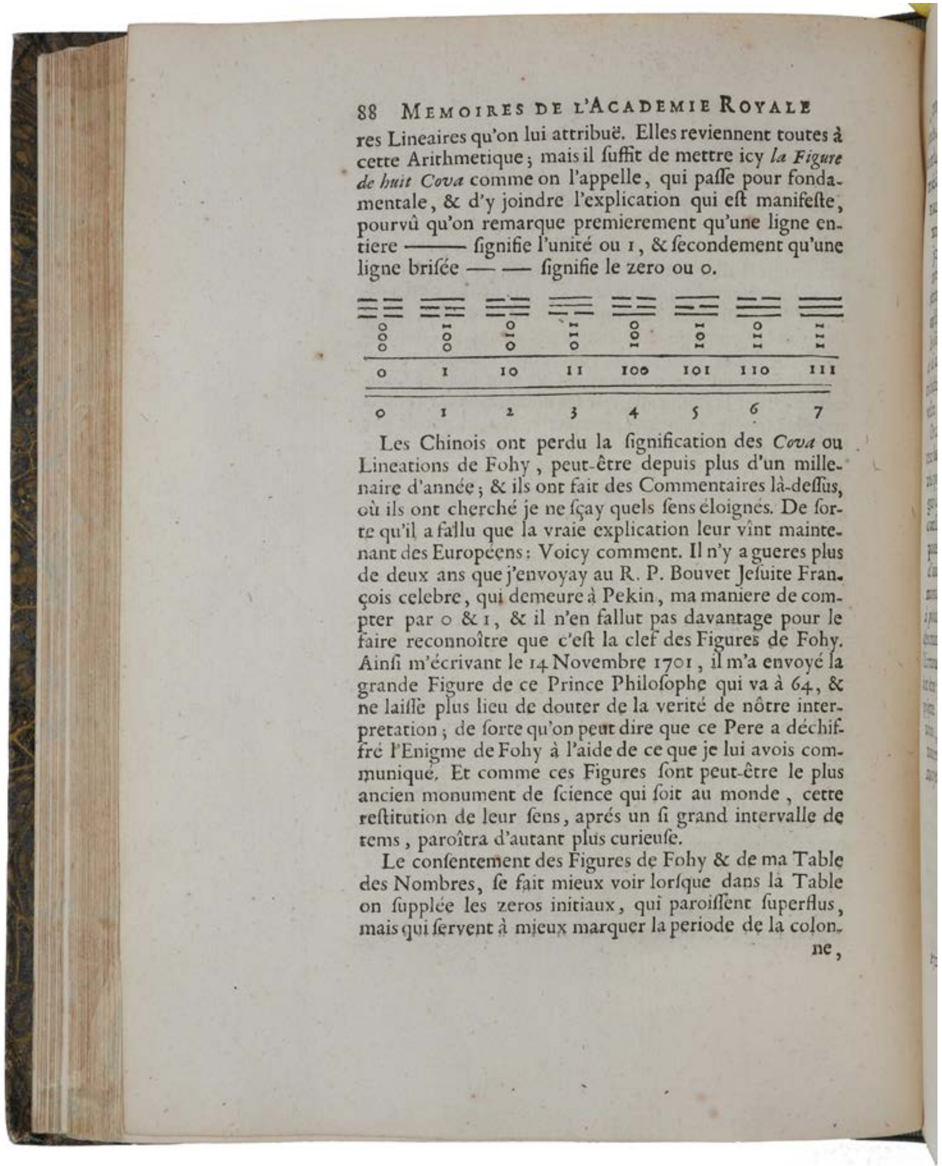
L iij

"Leibniz write to Bouvet on 15 February 1701, at the time he was compounding his essay for the Paris Academy, and it was therefore natural that he should describe for his correspondent the principles of his binary arithmetic, including the analogy of the formation of all the numbers from 0 to 1 with the creation of the world by God out of nothing. Bouvet immediately recognised the relationship between the hexagrams and the binary numbers and he communicated his discovery in a letter written in Peking on 4 November 1701. This reached Leibniz in Berlin, after a detour through England, on 1 April 1703. With the letter, Bouvet enclosed a woodcut of the arrangement of the hexagrams attributed to Fu-Hsi, the mythical founder of Chinese culture, which holds the key to the identification …

"Leibniz accepted Bouvet's discovery with great enthusiasm. Having no reason to doubt the antiquity of the Fu-Hsi arrangement of the hexagrams that Bouvet had sent him, he was evidently delighted that this figure – 'one of the most ancient monuments of science', as he described it – should have been found to be in agreement with his own binary arithmetic" (ibid., pp. 245-7).

"Within a week of receiving Bouvet's letter, Leibniz had communicated the discovery to his friend Carlo Mauritio Vota, the Confessor of the King of Poland, and sent to Abbe Bignoin for publication in the Memoires of the Paris Academy his Explication de l'Arithmetique binaire, qui se sert des seuls caracteres 0 & 1; avec des remarques sur son utilité, & sue ce qu'elle donne le sens des anciens figues Chinoises de Fohy. Ten days later he sent a brief account to Hans Sloane, the Secretary of the Royal Society" (ibid., p. 247).

"Owing to his separation from the real scholars of the time, who for political reasons shunned the Court circles on which he had to rely for his information, Bouvet had been mistaken in his belief in the antiquity of the Fu-Hsi arrangement



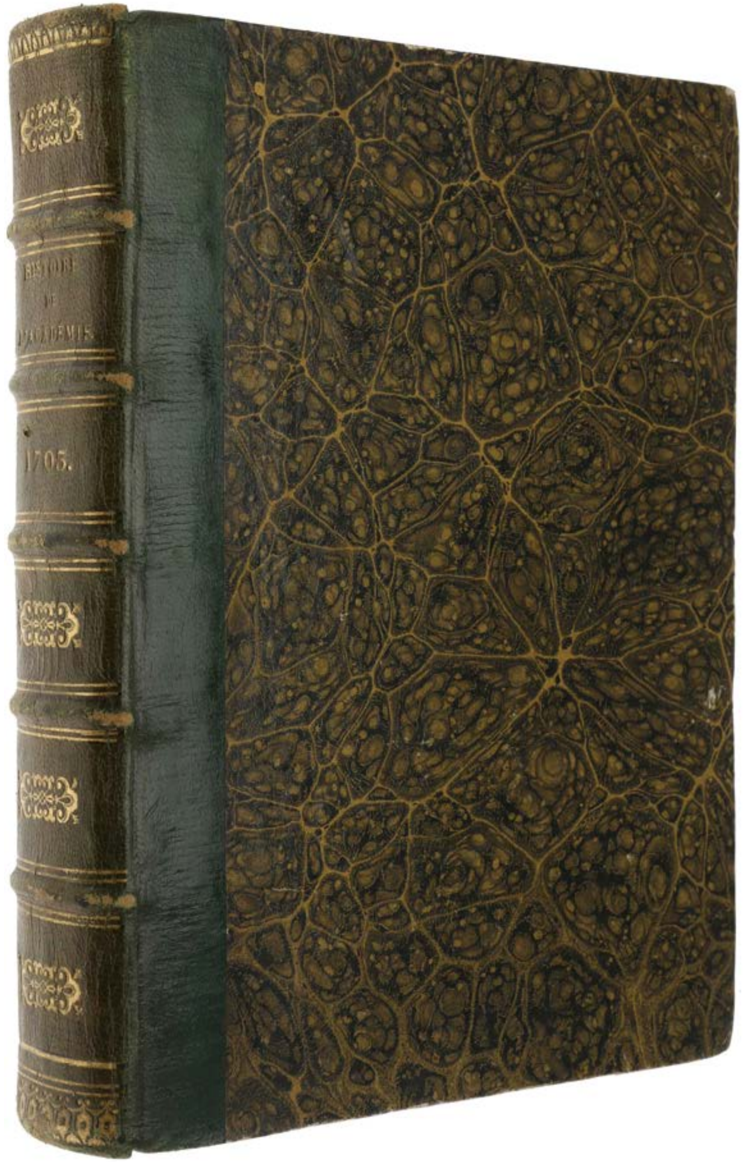
of the hexagrams. For this order was the creation of Shao Yung, who lived in the eleventh century … In the I ching the hexagrams are arranged in a different order, attributed to King Wen (ca. 1050 BC) … This order lacks even a superficial resemblance to a number system.

"Bouvet's great discovery, to which Leibniz gave hia enthisuasitc support, was therefore a misinterpretation based on bad Sinology. Generously but mistakenly, Leibniz had been willing to follow Bouvet in attributing his own invention to Fu-Hsi, thereby giving support to the myth that the ancient Chinese possessed advanced scientific knowledge which later generations had lost" (ibid., 247-8).

Nevertheless, combinatorial aspects susceptible to binary interpretations do exist in the Figures of Fu-Hsi, as has been demonstrated by F. van der Blij of the Mathematical Institute at Utrecht.

The article 'Nouvelle Arithmetique binaire' in the Histoire part of this volume is unsigned, but is actually by Bernard Le Bovier de Fontenelle. His article constituted an editorial comment on the 'Explication' of Leibniz.

"Fontenelle pointed out that ten need not be the base of our arithmetic, and that indeed certain other bases would have advantages over it. Base 12, for example, would simplify dealings with certain fractions such as 1/3 and 1/4. He also noted that numbers have two sorts of properties, essential ones and those dependent on the manner of expressing them. As an example of the former he cited the property that the sum of the first n odd numbers equals n2, and of the latter that a number divisible by 9 has a digit sum also divisible by 9. This same property would hold for 11 in the case of base 12. He reported that Leibniz had worked with the simplest of all possible bases, base two. This base was not recommended



for common use because of the excessive length of its number representations, but Leibniz considered it particularly suitable for difficult research and as possessing advantages absent from other bases. Fontenelle reported further that Leibniz had communicated this binary arithmetic in 1702, but had asked that no mention of it be made in the Histoire until he could supply an application. This application eventually came forth in the binary interpretation of the Figures of Fohy. The rest of Fontenelle's article is devoted to reporting that binary arithmetic was invented not only by Leibniz, but also by Professor Lagny at about the same time [i.e., Tomas Fantet de Lagny (1660-1734)]" (Glaser, p. 44).

This volume was reissued at Paris in 1710 (this is the edition reproduced on gallica), and later in octavo format at Amsterdam.

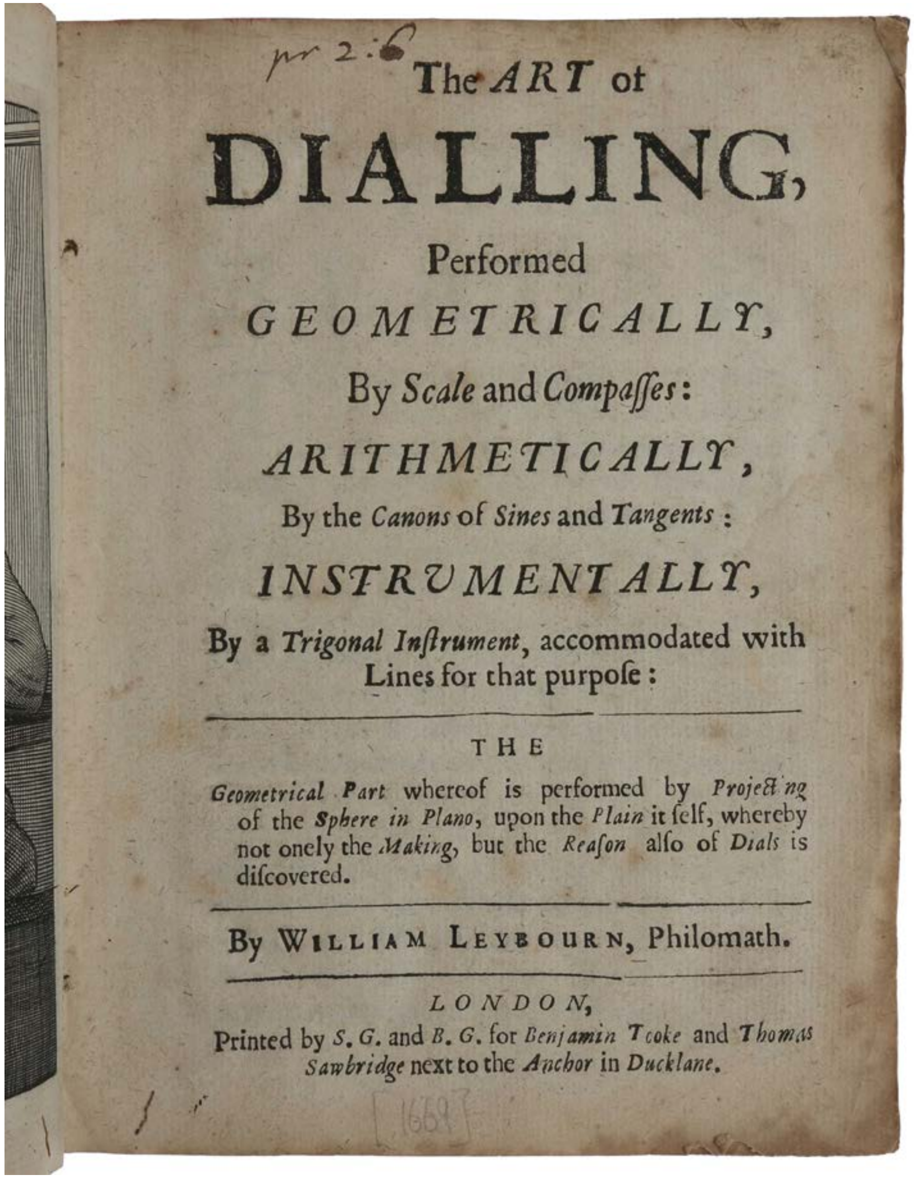
THE EXTREMELY RARE FIRST ISSUE

LEYBOURN, William. *The art of dialling performed geometrically by scale and compasses: arithmetically, by the canons of sines and tangents: instrumentally, by a trigonal instrument, accommodated with lines for that purpose; The geometrical part whereof is performed by projecting of the sphere in plano, upon the plain it self, whereby not only the making, but the reason also of dials is discovered.* London: Printed by S[arah]. G[riffin]. and B[ennet]. G[riffin]. for Benjamin Tooke and Thomas Sawbridge, [1669].

$3,500

Small 4to (189 x 146 mm), pp. [viii], 175, [1], with engraved portrait frontispiece of Leybourn and one folding plate. Contemporary calf (a little rubbed). A very good unrestored copy.

First edition, first issue, extremely rare, of the first of Leybourn's books on the subject. This issue has 'dialling:' in line 2 of the title and lacks imprint date; the more common (but still rare) second issue has 'Dialling,' and imprint date 1669. This, the Kenney copy, is one of perhaps only two known examples of the first issue, ESTC listing only the BL copy. "In 1669 Leybourn authored *The Art of Dialling*, a book on the use of sundials and astrolabes in determining the position of vessels at sea. The contemporary expansion of the Royal Navy and Merchant Marines created a significant demand for such manuals, and *The Art of Dialling* was well written, easy to understand and cheaply produced" (Wikipedia)."The design of sundials represented a steady source of income for independent mathematicians such as William Leybourn. It is quite straightforward to design a standard garden sundial on the horizontal plane with the gnomon angled toward the north celestial pole. It is another matter to design a dial on a wall of a building



that is situated at an odd angle to the cardinal compass points. Leybourn was well-known for his books on dialling that treat such arcane subjects as how to create a dial on a ceiling, the sun being reflected from a mirror attached to the windowsill. In this, the first of his several volumes on the subject, he does not consider these very unusual situations but introduces the reader to such fundamental concepts as how to measure the two required angles (reclination and declination) of the surface on which the dial is to be mounted. After a short tutorial on astronomy and the movement of the sun, he covers three different methods of designing dials: geometrically by ruler and compass, arithmetically by means of a table of sines and tangents, and finally by the use of a very simple instrument of his own design. The work concludes with a table of the sun's declination for each day of the year and a table of the latitudes for each of the major places in Britain. The frontispiece, a portrait of the author, is the same as that later used in his 1672 book *Panorganon*" (Tomash). Leybourn (1626-1716) enjoyed a fine reputation in his day, both as a fellow (described by John Gadbury as of 'a facetious, pleasant and cheerful disposition'), and as a mathematician, ranked by William Derham with William Oughtred and Jonas Moore, and some of his textbooks had a life of over a century. "A notable example of the mathematical career that flourished largely away from elite court circles and the universities is that of William Leybourn … Leybourn might with justification be thought one of the most significant London mathematicians of his day. His long life and career paralleled [Christopher] Wren's closely, though at a distinct social remove" (Edwards, p. 100). No other copies of this first issue located in auction records.

"Born in 1626, Leybourn worked originally with his brother Robert as a printer, based at Monkswell Street, Cripplegate. Whilst the Leybourn brothers produced many books for writers involved in technical experimentation and reform, William Leybourn increasingly gave up his printing work to write his own books on mathematics; to teach private pupils whom he boarded at his home in Southall; and to work as a mathematical practitioner, taking part in private and public projects such as the great fire survey, and the surveying of estates forfeited in the Civil War.

"Leybourn's practical work and his writing were clearly highly symbiotic in the maintenance of his career. Leybourn's books made evident his knowledge and competence in mathematics. They also directly advertised his services. The Line of Proportion, for instance, first published in 1667, offered a straightforward, accessible guide to the use of 'Gunter's line', a logarithmic series designed to help artisans with little mathematics compute areas and volumes mechanically. An unfussy duodecimo volume, it was first published in the immediate aftermath of the great fire and dedicated to the City grandees who oversaw the surveying of the ruins. As well as advertising Leybourn's employment in this survey, later editions of this simple, practical book were used to tout for further business with a wider clientele. One edition (1673) carries the following notice:

'If any Gentleman, or other Person, desire to be instructed in any of the Sciences Mathematical, as Arithmetick, Geometry, Astronomy, the Use of the Globes, Trigonometry, Navigation, Surveying of Land, Dialling, or the like; the Author will be ready to attend them at times appointed.

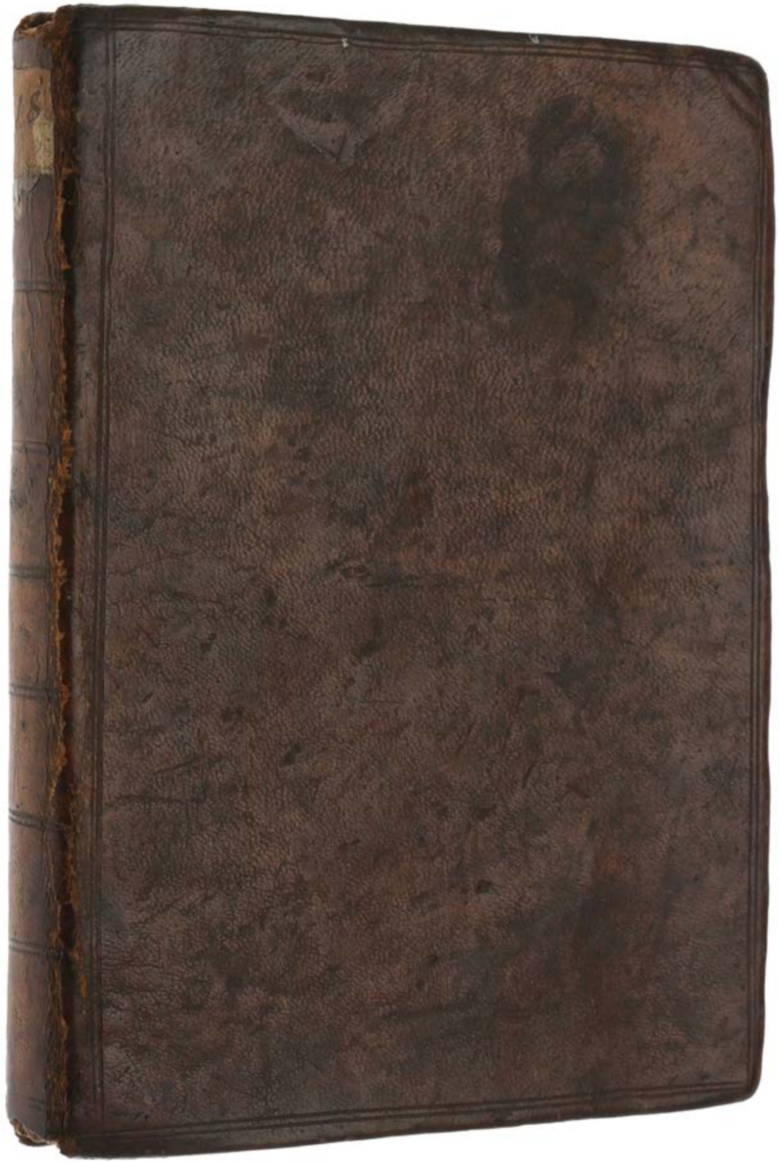
Also, If any Person would have his Land, or any Ground for Building Surveyed, or any Edifice of Building Measured, either for the Carpenters, Bricklayers, Plaisterers, Glaziers, Joyners or Masons work, he is ready to perform the same either for Master Builder or workman: …

You may hear of him where these Books are to be sold.'

"Self-marketing such as this is utterly characteristic of Leybourn's long and fertile publishing career. Leybourn published his first mathematical work, a treatise on surveying, in 1650. By 1682 he was able to fund production of a folio edition of his book on *Dialling* by subscription, expanding a text first published in quarto form in 1669 [offered here], and furnishing it with his portrait. An insider to the seventeenth-century print trade, Leybourn joined other early modern writers beginning to capitalize on the social currency of their knowledge; owning and selling shares in it. Having made a name for himself, Leybourn effectively franchised his name. He contributed prefaces to other writers' works, and published in many instances to promote instruments made by his associates: an early example of tie-in marketing. He was also cannily sensitive to the different markets for his books. Some, such as *The Line of Proportion*, are simply and cheaply produced, with the accent on practical use. *Cursus Mathematicus*, on the other hand, [a] relatively lavish folio production published in 1690 by subscription and costing twenty shillings, addresses itself to the leisured gentry … Leybourn was a pragmatic man, who promoted mathematical science in order to build his own successful career. But what he was selling wasn't simply practical utility. No less than [John] Dee, Leybourn recognized that the most valuable commodity in a trade in intellectual knowledge was one which somehow blended practical use and profit with the truth and virtue traditionally associated with liberal disinterest" (*ibid*., pp. 100-102).

Subsequent editions appeared in 1681, 1690, and 1700.

ESTC R17714 (BL only). Tomash L90 (second issue). Edwards, *Writing, Geometry and Space in Seventeenth-Century England and America*, 2006.



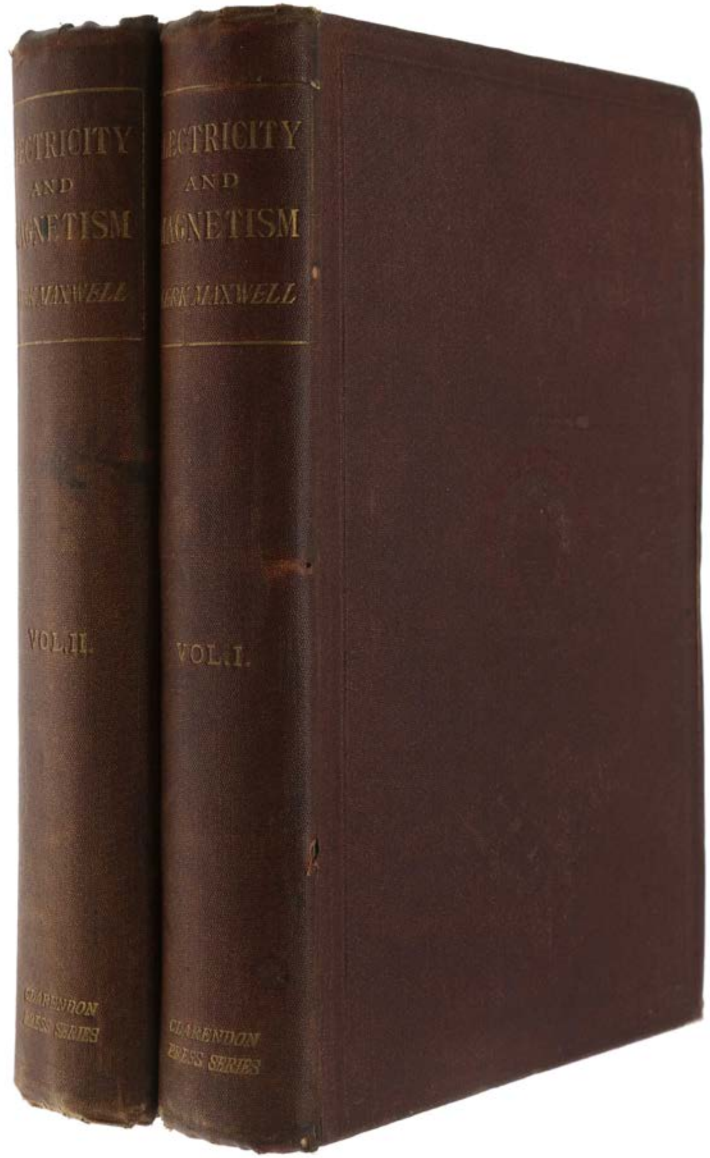
PMM 355 - LIGHT AS A FORM OF ELECTRICITY

MAXWELL, James Clerk. *A Treatise on Electricity and Magnetism.* Oxford: Clarendon Press, 1873.

