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THE ‘PRINCIPIA’ OF ELECTRODYNAMICS

AMPÈRE, André-Marie; Jöns Jacob von BERZELIUS; Michael FARADAY; Auguste de la RIVE; Félix SAVARY. Recueil d’observations electro-dynamiques. Paris: Crochard, 1822 [i.e., 1823].

$15,000

8vo (204 x 126 mm), pp. [ii], [1-3], 4-167, [1, blank], 169-250, 252-258, [1, blank], 259-378, [1], 358-360, 383, with 10 folding engraved plates (plates 1-5 signed by Adam after Girard), one small text woodcut. Contemporary red morocco gilt by Lefebvre, flat spine richly decorated and lettered in gilt, borders of covers gilt-tooled within double rules, inner gilt dentelles, all edges gilt. A very fine copy.

A beautiful copy bound in contemporary red morocco of the definitive version of this continually evolving collection of important memoirs on electrodynamics by Ampère (1775-1836) and others. "Ampère had originally intended the collection to contain all the articles published on his theory of electrodynamics since 1820, but as he prepared copy new articles on the subject continued to appear, so that the fascicles, which apparently began publication in 1821, were in a constant state of revision, with at least five versions of the collection appearing between 1821 and 1823 under different titles" (Norman). Some of the 25 pieces in the collection are published here for the first time, others appeared earlier in journals such as Arago’s Annales de Chimie et de Physique and the Journal de Physique. But even the articles that had appeared earlier are modified for the Recueil, or have additional notes by Ampère, to reflect his progress and changes in viewpoint in the intervening period. Many of the articles that are new to the present work concern Ampère’s reaction to Faraday's first paper on electromagnetism, ‘On some new electromagnetic motions, and on the theory of magnetism’, originally published in the
21 October 1821 issue of the *Quarterly Journal of Science*, which records the first conversion of electrical into mechanical energy and contains the first enunciation of the notion of a line of force. Faraday’s work on electromagnetic rotations would lead him to become the principal opponent of Ampère’s mathematically formulated explanation of electromagnetism as a manifestation of currents of electrical fluids surrounding ‘electrodynamic’ molecules. The *Receuil* contains the first French translation of Faraday’s paper followed by extended notes by Ampère and his brilliant student Félix Savary (1797-1841). Ampère’s reaction to Faraday’s criticisms are the subject of several of the articles in the second half of the *Receuil*. The collection also includes Ampère’s important response to a letter from the Dutch physicist Albert van Beek (1787-1856), in which “Ampère argued eloquently for his model, insisting that it could be used to explain not only magnetism but also chemical combination and elective affinity. In short, it was to be considered the foundation of a new theory of matter. This was one of the reasons why Ampère’s theory of electrodynamics was not immediately and universally accepted. To accept it meant to accept as well a theory of the ultimate structure of matter itself” (DSB). The volume concludes with a résumé of a paper read by Savary to the Académie des Sciences on 3 February 1823, and a letter from Ampère to Faraday, dated 18 April 1823 (which does not appear in the Table of Contents), showing that this definitive version of the *Receuil* was in fact published in 1823. Only three other copies of this work listed by ABPC/RBH.

The collection opens with the ‘Premier Mémoire’ [1] (numbering as in the list of contents, below), first published in Arago’s *Annales* at the end of 1820. This was Ampère’s “first great memoir on electrodynamics” (DSB), representing his first response to the demonstration on 21 April 1820 by the Danish physicist Hans Christian Oersted (1777-1851) that electric currents create magnetic fields; this had been reported by François Arago (1786-1853) to an astonished Académie des Sciences on 4 September. In this memoir Ampère “demonstrated for the first time that two parallel conductors, carrying currents traveling in the same direction, attract each other; conversely, if the currents are traveling in opposite directions, they repel each other” (Sparrow, Milestones, p. 33).

The first quantitative expression for the force between current carrying conductors appeared in Ampère’s less well-known ‘Note sur les expériences électromagnétiques’ [2], which originally appeared in the *Annales des Mines*. Ampère stated, without proof, that, if two infinitely small portions of electric current A and B, with intensities *g* and *h*, separated by a distance *r*, set at angles α and β to AB and in directions which created with AB two planes at an angle γ with each other, the action they exert on each other is

\[ gh \left( \sin \alpha \sin \beta \sin \gamma + k \cos \alpha \cos \beta \right)/r^2, \]

where *k* is an unknown constant which he stated could ‘conveniently’ be taken to be zero. This last assumption was an error which significantly retarded his progress in the next two years before he stated correctly that *k* = − 1/2 in his article [13], published for the first time in the *Receuil*. This article comprised ‘notes’ on a lecture [12] delivered to the *Institut* in April 1822 in which he
surveyed experimental work carried out by himself and others since 1821 (he also published for the first time there the words 'electro-static' and 'electro-dynamic'). The full theoretical and experimental proof of the correct value of \( k \) appeared in two articles in Arago’s *Annales* in 1822, \([19]\) and \([20]\), in an article by Savary \([22]\), and in experiments with de la Rive \([17]\) (see below).

On 20 January 1821 Ampère performed an experiment together with César-Mansuète Despretz (1798-1863) intended to support his own theory of the interaction of electric currents against a rival theory of Jean-Baptiste Biot (1774-1862) and Félix Savart (1791-1841) presented to the Académie on 30 October 1820. This was reported in article \([21]\), the first “experimentally based semi-axiomatic presentation of electrodynamics” (Hofmann, p. 316). A small cylindrical magnet was placed at the same distance from two perpendicular current carrying wires. The Biot-Savart theory predicted that the magnet would experience no net force; Ampère’s theory predicted that the magnet would experience a non-zero torque from the nearby currents. But when Ampère and Despretz performed the experiment the magnet did not move (p. 343). This defeat, together with illness and fatigue, caused Ampère to suspend his electrodynamical researches for several months. What little energy he could muster for electrodynamics was mainly devoted to correspondence.

According to Ampère, magnetic forces were the result of the motion of two electric fluids; permanent magnets contained these currents running in circles concentric to the axis of the magnet and in a plane perpendicular to this axis. By implication, the earth also contained currents which gave rise to its magnetism. It was not long, however, before Auguste Fresnel (1788-1827) pointed out to his friend Ampère that his theory had several difficulties, notably the fact that the supposed currents in magnets should have a heating effect which was not observed. Fresnel suggested that the electric currents circulated around each molecule, rather than around...
the axis of the magnet. In January 1821 Ampère publicly accepted Fresnel’s idea. Not everyone was convinced of the identity of electricity and magnetism, however. Humphry Davy (1778-1829) expressed doubts in a letter to Ampère of 20 February 1821 [7]. Ampère’s idea of magnetism created by circulating electric currents was also in direct opposition to a theory put forward by Johann Joseph von Prechtl (1778-1854), and supported by the great Swedish chemist Jöns Jacob Berzelius (1779-1848), according to which electromagnetism was ‘transverse magnetism’ – whereas Ampère eliminated magnetism and showed how all the phenomena could be accounted for by the action of two electric fluids, Prechtl and Berzelius reduced electromagnetism to magnetic action. Berzelius expressed this view in his letter [3]; Ampère responded in a letter to Arago [4].

In April 1821 Ampère wrote to Paul Erman (1764-1851), professor of physics at the University of Berlin and perpetual secretary of Berlin’s Royal Academy, in response to Erman’s Umrisse zu den physischen verhältnissen des von Herrn Professor Oersted entdeckten elektro-chemischen Magnetismus (Berlin, 1821). Ampère declared that his electric theory of magnetism was established “as solidly as a physical theory can be, since, in only admitting it at first as a hypothesis, it serves to predict and make known in advance all the magnetic phenomena formerly known, those which M. Oersted has discovered, and the new properties whose existence in voltaic conductors I have made known. When one finds such an agreement between the facts and the hypothesis from which one started, can one recognize it merely as a simple hypothesis? Is it not, on the contrary, a truth founded on incontestable proofs?” In the same letter Ampère calmly harvested Erman’s experimental discoveries as further confirmatory evidence. “The observations described in the memoir which you have been so good as to send me are all the more new proofs of it. For, if I am not mistaken, they could all be predicted according to the theory in which magnets are considered to be assemblages of what I call electric currents” (Hofmann, pp. 277-8). Erman’s experiments influenced Ampère’s investigations of induction in July 1821, in which he very nearly anticipated Faraday’s landmark discovery of electromagnetic induction a decade later (see below).

Ampère again stressed the ‘identity’ of electricity and magnetism in a lecture to the Académie on 2 April 1821 [5]. He also expressed his views on the nature of magnetism in a letter to Gaspard de la Rive (1770-1834) [8]. “Perhaps in an attempt to accommodate the positivistic inclinations of some of his Parisian colleagues, or to avoid the adoption of hypotheses, Ampère normally wrote on electricity and magnetism in a phenomenological vein, eschewing noumenal questions. But there were exceptions: [an] example occurred in a letter of 15 May 1821 to the Swiss physicist Gaspard de la Rive, which was published in the recipient’s journal Bibliotheque universelle. Adopting the two-fluid theory of electricity then prevalent in France, he spoke, rather in passing, of “the series of decompositions and of recompositions of the fluid formed by the reunion of the two electricities of which one regards electrical currents as composed” (p. 122). Thus at this time Ampère’s aetherian framework was based on electric current regarded as de- and recomposition of fluid(s), and magnetism construed in terms of these currents rotating around each magnetic molecule” (Grattan-Guinness, p. 927).

As far as Ampère was concerned, “The physical theory of electrodynamics was now complete. Given the concepts of the ether and the electromotive force of matter as Ampère had formulated them, all the observed effects could be explained; not only explained, but subjected to mathematical analysis. The combination was a potent one and the accuracy of Ampère’s calculations and the depths of his insight led many to embrace his theory. Ampère, however, was not satisfied with merely creating a model of electrodynamic action. By 1821 he was intoxicated by his vision and convinced that his electrodynamic molecules really existed. They must, then, also explain other areas of physics and chemistry.
"In his 'Answer to the Letter of M. van Beck' [i.e., van Beek] [11], published in October 1821, Ampère turned his attention once again to the problem of chemical combination … What determined whether a reaction would take place and if so, with what violence, was the electrical condition of the participating molecules. To explain the mechanism of chemical combination, Ampère had recourse to another analogy; molecules were not only like voltaic piles, but also like Leyden jars. The facts of electrochemistry proved "that the particles of substances are essentially in two opposed electrical states." In order to preserve its electrical neutrality, each molecule, therefore, decomposed the ambient ether to attract the electricity of the opposite sign. Ampère did not say if this was why each molecule was surrounded by electric currents but his use of the Leyden jar analogy would appear to rule out this possibility. The molecule, presumably, had both an inherent electrical charge and electric currents associated with it. It was the inherent static charge that caused chemical combination; the resultant combination of the two electricities gave rise to heat and light and both the material and energy relations of reactions could be understood in terms of the same mechanism … There can be no doubt that he took his own theory seriously as a general theory of matter. Nor was he alone in this. During the 1820's Becquerel in Paris and Auguste de la Rive (1801-73) in Geneva used the electrodynamic model in their researches in electrochemistry" (Williams, pp. 150-1).

Late in 1821, however, Ampère's satisfaction with his theory of magnetism was seriously challenged by Faraday's discovery of electromagnetic rotation, a development which thrust Faraday immediately into the first rank of European scientists. "In the autumn he had to face a powerful criticism from Faraday, whose paper 'On some new electro-magnetical motions' came out in a French translation [9] in Arago's Annales, soon after its appearance in a London journal. A seminal paper in Faraday's contributions to the topic, it announced that continuous
rotation could occur if a pivoted cylindrical magnet moved around a fixed wire, and also if a pivoted wire moved round a fixed magnet. In October he sent to Ampère and [Jean-Nicolas-Pierre] Hachette (1769-1834) one of his pieces of apparatus, and Ampère demonstrated its working to the Académie in November.

“From the theoretical point of view, the chief challenge to Ampère’s view was Faraday’s conviction that such motions could not be explained by theories based on inter-molecular forces. Faraday’s alternative, drawn from this and other experiments, was to give preference to curved ‘lines of force’; but Ampère was anxious to preserve his own approach. Accordingly, when the translation was prepared, he had a set of appendicial notes [10] made by a new helper, Félix Savary, polytechnicien of the promotion of 1815 and thus one of Ampère’s old students, and in 1821 principally a geographer by profession. Ampère added his name to these notes to indicate his agreement with them. In his second note Savary rejected Faraday’s implicit claim in the paper that the rotatory motion could be taken as a ‘primitive fact’ in electromagnetic phenomena, and in the next note he showed how that motion could be explained in Ampère’s terms” (Grattan-Guinness, p. 928).

“In his original article describing the discovery of a continuous rotation of one extremity of a current-carrying wire around a magnet, as well as the rotation of one extremity of a magnet around a current-carrying wire, Faraday stated the following: “Having succeeded thus far, I endeavoured to make a wire and a magnet revolve on their own axis by preventing the rotation in a circle round them, but have not been able to get the slightest indications that such can be the case; nor does it, on consideration, appear probable.” Ampère, on the other hand, considered that this new kind of motion might be produced in the laboratory. He was also the first to obtain it experimentally. He communicated his discovery to the Academy of Sciences of Paris in 7 January 1822 [14]. In order to obtain continuous rotation of a magnet around its axis, Ampère initially floated it in mercury by the help of a counterweight in its lower extremity. By closing the circuit, a constant current flowed vertically downwards through the upper extremity of the magnet, leaving laterally along its lower portion and going through the mercury. When this constant current was flowing through the magnet, it rotated around its axis relative to the ground” (Assis & Chaib, p. 123). Ampère wrote to Faraday in April 1823 describing these electromagnetic rotation experiments [24].

In the letter to van Beek [11] described earlier, Ampère described an experiment, suggested by Fresnel, to decide whether in a ring of copper macroscopic currents would be induced by a nearby coil or magnet. A first trial in July 1821 produced a negative result which fitted well into Ampère’s theory of molecular currents. When he repeated the experiment with a more powerful magnet in August 1822, however, he indeed obtained an effect, and realized that this was the induction of currents by magnets. But as a consequence of his struggle with Faraday’s rotations, he concentrated on his magnetic theory. Although the positive result of the induction experiment again opened the way for both interpretations of magnetization, it did not provide any positive hint concerning which of them should be preferred. Thus Ampère declared only that the result did not refer to his theory, and decided not to pursue it further. A decade later, when Faraday again discovered electromagnetic induction and gained great publicity, Ampère bitterly complained about his former disregard of the result.

Between 1821 and 1822, Gaspard de la Rive, van Beek and Faraday performed some experiments showing that the poles of a cylindrical magnet are not located exactly at the extremities of the magnet, as was predicted by Ampère’s theory. These experiments forced Ampère to modify his conception of microscopic
currents. In a letter addressed to Gaspard de la Rive, dated 12 June 1822 [15], Ampère included [a figure which] presents the equilibrium configuration of the microscopic currents around the particles of the magnet, due to the interaction of all microscopic currents. That is, due to the collective interactions between the small current-carrying loops, the planes of these molecular currents should no longer remain orthogonal to its magnetic axis … This final conception of molecular currents presented by Ampère, with their planes inclined relative to the axis of an uniformly magnetized bar, is accepted in its essence up to the present time” (Assis & Chaib, p. 105).

As described earlier, Ampère had concluded in his article [13] that the constant $k$ in his law for the force between current carrying wires should be equal to $-1/2$. This implied, however, that two collinear and parallel current elements should repel one another when both currents flowed in the same direction towards the same point in space. Sceptical about this prediction, he performed with Auguste de la Rive, in September 1822, in Geneva, an experiment to test it [reported in [17], pp. 284-5] … This experiment has received several names in the literature: “Ampère’s floating wire experiment”, “Ampère’s hairpin experiment” and “Ampère’s bridge experiment.” Ampère himself gave a very clear description: “Two very interesting electro-magnetic experiments have lately been made by M. Ampère, in the laboratory of M. de la Rive at Geneva. M. Ampère had been induced, from his mathematical investigations, to expect a repulsion between two portions of an electrical current passing in the same direction, and in the same right line, or that every part of an electrical current would repel the other parts, a result which may be comprehended by conceiving an endeavour in the current to elongate itself. The experiment which M. Ampère has contrived to illustrate this action of the current consisted of dividing a dish into two parts by a division across the middle, and filling each division with mercury, a piece of wire was then bent into the form
of the letter U, but the curved part was bent to one side, so that the two limbs of the wire might lie on the mercury one on each cell, and the bent part pass over the division without touching it. The wire was covered with silk, except a small portion at each extremity, by which the communication was established with the mercury” (Assis & Chaib, p. 145). “Ampere and Auguste de La Rive reported that as soon as a current was sent through the circuit, and regardless of the direction of this current, the originally stationary floating wire was propelled across the mercury pool away from the terminals connected to the power source. Ampere immediately attributed this phenomenon to repulsive forces between collinear pairs of current elements, that is, pairs in which one member is an element of the current in the mercury flowing between the bare end of the wire and the adjacent terminal, and the other is an element of one of the linear segments of the wire. Interpreted in these terms, the experiment represented a striking confirmation of the prediction Ampère had made to the Académie three months earlier. The importance Ampère ascribed to this demonstration was promptly reflected in the way he publicized it. For example, in sharp contrast to his ambiguous and incomplete descriptions of induction, the text he composed for his verbal report to the Académie includes a thorough and accurate account of the floating-wire demonstration” (Hofmann, pp. 317-8).

In his article [22], Savary provided further support for Ampère’s conclusion that \( k = -\frac{1}{2} \) by analyzing an experiment carried out in 1820 by the chemists Joseph Louis Gay-Lussac (1778-1850) and Jean-Joseph Welter (1763-1852). “Initially they utilized an unmagnetized steel ring which did not interact with a compass needle. If this ring was broken into pieces, its pieces also had no influence upon the magnetized needle. They then coiled a toroidal helix around this ring and a constant current flowed through it. The current was then turned off and the helix was removed out of the ring. The ring did not interact with a compass needle placed nearby. However, when the ring was broken into pieces, each piece did now interact with the magnetized needle. Each piece behaved now as a small magnet. That is, each small piece of the ring was magnetically polarized with a North and a South pole, so that it became magnetized” (Assis & Chaib, p. 149). Savary showed that the results of this experiment were possible only if \( k = 1 \) or \( -\frac{1}{2} \), and as previous experiments by Ampère had shown that \( k \) could not be positive he could conclude that \( k = -\frac{1}{2} \). “Savary’s contribution was well publicized by Ampère. He wrote several complimentary reviews for influential journals and wrote to la Rive that Savary’s presentation of his work to the Académie marked “a kind of epoch in the history of dynamic electricity”” (Hofmann, p. 321).