$25,000

Two volumes, 8vo (223 x 140 mm), pp. xxix, [3], 425, [5] and 13 plates (errata slip before leaf B1); xxiii [1], 444, [2] and 7 plates, 15 [1] (advertisements). Original publisher's blind-stamped plum cloth, some moderate wear to coners and hinges, exlibris and stamp of the Royal Society of Glasgow, a very good copy without any repairs. This first issue can be distinguished from the second from the eight leaves of publisher's advertisements in the rear of volume 2; the entry for Maxwell's Treatise itself states "just published."

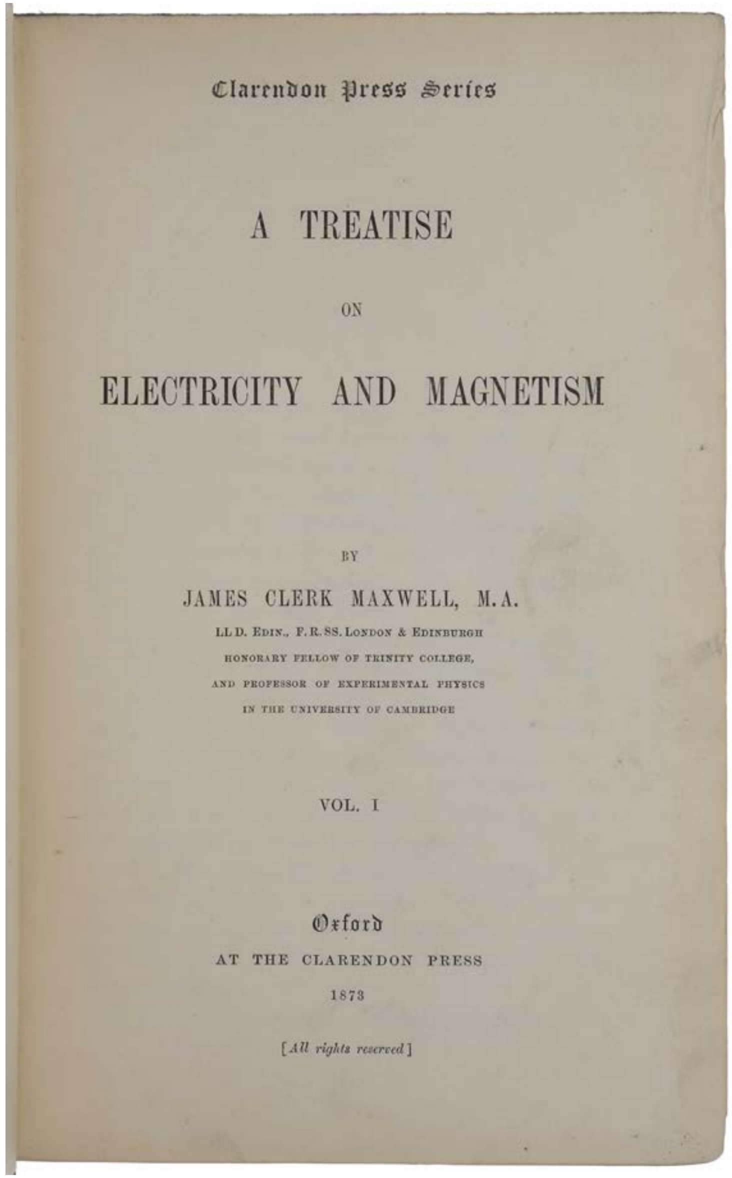
First edition, first issue, of Maxwell's presentation of his theory of electromagnetism, advancing ideas that would become essential for modern physics, including the landmark "hypothesis that light and electricity are the same in their ultimate nature" (Grolier/Horblit). "This treatise did for electromagnetism what Newton's *Principia* had done from classical mechanics. It not only provided the mathematical tools for the investigation and representation of the whole electromagnetic theory, but it altered the very framework of both theoretical and experimental physics. It was this work that finally displaced action-at-a-distance physics and substituted the physics of the field" (*Historical Encyclopedia of Natural and Mathematical Sciences*, p. 2539). "From a long view of the history of mankind — seen from, say, ten thousand years from now — there can be little doubt that the most significant event of the 19th century will be judged as Maxwell's discovery of the laws of



electrodynamics" (R. P. Feynman, in *The Feynman Lectures on Physics* II (1964), p. 1-6). "[Maxwell] may well be judged the greatest theoretical physicist of the 19th century ... Einstein's work on relativity was founded directly upon Maxwell's electromagnetic theory; it was this that led him to equate Faraday with Galileo and Maxwell with Newton" (PMM). "Einstein summed up Maxwell's achievement in 1931 on the occasion of the centenary of Maxwell's birth: 'We may say that, before Maxwell, Physical Reality, in so far as it was to represent the process of nature, was thought of as consisting in material particles, whose variations consist only in movements governed by [ordinary] differential equations. Since Maxwell's time, Physical Reality has been thought of as represented by continuous fields, governed by partial differential equations, and not capable of any mechanical interpretation. This change in the conception of Reality is the most profound and the most fruitful that physics has experienced since the time of Newton'" (Longair).

Provenance: Royal Philosophical Society of Glasgow (bookplate on front paste-down and stamp on half-title). "On 9th Nov. 1802, in response to an invitation, twenty-two citizens met in the Prince of Wales Tavern, Glasgow where they set up a committee to outline the principles for a Society 'for the improvement of the Arts and Sciences' in Glasgow. An important consideration was the establishment of a select library of scientific books … After the 1939/45 war many other professional societies were formed and there were other libraries for the 'arts and sciences' … In 1961 the building was sold, and the library of over 5000 volumes was dispersed" (https://royalphil.org/history/).

"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



Clarendon Press Series

A TREATISE

ON

ELECTRICITY AND MAGNETISM

BY

JAMES CLERK MAXWELL, M.A.

LL.D. EDIN., F.R.SS. LONDON & EDINBURGH

HONORARY FELLOW OF TRINITY COLLEGE,

AND PROFESSOR OF EXPERIMENTAL PHYSICS

IN THE UNIVERSITY OF CAMBRIDGE

VOL. I

Oxford

AT THE CLARENDON PRESS

1873

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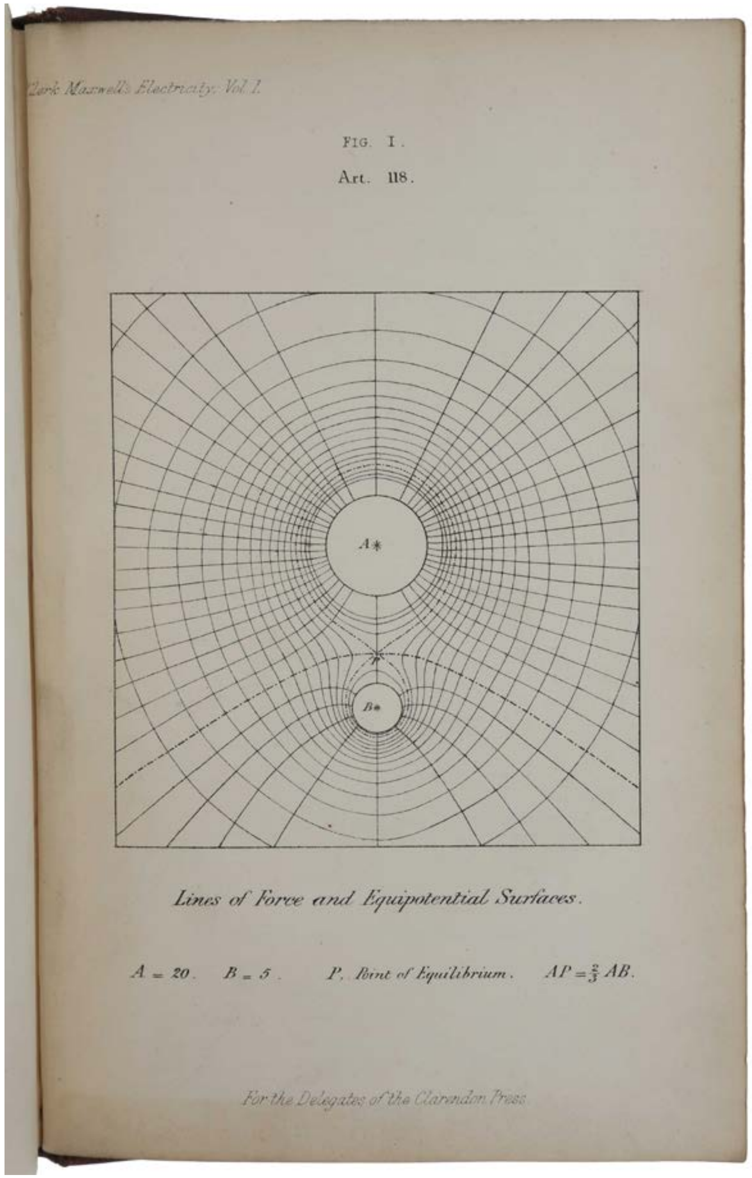
introduced. These earlier works were based upon mechanical analogies. In the paper of 1865, the focus shifts to the role of the fields themselves as a description of electromagnetic phenomena. The somewhat artificial mechanical models by which he had arrived at his field equations a few years earlier were stripped away. Maxwell's introduction of the concept of fields to explain physical phenomena provided the essential link between the mechanical world of Newtonian physics and the theory of fields, as elaborated by Einstein and others, which lies at the heart of twentieth and twenty-first century physics" (Longair).

The 1865 paper "provided a new theoretical framework for the subject, based on experiment and a few general dynamical principles, from which the propagation of electromagnetic waves through space followed without any special assumptions … In the *Treatise* Maxwell extended the dynamical formalism by a more thoroughgoing application of Lagrange's equations than he had attempted in 1865. His doing so coincided with a general movement among British and European mathematicians about then toward wider use of the methods of analytical dynamics in physical problems … Using arguments extraordinarily modern in flavor about the symmetry and vector structure of the terms, he expressed the Lagrangian for an electromagnetic system in its most general form. [George] Green and others had developed similar arguments in studying the dynamics of the luminiferous ether, but the use Maxwell made of Lagrangian techniques was new to the point of being almost a new approach to physical theory—though many years were to pass before other physicists fully exploited the ground he had broken …

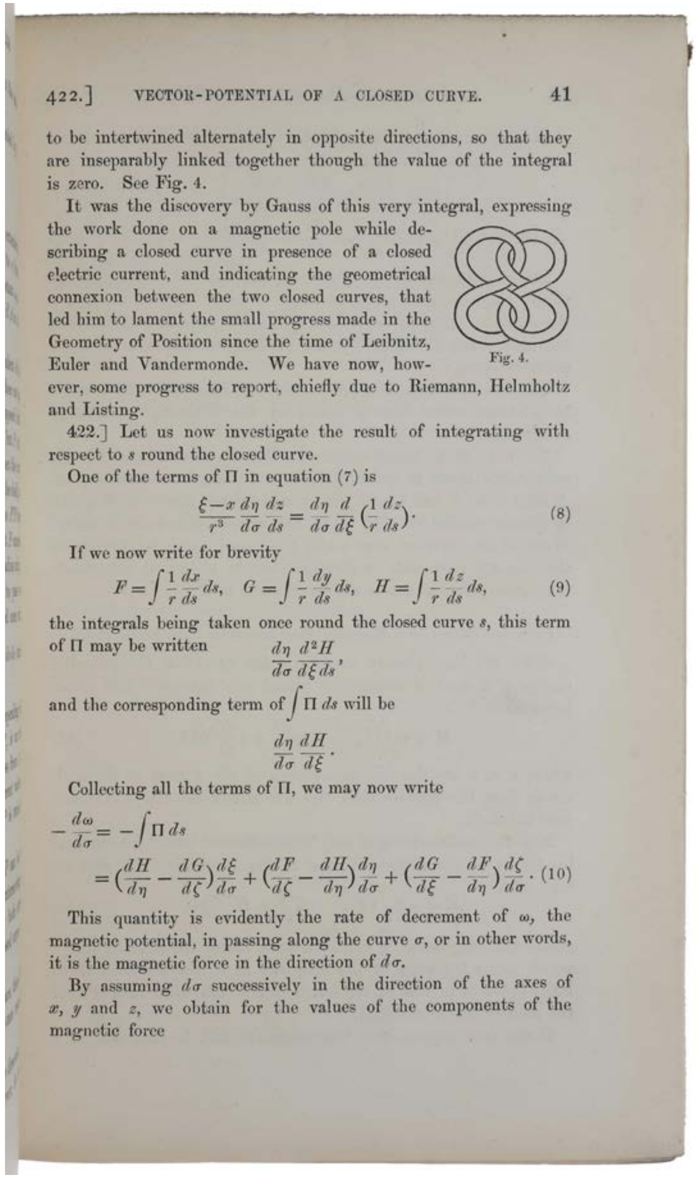
"In 1865, and again in the *Treatise*, Maxwell's next step after completing the dynamical analogy was to develop a group of eight equations describing the electromagnetic field … The principle they embody is that electromagnetic processes are transmitted by the separate and independent action of each charge



(or magnetized body) on the surrounding space rather than by direct action at a distance. Formulas for the forces between moving charged bodies may indeed be derived from Maxwell's equations, but the action is not along the line joining them and can be reconciled with dynamical principles only by taking into account the exchange of momentum with the field" (DSB).

"Maxwell once remarked that the aim of his *Treatise* was not to expound the final view of his electromagnetic theory, which he had developed in a series of five major papers between 1855 and 1868; rather it was to educate himself by presenting a view of the stage he had reached in his thinking. Accordingly, the work is loosely organized on historical and experimental, rather than systematically deductive, lines. It extended Maxwell's ideas beyond the scope of his earlier work in many directions, producing a highly fecund (if somewhat confusing) demonstration of the special importance of electricity to physics as a whole. He began the investigation of moving frames of reference, which in Einstein's hands were to revolutionize physics; gave proofs of the existence of electromagnetic waves that paved the way for Hertz's discovery of radio waves; worked out connections between electrical and optical qualities of bodies that would lead to modern solid-state physics; and applied Tait's quaternion formulae to the field equations, out of which Heaviside and Gibbs would develop vector analysis" (Norman).

DSB IX, p.198; Grolier/Horblit 72; Norman 1666 (second issue); PMM 355; Poggendorff III, p. 889; Wheeler Gift Catalogue 1872. Achard, 'James Clerk Maxwell, *A Treatise on Electricity and Magnetism*, First Edition (1873),' Chapter 44 in *Landmark Writings in Western Mathematics 1640-1940*, Grattan-Guinness (ed.), 2005. Malcolm Longair, '. . . a paper . . . I hold to be great guns': a commentary on Maxwell (1865) 'A dynamical theory of the electromagnetic field'. *Philosophical Transactions* A 373, No. 2039, 13 April 2015.



422.] VECTOR-POTENTIAL OF A CLOSED CURVE. 41

to be intertwined alternately in opposite directions, so that they are inseparably linked together though the value of the integral is zero. See Fig. 4.

It was the discovery by Gauss of this very integral, expressing the work done on a magnetic pole while describing a closed curve in presence of a closed electric current, and indicating the geometrical connexion between the two closed curves, that led him to lament the small progress made in the Geometry of Position since the time of Leibnitz, Euler and Vandermonde. We have now, however, some progress to report, chiefly due to Riemann, Helmholtz and Listing.

Fig. 4.

422.] Let us now investigate the result of integrating with respect to s round the closed curve.

One of the terms of Π in equation (7) is

$$\frac{\xi - x}{r^3}\frac{d\eta}{d\sigma}\frac{dz}{ds} = \frac{d\eta}{d\sigma}\frac{d}{d\xi}\left(\frac{1}{r}\frac{dz}{ds}\right). \qquad (8)$$

If we now write for brevity

$$F = \int \frac{1}{r}\frac{dx}{ds}\,ds, \quad G = \int \frac{1}{r}\frac{dy}{ds}\,ds, \quad H = \int \frac{1}{r}\frac{dz}{ds}\,ds, \qquad (9)$$

the integrals being taken once round the closed curve s, this term of Π may be written

$$\frac{d\eta}{d\sigma}\frac{d^2 H}{d\xi\, ds},$$

and the corresponding term of $\int \Pi\, ds$ will be

$$\frac{d\eta}{d\sigma}\frac{dH}{d\xi}.$$

Collecting all the terms of Π, we may now write

$$-\frac{d\omega}{d\sigma} = -\int \Pi\, ds$$

$$= \left(\frac{dH}{d\eta} - \frac{dG}{d\zeta}\right)\frac{d\xi}{d\sigma} + \left(\frac{dF}{d\zeta} - \frac{dH}{d\eta}\right)\frac{d\eta}{d\sigma} + \left(\frac{dG}{d\xi} - \frac{dF}{d\eta}\right)\frac{d\zeta}{d\sigma}. \qquad (10)$$

This quantity is evidently the rate of decrement of ω, the magnetic potential, in passing along the curve σ, or in other words, it is the magnetic force in the direction of $d\sigma$.

By assuming $d\sigma$ successively in the direction of the axes of x, y and z, we obtain for the values of the components of the magnetic force

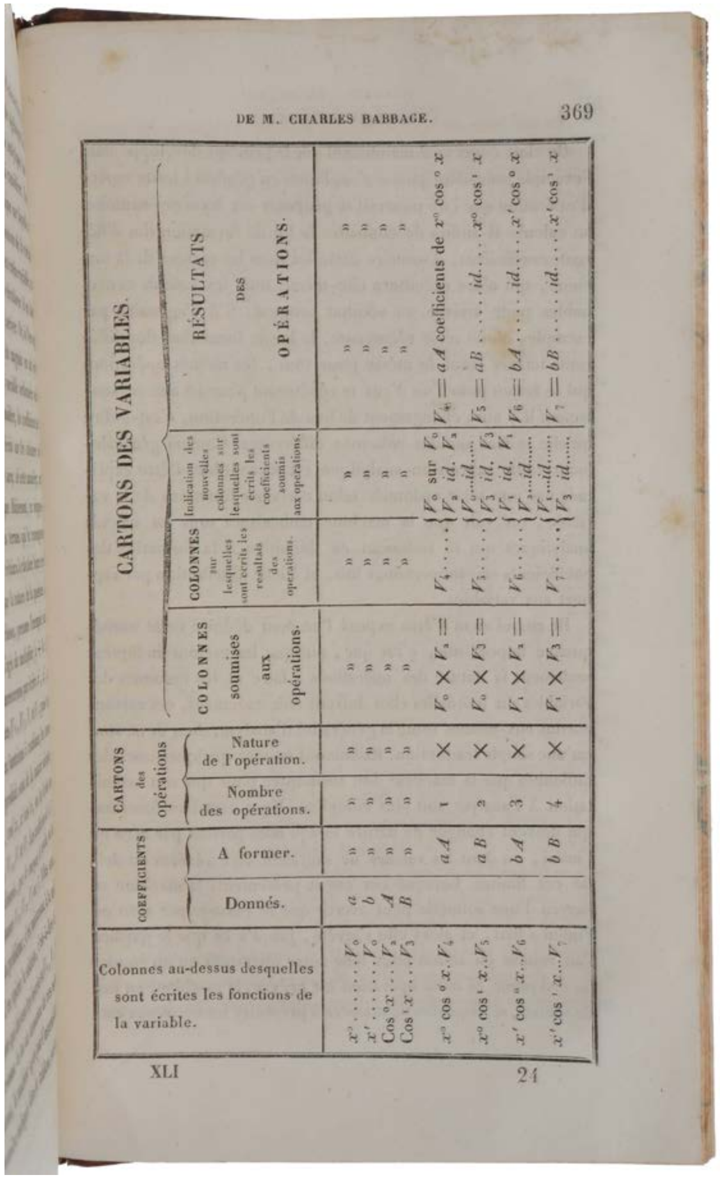
THE FIRST EXAMPLES OF COMPUTER PROGRAMS EVER PUBLISHED

MENABREA, Luigi Federico. *Notions sur la machine analytique de M. Charles Babbage.* Paris: Anselin, 1842.

$25,000

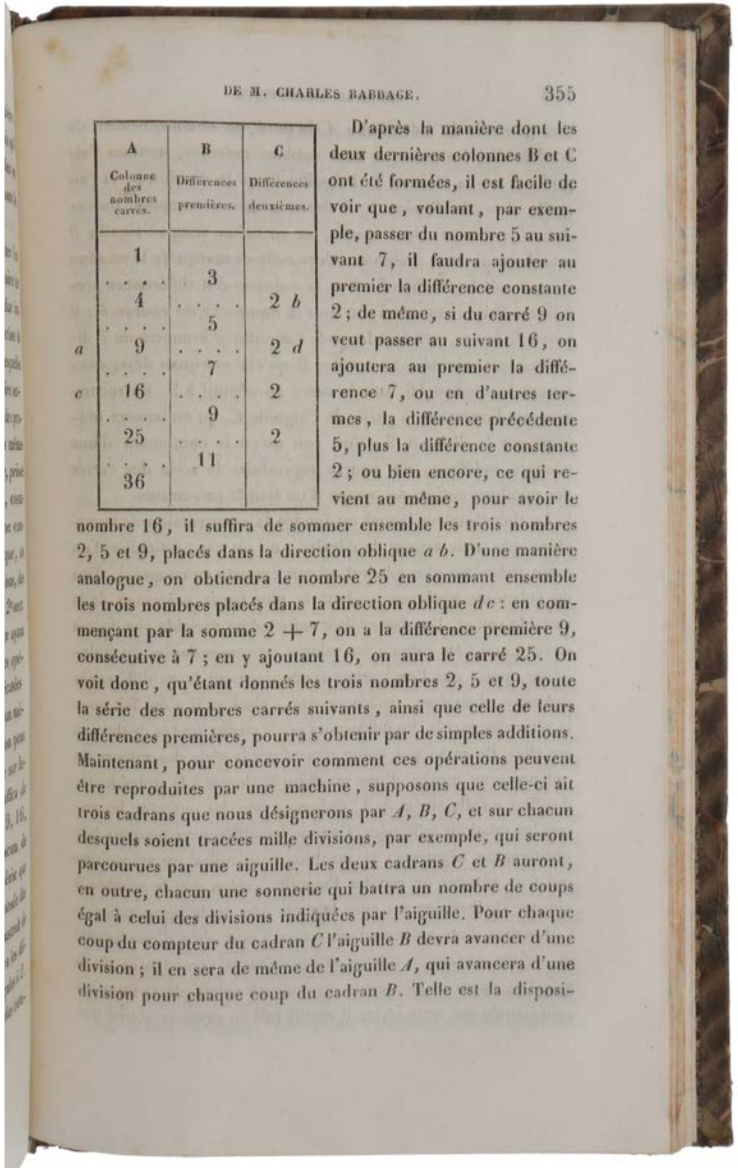
Pp. 352-376 in: Bibliothèque Universelle de Genève. Nouvelle série, Tome 41. 8vo (213 x 128 mm), pp. [5], 6-420, with folding table and plate, several tables printed within text. Contemporary quarter-calf and marbled boards, spine richly gilt with two lettering-pieces (binding a little rubbed, two old 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 receiving 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