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22. [SAVARY] Extrait fait par M. Savary du Mémoire qu’il a lu à l’Académie royale des Sciences, le 3 février 1823, pp. 345–354

23. [Observations additionelle], pp. 354–364

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The bibliographical complexity of this work is a direct result of Ampère’s *modus operandi*: “His work was marked by flashes of insight, and it often happened that he would publish a paper in a journal one week, only to find the next week that he had thought of several new ideas that he felt ought to be incorporated into the paper. Since he could not change the original, he would add the revisions to the separately published reprints of the paper and even modify the revised versions later if he felt it necessary” (Norman). Our version of the *Receuil* is more extensive than the most complete copy owned by Norman, and is probably that alluded to in the note to item 45 in the Norman catalogue: “Another, probably later version, has been noted with additional pages 361-378, plus an additional page of errata (p. 383) and ten instead of nine plates.” This copy additionally has pp. 223-236, which are missing from the Norman copy and to which the additional plate refers.

THE FOUNDATION WORK OF HYDROSTATICS


$17,500

Two works bound in one volume, 4to (189 x 137 mm). I. pp. [viii], 43, [1, blank]. II. pp. [viii], 47, [1, blank], with numerous woodcut diagrams in both works. Mid-seventeenth century red morocco, with central gilt arms of the Duke of Medina de las Torres, Felipe Ramirez de Guzmán, on both covers, roll tool borders and corner pieces enclosing the arms, surrounded by an acrostic inscription, lower cover with the emblematic device of three plants growing between reeds with a starry sky and motto 'Revolvta Foecundant' within a shield and the same acrologic inscription, spine gilt lettered and tooled, gilt edges (light damp staining at front cover, some sporadic foxing). A splendid copy.

First edition of both works, a spectacular copy in a mid-seventeenth century red morocco armorial binding from the Library of Felipe Ramirez de Guzmán (ca. 1600-1668), Duke of Medina de las Torres, Viceroy of Naples. The first work is the first complete edition of the foundation work of hydrostatics, Archimedes’ On Floating Bodies, which includes the eponymous ‘Archimedes’ principle’ of buoyancy; the second is the first published work on centres of gravity of solid bodies. "Archimedes – together with Newton and Gauss – is generally regarded as one of the greatest mathematicians the world has ever known, and if his influence
had not been overshadowed at first by Aristotle, Euclid and Plato, the progress of modern mathematics might have been much faster ... In hydrostatics [Archimedes] described the equilibrium of floating bodies and stated the famous proposition – known by his name – that, if a solid floats in a fluid, the weight of the solid is equal to that of the fluid displaced and, if a solid heavier than a fluid is weighed in it, it will be lighter than its true weight by the weight of the fluid displaced" (PMM, p. 44). For his edition of On Floating Bodies, Commandino (1509-75) used a Latin translation, from a now lost Greek text, by Flemish Dominican William of Moerbeke (1215-86) in 1269 (Moerbeke's holograph remains intact in the Vatican library, Codex Ottobonianus Latinus 1850); for this work he had no access to a Greek text, unlike the five other Archimedean works he had previously translated. But the Greek text used by Moerbeke was corrupt and lacked the proofs of two crucial propositions. In addition, Archimedes used certain results on the centres of gravity of solid bodies, but his work on this subject has not survived. Commandino not only cleaned up the corrupted text, he also supplied the missing proofs and further took it upon himself to prove the necessary results about centres of gravity in the form of a self-contained treatise, De Centro Gravitatis Solidorum. Commandino uses the Archimedean methods of exhaustion and reductio ad absurdum, and De Centro Gravitatis Solidorum may justly be regarded as a reconstruction of Archimedes' lost work on centres of gravity. Their very close relationship makes it particularly appropriate to find these two works bound together, as here. A second translation of On Floating Bodies, published by Curtius Trioianus from the legacy of Niccoló Tartaglia (1499-1557), appeared in the same year (the brief Book I had been published in 1543), but according to Rose (p. 153) this is a direct transcript of a copy of the Moerbeke translation, retaining all the errors and making no attempt to fill in the lacunae. Commandino's "masterful version ... was far more influential than the version of On Floating Bodies ... published under Tartaglia's direction" (Clagett). "In the sixteenth century, Western mathematics emerged swiftly from a millennial decline. This rapid ascent was assisted by Apollonius, Archimedes, Aristarchus, Euclid, Eutocius, Hero, Pappus, Ptolemy, and Serenus – as published by Commandino" (DSB).

Born in Urbino, Commandino studied Latin and Greek at Fano, then returned to Urbino where he studied mathematics. Later he studied medicine at Padua, and after returning home again he became personal physician to the Duke of Urbino. There he met Cardinal Ranuccio Farnese, the brother of the Duke's wife, who was to become his most important patron. In the early 1550s the Cardinal persuaded Commandino to move to Rome as his personal physician; while there he became friendly with Cardinal Cervini, who was elected Pope Marcello II in 1555. But following Cervini's death shortly after his election, both Commandino and Farnese returned to Urbino, where Commandino continued in the service of the Duke and Cardinal. But Commandino's true love was mathematics, and in 1558 he published his edition of Archimedes' Opera, which he dedicated to Farnese (this did not contain any of Archimedes' works on mechanics). Also in 1558 Commandino published a work he had begun in Rome, namely Commentarius in Planisphaerium Ptolemaei, in which he gave an account of Ptolemy's stereographic projection of the celestial sphere. In 1562 he published his edition of Ptolemy's work on the calibration of sundials, De Analemmate.

"In July 1564 Ranuccio Farnese was appointed to the see of Bologna and by 1565 the Cardinal and Commandino were settled there. At Bologna in that year Commandino published his edition of Archimedes' On Floating Bodies together with his own De Centro Gravitatis. Since the Greek text of the Archimedean work was then unknown, Commandino availed himself of the same manuscript of the Moerbeke translation that he had used for his 1562 edition of Ptolemy's De Analemmate ... in the dedication to De Centro Gravitatis, Commandino states that Cervini, when still a Cardinal, had given Commandino the early Latin version. This immediately raises the problem of precisely which manuscript
Commandino received from Cervini. There are indeed very few codices of the Moerbeke translation … the probability seems to me to be that Cervini loaned, rather than gave, Commandino the autograph Ottob. Lat. 1850 …

“The dedication to Ranuccio Farnese explains that the delay in publishing On Floating Bodies is due to the far greater difficulty of the material and the corruption of the text. Here especially he has felt the lack of a Greek text. But even so, Commandino has seen that the earlier translator’s (Moerbeke’s) Greek text must have been corrupt and defective since two of the proofs are missing, thus disturbing the admirable sequence of mathematical argument. This problem was enhanced by Archimedes’ accepting as evident a great many proofs and facts on conics which had been discovered by earlier mathematicians. The ideas in question, however, are not so evident to moderns and so in order to render the text fully intelligible Commandino has had to resort often to the Conics of Apollonius. But many of the writings of the other pre-Archimedean mathematicians have now been lost, a matter of great regret to Commandino who confesses that he cannot admire too much the skill of the Greek mathematicians … For this reason, Commandino has now with great effort prepared the present edition of Archimedes’ book. The errors of the anonymous translator are now emended, the corrupt passages have been cleaned up, and the lacunae filled in. Commandino has also used Apollonius to illuminate many of the facts taken for granted by Archimedes” (Rose, pp. 200-201). The study of conics necessitated by Commandino’s work on On Floating Bodies led in the following year to the publication of his edition of Apollonius’ Conics, which served as the standard edition until the 18th century.

Book I of On Floating Bodies introduced the concept of fluid pressure and initiated the science of hydrostatics. It contains the result on which the rest of the work rests, Archimedes’ Law of Buoyancy (Propositions 6 & 7). As Heath
(pp. 259-261) suggests, this principle led to Archimedes’ fabled Eureka! moment, when he realised how to determine whether a certain crown supposed to have been made of gold did not in reality contain a certain proportion of silver (by weighing the crown both in air and when immersed in water, he could determine the specific gravity of the crown, which could then be compared with the known specific gravity of gold). Book I concludes (Propositions 8 & 9) with a simple, elegant geometric proof that a floating segment of a homogeneous solid sphere, the planar base of which is either completely above the fluid surface or completely below it, is in stable equilibrium when and only when its base is parallel to the surface of the fluid. The mechanical tools used were the Law of Buoyancy, the Law of the Lever (from De Aequeponderantibus), and the equilibrium condition that the centre of gravity of the floating body must lie on the same vertical line as its centre of buoyancy (the centre of gravity of its submerged portion). The proof of Proposition 8, which deals with the case in which the base is above the fluid surface, was absent from Moerbeke’s translation (and hence from Tartaglia’s edition), and was supplied by Commandino.

Book II of On Floating Bodies contains many sophisticated ideas and complex geometric constructions and is considered to be Archimedes’ most mature work, commonly described as a tour de force (Clagett, Biographical Dictionary of Mathematicians, vol. 1 (1991), p. 95). In this book Archimedes extended his stability analysis of floating bodies from segments of a sphere to segments of a paraboloid of revolution (or conoid) of various shapes and relative densities, but restricted to the case in which the base of the conoid lies either entirely above or entirely below the fluid surface (the general case can only be completed by using modern mathematical and computational techniques). Although applications are not indicated, it is surely probable that this study was motivated by the problem of the stability of ships. The crucial result is Proposition 2, which gives the condition for stability of a floating segment of a conoid. Moerbeke’s translation contains only the first three introductory paragraphs of the proof of this difficult proposition; the remainder was supplied by Commandino (Heath, pp. 264-266). The proof of Proposition 2 “was especially important and difficult for Commandino since it required fore-knowledge of the determination of the centre of gravity of a paraboloid segment. Archimedes evidently knew the method for determining this, but in none of his then extant works, nor in any other known Greek text, is this method described. In order to rediscover this particular method and to complete the proof, Commandino was therefore compelled to undertake an investigation of the theory of the centres of gravity of solids.

“Commandino’s researches on this are embodied in De Centro Gravitatis, published with the Archimedean edition at Bologna in 1565, and dedicated to Cardinal Alessandro Farnese, the brother of Ranuccio. Commandino begins the dedication by pointing out the notable lack of any classical text on the centres of gravity of solids although Archimedes has dealt with the centres of gravity of planes in his De Aequeponderantibus. Referring to Cervini’s ‘gift’ to him some years previously of On Floating Bodies, Commandino says that a reading of that text had convinced him, however, that either Archimedes or some other mathematician had written a treatise on the subject of solids. This was especially obvious in the case of a certain proposition (book II, prop. 2). An assiduous investigation of Archimedes and other writers persuaded Commandino that he might undertake some sort of treatise on the subject, if not a complete account. When working on this, a certain book of Francesco Maurolico (1494-1575), in which the author affirmed that he had already written a treatise on the centre of gravity of solids, came into the hands of Commandino. (This was probably the spherics collection of 1558 [Theodosii sphaericorum … Messina: Pietro Spira] which contains an Index Lucubrationum that includes the relevant treatise.) Hearing this, Commandino delayed his book for some time in expectation of the appearance of the work of Maurolico, whom he names honoris causa [Maurolico’s work on centres of gravity was not published
until 1685]. But after long delay Commandino has now decided to publish his own work, particularly as it complements his edition of On Floating Bodies which is now in press. Since he is the first mathematician ever to treat of the subject in print, Commandino hopes that any errors will be ascribed to his desire to benefit other students” (Rose, pp. 201-202).

The most important (and most difficult) result proved by Commandino in De Centro Gravitatis is that the centre of gravity of a segment of a conoid is situated on the axis two-thirds of the distance from the vertex to the base – this is the result that was assumed by Archimedes in his proof of Book II, Proposition 2 of On Floating Bodies. To prove it, Commandino uses the method of exhaustion: he divides the axis of the conoid into \( n \) equal segments, each of length \( h \), say, and then constructs segments of circular cylinders with axis each of these line segments, which are as large as possible subject to being contained in the conoid. This results in an inscribed solid composed to sections of circular cylinders. Using the fact that the extremities of these cylindrical segments lie on a parabola, Commandino shows that the volume of the \( k \)th segment from the vertex of the conoid is proportional to \( k \). Taking moments about the vertex he deduces that the distance of the centre of gravity of the inscribed body from the vertex is

\[
2H/3 - h/6,
\]

where \( H = nh \) is the height of the conoid. Similarly, Commandino constructs a circumscribed body consisting of segments of circular cylinders and finds that its centre of gravity is at a distance

\[
2H/3 + h/6
\]
from the vertex. We would now use a convergence argument, allowing \( n \) to tend to infinity and \( h \) to zero, to reach the desired conclusion that the centre of gravity of the conoid is at a distance \( 2H/3 \) from the vertex, but Commandino followed the method of *reductio ad absurdum* used by Archimedes: the centre of gravity of the conoid must be between those of the inscribed and circumscribed solids, so assuming that its distance from the vertex is other than \( 2H/3 \) leads to a contradiction by taking \( h \) sufficiently small (or, equivalently, \( n \) sufficiently large). We now know that Archimedes had determined the centre of gravity of a conoid using a similar technique in the *Method*, which was unknown in Commandino's time. The only extant copy is contained in the Archimedes Palimpsest, discovered by Heiberg in 1906; this also contains the only extant Greek text of *On Floating Bodies*.

Although Commandino's work on centres of gravity was motivated by his efforts to complete *On Floating Bodies*, it led to later developments in the theory of indivisibles and integral calculus by Cavalieri, Torricelli, Wallis, Leibniz, Newton and others. "The application of the method of moments to centre of gravity determinations was important for the development of the calculus in that it provided a valuable field for the deployment of infinitesimal methods through which the concept could be approached both arithmetically and geometrically. It linked volumetric and area determinations, thereby providing a basis for the geometric transformations which played such a fundamental role in integration before the development of any general concept of function. An important paper of Leibniz, the *Analysis tetragonistica ex centrobaryces*, shows clearly that these processes were not only important in anticipation of the calculus but also that they played a significant role in its actual invention" (Baron, p. 91).

Rose (p. 185) emphasizes the importance of Commandino to the mathematical renaissance of the sixteenth century: "Perhaps the clearest perception of the mathematical renaissance is to be found in the writings of the Urbino school. Not only did Commandino, Guidobaldo dal Monte (1545-1607) and Bernardino Baldi (1533-1617) pursue the revival of Greek mathematics and the restoration of mathematical certainty, but in their thought there also emerged a strong sense of the historical development of mathematics. The idea of a mathematical renaissance is especially evident in the tributes paid to the founder of the Urbino school by his two important pupils. Guidobaldo writes in 1577: ‘Yet in the midst of that darkness (though there were also some other famous names) Federico Commandino shone like the sun. He by his many learned studies not only restored the lost heritage of mathematics, but actually increased and enhanced it. For that great man was so well endowed with mathematical talent that in him there seem to have lived again Archytas, Eudoxus, Hero, Euclid, Theon, Aristarchus, Diophantus, Theodosius, Ptolemy, Apollonius, Serenus, Pappus and even Archimedes himself, for his commentaries on Archimedes smell of the mathematician’s own lamp. And lo! just as he had been suddenly thrust from the darkness and prison of the body (as we believe) into the light and liberty of mathematics, so at the most opportune time he left mathematics bereft of its fine and noble father and left us so prostrate that we scarcely seem able even by a long discourse to console ourselves for his loss.’ And Baldi: ‘Commandino with the greatest diligence and insight restored to light, to dignity and to splendour the works of nearly all the principal writers of the age in which mathematics had flourished.’"

It is highly unusual to find an important scientific book bound as elaborately as the present volume. J. Basil Oldham in *Shrewsbury School Library Bindings* (Oxford, 1943, pp. 120-121) notes the following regarding an almost identical binding on another book bound for de Guzmán: On both covers there is a "narrow border formed by a simple conventional foliage roll, with a foliage ornament in each angle; in centre, an heraldic stamp; a shield, surrounded with the following letters
in circles CGDDMAHPPMIGPCLA, and surmounted by a coronet under which is a scroll bearing the letters FEI. On the upper cover: arms: two coats impaled: Dexter (arms of Felipe Ramirez de Guzman, Duke of Medina de las Torres, Marquis of Torrel): Two caldrons checky with snakes issuing therefrom, flanked in saltire by ten ermine-tails (5 and 5), within a bordure gobony of Castile and Leon; Sinister (arms of Anna Caraffa, Duchess of Sabbioneta, Mondragone and Trajetto, Princess of Stigliano): Quarterly of six (two in chief and four in base): 1. Per fesse (a) three bars (Caraffa) and (b) a band counter-embattled between six stars (Aldobrandini); 2. a cross patty between four eagles crowned, and over all an escutcheon quarterly of three bars and a lion rampant (Gonzaga); 3. four pallets (Aragon); 4. per fesse a castle (Castile) and a lion (Leon); 5. four pallets flanked in saltire by two eagles crowned (Sicily); 6. a column ensignd by a crown (Colonna). On the lower cover: arms (unidentified): Upon a terrace in base, a plant growing between reeds or tufts of grass; in chief an arched band inscribed REVOLUTA FOECUNDANT, with, beneath it, and ranged in the same manner, three rows of stars. Ramiro de Guzmán’s arms impale those of his second wife, “Anna Caraffa, daughter of Antonio Caraffa, Duke of Mondragone, and Elena Aldobrandini. He had previously married Marie de Guzman, daughter of Gaspar de Guzman, Count of Olives, Philip IV’s minister, to whose titles, through his marriage, he succeeded on Olives’ death in 1645, for which reason he used the acrologic inscription round the shields which Olives had used as an adjunct to his armorial insignia. The letters (C and G being transposed towards the end) stand for: ‘Comitatui grandatum ducatum ducatum marchionatum marchionatum arcis hispalensis perpetuum praefecturam magnam Indiarum chancellariatum primam Guzmanorum lineam addidit.’ The letters FEI stand for: ‘Fortuna etiam invidente.’ As the owner of the book would not be likely to use the boastful inscription of his father-in-law until he had, by the latter’s death, succeeded to his titles, the book was probably not bound till after 1645, and in Spain,
not Naples, because by that time the owner had ceased to be Viceroy of Naples.” De Guzmán’s library was acquired en bloc by the English diplomat Sir William Godolphin (1635-96), who spent the years 1667-96 in Spain, serving as ambassador from 1672 to 1678.

PMM 288 - DISCOVERY OF THE MAMMALIAN OVUM


$36,000

4to, pp. [viii], 40, [2, corrigenda], coloured engraved plate. Original grey boards with ornamental border pasted to the front cover (spine and corners worn, previous owner’s signature on front board), cloth folding case.