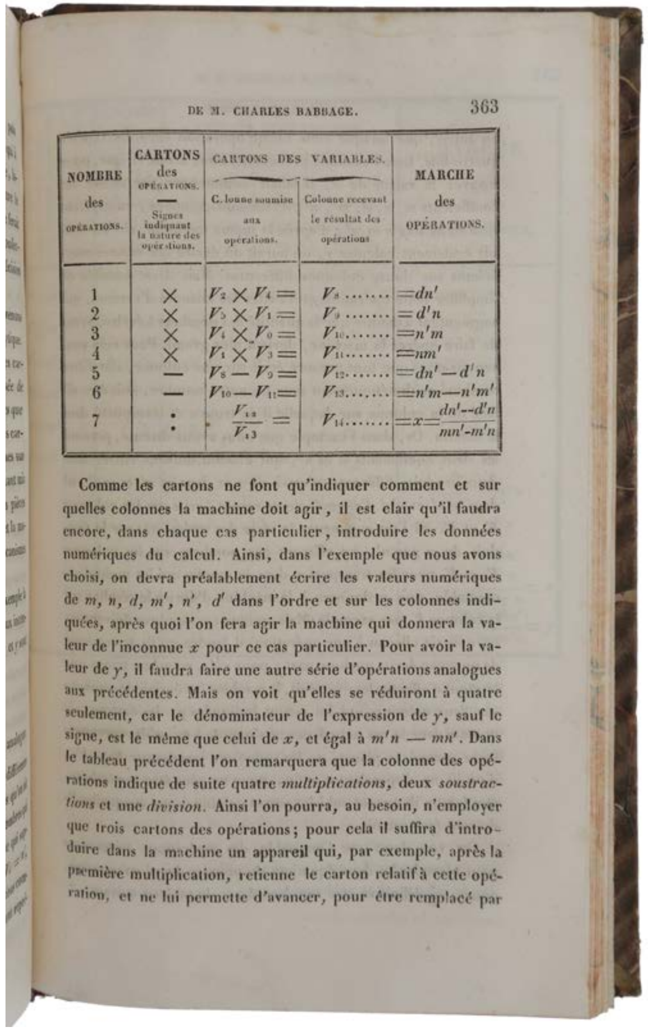
DE M. CHARLES BABBAGE. 355

	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 disposiwas 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 previous calculations. By a series of particular examples, Babbage gradually led his audience to understand and accept the general principles of his engine. In



DE M. CHARLES BABBAGE. 363

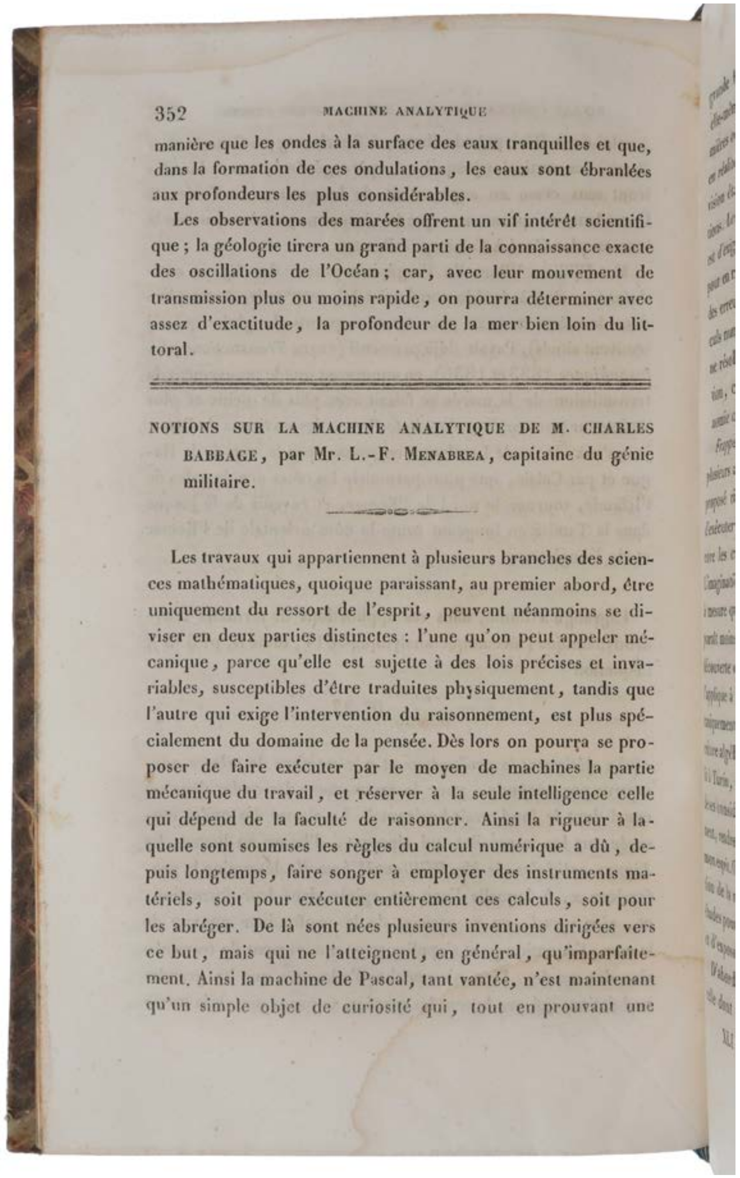
NOMBRE des OPÉRATIONS.	CARTONS des OPÉRATIONS. — Signes indiquant la nature des opérations.	CARTONS DES VARIABLES.		MARCHE des OPÉRATIONS.
		Colonne soumise aux opérations.	Colonne recevant le résultat des opérations	
1	$\times$	$V_2 \times V_4 =$	V_8	$= dn'$
2	$\times$	$V_5 \times V_1 =$	V_9	$= d'n$
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 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 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



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.

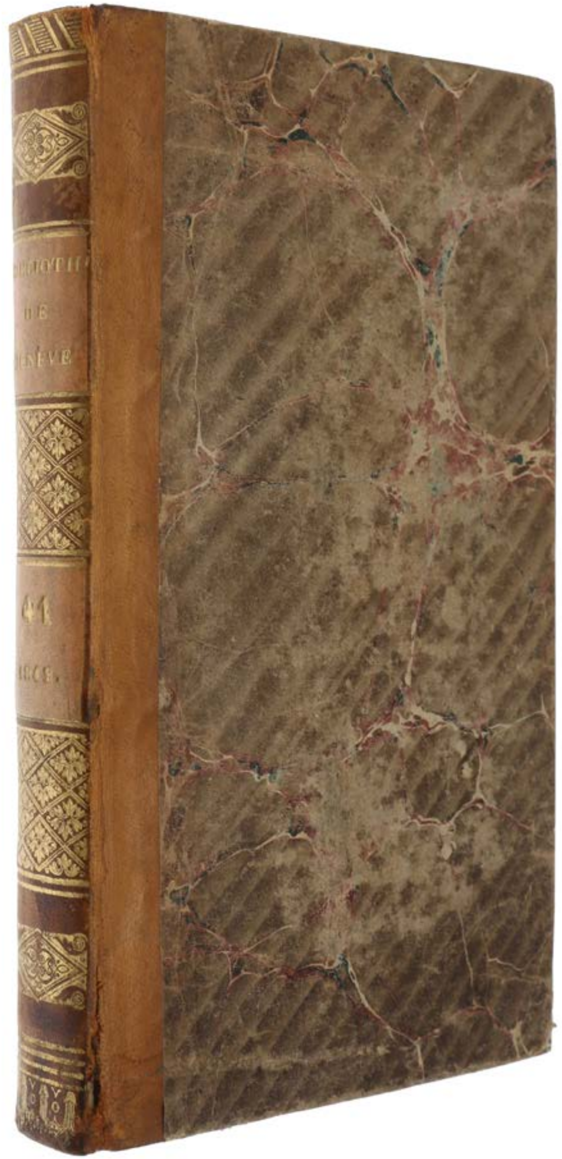
NOTIONS SUR LA MACHINE ANALYTIQUE DE M. CHARLES BABBAGE, par Mr. L.-F. MENABREA, capitaine du génie militaire.

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 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.



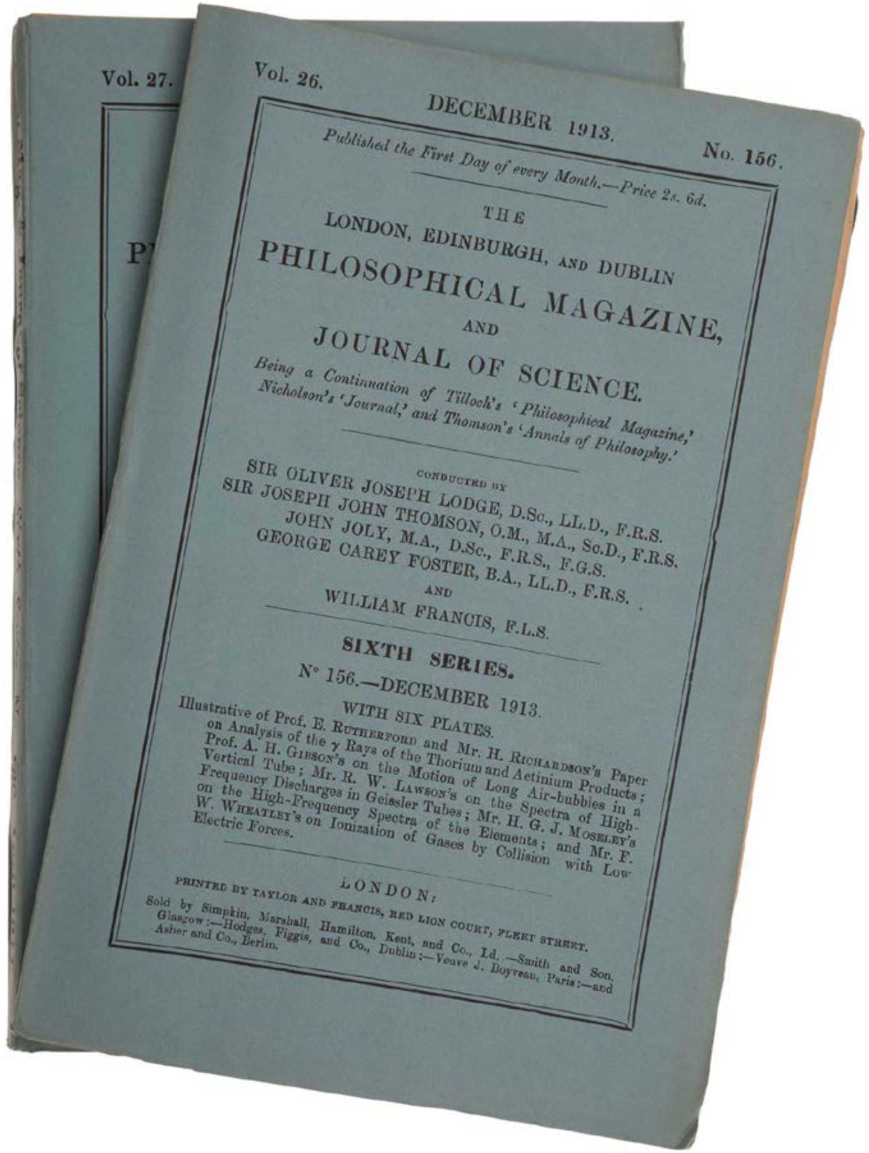
PMM 407 - THE ATOMIC TABLE

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.



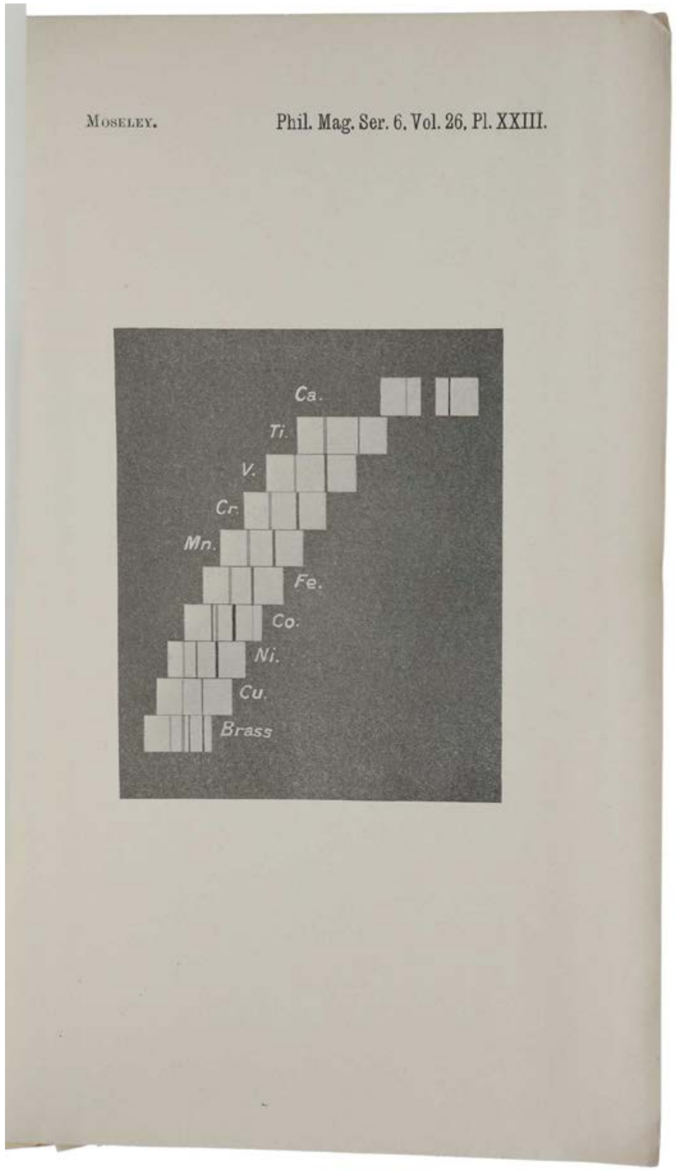
"Before 1913 the order of the elements in the periodic system was universally taken to be given by the atomic weight. Although this caused some anomalies, such as that related to the 'reversed' atomic weights of tellurium (Te = 127.6) and iodine (I = 126.9), the convention or dogma of atomic weight being 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.

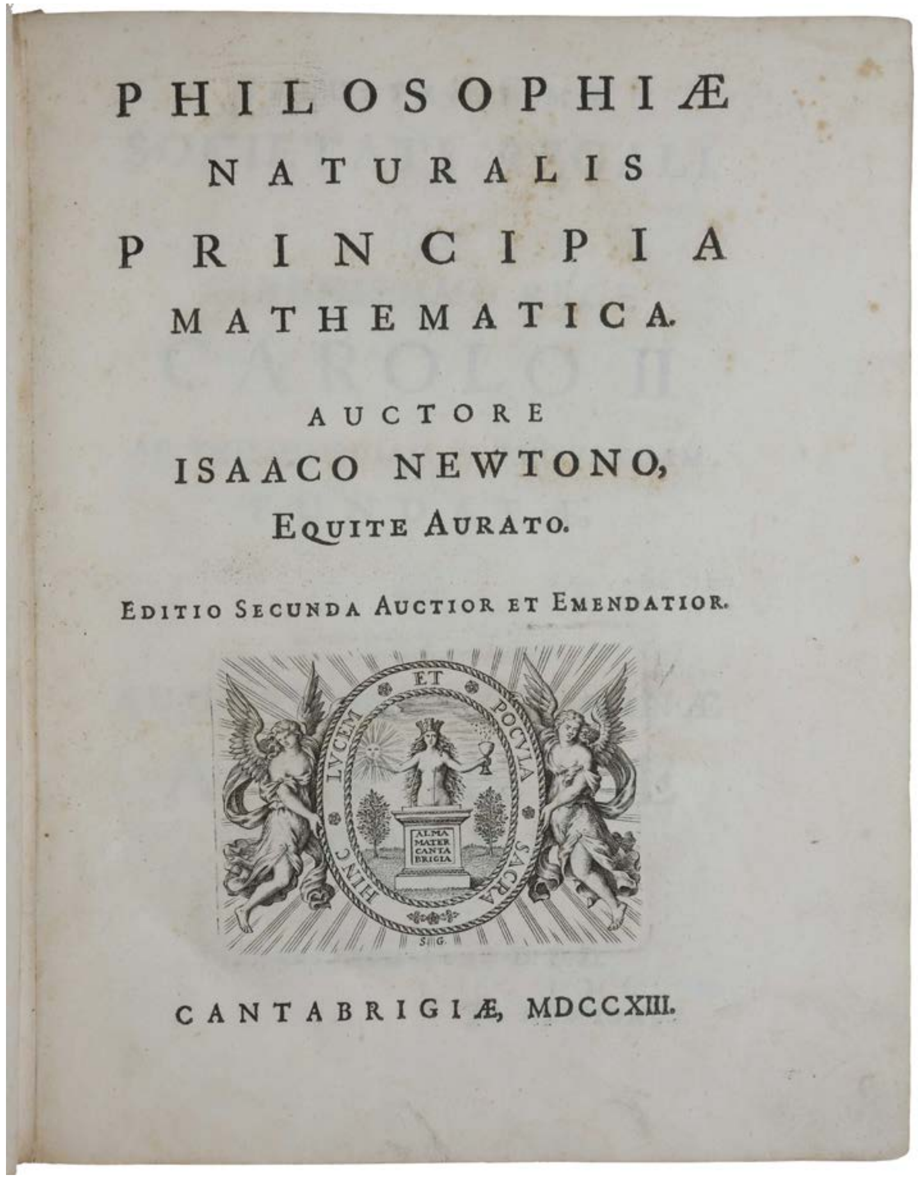
THE GREATEST WORK IN THE HISTORY OF SCIENCE

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).

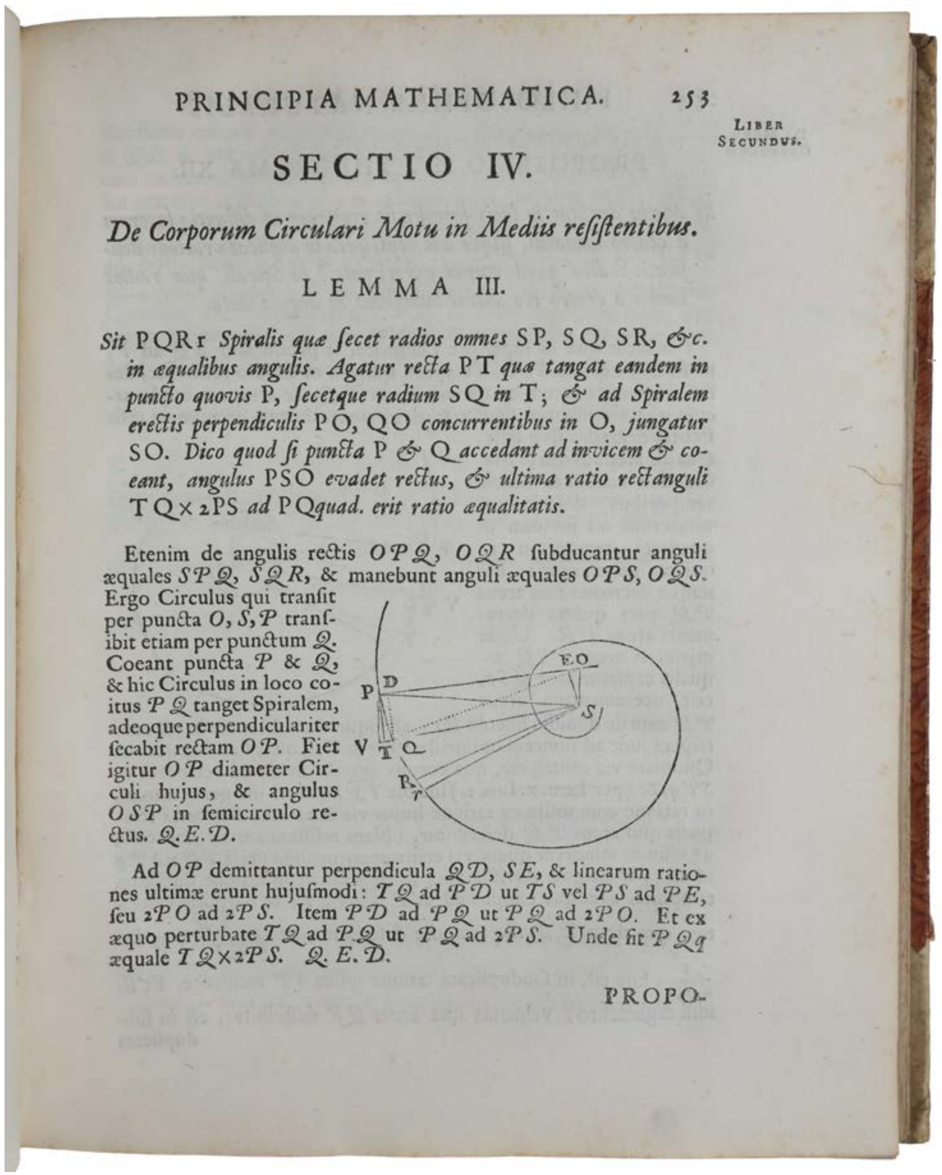
Earlier, to Boyle, he had spoken of an 'etherial substance' diffused through all space. Oddly enough Newton accepted the misrepresentation of his views without complaint, public or private.

"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



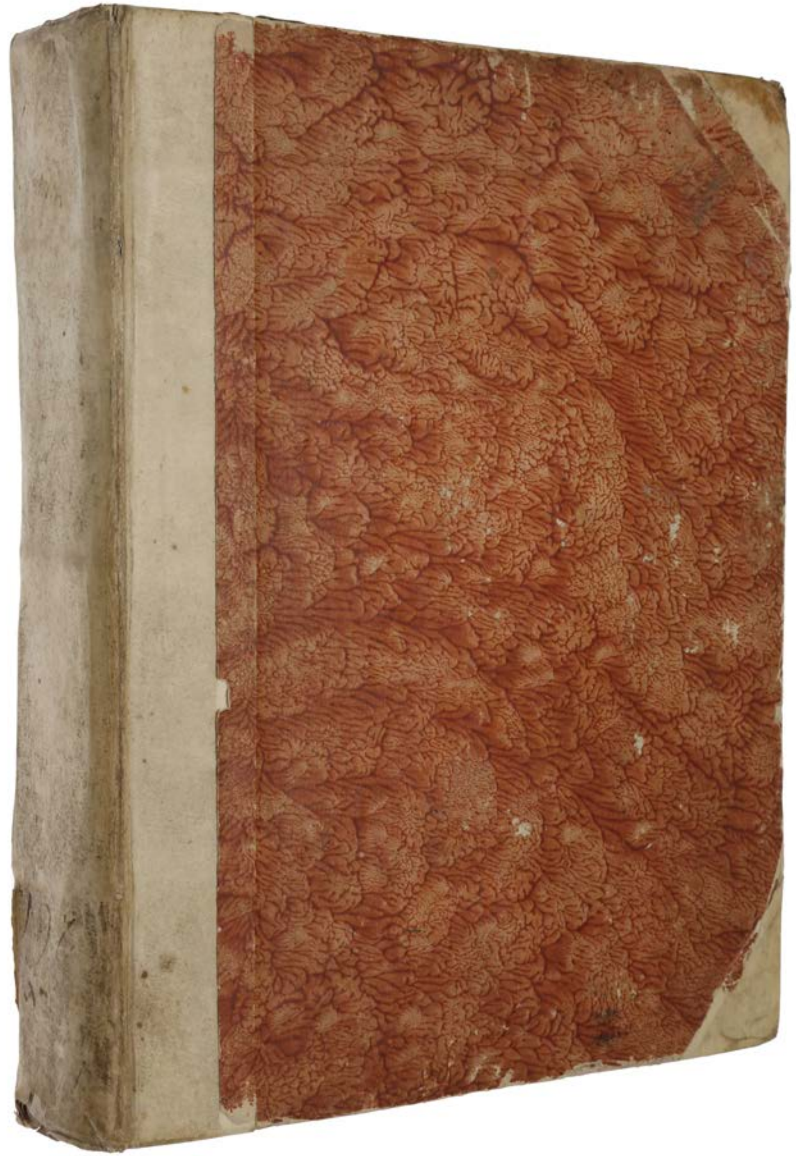
manner not at all human … in a manner utterly unknown to us'. We know God only through his works, 'by his most wise and excellent contrivances of things'. There seems little room in Newton's austere theology for anything like a personal God. Indeed, he went out of his way to dismiss such an option. God, 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.