First edition, rare, especially in original boards as here, of von Baer’s landmark paper, in which he announced the discovery of the mammalian ovum. The idea that all animals begin as eggs had been current at least since the seventeenth century, when William Harvey, in his *De Generatione Animalium* (1651), defended it against the false notions of spontaneous generation and the “preformation” of the foetus. Harvey’s theory was strengthened in 1672, when Reinier de Graaf published his observations of the Graafian vesicle (which contains the ovum) and the process of ovulation; and in 1825, when Johann Evangelista Purkinje announced his discovery of the germinal vesicle in the embryo. However, the mammalian ovum itself remained unobserved until von Baer, in his experiments with dogs and other mammals, “plot[ted] the course of ovulation and fertilization from its later stages back to the ovary and there … identif[ied] the minute cell which was the ovum” (PMM). In von Baer’s own words, “when I observed the ovary . . . I discovered a small yellow spot in a little sac, then I saw these same spots in several others, and indeed in most of them—always in just one little spot. How strange,
I thought, what could it be? I opened one of these little sacs, lifting it carefully with a knife onto a watchglass filled with water, and put it under the microscope. I shrank back as if struck by lightning, for I clearly saw a minuscule and well developed yellow sphere of yolk” (quoted in Baer, ‘On the Genesis of the Ovum of Mammals and Man,’ tr. O’Malley, *Isis* 47 (1956), p. 120). Von Baer concluded that every sexually reproducing animal – including man – develops originally from an egg cell, “a unifying doctrine whose importance cannot be overemphasized” (DSB). For this concept and for his further researches in embryology, contained in his monumental *Entwickelungsgeschichte der Thiere* (1828--1837), Garrison and Morton have named von Baer “the father of modern embryology.” ABPC/ RBH list only two copies in original boards in the last 35 years (Sotheby’s, June 8, 2011, lot 65, £15,000 (rebacked); Sotheby’s NY, November 16, 2001, lot 7, $30,650 (Friedman copy)).

“Earlier researchers had used microscopes to look at eggs and to try to explain early development. Mid-17th century scientists, such as Marcello Malpighi and Nicolas Steno, both in Italy, claimed that living beings developed from a corpuscular element called the ovum, which in Latin means egg, as its function corresponded to the birds’ eggs … In 1651, the physician William Harvey had employed the Latin word ovum to refer to the beginning of animal life in his *Exercitationes de generatione animalium* (Exercises on Animal Generation). Harvey provided no evidence for such a claim. From the 17th to the 19th century, other scholars in Europe, such as Regner de Graaf, William Cruikshank, Jean-Luis Prévost, and Jean-Baptiste André Dumas had observed the ovum in mammals. However, their contributions were imprecise about the place in which the ovum was likely generated …

“The pamphlet has an introduction and six chapters. In the introduction, von Baer first praises the Imperial Academy of Sciences in Saint Petersburg, Russia, and the works of its scholars. Second, he outlines the conceptual background of his discovery, in particular the debate about the relationship between Graafian follicles and the ovum. Scholars involved in that debate either argued that a Graafian follicle was actually the ovum, as had de Graaf in 1672, or that it did not correspond to the ovum, as had Cruikshank argued in 1797 and Prévost and Dumas in 1824. Third, von Baer states that the goal of his research is to resolve that debate by assessing the relationship between a Graafian follicle and the ovum. After introducing the historical context, von Baer writes that the main organism he used for his research was the dog.

“In Chapters One and Two, von Baer describes the first stages of development in the dog embryo, and he names its different parts. He writes about the first stages of development, and he describes the embryo’s shape, color, and position of the anatomical structures. Additionally, von Baer notes that in more advanced stages of development the ovum lies in the uterus, while in less advanced stages it lies in the oviducts. His observations, and the similarity of the ovum between the early and later stages, enabled von Baer to infer that the ovum passes through the oviducts before reaching the uterus.

“In Chapter Three, von Baer writes about the dog’s ovum as he found it in the ovary. Von Baer claims that the ovum is not exactly the same as the Graafian follicle, as some scholars had thought, and that it lies inside the follicle. In Chapter Four, von Baer describes the formation of the Graafian follicle by comparing how that phenomenon occurs in different mammals. Such a comparison demonstrated that in all of those mammals the ovum is formed in the same way. In Chapter Five and Six, von Baer describes the development of mammals in general, and he summarizes the course of the ovum from the ovary to the oviducts to the uterus. Additionally, he compares the ovum of mammals with the ovum of other animals, such as birds. Von Baer concludes that all animals develop from an ovum. Von
Baer’s statement that reproduction begins with a corpuscular element rather than with liquid matter influenced debates concerning generation, because it disproved a claim of Albrecht von Haller’s, who worked in Switzerland, that development starts from fluids …

“Although von Baer was skeptical of common ancestry and natural selection, Charles Darwin’s portrayal of development in The Origin of Species was the same as von Baer’s: branching and epigenetic. Darwin also provided the same critiques of recapitulation as had von Baer; Darwin said that adult forms of one animal do not show themselves in other animal’s development, and that only the embryos look similar to one another. Darwin also wrote that embryology provided the strongest class of facts in support of his theory of evolution …

“Karl Ernst von Baer was born on 28 February 1792 in Piep, Estonia, to first cousins Juliane Louise von Baer and Magnus Johann von Baer. As one of ten children, von Baer spent his childhood in Coburg with his father’s brother Karl and his wife, Baroness Ernestine von Canne. Although his uncle and father encouraged military life, von Baer chose to attend the University of Dorpat, where he began medical studies in August 1810. At Dorpat, von Baer studied botany, physics, and physiology, and was influenced by professor of physiology Karl Friedrich Burdach. After completing his MD degree in September 1814, von Baer traveled to Berlin and Vienna to continue his education. In 1815 he proceeded to Würzburg to further his medical studies and there he met physiologist and anatomist Ignaz Döllinger as a result of his interest in botany. From 1815-1816 von Baer studied comparative anatomy with Döllinger, who encouraged him to research the development of the chick. However, von Baer was unwilling or unable to spend the time and money necessary to pursue this area of study and instead returned to Berlin during the winter of 1816-1817 to train in practical anatomy.
"In August 1817 von Baer became a prosector in anatomy in Königsberg at the invitation of Karl Friedrich Burdach. In 1819 he became Extraordinary Professor of Anatomy and in 1826 Ordinary Professor of Zoology. During his time in Königsberg, von Baer taught zoology, anatomy, and anthropology, founded a zoological museum, acted as director of the botanical gardens, and served as dean of the medical faculty and as rector of the university.

"Most of von Baer's contributions to embryology were from 1819-1834 while at Königsberg. During this time, he returned to the study of embryology and made considerable advances in the understanding of extraembryonic membrane development and function in the chick and in mammals. In this work he built on the results of research he had carried out collaboratively in Würzburg with Christian Heinrich Pander, as well as on Pander's own work on chick embryology. Karl Ernst von Baer also introduced the term "spermatozoa", for what had previously been referred to as "animalcules" in the seminal fluid, which he believed to be parasites. In 1826 von Baer discovered mammalian eggs in the ovary of Burdach's dog, completing a search that began centuries before … He encapsulated his thinking into four statements that are now known as 'von Baer's Laws' [described in Entwickelungsgeschichte der Thiere]. The first law says that the general features of a large group of animals appear earlier in the embryo than the special features. The second law says that less general characters are developed from the most general, and so forth, until finally the most specialized appear. The third law is that instead of passing through the stages of other animals, each embryo of a given species departs more and more from them. Finally, the fourth law concludes from the previous three that the embryo of a higher animal is never like the adult of a lower animal, but only like its embryo.

"After the death of his brother Louis, von Baer returned with his family to St. Petersburg to retain the family estate. He then entered the Academy of Sciences in St. Petersburg as a Full Member in Zoology in December 1834 after refusing previous offers while in Königsberg. After working at the academy as a librarian, academician, and professor of anatomy and physiology, von Baer retired from active membership in 1862 but continued to work as an honorary member until 1867. After returning to Dorpat, von Baer died on 28 November 1876" (The Embryo Project Encyclopedia, embryo.asu.edu).

In 1828, von Baer provided a commentary to his work, titled Commentar zu der Schrift: De Ovi Mammalium et Hominis Genesi (Commentary on the Work: De Ovi Mammalium et Hominis Genesi). The first translation of De ovi mammalium et hominis genesi appeared in French in 1829 as a book edited by Gilbert Breschet. Benno Ottow translated von Baer's article into German in 1927. Charles Donald O'Malley translated von Baer's article into English and published it in 1956.

Dibner 196; Garrison-Morton 477; Horblit, One Hundred Books Famous in Science 9b; Lilly Library Notable Medical Books 181; PMM 288a.
THE PRINCIPLE OF CONSERVATION OF ENERGY

BERNOULLI, Johann. Autograph letter signed, with important scientific content concerning the ‘vis viva’ controversy, from Basel, dated 27 July 1728, to Gabriel Cramer, ‘Professor of Mathematics,’ presently in London.

$18,500

Two pages on a single sheet (230 x 170 mm), folded for posting, corner torn from wax seal, small tears in the vertical fold repaired with Japanese paper.

An important autograph letter from Johann Bernoulli (1667-1748), then one of the elder scientific statesmen of Europe, and still one of its greatest mathematicians, to his gifted student Gabriel Cramer (1704-52), who despite his youth had been appointed to the chair of mathematics at Geneva the previous year, but was now traveling through Europe and England making the acquaintance of the leading mathematicians of the day. The letter concerns the problem of vis viva (‘forces vives’, ‘living force’), one of the most controversial topics of the day. This was the question of whether it is (to use modern terminology) momentum (‘quantity of motion’, mass x velocity) or kinetic energy (‘living force’, mass x velocity^2) which is the true measure of the ‘force’ between colliding bodies in motion. As with so many other issues, this controversy pitted the supporters of Leibniz against those of Newton. Bernoulli had recently published a major contribution to the dispute, Discours sur les Loix de la Communication du Mouvement (1727), supporting the Leibnizian position, in which he presented an analysis of vis viva in terms of balls moved by releasing compressed springs. This was attacked by the young English Newtonian Benjamin Robins (1707-51) in May 1728 in an article in The Present
State of the Republic of Letters, in which he gave a detailed discussion of the impact of elastic bodies. This article won Robins many admirers in England. In the present letter, Bernoulli refutes Robins’ article, and writes that an experiment proposed by another English Newtonian, James Jurin (1684-1750), and carried out by the London instrument maker George Graham (1673-1751), involving dropping a lead weight onto an elastic plate, ‘prouve rien contre la théorie des forces vives’. Bernoulli also responds to a misunderstanding by Cramer of a point in his Discours which Cramer had raised in an earlier letter. In a postscript, Bernoulli conveys the compliments of his nephew, the mathematician Nicolas Bernoulli (1687-1759). Letters by Bernoulli are rare on the market, particularly those with significant scientific content.

Translation:

Monsieur Robert Caille

Marchand Banquier, pour faire tenir à Monsieur Cramer, Professeur en mathematique present à Londres

Monsieur,

Ce mot de lettre nest que pour vous don[n]er avis que je vous ecrivis jeudi passé une reponse à la votre du 22. Juin, que jai addressée à Mr. de Mairan à Paris. Elle contient quelques reflexions generales sur la piece de Mr. Robins, et une reponse à l’objection tirée de l’experience avec la plaque de cuivre par laquelle étant en oscillation on laisse tomber un poids de plomb. J’ai fait voir que cette experience ne prouve rien contre la theorie des forces vives, et qu’elle est semblable à celle qu’on ferait avec deux corps sans ressort dont l’un en mouvement choquerait directement l’autre en repos, auquel si le choquant étoit egal, ne lui com[m]uniqueroit que la moitié de la vitesse et iroit avec lui après le choc de compagne, en sorte que la moitié de la force vive paroirà être perdue. Je vous avois aussi ecrit une tres grande lettre datée du 23. Mai en reponse à vos deux precedentes du 10. Mars et 15 Avril, mais de laquelle vous ne faites pas mention dans votre derniere du 22 Juin, auquel temps vous prairés [pourriez] déjà avoir reçu la mienne; ce silence me mettant en peine, je vous prie de m’en tirer au plutot pour savoir si en fin elle vous a été rendue : vous y aurés trouvé bien des choses pour la confirmation de la theorie des forces vives et une ample solution à votre difficulté, qui consistoit à me demander, d’ou vient que c’est l’increment de la vitesse, et non pas celui de la force vive, qui dans un temps infiniment petit est proportionel à ce temps et à la pression: c’ est-à-dire, pourquoi il faut faire du = pdx/u, et non pas df = pdx/u ?

Je finis en vous temoignant que je suis toujours avec la plus parfait consideration

Monsieur votre tres humble et tres obeissant serviteur

J Bernoulli

Bale, ce 27. Juillet 1728

S. Mon neveu vous fait ses compliments ; il y a quelques semaines qu’il vous a ecrit une lettre sous l’adresse de Mr. Caille : dont je me serais aussi toujours en vous ecrivant.

Translation:

Mr Robert Caille

Merchant Banker, for the attention of Mr. Cramer, Professor in mathematics present in London
Sir,

This brief letter is only meant to give notice that I wrote you last Thursday a reply to your letter of 22 June which I addressed to Monsieur de Mairan in Paris. It contains some general reflections on Mr. Robins’ essay, and a response to the objection derived from the experiment with an oscillating copper plate on which a lead weight is dropped. I have shown that this experiment proves nothing against the theory of the live force, and that it is similar to that which would be made with two inelastic bodies, one of which in motion would directly collide with the other at rest. If the colliding body was equal to the one at rest, it would convey to it only half the speed and go with it after the collision, so that half the force will appear to be lost. I also wrote you a very long letter dated 23 May in reply to your two previous ones of 10 March and 15 April, which you do not mention in your last letter of 22 June, though at that time you may have already received mine; this silence is distressing and I beg you to put an end to it and let me know if eventually it was delivered to you; you will have found [in it] many things confirming the theory of live forces and an ample solution to your difficulty, which consisted in asking me, how come that it is the increment of speed, and not that of the live force, which in an infinitely small time interval is proportional to this interval and to pressure; that is to say, why should we do $du = pdx/u$, not $df = pdx/u$?

I am ending this letter with the renewed assurance that I remain, with the most perfect consideration

Your very humble and very obedient servant

J Bernoulli

Basel, 27 July 1728

P.S. My nephew sends you his compliments; a few weeks ago, he wrote you a letter, care of Mr. Caille, whose address I also always use when writing to you.

“The vis viva problem, first formulated by Leibniz in the 1680s, centred on how to define quantity of motion and, to some extent, how to define force. Descartes had argued that the quantity of motion (or force) in the universe must remain constant, and that its measurement was the product of the quantity of matter and velocity ($mv$). Leibniz disagreed with Descartes’ assumption and argued that $mv^2$ (what he called vis viva) defined the quantity of motion. (For clarity today, we would characterize the controversy as a confusion between conservation of momentum and conservation of kinetic energy, but in the mid-eighteenth century there was no concept of energy as we now understand it, nor was there agreement about what constituted force).

“In 1717, the correspondence between Leibniz and Samuel Clarke was published, which revealed that the Newtonians were the allies of the Cartesians on this particular point … The importance of the vis viva controversy was further revealed by the fact that in 1724 the Paris Académie des Sciences set the communication of motion as the subject of their prize competition. Although he did not win, Jean Bernoulli contributed a widely acclaimed essay supporting Leibniz [Discours sur les Loix de la Communication du Mouvement]” (Correspondence of James Jurin 1684-1750, p. 40).

“In his prize essay, Bernoulli started by denying the very existence of hard bodies ‘in the vulgar sense’, and went on to model the force of collision by analogy with the compression and release of springs. He predicated his analysis on the fundamental elasticity of matter, following Leibniz. Hardness, or ‘rigidity’, was equivalent to perfect elasticity, said Bernoulli. By looking at the motion imparted by springs to rigid bodies, he was able to show that the force of the spring was
proportional to the square of the velocity it gave to the body. He presented his ideal springs as a thought experiment, to give a measure of concreteness to the abstract consideration of force and motion. Bernoulli framed his systematic and comprehensive analysis of collisions as a demonstration of the true measure of living force, or the force of motion – something that he claimed Leibniz had only proved indirectly. His demonstration used the integral calculus to obtain the equation \( \frac{1}{2}v^2 = \int p \, dx \) (for unit mass) [where \( p \) is the pressure of the spring]. From what he called ‘the familiar law of acceleration’, according to which pressure equals \( mdv/dt \), he deduced that the *vis viva* produced or destroyed by the action of a spring (or by the elastic surface of a body) is proportional to the distance through which the spring extends, or is compressed” (Terrall, pp. 192-3).

In May 1798 the English mathematician and military engineer Benjamin Robins published a point-by-point refutation of Bernoulli’s prize essay (*The Present State of the Republick of Letters*, Vol. 1, Article XXIII, pp. 357-372), to which Bernoulli refers in the present letter. Robins concludes (p. 372): “I think, I have proved, that nothing Mr. Bernoulli has urged in defence of Mr. Leibniz’s opinion, is in any way conclusive; that many parts of his discourse are contradictory; and that all his determinations of the laws of motion are wrong, since they are by him applied to bodies, which only perfectly restore themselves; whereas they are true in none but such as restore themselves in the same time they were compressed.”

“The Swiss mathematician Gabriel Cramer wrote Jurin an extensive letter in January 1729, once again arguing the case for *vis viva*. Cramer, who resided in London in the late 1720s and became acquainted with Jurin at that time, had visited s’Gravesande in Leyden on his return to Geneva and had corresponded with Bernoulli. Cramer became convinced of the correctness of *vis viva*” (*Correspondence of James Jurin 1684–1750*, pp. 40-41).

In his January 1729 letter Cramer gives further detail on the topics touched upon in the offered letter, in particular the experiment in which a heavy weight is dropped on to an elastic plate (though the plate is made of leather rather than copper). He writes: “I received two or three Letters from Mr Bernoulli about the remarks of Mr Robins upon his Discourse, and about the Experiment made under your direction by Mr Graham against his theory. About this, he says it appears plainly that Mr Robins has mistaken his sense in many places, whether on purpose or no, he does not care to determine; that he lends him ridiculous opinions which he never had; and refutes absurdities which Mr Bernoulli never wrote … About the Experiment he writes me so: ‘In the experiment made with a sheet of leather, on which a smooth lead weight is gently dropped when it is at the base of its oscillation: I will tell you, Sir, that earlier I had a similar experiment in mind; Mr Euler, presently at Petersburg, could testify to this, but before I could carry out the experiment, I soon recognized that it was not at all suitable to decide the controversy; thus I neglected it in the hope that it was bound to show what you say it has. I am surprised that this affair has troubled you so much that you have not found a suitable response in this area. The objection is similar to that made for two bodies without elasticity. It is clear that in these cases half of the *vis viva* is consumed during the compression without being restored, because the *vis viva* is communicated either to the surrounding matter, or to the small internal particles of the two bodies, in order to make them vibrate. When the lead weight suddenly joins the sheet of leather and the two bodies are no longer separated, they are forced to move together; thus they present a case similar to the two non-elastic bodies A and B, combined by impact into one body, which has no other difference than this: that the body A united to the body B pushes it before itself, here the sheet of leather having received the weight carries it with itself. But perhaps you will tell me that between the sheet & the lead there is no conceivable compression. But I respond that the fibers of the 2 surfaces interweave, that the fibers of leather in between the two can contract and expand a little so that there
is no space created by the compression of the bodies A & B; from which it follows that half of the vis viva is used in expanding these fibers & thus causing a vibration in the small particles of leather & lead, & the other half is conserved in the shared movement of the total mass (ibid., pp. 376-7).