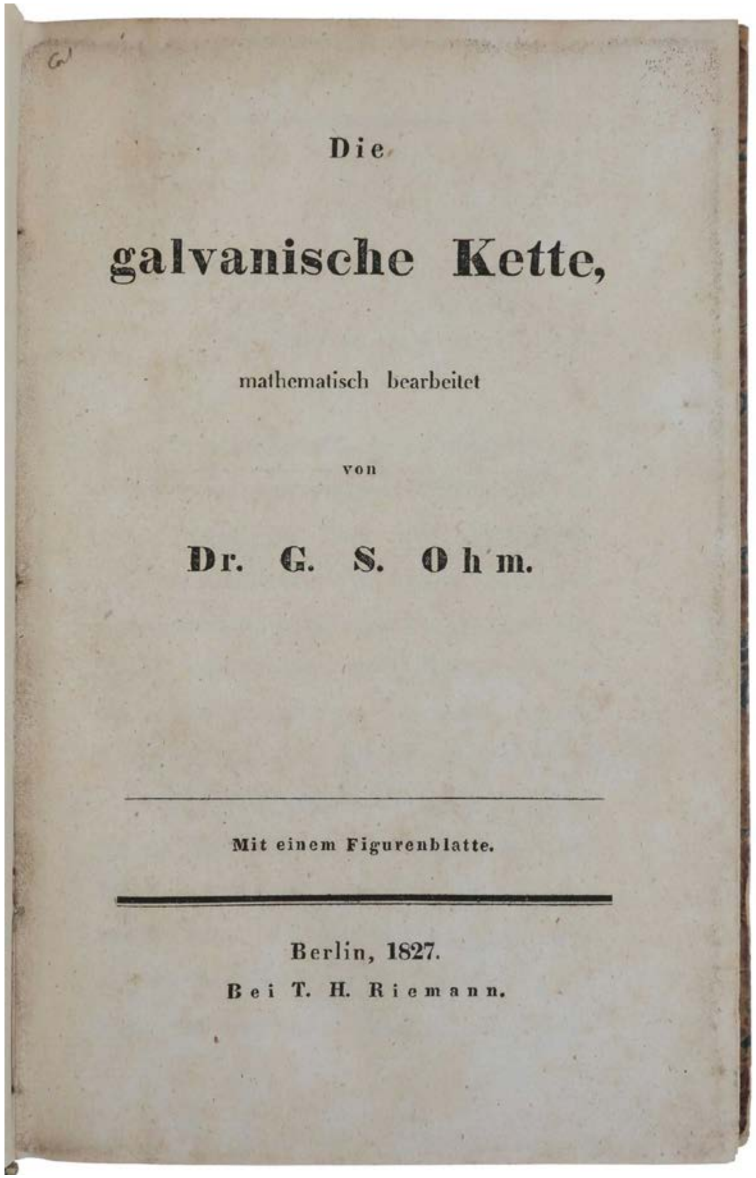
PMM 289 - MEASURING ELECTRICITY

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).

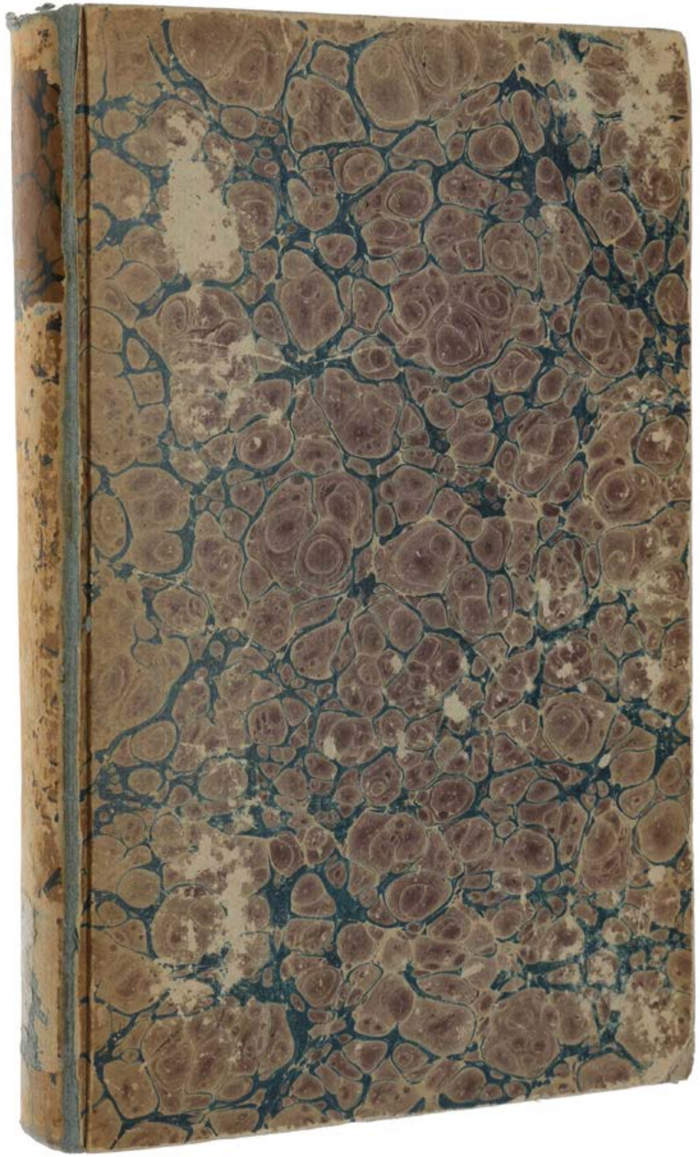
"The expression "investigated mathematically" in the title of Ohm's book described his objective: to deduce the properties of the galvanic circuit from a set of "fundamental laws." The first of these laws states that electricity passes only between adjacent particles of the conductor and that the quantity 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 electro-magnetism 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.

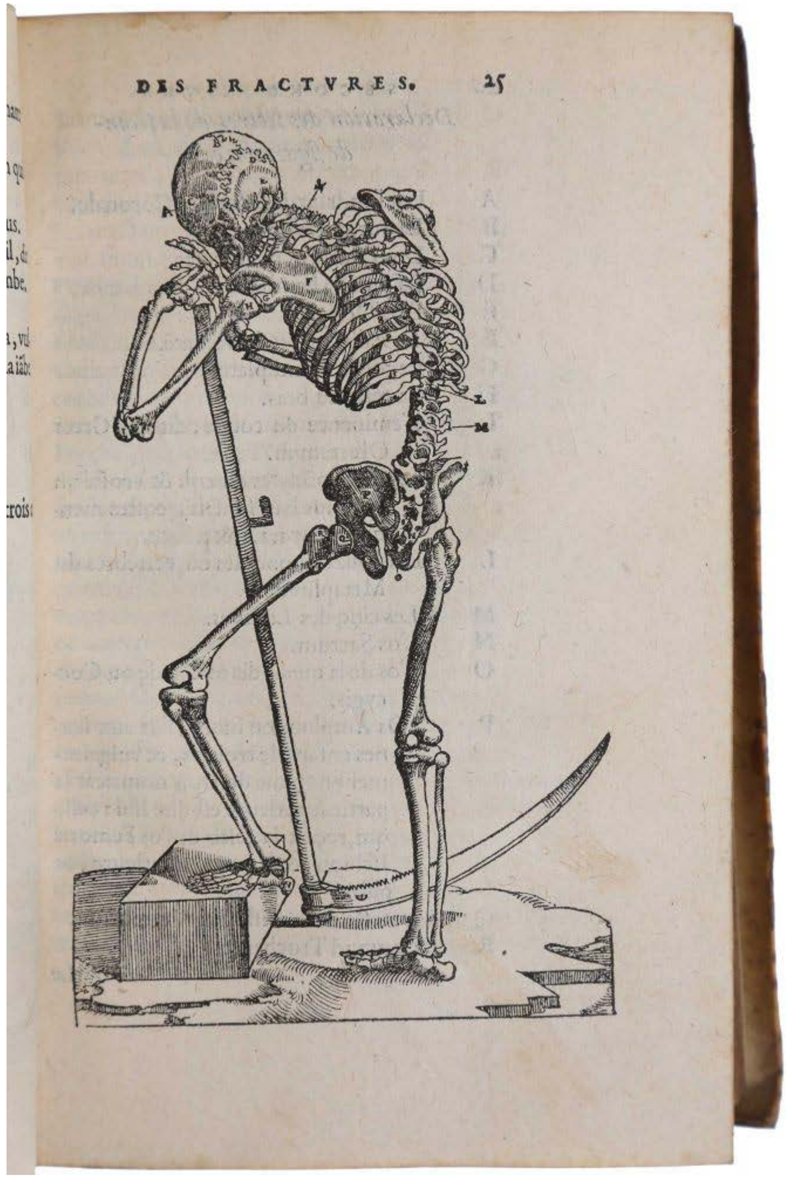
A FINE COPY OF PARÉ'S CHEF D'OEUVRE

PARÉ, Ambroise. *Cinq livres de chirurgie. 1. Des bandages. 2. Des fractures. 3. Des luxations, avec une Apologie touchant les harquebousades. 4. Des morsures & piqueures venimeuses. 5. Des gouttes. [Bound with:] Traicté de la peste, de la petite Verolle & Rougeolle: avec une brefue description de la Lepre.* Paris: André Wechel, 1572; 1568.

$185,000

Two works in one volume, 8vo (170 x 109 mm). I. Pp. [xxiv], 470, [2]. Title within woodcut allegorical border, woodcut portrait of Paré on verso, 41 woodcuts in text, most full-page, complete with colophon leaf with woodcut printer's device on verso. (Title with tiny marginal chip at foot, extremely minor marginal worming affecting gatherings n-q and v, occasional pale soiling or spotting.). II. Pp. [xvi], 235 (recte 275), [4] (last leaf blank). Title within woodcut allegorical border, complete with 'Au Lecteur' leaf with very large woodcut printer's device on verso. (Some dampstaining from gathering O to end, minor marginal worming affecting last 5 leaves and endpapers.) Contemporary vellum, yapp edges (some soiling, remains of ties).

First editions of two of Paré's most important works, both very rare, and in fine condition. "The *Cinq livres* contains all new material. It had been called by several serious writers Paré's chef d'oeuvre ... in it appears the first description of the fracture of the head and of the femur. Secondly, it is the first appearance of the whole teaching of bandages, fractures, and dislocations which has come down to us from the ancients, broadened by Paré's own experience ... It is undoubtedly one of his most important works" (Doe 19). The *Traicté de la peste* was written from direct experience of the plague: "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 14). Both works are very rare. "Paré's original books, all very rare today, were handy volumes, small enough for the field surgeon's knapsack" (Hagelin, p. 35). ABPC/RBH list only two other apparently complete copies of the *Cinq livres* (Parke Bernet, 1963 & Sotheby's, 2016), and one of the *Traicté de la peste* (Sotheby's, 2005).

Provenance: 'C.P.L.C. du bon desser' (contemporary ink inscription in lower margin of a2 recto); François Moutier (20th-century bookplate).

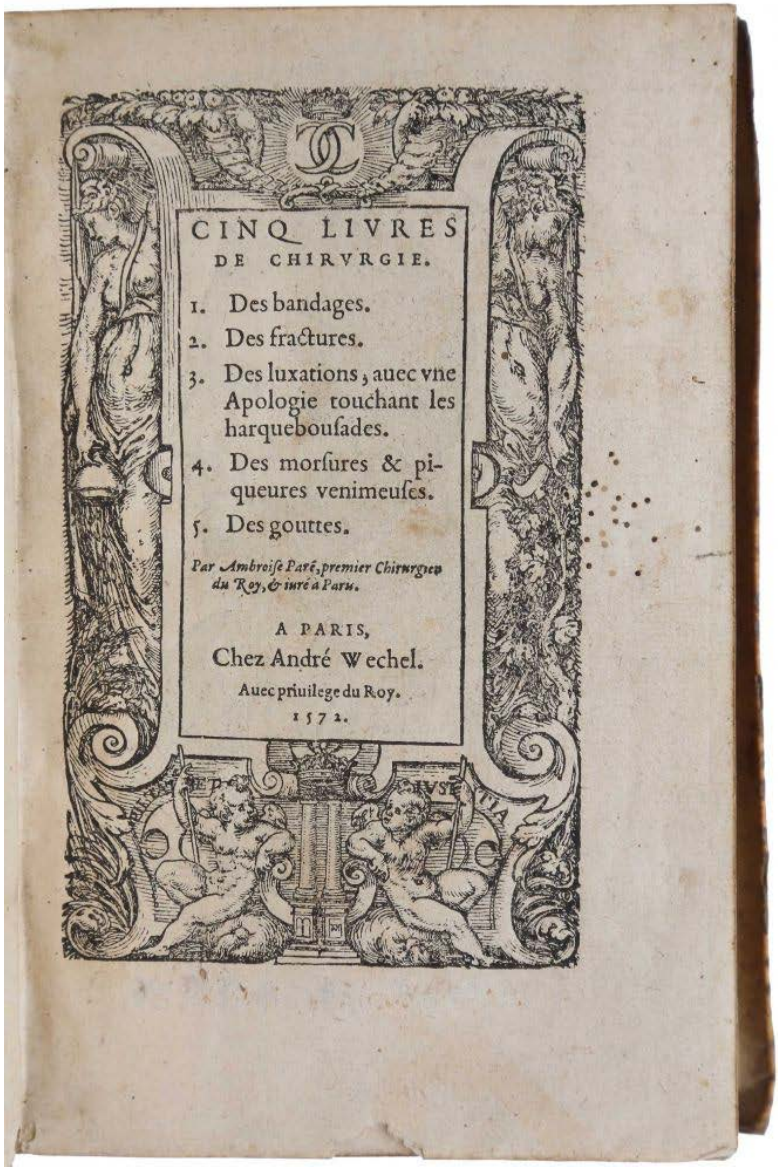
"The barber-surgeons before Paré expected that any sort of surgical technique would require that the patient experience pain, sometimes pain so extreme that the subject would lose consciousness during the procedure. His realization that one might act gently in the capacity of a surgeon and that such gentleness actually might improve the lot of his patients was transformative. Pain relief was extremely limited in the 16th century — opium, henbane, mandrake, and strong spirits being the only offerings — and a quick, painful procedure often meant survival in a pre-antibiotic era. Tremendous pain was an accepted part of surgery. For Paré, the benefits of a gentle hand during surgery would soon become a clear means of reducing the suffering of his patients.

"Paré made his break from the traditional practices in 1537 when he ran out of the boiling oil solution conventionally used to 'detoxify' and cauterize wounds caused by gunpowder-driven projectiles. He replaced this harsh treatment with a soothing balm made from egg yolks, rose oil, and turpentine. The next morning, he was astonished to find the recipients of his new treatment were resting easily while those who suffered the cauterizing oil were 'feverish' and afflicted with 'great pain and swelling about the edges of their wounds'.

"Seeing the dramatic difference between the 'proper' and improvised treatments, Paré resolved to only treat cases with procedures he had personally observed to be useful. This resulted in such innovations as the use of ligatures in amputations, treatments for sucking chest wounds, and a cure for chronic ulcers of the skin. Although this experimentally driven medicine did not come to define the physician's practice until the rise of the Paris Clinic in the 19th century, these first writings established an important foundation of empiricism in European medicine" (Drucker).

"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).

Janet Doe's bibliography makes special note of the scarcity of the *Cinq livres*: "Malgaigne, who could locate no copy of this book, makes an erroneous guess in ascribing to it Pare's book on tumors. It is extraordinary that no copy has come to light in recent years [Doe's work was published in 1937] till Haberling's report of the Freiburg one in 1928. Haeser, in 1881, mentions its existence; but



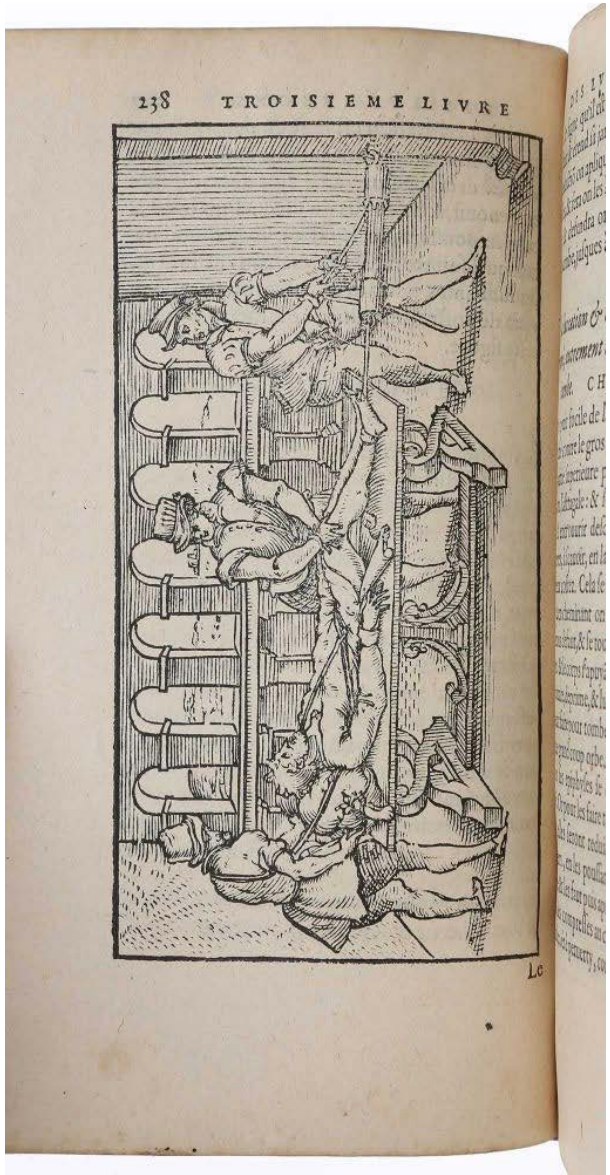
Le Paulmier in 1884, Paget in 1897, and Packard in 1921 all refer to it as probably lost. This present census reveals fourteen copies extant, all but three of which are in large libraries" (Doe, p. 70). Doe lists the following copies: 1. Boston Medical Library. 2. Bibliothèque Nationale. 3. Bibliothèque Publique, Besançon. 4. Dr. Logan Clendining, Kansas City. 5. Dr. Harvey Cushing. 6. Bibliothek der Deutschen Gessellshaft für Chirurgie, Berlin. 7. National-Bibliothek, Vienna. 8. Royal College of Surgeons, London. 9. Sächsiche Landesbibliothek, Dresden. 10. Universitäts-Bibliothek, Freiburg. 11. Bibliothèque de l'Université, Louvain. 12. Biblioteca Apostolica Vaticana. 13. Dr. Erik Waller, Sweden. 14. Zentrabibliothek, Zürich.

"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' [quotation from the English translation, *A Treatise of the Plague*, London: Thomas Johnson, 1630]" (Cunningham & Grell, pp. 280-281).

"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" (Hagelin, pp. 34-5).

I. Cushing P64; *En Francais dans le Texte* 66; Tchemerzine (1977) V, 37 ("de la plus grande rarete"); Waller 7166. Not in Adams, Eimas, Durling, Norman or Wellcome. II. Durling 3526; Waller 7162. Not in Adams, BM STC, Osler, Honeyman or Norman. 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; Drucker, 'Ambroise Paré and the Birth of the Gentle Art of Surgery,' *Yale Journal of Biology and Medicine* 81 (2008), pp. 199–202; 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; Not in Norman.



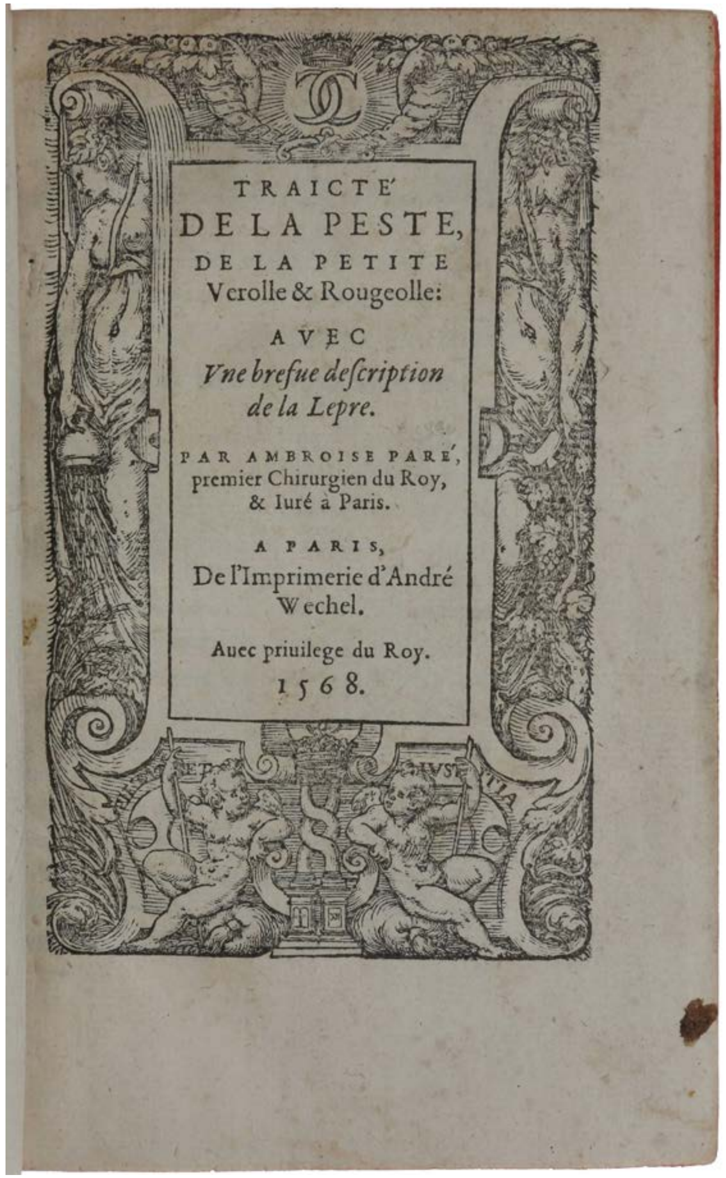
THE PLAGUE, SMALLPOX, AND MEASLES

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.'

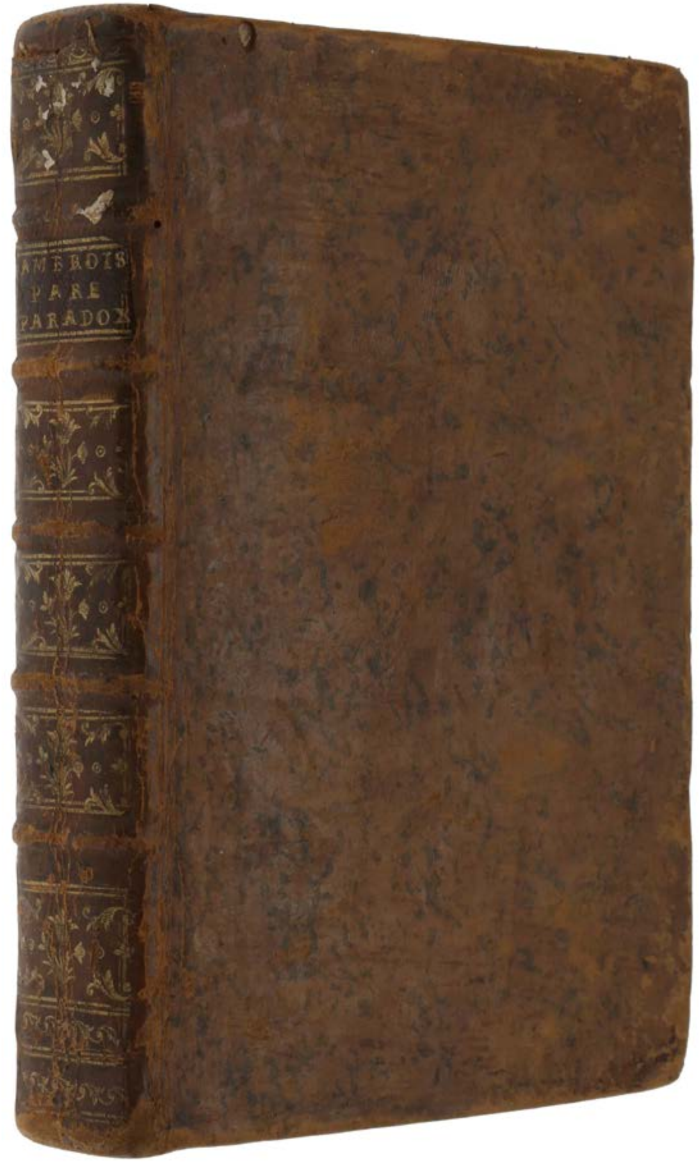
[This and subsequent quotations from Paré are from the English translation of the present work, *A Treatise of the Plague*, London: Thomas Johnson, 1630.]

"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.

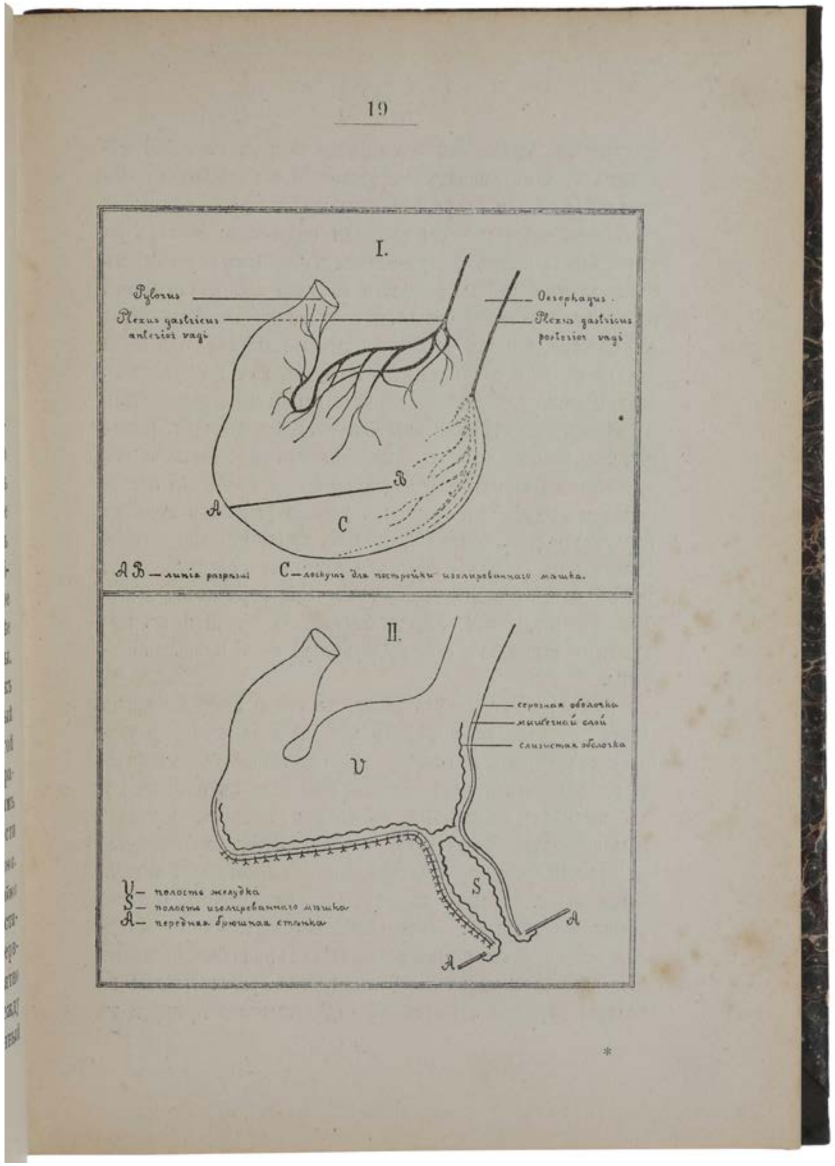
PMM 385 - CONDITIONED REFLEXES

PAVLOV, Ivan Petrovitch. *Lektsii o rabotie glavnikh pishtshevaritelnikh zhelyos.* St. Petersburg: I. N. Kushnereff & Ko., 1897.

$17,500

8vo (187 x 131 mm), pp. [vi], ii, 223, [1]. Contemporary Russian half calf over marbled boards, upper capital chipped, some rubbing to hinges, light spotting to first and final leaves. A very good and unrestored copy.

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.

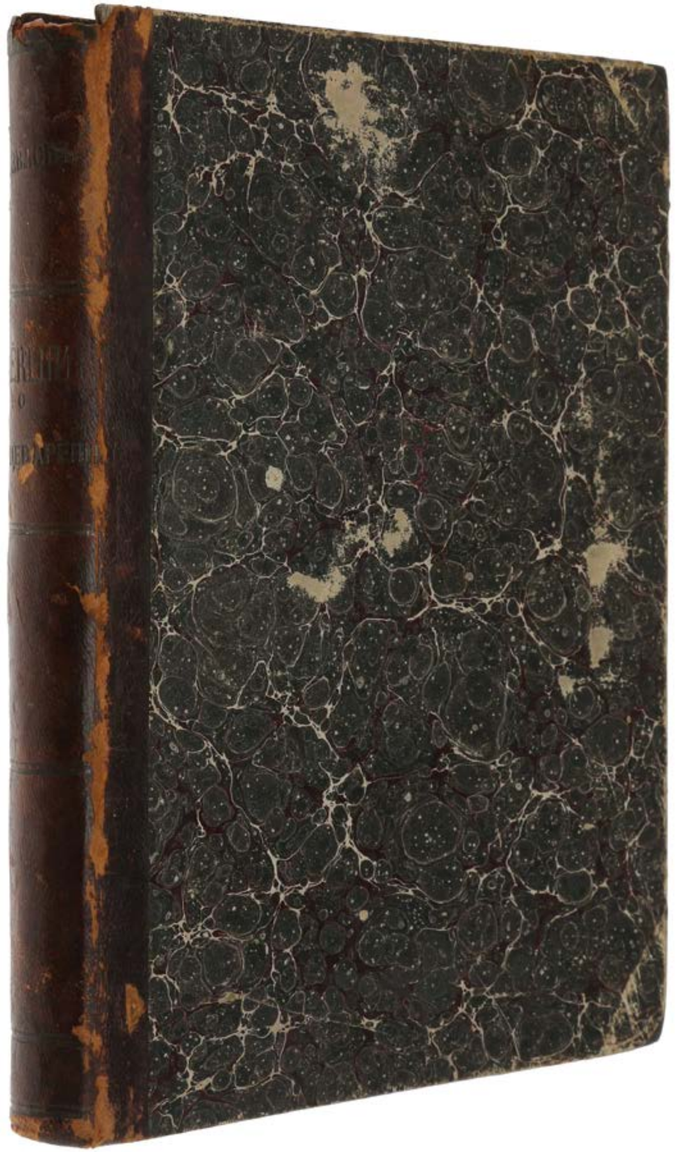
"Pavlov became passionately absorbed with physiology, which in fact was to remain of such fundamental importance to him throughout his life. It was during this first course that he produced, in collaboration with another student, Afanasyev, his first learned treatise, a work on the physiology of the 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).

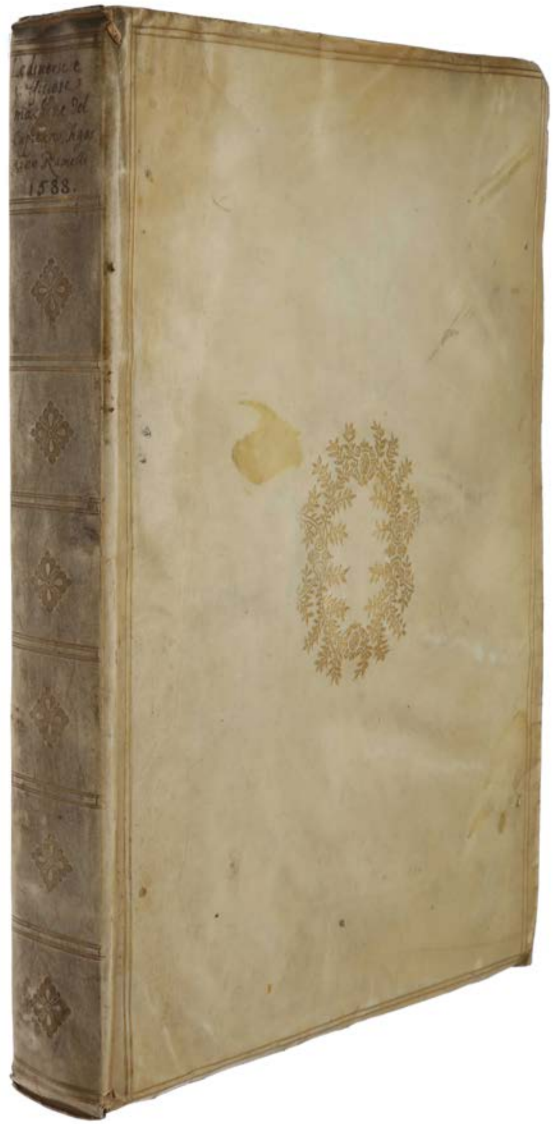
Pavlov's discovery of the conditioned reflex has gained growing significance in politics and sociology. He concluded that even such concepts as freedom, curiosity and religion were conditioned reflexes of the brain. '"Essentially, only one thing in life is of real interest to us — our psychical experience,"' 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.

AN XCEPTIONALLY FINE COPY IN UNTOUCHED GILT VELLUM

RAMELLI, Agostino. *Le Diverse et Artifiose Machine... Nelle quali si contengono varii et industriosi Movimenti, degni di Grandissima Speculatione, per cavarne beneficio infinito in ogni sorte d'operatione.* Paris: for the author, 1588.

$225,000



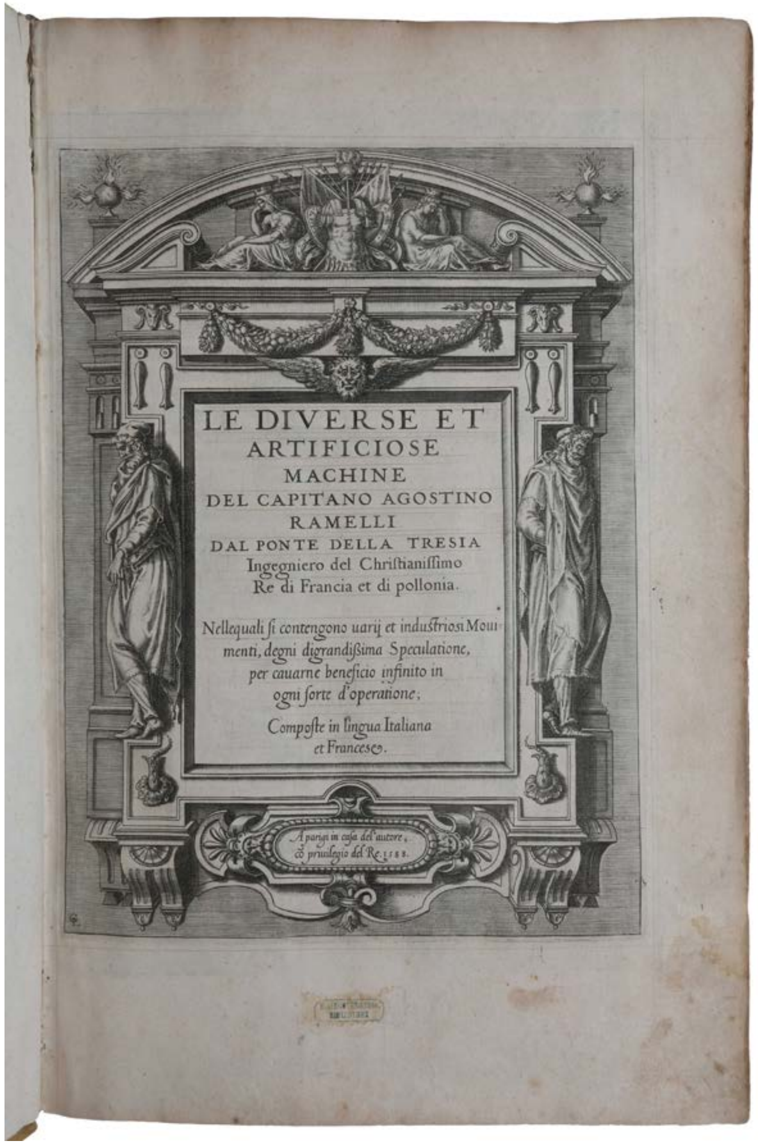
Folio (354 x 228 mm). Ruled in red throughout. Roman (French) and italic (Italian) types. Engraved title within architectural frame by Leonard Gaultier, each leaf of text printed within a border of typographical fleurons, engraved portrait of Ramelli by Gaultier on title-page verso, 194 engravings (174 full-page, 20 full-sheet) numbered I-CXCV (CXLVIII and CXLIX combined on a single engraving), three signed with the cipher 'JG' (CL-CLII). (Four leaves comprising n1 [f.97], o1 [f.105, mis-signed n1], o8 [f.112] and n8 [f.104] misbound, very short minor tears to i4 and T2, tiny marginal chips to i8 and V3, small marginal chip repaired on P2, occasional light spotting and browning.) Contemporary French limp vellum, covers framed with gilt double fillet enclosing gilt centre ornament of laurel leaf tools, flat gilt spine, lettered at head of spine in ink manuscript, gilt edges (small stain on upper cover of binding, spine lightly soiled, lacking ties), modern green cloth slipcase.

First edition, a magnificent copy bound in contemporary gilt vellum, and with outstanding provenance, of one of the most famous illustrated books of the sixteenth century and a landmark in book design. "The plates in Ramelli's treatise are artistically as well as technologically superb, the bilingual text beautifully printed, and both plates and text surrounded by handsome borders of typographic ornaments. The reasons for this sumptuousness were twofold:

first Ramelli had dedicated the book to his patron Henri III; and second, he had previously had several designs stolen from him by a trusted associate (probably Ambroise Bachot, later engineer to Henri IV), who published them in corrupt and mutilated form and claimed them as his own. As a result of this experience Ramelli planned his treatise as a particularly lavish work that would be difficult to counterfeit, and produced and published it from his own house where he could maintain absolute control over the project' (Norman). Together with Agricola's *De Re Metallica* (1556), Ramelli's work was the most influential and copied of all the early illustrated manuals of inventions and machines. Its influence was felt in such later works as Böckler's *Theatrum machinarum* (1662), and it was even copied in China, where it had been taken by Jesuit missionaries. This is without doubt one of the finest copies to have appeared on the market, the only comparable copy being that of Nicolas-Claude Fabri de Peiresc in the Norman library (Christie's New York, 18 March 1998, lot 168, $200,500; subsequently sold at the Freilich sale, Sotheby's New York, 11 January 2001, lot 449, $291,750).

Provenance: Francoise d'Espinay de Bretagne, first wife of Henri de Schomberg, comte de Nanteuil (1604 gift inscription on front free endpaper); Princes of Liechtenstein (stamp on title); Otto Schäfer (sold Sotheby's 27 June 1995, lot 163, £38,900).

"Ramelli was born in northern Italy, probably in 1531. As a young man he served under the famous Italian warlord, Gian Giacomo de' Medici, Marquis of Marignano, and became trained in mathematics and military engineering. His reputation grew and he eventually left for France to serve under the Duke of Anjou, later King Henry III. His year of death is unknown and usually given as 'circa 1600,' but since documents exist to show that he was still alive in 1608, circa 1610 is a more realistic approximation.

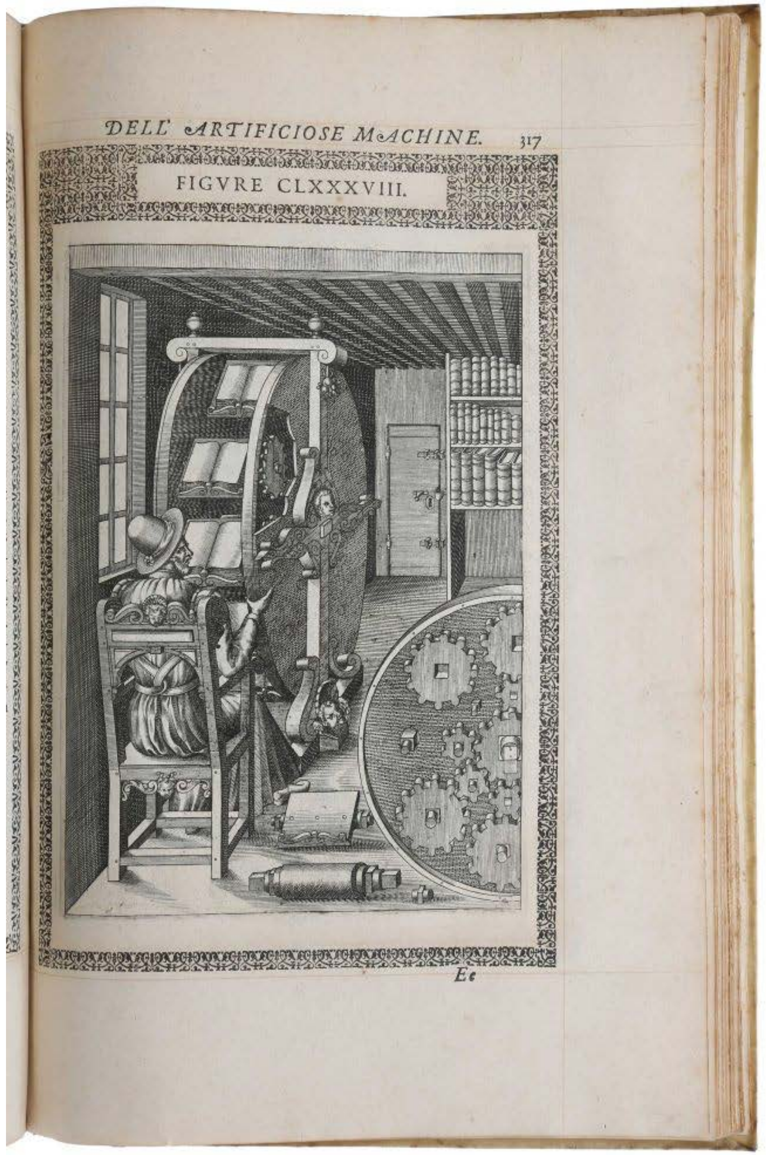


"Ramelli was greatly influenced by the increasing importance placed on mathematics and geometry as an important tool for engineers and artists, and particularly by the writings of Guidobaldo del Monte (1545-1607) and Petrus Ramus (1515-1572). Ramelli's interest in mathematics is demonstrated in the preface to his book, 'On the excellence of mathematics in which is shown how necessary mathematics are for learning all the liberal arts.' Ramelli also wanted to make his book accessible to many engineers so, as an Italian living in France, he produced both Italian and French descriptions of the machines.

"The book itself is a fine example of the exquisite work of late sixteenth-century French printers and artists. It is a large book in folio format thus allowing great detail to be placed in the numerous engraved plates which total 195 in all (although plates 148 and 149 are combined into one image). Twenty of the plates are two-page spreads. Ramelli's bilingual descriptions are much more detailed than those found in previous illustrated books of machines (popularly called 'theaters of machines') by Jacques Besson (*Theatrum instrumentorum et machinarum*, 1569) and Jean Errard de Bar-le-Duc (*Le premier livre des instruments mathématiques mechaniques,* 1584).

"Ramelli's book had a great influence on future mechanical engineering as can be see in Georg Andreas Böckler's work, *Theatrum machinarum novum*, 1662, where he copied eighteen of Ramelli's plates. Ramelli's influence can also be seen in the well-known works of Grollier de Servière (*Recueil d'ouvrages curieux de mathematique et de mecanique*, 1719) and Jacob Leupold (the multi-volume set *Theatrum machinarum*, 1724-1739). Leupold's work helped pass along Ramelli's ideas to a large population of eighteenth-century engineers.

"Of the 195 machines pictured in the book, the majority are of devices designed



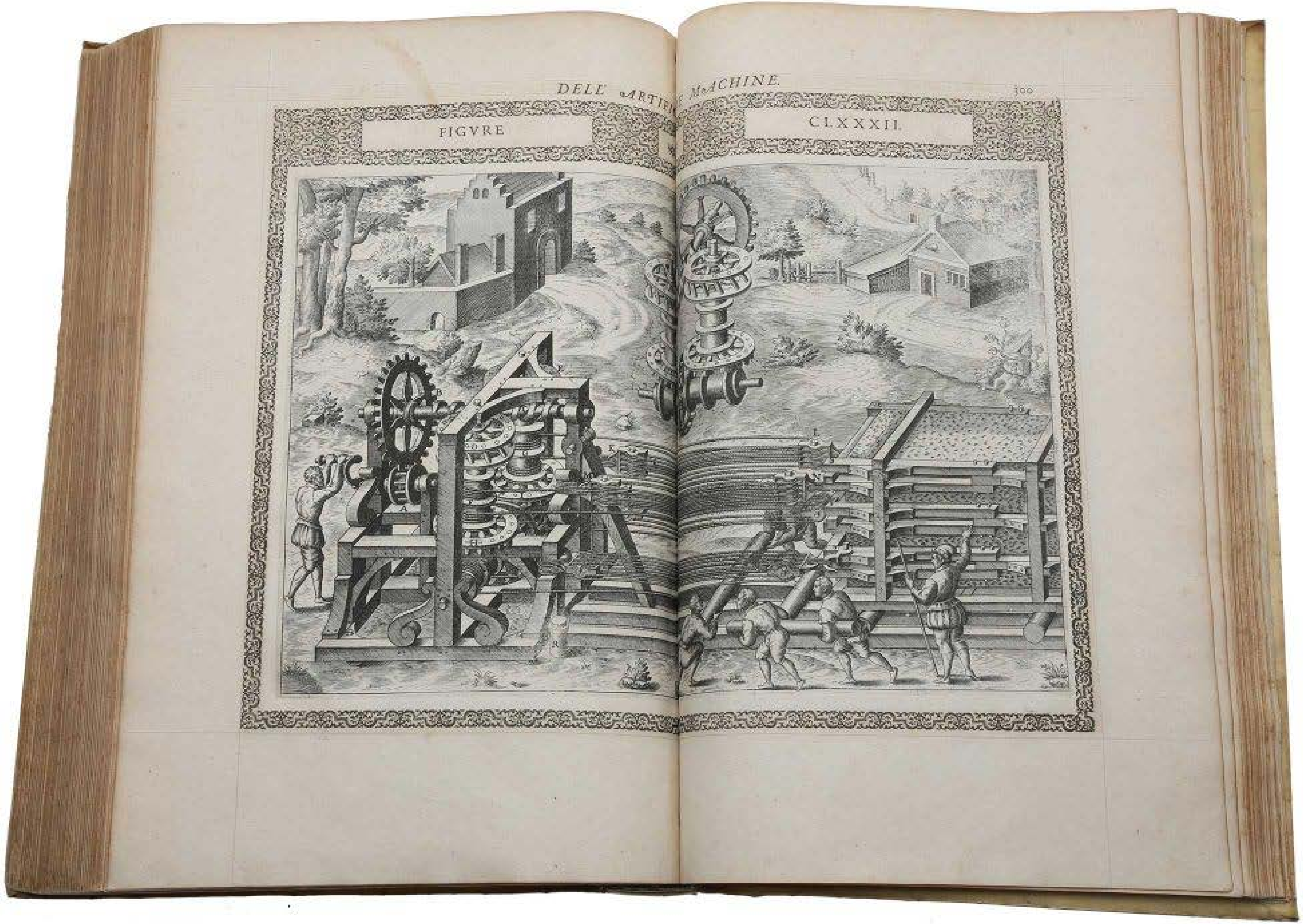
to raise water. The breakdown is as follows:

- 110 Water-raising machines
- 21 Grain mills
- 4 Other mills
- 10 Cranes
- 7 Machines for dragging large objects
- 2 Machines to raise excavated earth
- 2 Cofferdams
- 4 Fountains and artificial bird-calls
- 1 Book wheel
- 15 Military bridges
- 14 Screw jacks and other breaking devices
- 4 Hurling engines
- 1 Gunner's quadrant

"Bachot was an apprentice and assistant to Ramelli, eventually becoming an architect and engineer to King Henry IV. As described in Gnudi's introduction to her translation of Ramelli, during the sixteen years he spent with Ramelli, Bachot learned a great deal about engineering but had a falling out with the elder engineer and attempted to pass off some of Ramelli's machine designs as his own in an attempt to gain patronage. These designs were published in 1587 in a book by Bachot, *Le Timon*, and the similarity in style between Bachot's engravings and Ramelli's is impressive. After intense research, Gnudi concluded that Bachot engraved the plates for his own work and most of those produced for Ramelli's book as well. After their falling out in 1587, Ramelli still used the plates Bachot engraved for him rather than have them redone. Gnudi never claimed that Bachot produced the original drawings, but rather engraved the plates after Ramelli's drawings.

"Only the one edition of the book was issued during Ramelli's lifetime. In 1620, a German translation appeared in Leipzig as *Schatzkammer, mechanischer Künste*..., published by Henning Grossen den Jüngern with the illustrations re-engraved by Andreas Bretschneider. A number of facsimile reprints have appeared since 1970 and in 1976 an English translation was prepared by Martha Teach Gnudi and Eugene S. Ferguson and published jointly by the Johns Hopkins University Press and the Scholar Press" (Brashear).

Adams R-52; Cockle 788; Dibner 173; Mortimer (French) II.452; Norman 1777; Riccardi I.341; Wellcome 5323. Brashear, 'Ramelli's Machines: Original drawings of sixteenth century machines' (sil.si.edu/ondisplay/ramelli/intro.htm). Brun, *Le livre français illustré de la renaissance* (ed. 1969), p. 280; M.T. Gnudi, 'Agostino Ramelli and Amrboise Bachot,' *Technology and Culture* 15 (1974), pp. 614-25; modern edition and translation, ed. Gnudi & Ferguson (1976).


DELL' ARTIFICIOSE MACHINE.
300
FIGVRE
CLXXXII.