In modern terms, we would say that when a body of mass \( m \) travelling with velocity \( v \) impacts inelastically with a second body of the same mass initially at rest, the total momentum \( mv \) is conserved; since the total mass of the two bodies after the collision is \( 2m \), the velocity of the combined body must be \( v/2 \). The total kinetic energy before the collision is \( \frac{1}{2}mv^2 \); after the collision it is \( \frac{1}{2}(2m)(v/2)^2 = \frac{1}{4}mv^2 \). The energy lost, we would say, is transformed into sound or heat in the collision, essentially in agreement with Bernoulli who says that it resides in the vibrations of the small particles of the bodies and the surrounding matter.

Gabriel Cramer was appointed to the chair of mathematics at the Académie de Calvin in Geneva, jointly with Giovanni Ludovico Calandrini, in 1727. “This appointment provided that the men share both the position’s duties and its salary. It was also provided that they might take turns traveling for two or three years 'to perfect their knowledge,' provided the one who remained in Geneva performed all the duties and received all the pay” (DSB). “In May 1727 Cramer was given his first opportunity to travel, visiting Basel, Leiden, London and Paris. He lost no time in establishing his first professional scientific connection with a group of Leibnizian mathematicians in Basel, known as the Bernoulli circle. Cramer spent six months there studying principally with Johann (I) Bernoulli. This was a singular honor since the elder Bernoulli accepted only the most promising students. In Basel Cramer met other members of the Bernoulli circle: Leonhard Euler, and Daniel and Nicholas Bernoulli, establishing lifelong friendships with each. Johann Bernoulli and his brother Jacob (I) (1654-1705) can be regarded as the major avenue by which Leibnizian physics and mathematics were introduced into Switzerland. Evidence of the high esteem in which Cramer was held by Johann Bernoulli was his selection as editor of the two elder Bernoullis’ works. According to Jacob Vernet, the Genevan theologian who wrote Cramer’s 'Eloge historique' in the Nouvelle Bibliothèque Germanique, … Bernoulli had urged Cramer to write a work defending vis viva, but Cramer had refused saying that he did not want to begin his career with a polemical work. Vernet reported that when Cramer visited London he did, however, propose several experiments designed to throw light on the famous question” (Dawson, pp. 65-66).

BERNOUlli, Johann.
PMM 263 - THE FATHER OF AERIAL NAVIGATION


$24,000


First edition, journal issues in the original printed wrappers, extremely rare thus, of “the first and the greatest classic of aviation history, laying the foundations of the science of aerodynamics” (PMM) and setting out the correct conception of the modern aeroplane. “The true inventor of the aeroplane and one of the most powerful geniuses in the history of aviation’: these are the words used by the French historian Charles Dollfus to describe Sir George Cayley (1773-1857), a scholarly Yorkshire baronet who until recently was virtually ignored by historians of applied science. Cayley, who lived and did most of his work at Brompton Hall, near Scarborough, first had his aeronautical investigations fired by the invention of the balloon in 1783 – when he was ten – and his active concern with flying lasted until his death in 1857. In the year 1796 he made a helicopter model on the lines of that invented by Launoy and Bienvenu, a device he later improved and modified. Then, within a few years, with no previous workers to guide him or suggest the lines of
approach, he arrived at a correct conception of the modern aeroplane, and so laid
the secure foundations for all subsequent developments in aviation. It was in the
year 1799 that Cayley took his first and most decisive step towards inaugurating
the concept of the modern aeroplane: the proper separation of the system of
thrust from the system of lift. This was the crucial breakaway from the ornithopter
tradition of previous centuries: it meant picturing the bird with its wings held rigid
as if in gliding flight, and propelled by some form of auxiliary mechanism. Then,
during the most fruitful decade of his life (1799-1809), Cayley made his basic
experiments, which included testing both model and full-size gliders, and arrived
at his mature conception of aircraft and aerodynamics. It was almost an accident
that he gathered together his notes and published them. For it was in Nicholson's
Journal, for November 1809, February 1910, and March 1810, that there appeared
Cayley's triple paper 'On Aerial Navigation' (ibid.). ABPC/RBH lists only three
copies in the last half-century, none of them in original printed wrappers.

“The 2007 discovery of sketches in Cayley's school notebooks (held in the archive
of the Royal Aeronautical Society Library) revealed that even at school Cayley
was developing his ideas on the theories of flight. It has been claimed that these
images indicate that Cayley identified the principle of a lift-generating inclined
plane as early as 1792” (Wikipedia, accessed May 15, 2019).

“IT was in 1804 that Cayley began to write his famous paper on Aerial Navigation,
a work he was not able to complete for four years … One hundred and fifty
years after Cayley began his essay, von Karman wrote in his book Aerodynamics
(published this year [1954]), 'The idea that sustentation can be accomplished by
moving inclined surfaces in the flight direction, provided we have mechanical
power to compensate for the air resistance, was probably clearly defined for the
first time by an Englishman, Sir George Cayley, in his papers published in 1809-10
on aerial navigation … in his paper he clearly defined and separated the problem
of sustentation, or in modern scientific language the problem of lift, from the
problem of drag' … It was after a brief but significant discussion on the forces
on a bird in gliding flight that Cayley made the statement: ‘The whole problem
is confined within these limits—To make a surface support a given weight by
the application of power to the resistance of the air.’ This paper provided the first
clarification of ideas about mechanical flight and was the first to lay down the
main principles.

“The resistance of a plane in a moving stream of air, at various angles of incidence,
was unknown. In his paper Cayley refers to 'many carefully repeated experiments'
to obtain the pressures on a plane, but it was not until the discovery of his note-
book in 1933 that it was known how astonishing these experiments were. Cayley
records that they were made with a home-made whirling arm apparatus, to find
the pressure on a flat plate, one foot square, at angles of incidence from 3 deg to 18
deg, in 3 deg steps. He was well aware of the difficulties of obtaining exact results,
carried out further tests, using a model glider, with an adjustable tailplane
and a movable centre of gravity, to test his results.

“In this paper Cayley briefly touches upon the helicopter, the principle of which he
demonstrates with a model using two sets of contra-rotating airscrews made from
birds' feathers. 'For the mere purpose of ascent this is perhaps the best apparatus,'
he declares, 'but speed is the great object of this invention, and this requires
a different structure.' He discusses the problem of the lateral and longitudinal
stability of a fixed-wing machine and 'aided by a remarkable circumstance that
experiment alone could point out,' shows that at very acute angles of incidence
the centre of pressure moves considerably in front of the centre of gravity of a
wing. This was the first statement made of the centre-of-pressure movement.
Light construction, light engines, and minimum forward resistance were the key
features of all Cayley's ideas about heavier-than-air craft. 'In thinking of how to
construct the lightest possible wheel for aerial navigation cars; he wrote in 1808, ‘an entirely new mode of manufacturing this most useful part of locomotive machines occurred to me—vide, to do away with wooden spokes altogether, and refer the whole firmness of the wheel to the strength of the rim only, by the intervention of tight cording.’ In a later paper he pointed out that the wheel was an incumbrance during flight, a cogent reason why it should be as light as possible” (Pritchard, pp. 701-2).

“His emphasis on lightness led him to invent a new method of constructing lightweight wheels which is in common use today. For his landing wheels, he shifted the spoke’s forces from compression to tension by making them from tightly-stretched string, in effect ‘reinventing the wheel’. Wire soon replaced the string in practical applications and over time the wire wheel came into common use on bicycles, cars, aeroplanes and many other vehicles” (Wikipedia).

The first part of Cayley’s paper is devoted to issues of propulsion and aerodynamics. He noted that steam-engines would be a factor of ten too heavy to act as sources of propulsion and adds that “lightness is of so much value in this instance, that it is proper to notice the probability that exists of using the expansion of air by the sudden combustion of inflammable powders or fluids with great advantage … Probably a much cheaper engine of this sort might be produced by a gas-tight apparatus and by firing the inflammable air with a due proportion of common air under a piston.” He then gives a brief indication of what had been done, and what might be achieved, using spirit of tar or gas as the combustible fluids.

Turning to questions of aerodynamics, Cayley uses the example of bird-flight to explain the action of the lifting wing. The wing’s total resistance is taken to act perpendicularly to the wing’s surface, the triangle of forces then being employed so as to determine the wing’s lift and drag components. The lift, of course, is
always known, being equal to the weight of the bird or aeroplane. According to Cayley's assumption concerning resistance direction, the wing's drag force is also known, being related to lift by the tangent of the wing's incidence angle. However, there is a further “direct resistance” due to the bulk of the bird, or to the aeroplane's remaining structure. He then turns to his belief in the superior lifting ability of the bird's cambered wing and here provides a perceptive conjecture as to its cause. He suggests that, at the leading edge, the air's upward motion over the upper surface's convexity “creates a slight vacuity” there. Meanwhile, “the current is constantly received under the anterior edge of the surface, and directed upward into the cavity … The fluid accumulated thus within the cavity has to make its escape at the posterior edge of the surface, where it is directed considerably downward; and therefore has to overcome and displace a portion of the direct current passing with its full velocity immediately below it; hence whatever elasticity this effort requires operates upon the whole concavity of the surface, excepting a small portion of the anterior edge." Here we meet for the first time some of the rudiments of our understanding that lift is created by the ability of a wing to remove leading edge upflow and then impart trailing edge downflow. The lift force is thus the consequent reaction on the wing due to its imposition of a vertically downward change of momentum to the air's motion. The subject of “direct resistance” is returned to in the closing pages of part three of the paper. He comments that “It has been found by experiment, that the shape of the hinder part of the spindle is of as much importance as that of the front, in diminishing resistance. This arises from the partial vacuity created behind the obstructing body. If there be no solid to fill up this space, a deficiency of hydrostatic pressure exists within it, and is transferred to the spindle. This is seen distinctly near the rudder of a ship in full sail, where the water is much below the level of the surrounding sea. The cause here, being more evident, and uniform in its nature, may probably be obviated with better success; in as much as this portion of the spindle may not differ essentially from the simple cone. I fear however, that the whole of this subject is of so dark a nature, as to be more usefully investigated by experiment, than by reasoning.”