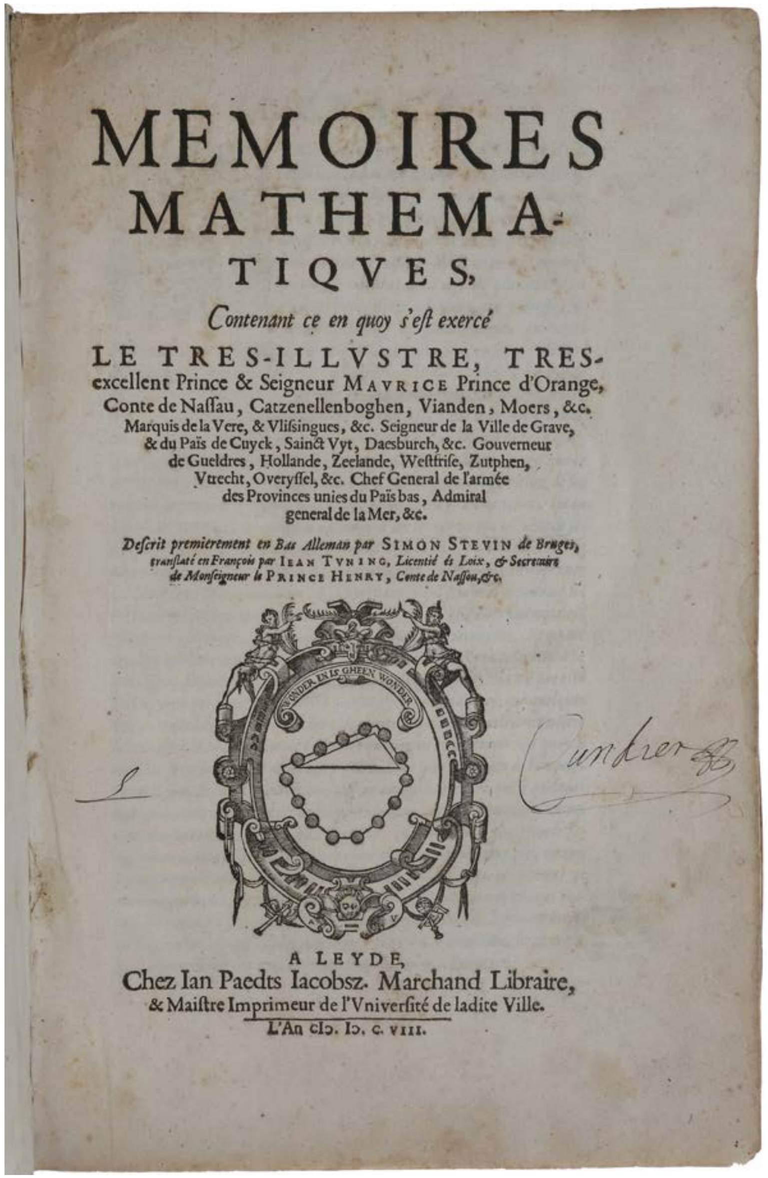
ONE THE MOST ORIGINAL SCIENTISTS OF THE 16TH CENTURY

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



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.

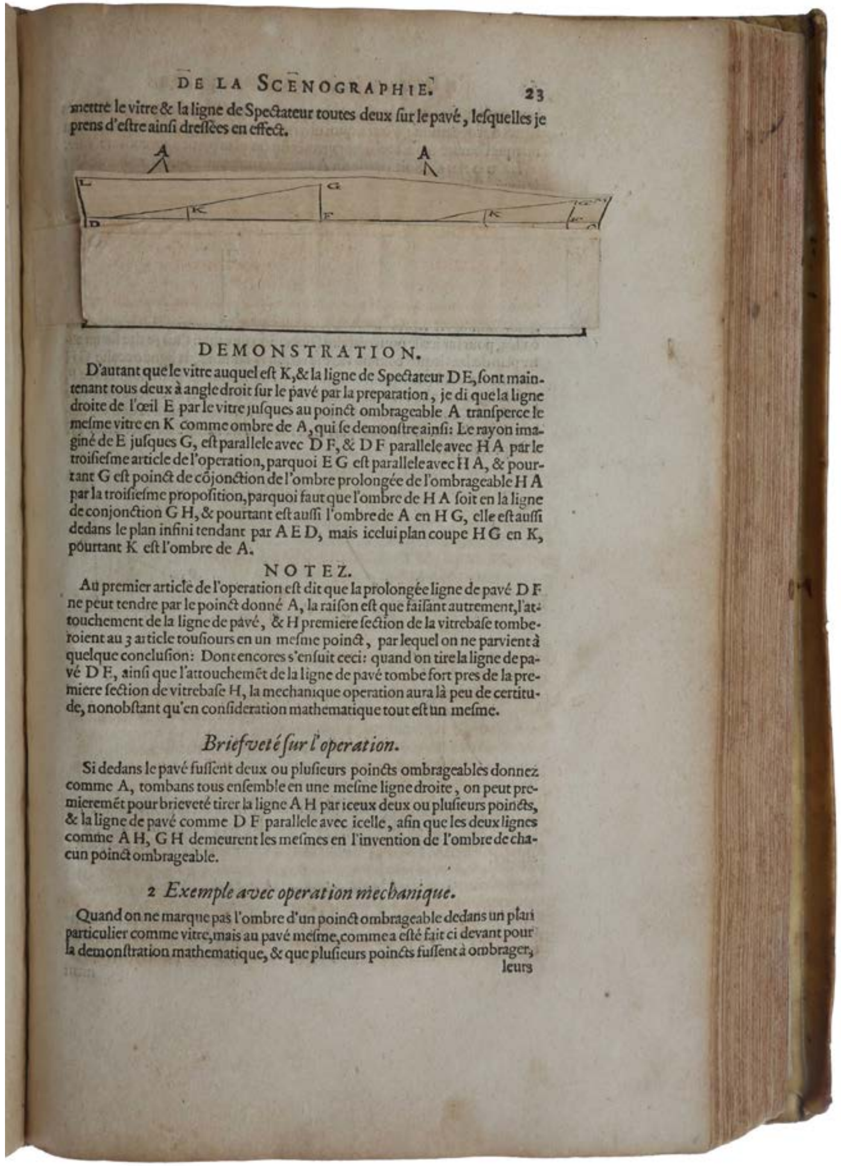
The first part of the work, entitled *Cosmographie* (1608), is a treatise on the trigonometrical techniques used in the observation of the heavens, together with extensive tables of sines, tangents and secants. "The first to use the term trigonometry seems to have been Pitiscus, whose book *Trigonometria* 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



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 …

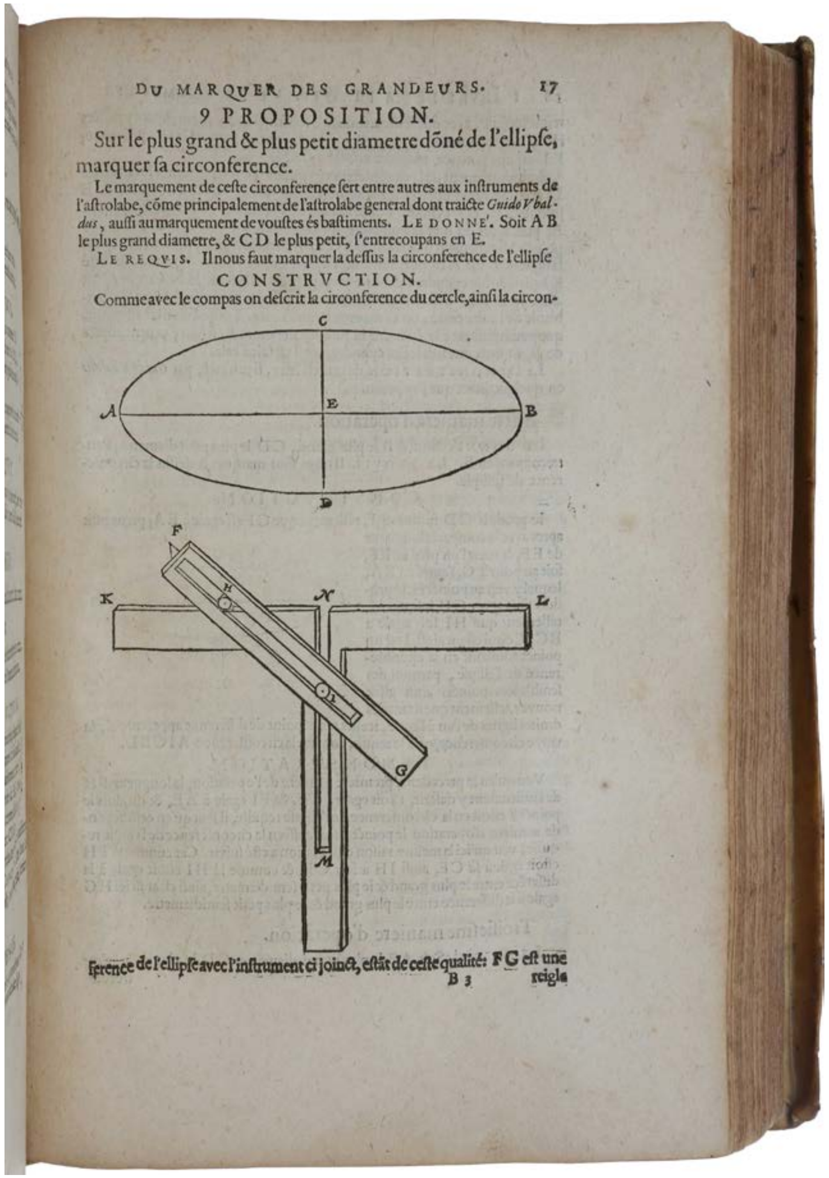
"In Book IV we find a theory of proportions. It is shown how areas and volumes proportional to given line segments can be found. The most interesting part is that in which the two mean proportionals between two line segments are discussed. As in the *Problemata Geometrica,* reference is made to Hero's 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


DU MARQUER DES GRANDEURS. 17

9 PROPOSITION.

Sur le plus grand & plus petit diametre dõné de l'ellipſe, marquer ſa circonference.

Le marquement de ceſte circonference ſert entre autres aux inſtruments de l'aſtrolabe, cõme principalement de l'aſtrolabe general dont traicte *Guido Vbaldus*, auſſi au marquement de vouſtes és baſtiments. LE DONNE'. Soit A B le plus grand diametre, & C D le plus petit, ſ'entrecoupans en E.

LE REQVIS. Il nous faut marquer la deſſus la circonference de l'ellipſe

CONSTRVCTION.

Comme avec le compas on deſcrit la circonference du cercle, ainſi la circon-

ference de l'ellipſe avec l'inſtrument ci joinct, eſtãt de ceſte qualité: F G eſt une reigle

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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 $x = 10, 100, 1000, \dots$ 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.

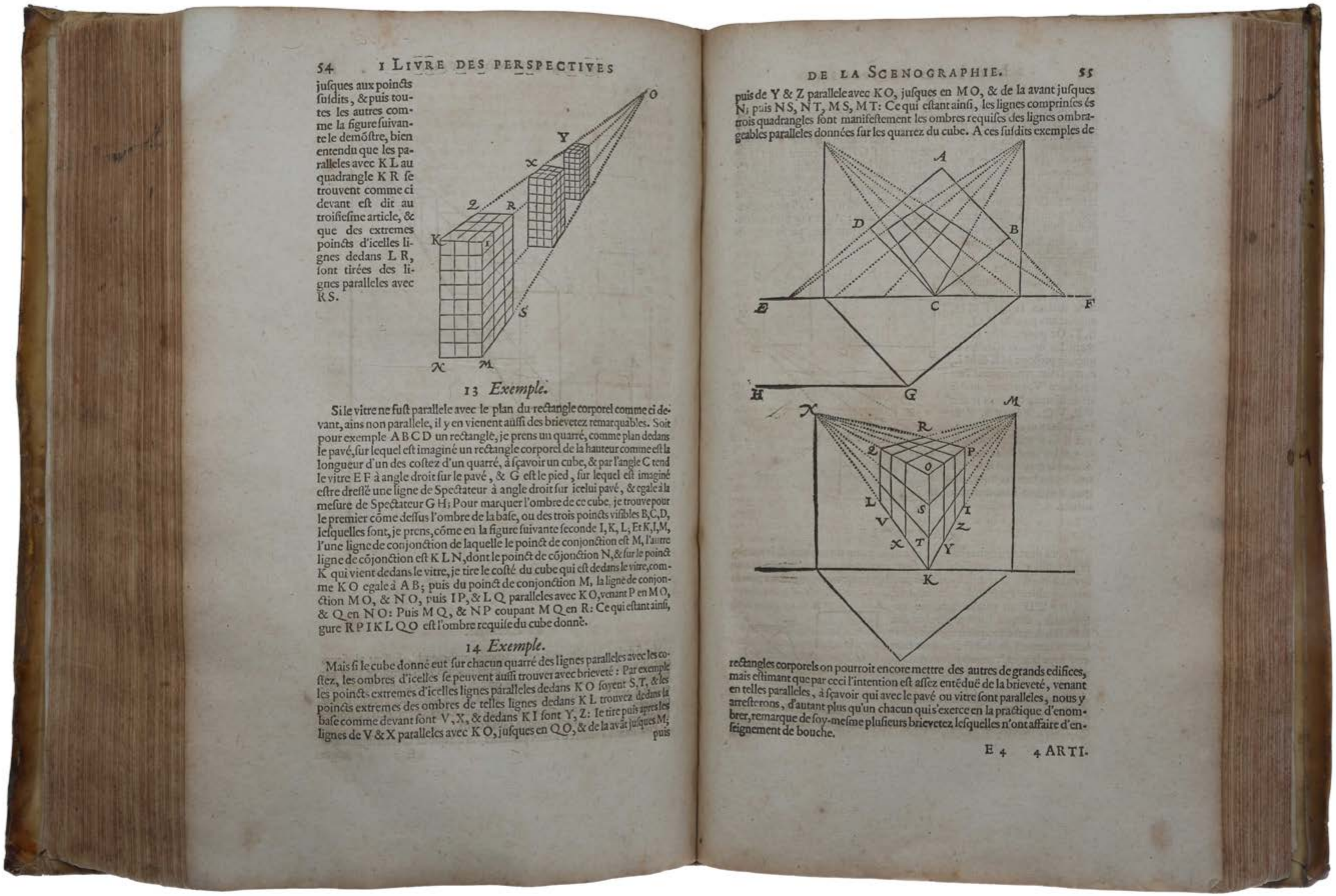
"The method can also be applied if the root is not an integral number. If $x^3 = 300x + 33900000$ we find $323 < x < 324$. Then write $x = 3230/10$ and proceed as above, first with 1/10, then 1/100, etc. This can go on indefinitely. If, for instance, the root were $x = 5/6$, the method gives first 8/10, 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 demõstre, 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 rectangle, 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 cõme dessus l'ombre de la base, ou des trois poincts visibles B, C, D, lesquelles sont, je prens, cõme 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 cõjonction est K L N, dont le poinct de cõjonction 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 avãt 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 entẽduë 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.

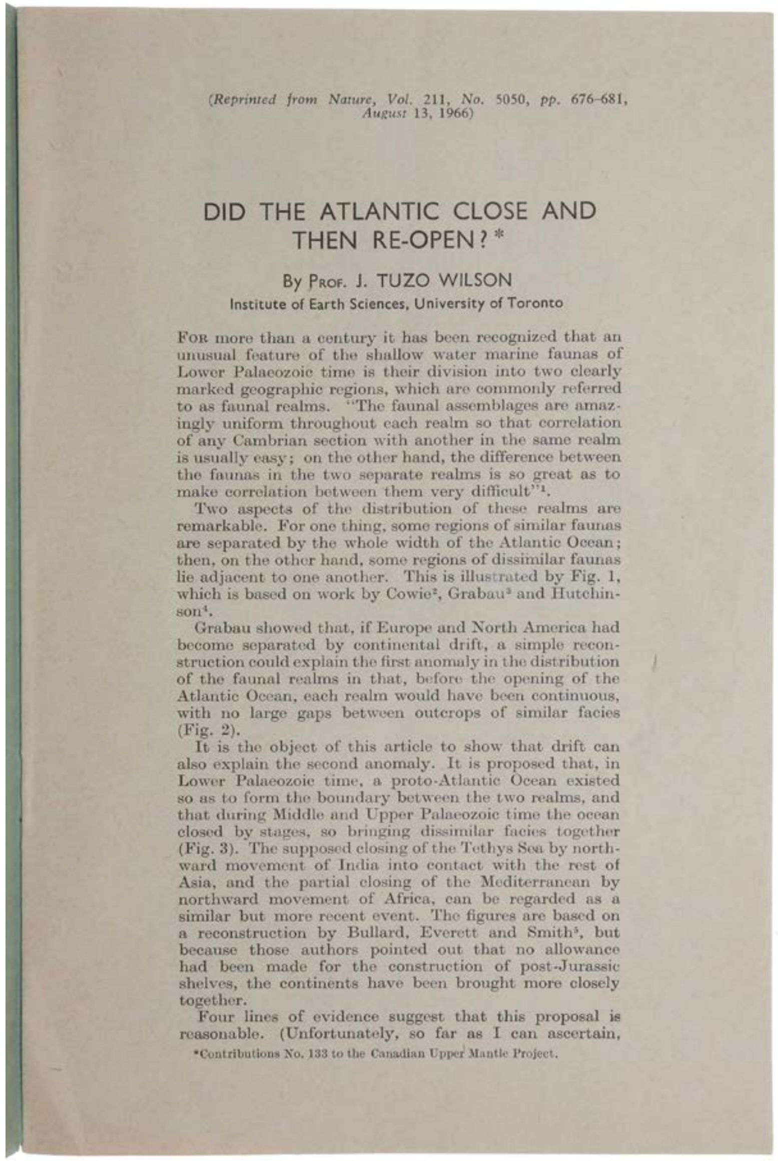
CONTINENTAL DRIFT

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 …

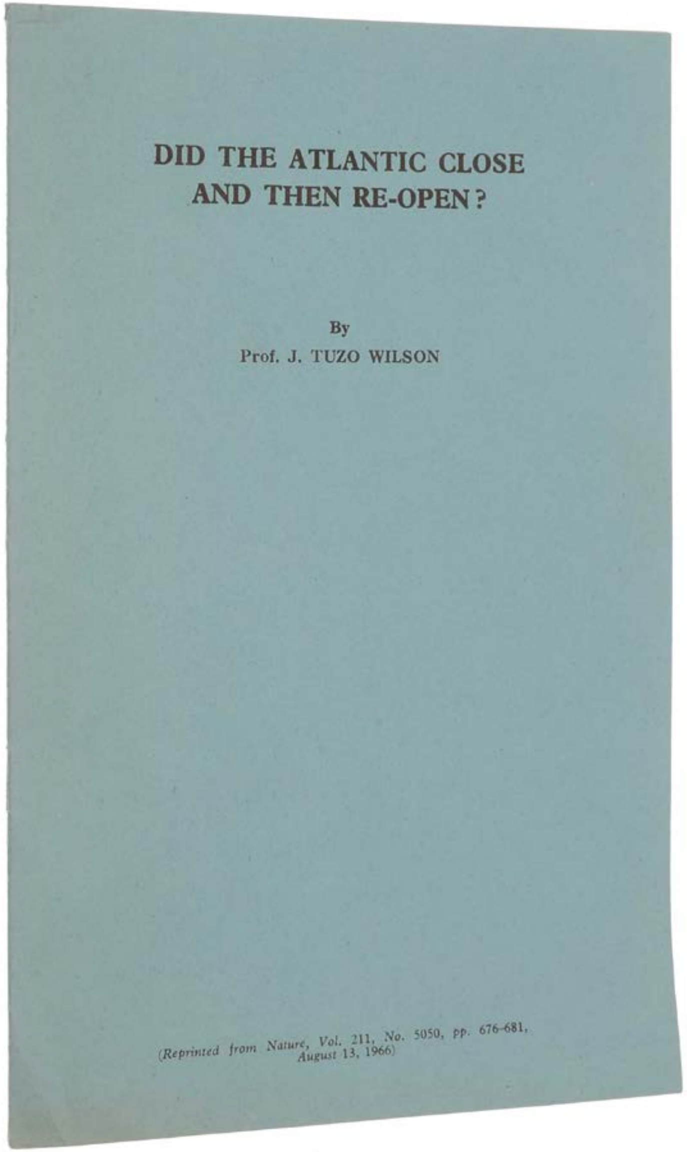
"Wegener did not speculate about how his Pangea had come together. But as the new plate tectonics emerged from studies of the ocean floor and began to revitalize drift theory, the time was ripe to see the break-up of Pangea as part of a bigger process. Professor Kevin Burke of the University of Houston, 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.

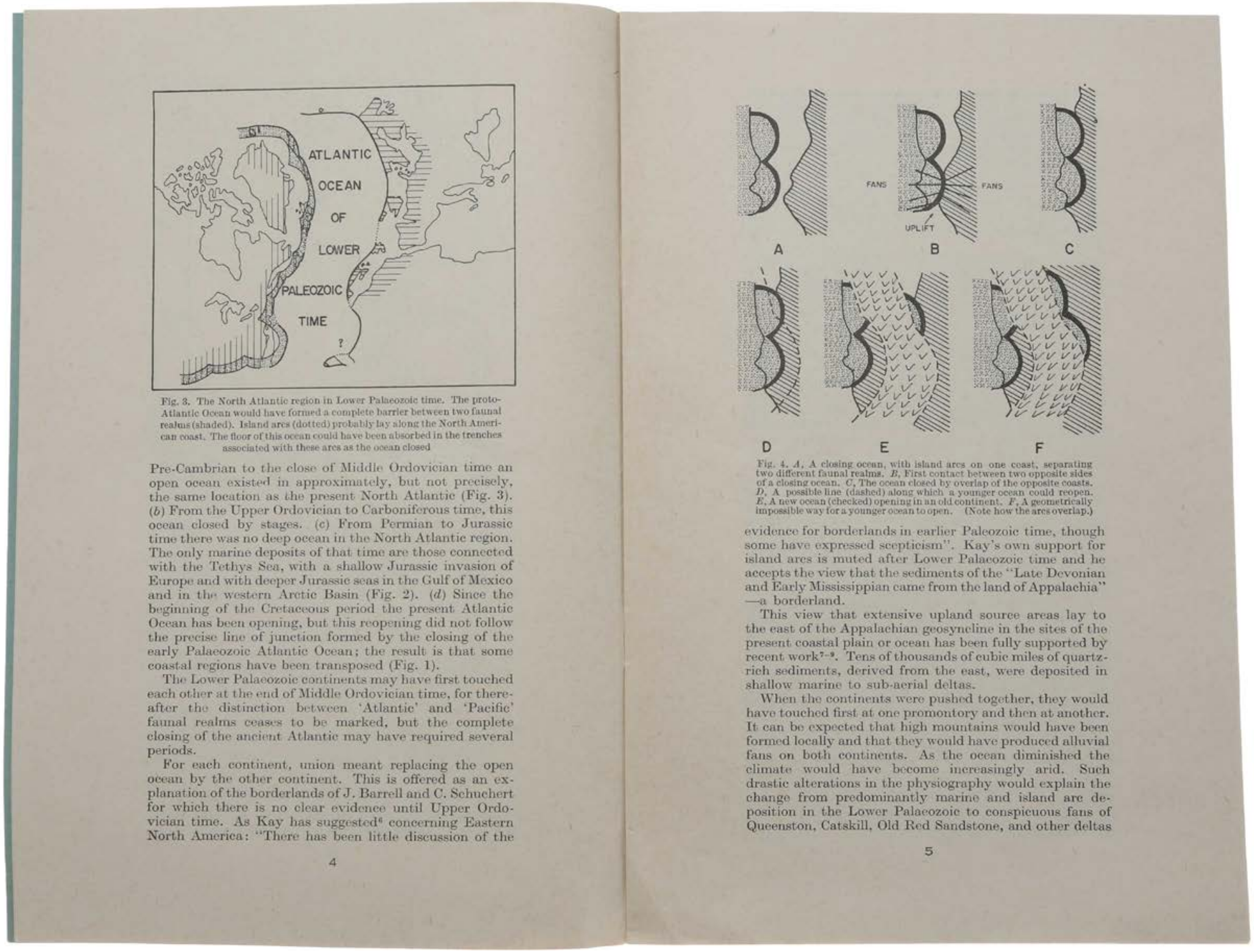


Fig. 3. The North Atlantic region in Lower Palaeozoic time. The proto-Atlantic Ocean would have formed a complete barrier between two faunal realms (shaded). Island arcs (dotted) probably lay along the North American coast. The floor of this ocean could have been absorbed in the trenches associated with 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

4

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

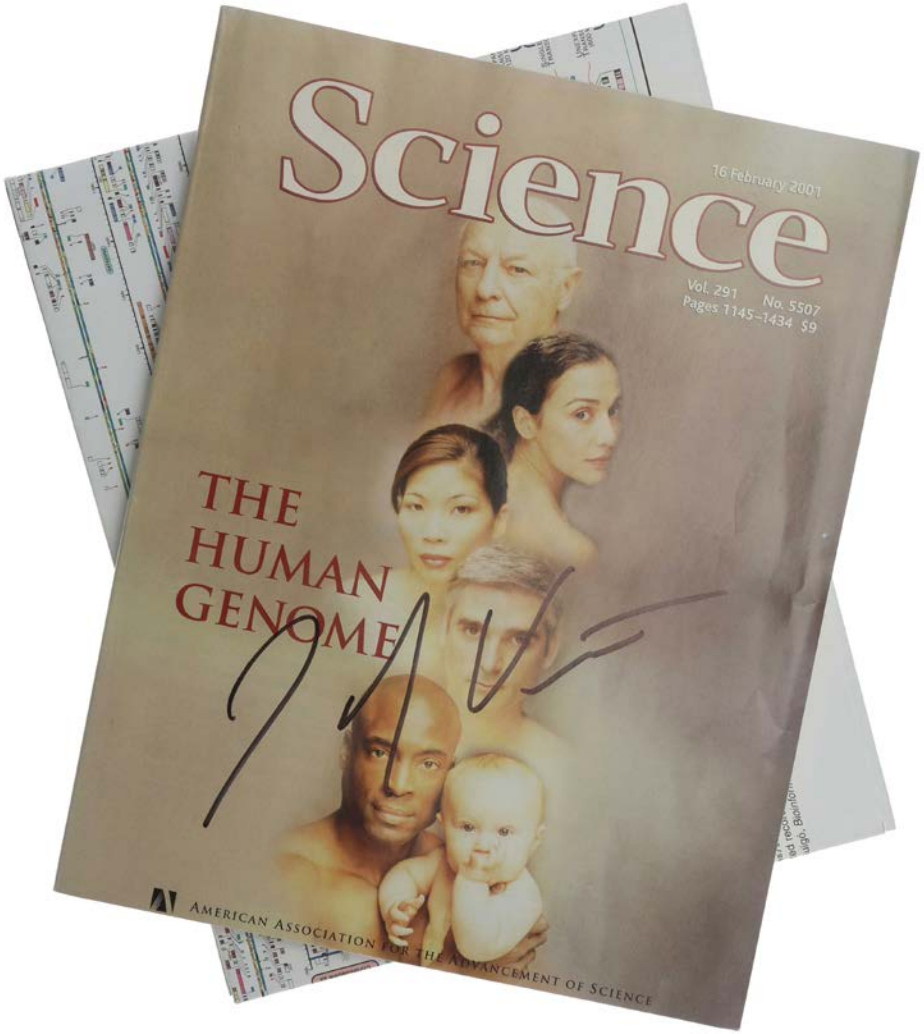
SEQUENCING THE HUMAN GENOME

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.

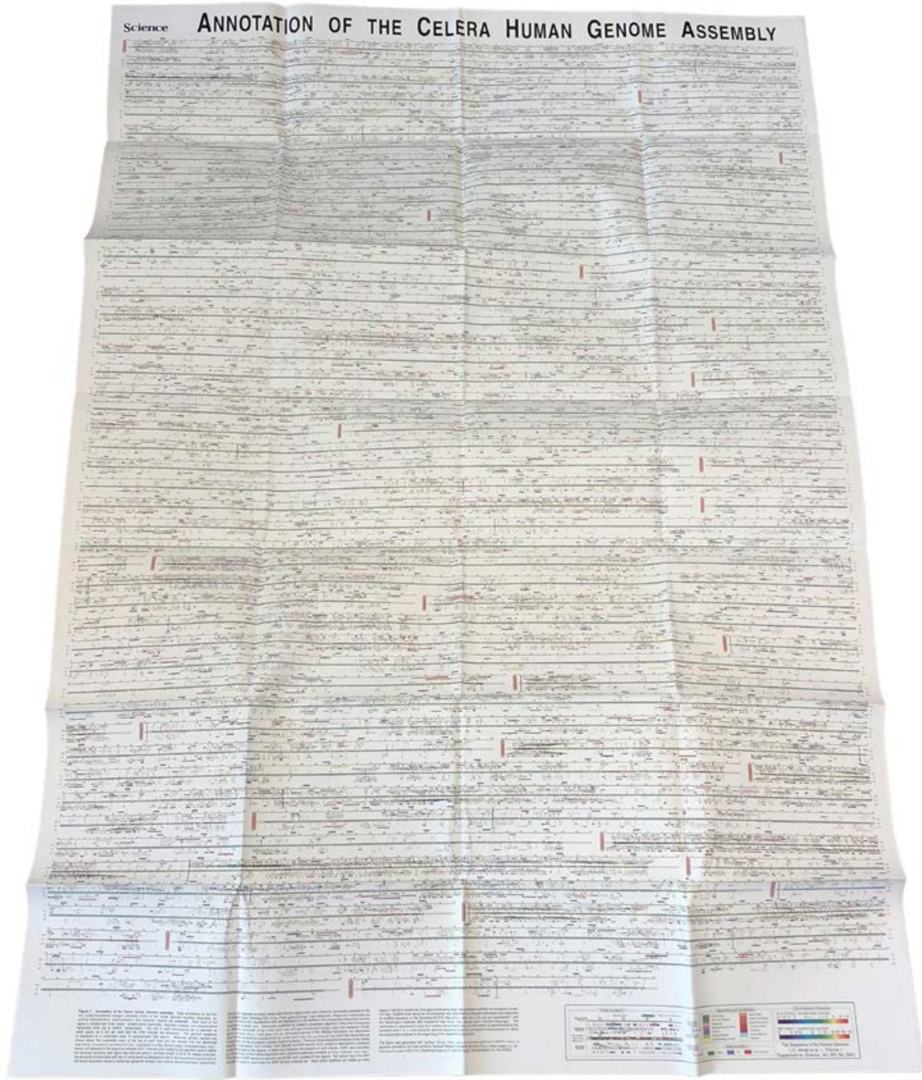
In the publicly funded HGP, researchers collected blood (female) or sperm (male) samples from a large number of donors. Only a few of many collected samples were processed as DNA resources. Thus the donor identities were protected so neither donors nor scientists could know whose DNA was sequenced. In 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.

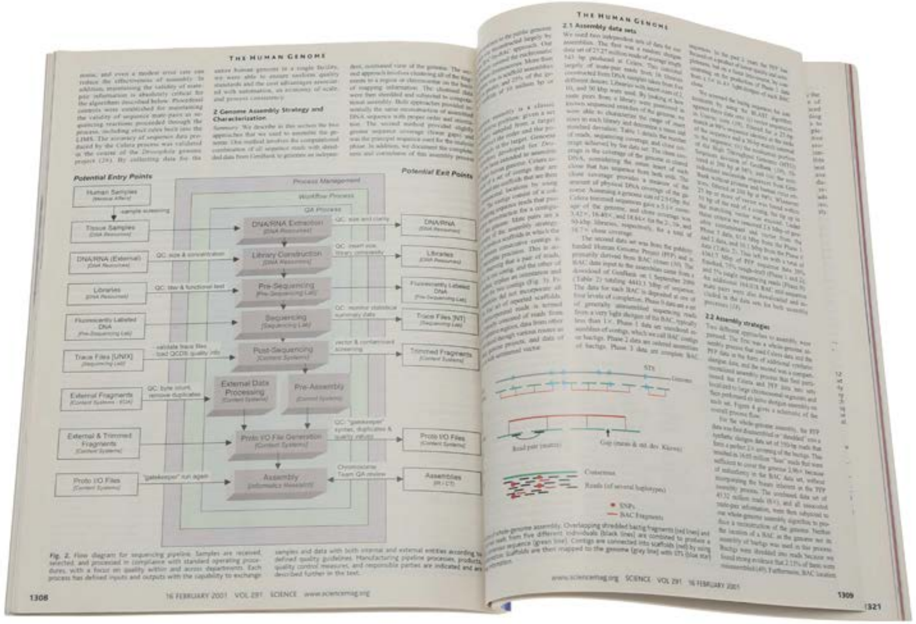
"Venter left the NIH in 1992 and, with the backing of the for-profit company Human Genome Sciences, in Gaithersburg, MD, established a research arm, The Institute for Genomic Research (TIGR). At the institute a team headed by American microbiologist Claire Fraser, Venter's first wife, sequenced the genome 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).



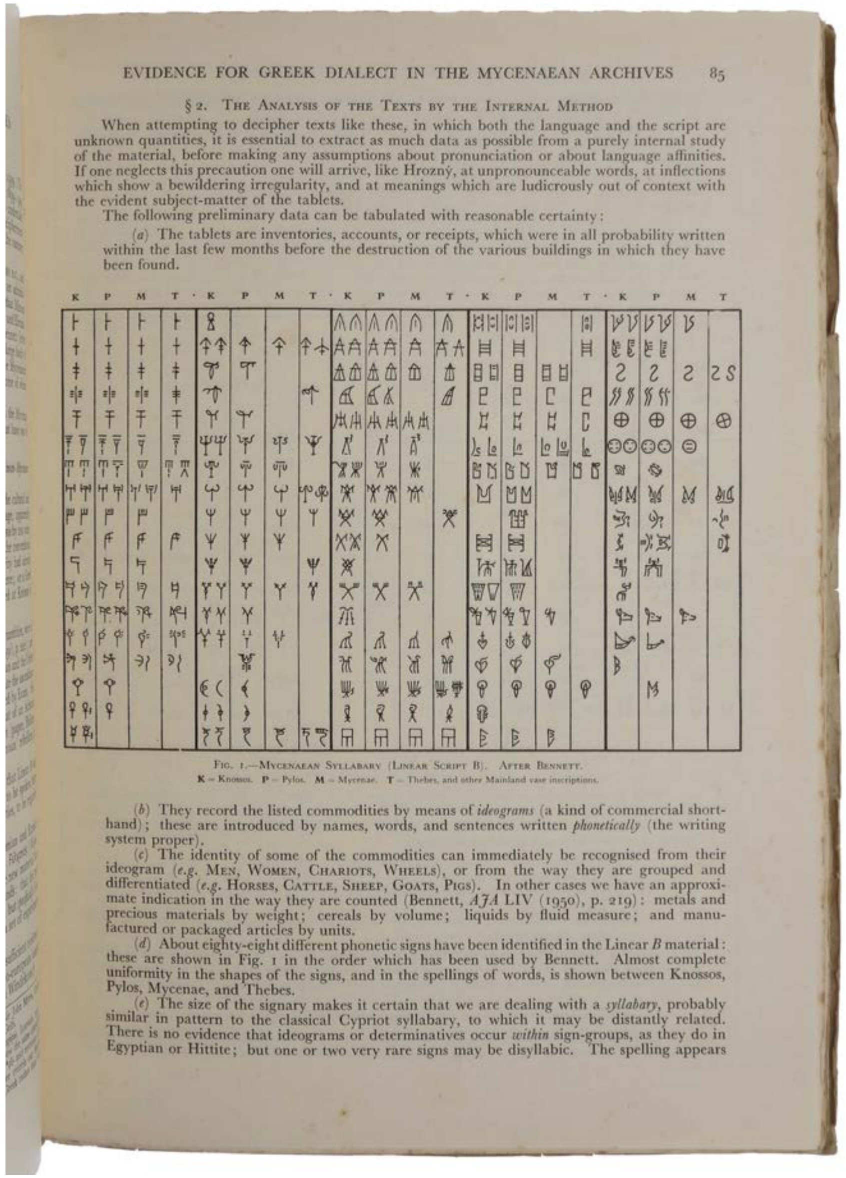
ONE OF THE GREAT REAKTHROUGHS IN CLASSICAL SCHOLARSHIP

VENTRIS, Michael & CHADWICK, John. *Evidence for Greek Dialect in the Mycenaean Archives.* London: Council of the Society for the Promotion of Hellenic Studies, 1953.

$1,500

Pp. 84-103 in Journal of Hellenic Studies, Vol. 73, 1953. 8vo, pp. xv, 210, with frontispiece photographic portrait and seven photographic plates. Original printed wrappers.

First edition of Ventris' historic paper describing his decipherment of the ancient Minoan script known as 'Linear B', ""the greatest advance in classical scholarship in the last 100 years … The decipherment of Linear B opened up, and indeed created, a whole new branch of scholarship. It added about 500 years to our knowledge of Greek, catapulting our understanding of early Greek history and society back into the second millennium BC, to the end of the Bronze Age at about 1200BC," said Dr Torsten Meissner, organiser of today's conference. "Suddenly the places of the figures of Greek mythology - like the legendary King Minos of Knossos or Homeric heroes like Nestor, king of Pylos, or Agamemnon, king of Mycenae – could be placed in a real setting through the clay tablets that record their administrative and political organization"" (*Cracking the code: the decipherment of Linear B 60 years on*, University of Cambridge, 13 October 2012 (cam.ac.uk/research/news/cracking-the-code-the-decipherment-of-linear-b-60-years-on#sthash.Xn1DnMd3.dpuf).


EVIDENCE FOR GREEK DIALECT IN THE MYCENAEAN ARCHIVES 85

§ 2. The Analysis of the Texts by the Internal Method

When attempting to decipher texts like these, in which both the language and the script are unknown quantities, it is essential to extract as much data as possible from a purely internal study of the material, before making any assumptions about pronunciation or about language affinities. If one neglects this precaution one will arrive, like Hrozný, at unpronounceable words, at inflections which show a bewildering irregularity, and at meanings which are ludicrously out of context with the evident subject-matter of the tablets.

The following preliminary data can be tabulated with reasonable certainty:

(a) The tablets are inventories, accounts, or receipts, which were in all probability written within the last few months before the destruction of the various buildings in which they have been found.

Fig. 1.—Mycenaean Syllabary (Linear Script B). After Bennett.
K = Knossos. P = Pylos. M = Mycenae. T = Thebes, and other Mainland vase inscriptions.

(b) They record the listed commodities by means of *ideograms* (a kind of commercial shorthand); these are introduced by names, words, and sentences written *phonetically* (the writing system proper).

(c) The identity of some of the commodities can immediately be recognised from their ideogram (*e.g.* Men, Women, Chariots, Wheels), or from the way they are grouped and differentiated (*e.g.* Horses, Cattle, Sheep, Goats, Pigs). In other cases we have an approximate indication in the way they are counted (Bennett, *AJA* LIV (1950), p. 219): metals and precious materials by weight; cereals by volume; liquids by fluid measure; and manufactured or packaged articles by units.

(d) About eighty-eight different phonetic signs have been identified in the Linear *B* material: these are shown in Fig. 1 in the order which has been used by Bennett. Almost complete uniformity in the shapes of the signs, and in the spellings of words, is shown between Knossos, Pylos, Mycenae, and Thebes.

(e) The size of the signary makes it certain that we are dealing with a *syllabary*, probably similar in pattern to the classical Cypriot syllabary, to which it may be distantly related. There is no evidence that ideograms or determinatives occur *within* sign-groups, as they do in Egyptian or Hittite; but one or two very rare signs may be disyllabic. The spelling appears

“When during the early 20th century archaeologists excavated some of the most famous sites of Ancient Greece – notably Knossos on the island of Crete and Mycenae and Pylos on the mainland – they found large numbers of clay tablets inscribed with a type of script that baffled them. It was significantly different to any other script known at the time. Moreover, it was immediately clear that there were at least two variants of this type of writing.

“These scripts – characterised by about 90 different characters, and on the clay tablets interspersed with signs for numerals as well as the depiction of every-day objects and commodities such as pots, cloth and grain – acquired the name ‘Linear’. Linear because they were more abstract and characterised by a more linear style than the earlier hieroglyphic type of writing, also found on Crete. The two variants were given the names Linear A and B. It was clear that Linear A was the earlier type, much rarer and restricted to the island of Crete. The younger type B was found in significantly larger numbers and found at Knossos, Mycenae and Pylos. Since the original excavations evidence for the same type of writing has come to light at other places, including Thebes and Tiryns on the Greek mainland and Chania on Crete …

“In the wake of some of the most famous excavations in history, the classicists who put their minds to the tantalising puzzle of deciphering Linear B included the best-known names in the field. After the German scholar Heinrich Schliemann had excavated Troy (or a site compatible with Homer’s famous city) and Mycenae and thereby opened the door to Greek archaeology of the second millennium BC, the British archaeologist Arthur Evans discovered these inscribed tablets in large numbers at Knossos in the year 1900. Evans and other scholars knew that the tablets held the key to a fuller understanding of the Mycenaean civilisation. But deciphering what was inscribed on them seemed an impossible task, given that both the script and the language behind it were unknown.

“After many unsuccessful attempts by would-be decipherers from all over the world, it was a brilliant British amateur called Michael Ventris who was to prove pivotal in the unlocking of the secrets of Linear B” (*ibid*.).

“Michael George Francis Ventris (1922–1956) was born in Wheathamstead, England, and educated privately in England and Switzerland and later at Stowe School from 1935 to 1939. As a boy, Ventris was fascinated with the classics, and much of his early education was of an informal nature, obtained primarily though books, travel, and self-taught languages. Ventris could speak six European languages as well as read Latin and classical Greek.

“In 1936, during a school trip to the Burlington House Exhibition, 14-year-old Ventris heard the famed archaeologist Sir Arthur Evans lecture on the Linear B script that he had discovered at Knossos. This encounter sparked Ventris’s lifelong obsession with Linear B. Four years later, in 1940, his first attempt at the decipherment of Linear B, “Introducing the Minoan Language,” was published in the *American Journal of Archaeology*, although he would later reject the theories presented in the paper.

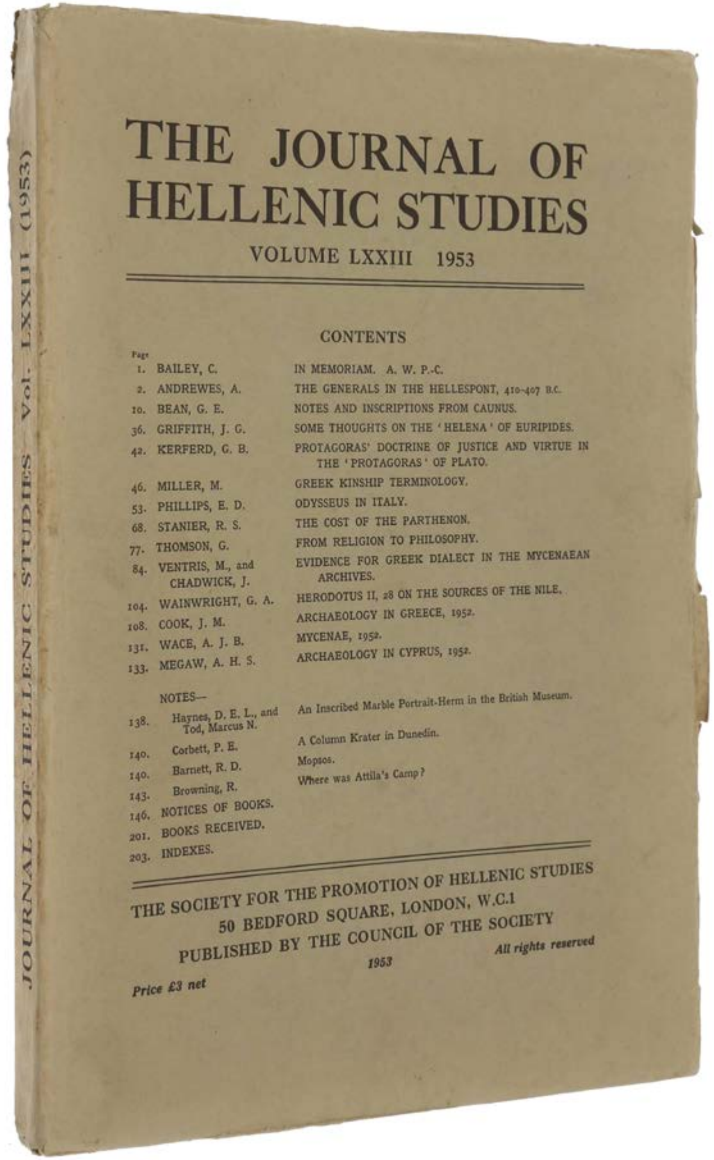
“In 1940, Ventris entered the Architectural Association School. His studies were interrupted shortly thereafter by World War II, during which he served in the Royal Air Force as a navigator. Upon completion of his service in 1946, Ventris reentered the Architectural Association School, and he earned his diploma with honors in 1948. During the next few years, Ventris returned to work on the ancient scripts during his spare time, creating a “work group” of colleagues that exchanged notes through correspondence. To members of the group Ventris circulated his work notes, which detailed his progress step by step. In addition, Ventris circulated a questionnaire to survey the progress of others working on decipherment, including, among others, Alice E. Kober and Emmett L. Bennett, Jr.

The results of this survey were compiled into the “Mid-Century Report” in 1950.

“Ventris’ progress on the decipherment of Linear B was influenced heavily by the work of other scholars, particularly that of Kober. She had noted that certain words in Linear B inscriptions had changing word endings, or declensions in the manner of Latin or Greek. Using this clue, Ventris constructed a series of grids associating the symbols on the tablets with consonants and vowels. Although *which* consonants and vowels they were could not yet be identified, Ventris learned enough about the structure of the underlying language to begin guessing.

“In 1951, additional Linear B tablets were discovered on the Greek mainland and were subsequently published, and Ventris began to suspect that some of the sequences of symbols he encountered on in the tablet texts were actually names. Noting that certain names appeared only in the Linear B texts, Ventris made an inspired guess that the names applied to cities on the island. Using the symbols he could decipher from this, Ventris soon decoded much of the text and determined that the underlying language of Linear B was in fact an archaic form of Greek. In June of 1952, Ventris announced his discovery over a British radio program.

“With the assistance of Cambridge linguist John Chadwick, Ventris assembled striking evidence supporting his theory. Their historic paper, “Evidence for Greek Dialect in the Mycenaean Archives,” was published in 1953, followed by an almost universal acceptance of the decipherment. Tragically, however, Ventris was killed only a few years later in an automobile accident on September 6, 1956” (Michael Ventris Papers, University of Texas at Austin).



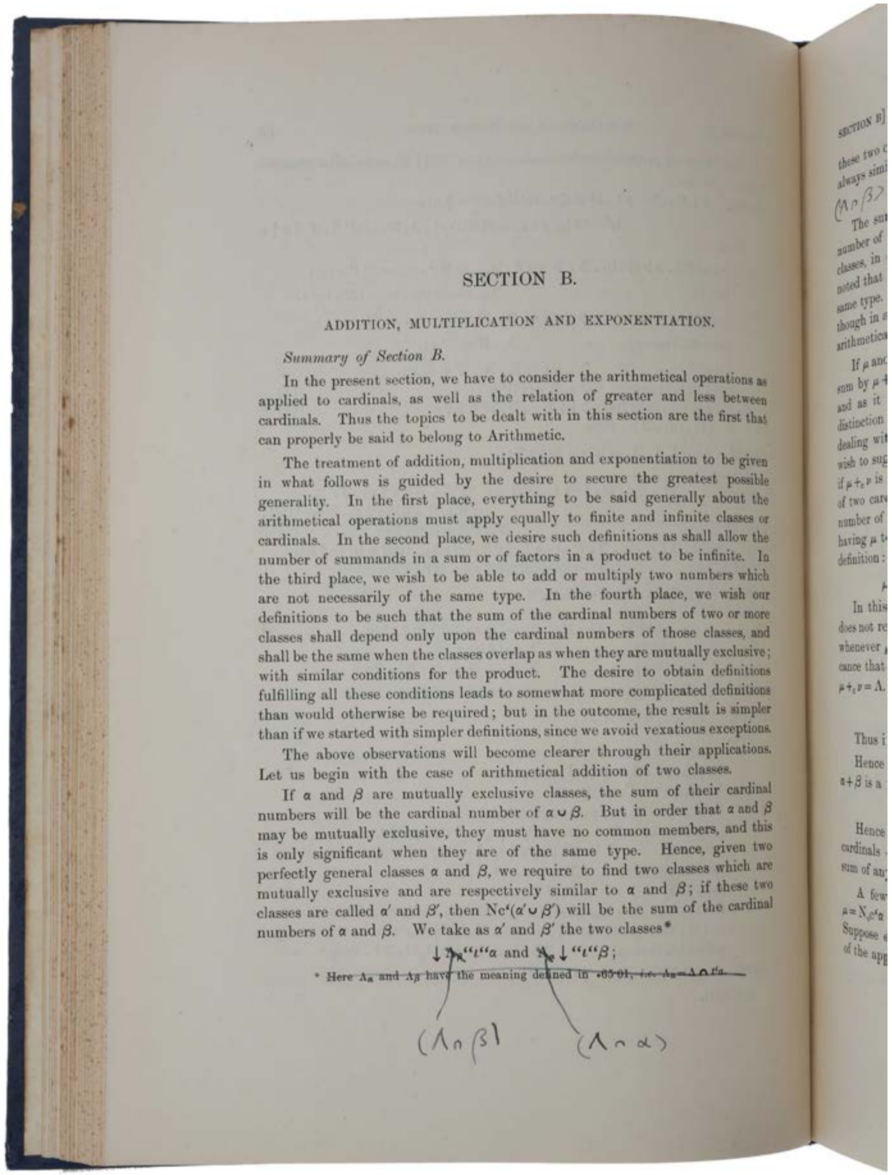
RUSSELL'S COPIES OF THE CORRECTED GALLEY PROOFS

WHITEHEAD, Alfred North & RUSSELL, Bertrand. *Principia Mathematica. Volumes I & II.* Cambridge: at the University Press, 1910-12 [but 1909 or 1910].

$150,000

Two volumes, large 8vo: I. (260 x 176mm), pp. [i-v], vi-xiii, [2, errata], 666; II. (260 x 178mm) pp. [i-v], vi-xxxiv, [1-4], 5-772. Bound without the single terminal blank leaf in vol. I and the two initial blanks in vol. II of the published version. Uniform contemporary blue-gray cloth, spines lettered and ruled in gilt.

A remarkable and important discovery, these are Russell's copies of the corrected galley proofs of the first two volumes of this monumental work. There are numerous autograph corrections in the text, almost certainly in Russell's hand, almost all of which were incorporated in the final published version, which thus differs significantly from the preliminary version represented by these volumes. Although the writing was very much a collaborative effort, correction of the proofs is known to have been carried out by Russell alone. A few of the corrections in our volumes were not incorporated into the published version of the first edition (although they were in the second edition), perhaps due to an oversight by the printer, or perhaps because Russell made these corrections after the final printing of the first edition. The bindings also differ from those of the published version: they are in differently coloured cloth, do not carry the publisher's logo, and were clearly intended as interim bindings solely for the authors. The bindings are uniform, reflecting the fact that the first two volumes of *Principia Mathematica* were printed at almost the same time, even though the second volume was



published two years after the first; the final volume was printed some three years after the first two, presumably explaining why the proofs of vol. III became separated from those of the first two volumes offered here. The evidence we have suggests that these are very probably the revised proofs known to have been given in 1914 by Russell to the Anglo-Polish logician Michael H. Dziewicki, and which then passed to his descendants (see below). Probably named after Isaac Newton's great work, "*Principia Mathematica* was Whitehead and Russell's detailed account of their 'logicist' thesis that mathematics could be derived solely from logical concepts and by logical methods…[it] has had an influence, direct and indirect, of near Newtonian proportions upon the spheres of its chief influence: mathematical logic, set theory, the foundations of mathematics, linguistic analysis and analytical philosophy" (Grattan-Guinness, p. 89). "Whether they know it or not, all modern logicians are the heirs of Whitehead and Russell" (*Palgrave*, p. 20). "After the failure of Frege's *Grundgesetze*, due to Russell's paradox, it was the *Principia Mathematica* of Whitehead and Russell which first successfully developed mathematics within a logical framework" (*ibid*., p. 21).

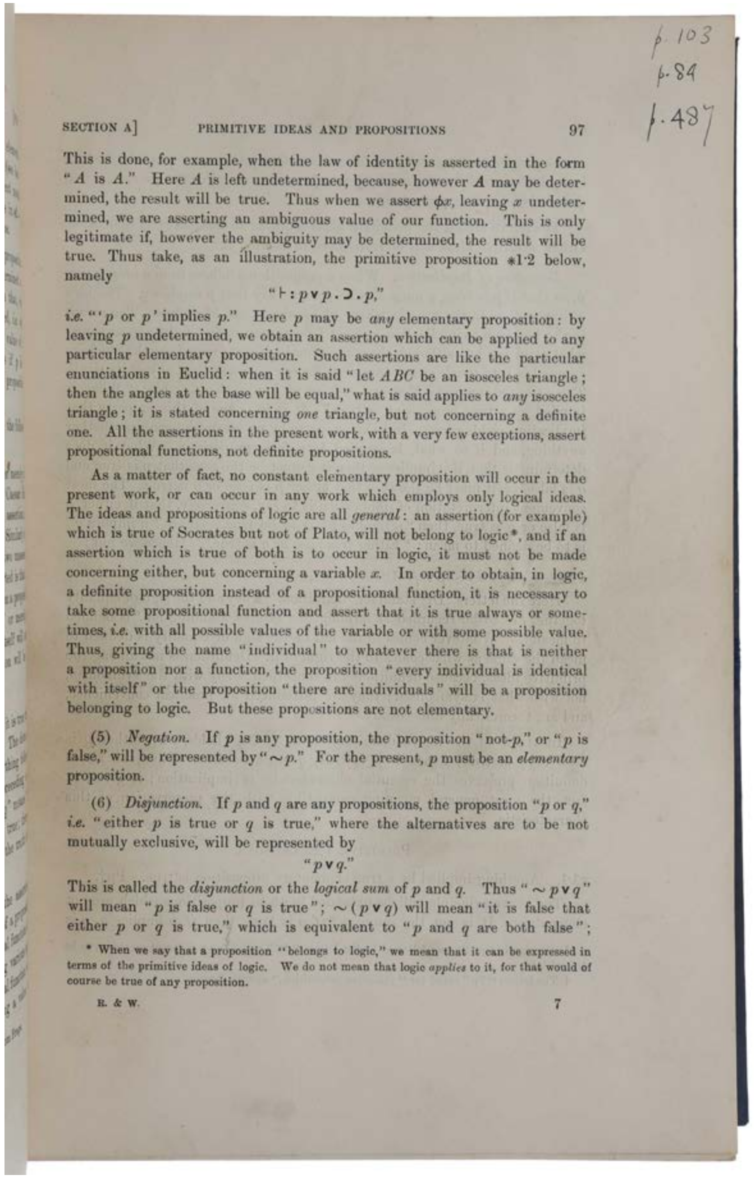
"Gottlob Frege had attempted to demonstrate logicism about arithmetic (though not geometry) in the period from 1879, when his first book, *Begriffsschrift*, was published, to 1903, when the second volume of his *Grundgesetze der Arithmetik* appeared. However, in 1902, as that second volume was in press, Russell had written to him informing him of the contradiction that he had discovered in Frege's system. Frege had attempted to respond to the contradiction – now known as Russell's paradox – in a hastily written appendix, but he soon realized that his response was inadequate and abandoned his logicist project. It was left to Russell to find a solution to the paradox and to reconstruct the logicist program accordingly. The final result was Russell's ramified theory of types and *Principia Mathematica* itself, but this theory and the logicist reconstruction in which it is embedded took a decade to develop" (*ibid.*, p. viii).

"*Principia Mathematica* had its origins in Russell's discovery of the work of Peano at the International Congress of Philosophy held in Paris in the summer of 1900, which Peano and his supporters attended in force. To that time Russell had been working for several years attempting to develop a satisfactory philosophy of mathematics. Despite some philosophical successes … a satisfactory outcome had always eluded him. At the conference, however, he very quickly realized that the Peano school had a set of techniques of which he could make use, and on his return from the conference he immediately set about applying them. As a result, he quickly rewrote *The Principles of Mathematics*, which he had started in 1899, finishing the new version by the end of the year. It was published, after some delay and substantial revisions of Part I, in 1903, billed as the first of two volumes. It was intended as a philosophical introduction to, and defence of, the logicist program that all mathematical concepts could be defined in terms of logic and that all mathematical theorems could be derived from purely logical axioms. It was to be followed by a second volume, done in Peano's notation, in which the logicist program would actually be carried out by providing the requisite definitions and proofs. At about the same time that Russell was finishing *The Principles of Mathematics*, he began the collaboration with his former teacher, Whitehead, that produced, many years later, *Principia Mathematica*.

"Whitehead in 1898 had published *A Treatise on Universal Algebra*, another first volume, in which a variety of symbolic systems were interpreted on a general, abstract conception of space. Again much detailed formal work was held over for the second volume. By September 1902 the two second volumes had merged, both authors having decided to unite in producing a joint second volume to each of their projects. This in turn grew until it constituted the three volumes of *Principia Mathematica*. The reason for the long delay in completing [*Principia Mathematica*] … was the difficulty in dealing with a paradox that Russell had discovered around May 1901 in the set-theoretic basis of the logicist system. The natural initial supposition of that system was that a class would correspond to each propositional function of the system, intuitively the class of terms which satisfied that propositional function. This being the case, there would be a class corresponding to the propositional function '*x* is not a member of itself', and this class would be a member of itself if and only if it was not a member of itself. The problem of restricting the underlying logic so that this result could not arise while leaving it strong enough to support the mathematical superstructure that Russell and Whitehead wished to build on it absorbed many years of intense labour" (*ibid*., pp. xvi-xvii).

"*Principia Mathematica* proved to be remarkably influential in at least three ways. First, it popularized modern mathematical logic to an extent undreamt of by its authors. By using a notation superior to that used by Frege, Whitehead and Russell managed to convey the remarkable expressive power of modern predicate logic in a way that previous writers had been unable to achieve. Second, by exhibiting so clearly the deductive power of the new logic, Whitehead and Russell were able to show how powerful the idea of a modern formal system could be, thus opening up new work in what soon was to be called metalogic. Third, *Principia Mathematica* re-affirmed clear and interesting connections between logicism and two of the main branches of traditional philosophy, namely metaphysics and epistemology, thereby initiating new and interesting work in both of these areas.

"As a result, not only did *Principia* introduce a wide range of philosophically rich notions (including propositional function, logical construction, and type theory), it also set the stage for the discovery of crucial metatheoretic results (including those of Kurt Gödel, Alonzo Church, Alan Turing and others). Just as importantly, it initiated a tradition of common technical work in fields as diverse as philosophy, mathematics, linguistics, economics and computer science" (*Stanford Encyclopedia of Philosophy*).



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SECTION A] PRIMITIVE IDEAS AND PROPOSITIONS 97

This is done, for example, when the law of identity is asserted in the form "*A* is *A*." Here *A* is left undetermined, because, however *A* may be determined, the result will be true. Thus when we assert ϕx, leaving x undetermined, we are asserting an ambiguous value of our function. This is only legitimate if, however the ambiguity may be determined, the result will be true. Thus take, as an illustration, the primitive proposition ∗1·2 below, namely

$$\text{“}\vdash : p \vee p \,.\supset .\, p,\text{”}$$

i.e. "'*p* or *p*' implies *p*." Here *p* may be *any* elementary proposition: by leaving *p* undetermined, we obtain an assertion which can be applied to any particular elementary proposition. Such assertions are like the particular enunciations in Euclid: when it is said "let *ABC* be an isosceles triangle; then the angles at the base will be equal," what is said applies to *any* isosceles triangle; it is stated concerning *one* triangle, but not concerning a definite one. All the assertions in the present work, with a very few exceptions, assert propositional functions, not definite propositions.

As a matter of fact, no constant elementary proposition will occur in the present work, or can occur in any work which employs only logical ideas. The ideas and propositions of logic are all *general*: an assertion (for example) which is true of Socrates but not of Plato, will not belong to logic*, and if an assertion which is true of both is to occur in logic, it must not be made concerning either, but concerning a variable *x*. In order to obtain, in logic, a definite proposition instead of a propositional function, it is necessary to take some propositional function and assert that it is true always or sometimes, *i.e.* with all possible values of the variable or with some possible value. Thus, giving the name "individual" to whatever there is that is neither a proposition nor a function, the proposition "every individual is identical with itself" or the proposition "there are individuals" will be a proposition belonging to logic. But these propositions are not elementary.

(5) *Negation*. If *p* is any proposition, the proposition "not-*p*," or "*p* is false," will be represented by "$\sim p$." For the present, *p* must be an *elementary* proposition.

(6) *Disjunction*. If *p* and *q* are any propositions, the proposition "*p* or *q*," *i.e.* "either *p* is true or *q* is true," where the alternatives are to be not mutually exclusive, will be represented by

$$\text{“}p \vee q.\text{”}$$

This is called the *disjunction* or the *logical sum* of *p* and *q*. Thus "$\sim p \vee q$" will mean "*p* is false or *q* is true"; $\sim(p \vee q)$ will mean "it is false that either *p* or *q* is true," which is equivalent to "*p* and *q* are both false";

* When we say that a proposition "belongs to logic," we mean that it can be expressed in terms of the primitive ideas of logic. We do not mean that logic *applies* to it, for that would of course be true of any proposition.

R. & W. 7

"Although they divided up the initial responsibility for the various parts of the project, they checked each other's work and in the end produced a truly collaborative text. 'Whitehead and I make alternate recensions of the various parts of our book,' Russell explained to P. E. B. Jourdain in a letter of March 1906, 'each correcting the last recension made by the other.' There were relatively few periods of extensive personal contact between them; indeed, during the period from the autumn of 1906 to the autumn of 1909, when most of *Principia Mathematica* was actually written, Whitehead lived in Cambridge and Russell in Oxford" (Grattan-Guinness, p. 90).

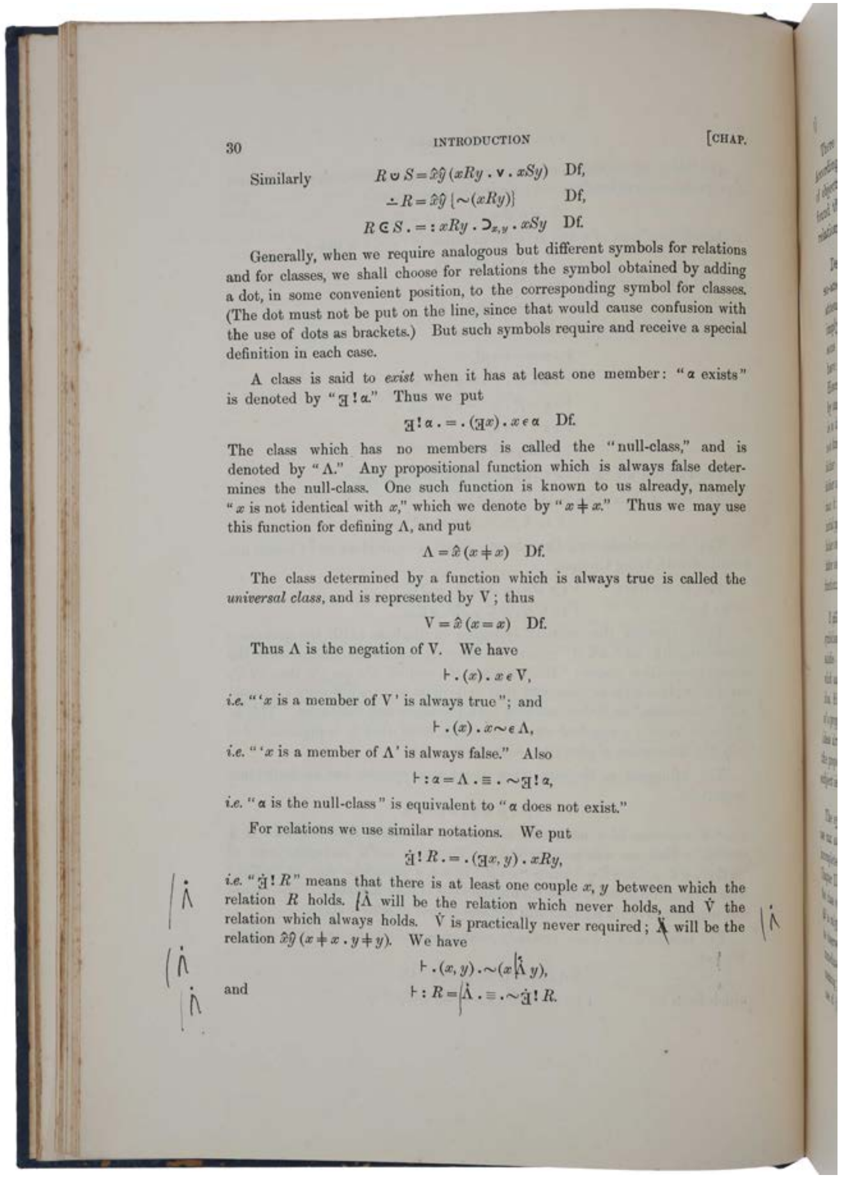
The original autograph manuscript of *Principia Mathematica*, comprising some 5000-6000 sheets, is almost entirely lost – only one sheet from vol. II and three half-sheets from vol. III are known to have survived (all in institutional collections). "Russell describes the first 4000 leaves [comprising the text of vols. I & II, and perhaps part of vol. III] as being packed into 'two large crates' when they were ready to be sent to Cambridge University Press on 18 October 1909, and he indicated that he held back a quantity that could easily be finished later. While colourful rumours have circulated as to the fate of the bulk of the manuscript, it is likely that Russell destroyed the manuscript 'copy' after each printer's sheet of proofs was proofread and returned to the printer for correction. This was evidently Russell's practice with published articles from the period … the whole manuscript was not kept intact until some dramatic incident resulted in the loss or disappearance of the whole manuscript. It seems rather to have been destroyed or the leaves reused as they were returned to Russell with the proofs … Russell told Lady Ottoline, after receiving 'a new lot of proofs from the Press': 'I enclose a page of the MS (which please burn) to amuse you. Every one of the numbers on the left is a reference, which has to be verified.' If Russell had been reaccumulating the manuscript as it was returned by the printer, he is unlikely to have told her to burn the specimen leaf. Our hypothesis is that Russell burnt the manuscript partially and serially, that is, as he was done with each portion of it. Its vast bulk was surely a consideration against reaccumulating it" (Linsky & Blackwell, pp. 145-6 & 150). "

"Yet to verify or trace the references, the authors needed an up-to-date text of *Principia*. A copy of the manuscript was not possible, and we have just seen that Russell had no interest in retrieving the specimen leaf from Ottoline. In the long periods before Volumes I and II were published, the authors, or at least Russell, probably maintained a duplicate set of revised proof sheets. [Footnote:] Such sheets may survive. Russell gave the Polish editor of Wycliffe's *Logica*, Michael H. Dziewicki, an incomplete set in 1914; the whereabouts (if any) of his papers are still unknown. This sort of gift was not unique for Russell. In 1923 he offered Hans Reichenbach his proof sheets of the second edition of Volume I down to page 304" (*ibid*., p. 150).

We acquired these volumes from a source in Australia, and it is obviously an interesting question how these important volumes arrived there. Our hypothesis is that they were taken there by Michael Henry Dziewicki's son, Joseph Henry Dziewicki (1871-1952), who was by 1925 living in Queensland. Michael H. Dziewicki (1851-1928) was born in England. He completed his initial logico-philosophical training in France. From 1896 to 1928 he taught English at the Jagiellonian University in Krakow, where he met Ludwig Wittgenstein in 1915. As a translator of Polish prose and poetry into English, he can be considered a propagator of Polish culture in English-speaking cultures. He is the author of two novels, and published several articles in the philosophical journals *Mind* and *Proceedings of the Aristotelian Society*. His most important scholarly achievement is a critical edition of the writings of John Wycliffe (1330-1384), published in several volumes from 1889 to 1909. His work on logic-related issues in the works of Wycliffe led him to enter into a correspondence with Bertrand Russell which lasted from 1912/13 to 1923.

The first volume of *Principia Mathematica* was published in December 1910 in an edition of 750 copies, priced 25 shillings; volumes II and III had a print run of only 500 copies, and were priced at 30 shillings and 21 shillings, respectively. A fourth volume, dealing with the applications to geometry, was planned and much of it written, by Whitehead alone, but never published and the manuscript was destroyed shortly after his death in 1947. The book was not a best seller. In his review of the book in the *Times Literary Supplement*, the leading Cambridge mathematician G. H. Hardy wrote: "Perhaps twenty or thirty people in England may be expected to read this book." Erwin Schrödinger went further: he said that he didn't believe that even Whitehead and Russell themselves had read all of it. Nevertheless, by the early 1920s the work was going out of print and a second edition, with three new appendices and a long new introduction, all written by Russell himself, was published in 1925-27. After 1927 Russell withdrew from logic. "After the massive achievement of *Principia Mathematica*, nothing remained but a job of cleaning up the formal foundations, a task that was essentially completed by the early 1930s, as a result of the work of Ramsey, Gödel and Tarski" (Palgrave, p. 14).

Norman 1868; Blackwell & Ruja A9.1a; Church, *Bibliography of Symbolic Logic*, 194.1-1 (one of a handful of works marked by Church as being "of special interest or importance"); Martin 101.01-03; Kneebone, *Mathematical Logic* (1963), p. 161*ff*; *Landmark Writings in Western Mathematics 1640-1940*, Chapter 61; *The Palgrave Centenary Companion to Principia Mathematica*, N. Griffin & B. Linsky (eds.), Palgrave Macmillan, 2013; I. Grattan-Guinness, 'The Royal


30 INTRODUCTION [CHAP.

Similarly $R \mathbin{\dot\cup} S = \hat{x}\hat{y}(xRy \,.\vee.\, xSy)$ Df,

$\dot{-}R = \hat{x}\hat{y}\{\sim(xRy)\}$ Df,

$R \mathbin{\dot\subset} S \,.=:\, xRy \,.\supset_{x,y}.\, xSy$ Df.

Generally, when we require analogous but different symbols for relations and for classes, we shall choose for relations the symbol obtained by adding a dot, in some convenient position, to the corresponding symbol for classes. (The dot must not be put on the line, since that would cause confusion with the use of dots as brackets.) But such symbols require and receive a special definition in each case.

A class is said to *exist* when it has at least one member: "α exists" is denoted by "$\exists!\alpha$." Thus we put

$$\exists!\alpha \,.=.\, (\exists x) \,.\, x \in \alpha \quad \text{Df.}$$

The class which has no members is called the "null-class," and is denoted by "Λ." Any propositional function which is always false determines the null-class. One such function is known to us already, namely "x is not identical with x," which we denote by "$x \neq x$." Thus we may use this function for defining Λ, and put

$$\Lambda = \hat{x}(x \neq x) \quad \text{Df.}$$

The class determined by a function which is always true is called the *universal class*, and is represented by V; thus

$$V = \hat{x}(x = x) \quad \text{Df.}$$

Thus Λ is the negation of V. We have

$$\vdash .\, (x) \,.\, x \in V,$$

i.e. "'x is a member of V' is always true"; and

$$\vdash .\, (x) \,.\, x \sim\in \Lambda,$$

i.e. "'x is a member of Λ' is always false." Also

$$\vdash : \alpha = \Lambda \,.\equiv.\, \sim\exists!\alpha,$$

i.e. "α is the null-class" is equivalent to "α does not exist."

For relations we use similar notations. We put

$$\dot{\exists}!R \,.=.\, (\exists x, y) \,.\, xRy,$$

$\dot\Lambda$ *i.e.* "$\dot{\exists}!R$" means that there is at least one couple x, y between which the relation R holds. $\dot\Lambda$ will be the relation which never holds, and $\dot{V}$ the relation which always holds. $\dot{V}$ is practically never required; $\dot\Lambda$ will be the relation $\hat{x}\hat{y}(x \neq x \,.\, y \neq y)$. We have $\dot\Lambda$

$\dot\Lambda$ $$\vdash .\, (x, y) \,.\sim(x \dot\Lambda y),$$

$\dot\Lambda$ and $$\vdash : R = \dot\Lambda \,.\equiv.\sim\dot{\exists}!R.$$

Society’s financial support of the publication of Whitehead and Russell’s *Principia Mathematica*,’ *Notes and Records of the Royal Society of London*, Vol. 30 (1975), pp. 89-104’ B. Linsky & K. Blackwell, ‘New manuscript leaves and the printing of the first edition of *Principia Mathematica*,’ *Journal of Bertrand Russell Studies* 25 (2005), pp. 141-154.

Autograph corrections (all but one in ink) in text as follows:

Volume I
1 page-refs: 15, 34, 103, 30
15 delete ‘of’ (not changed in the published version)
17 page-ref: 30
30 four lambda corrections
33 page-ref: 34
34 one lambda correction
97 page-refs: 103, 84, 487
103 ‘assumption’ -> ‘assertion’; q -> r (not changed in the published version)
241 page-refs: 241, 242; two lambda corrections
243 three lambda corrections
371 page-ref: 641
487 95 -> 94 (not changed in the published version)
497 page-ref: 503
503 88*38 -> 88*36 (not changed in the published version)
666 at bottom of page: ‘S.’ (in pencil)

Volume II
1 page-ref: 101
66 two subscripts to conjunction
67 two subscripts to conjunction
75 two subscripts to conjunction
76 two subscripts to conjunction; ‘… throughout’
101 ‘:’ -> ‘.’
616 ‘P’ - > ‘Q’ (not changed in the published version