In part two of the paper Cayley addresses the problems of stability and control. He begins by describing the first successful parachute descent by André Jaques Garnerin (1769-1823) in 1797. Having no vent at its apex and therefore no doubt suffering alternate spillage around its canopy edge, Garnerin's parachute produced a markedly oscillatory descent. This instability Cayley seizes on in his search for a means of providing lateral stability for aeroplanes. He believes this instability to be due merely to direct resistance differences across the canopy when tilted. Using a two-dimensional analogy, Cayley's argument is based entirely on the reduced resistance of a plate, at incidences less than normal to a stream, in comparison with the normal case. From this he argues that an inverted parachute canopy should be stable, a conclusion which he immediately applies to the aeroplane so as to suggest wing dihedral: Cayley has arrived at dihedral provision so as to enhance lateral stability.

Cayley then turns to longitudinal stability. Despite Cayley's failure to grasp the stabilising function of the tailplane at this stage, he nonetheless realises the necessity of having a movable horizontal tail surface for the purposes of retrimming for different flight speeds. “From a variety of experiments upon this subject I find, that, when the machine is going forward with a superabundant velocity, or that which would induce it to rise in its path, a very steady horizontal course is effected by a considerable depression of the rudder, which has the advantage of making use of this portion of the sail in aiding the support of the weight. When the velocity is becoming less, as in the act of alighting, then the rudder must gradually recede from this position, and even become elevated, for the purpose of preventing the machine from sinking too much in front, owing to the combined effect of the want of projectile force sufficient to sustain the centre
of gravity in its usual position, and of the centre of support approaching the centre of the sail.” A further function of the tail, as Cayley sees it, is for steering: “The powers of the machine being previously balanced, if the least pressure be exerted by the current, either upon the upper or under surface of the rudder, according to the will of the aeronaut, it will cause the machine to rise or fall in its path, so long as the projectile force is continued with sufficient energy.”

Cayley's thinking on structural design is contained entirely within the third part of the triple-paper, this being otherwise largely devoted to flapper propulsion systems. He offers the following general principles: “Diagonal bracing is the great principle for producing strength without accumulating weight; and, if performed by thin wires, looped at their ends, so as to receive several laps of cordage, produces but a trifling resistance to the air, and keeps tight in all weathers. When bracings are well applied, they make the poles, to which they are attached, bear endwise. The hollow form of the quill in birds is a very admirable structure for lightness combined with strength, where external bracings cannot be had; a tube being the best application of matter to resist as a lever; but the principle of bracing is so effectual, that, if properly applied, it will abundantly make up for the clumsiness of human invention in other respects; and should we combine both these principles, and give diagonal bracing to the tubular bamboo cane, surfaces might be constructed with a greater degree of strength and lightness, than any made use of in the wings of birds.” Cayley's suggestion of diagonal wire bracing coupled to his earlier ideas on hollow tubular members proved apt advice, as later constructors were to demonstrate.

After the publication of the triple-paper, Cayley turned to a variety of other activities. He retained his interest in aeronautics but concentrated mainly on airships and ornithopter designs. Indeed, he remained largely silent on the
aeroplane until prompted to return to it by the publication of Henson’s design for his ‘Aerial Steam Carriage’ in 1843. “Around 1843 he was the first to suggest the idea for a convertiplane, an idea which was published in a paper written that same year. At some time before 1849 he designed and built a biplane in which an unknown ten-year-old boy flew. Later, with the continued assistance of his grandson George John Cayley and his resident engineer Thomas Vick, he developed a larger scale glider (also probably fitted with ‘flappers’) which flew across Brompton Dale in front of Wydale Hall in 1853” (Wikipedia).

PMM 260 - THE METRIC SYSTEM


$20,000

*Three vols., 4to, pp. [iv] [ii] 180, 551 and 8 plates; xxiv 844 and 11 plates; [iv] 4, 704, 62 (index) and 9 plates. Contemporary boards, uncut, spines with some very well done restoration.*

First edition, uncut in contemporary boards, of the foundation work of the metric system. "For many centuries there were no general standards for measurement: every trade and craft had its own peculiar units and they differed even in various regions of the same country. Since the development of international trade in the Middle Ages this chaotic situation had become more and more tiresome, but all efforts towards standardization were strongly resisted by vested interest … We owe the introduction of an international metric system to the French Revolution. In 1790 the Académie des Sciences, at the request of Talleyrand, set up a commission to consider the question: among its members were J. C. Borda, Lagrange, Laplace, G. Monge and Condorcet. In 1791 they reported that the fundamental unit of length should be derived from a dimension of the earth: it should be the ten-millionth part of a quadrant of the earth's meridian extending between Dunkirk and Barcelona. As the distance was already approximately known, a provisional meter was at once adopted. The new unit of weight was to be the gram: the weight of one cubic centimeter of water at 4° C. The Constituent Assembly set up a general commission of weights and measures to carry these proposals into effect and in 1795 a law was passed introducing the metric system into France with provisional
standards. The astronomers Jean Baptiste Joseph Delambre and Pierre Francois André Méchain were charged with the task of measuring accurately the newly adopted length along the meridian arc between Dunkirk and Barcelona. Owing to the disturbances of the revolutionary period their work was much impeded, but in 1799 their measurement was completed. The above work, *Base du système métrique decimal*, embodies their report. The length of a meter (equaling 39.37 English inches) was marked on a platinum bar, and the unit of weight was also constructed of platinum, being the weight of a cubic decimeter, or liter, of pure water at its maximum density. These original bars remained the basic standards until 1875 and are still preserved in Paris. The metric system was gradually accepted by most nations – with the notable exceptions of England and (for weights and measures) the United States; but optional use was legalized in 1864 (England) and 1866 (U.S.A.) and its general adoption in England was proposed in 1865. After meetings of an international commission in 1872 the International Bureau of Weights and Measures was set up in 1875. It is now situated near Sèvres and has since remained the international center for all questions of standards. New units made from a bar of platinum alloyed with 10 per cent iridium were constructed, copies of which were distributed to the various participating countries” (PMM).

"Measures in the eighteenth century not only differed from nation to nation, but within nations as well. This diversity obstructed communication and commerce, and hindered the rational administration of the state. It also made it difficult for the savants to compare their results with those of their colleagues. One Englishman, traveling through France on the eve of the Revolution, found the diversity there a torment. “[I]n France,” he complained, “the infinite perplexity of the measures exceeds all comprehension. They differ not only in every province, but in every district and almost every town…” Contemporaries estimated that under the cover of some eight hundred names, the Ancien Régime of France employed a staggering 250,000 different units of weights and measures.

“In place of this Babel of measurement, the savants imagined a universal language of measures that would bring order and reason to the exchange of both goods and information. It would be a rational and coherent system that would induce its users to think about the world in a rational and coherent way. But all the savants’ grand plans would have remained fantasy had not the French Revolution -- history’s great utopian rupture -- provided them with an unexpected chance to throw off the shackles of custom and build a new world upon principled foundations. Just as the French Revolution had proclaimed universal rights for all people, the savants argued, so too should it proclaim universal measures” (Alder). "In 1788 the Académie des Sciences decided to establish a “uniform system of measures” founded on some “natural and invariable base.” The plan for the new system of measures was formally approved by a decree of the Assembly of 8 May 1790, proposed by Talleyrand; it was approved by Louis XVI on the following 22 August. A commission on the metric system, consisting of Borda, Lagrange, Laplace, Monge, and Condorcet, was thereupon appointed by the Academy. In a report submitted on 19 March 1791, the commissioners rejected two proposed bases for the fundamental unit of measure: the length of a seconds pendulum (at 45° latitude), and one-quarter of the terrestrial equator. Instead they chose one-quarter of a terrestrial meridian, the common practical unit to be a ten-millionth part of this quantity. Accordingly, it was proposed to make a careful and accurate measure along an arc of the meridian through Dunkerque (which had in part been measured by the Cassinis in 1718 and in 1740), extending as far south as Barcelona.

“Three fundamental tasks were envisaged. First, to determine the exact difference in longitude between Dunkerque and Barcelona (and to make any needed latitude determinations in between); second, to check by new observations and calculations the triangulations used earlier to find the distance between Dunkerque and Perpignan; third, to make new measurements that could serve
for successive triangulations. Clearly a major part of this assignment would be to compute carefully the difference in actual lengths (in toises) corresponding to the same difference in latitude at various points along the meridian, so as to be able to determine the actual shape of the earth. While these operations were being performed, other scientists would be engaged in establishing a standard of mass. The instruments, chiefly made by Lenoir according to the plans of Borda, were ready by June 1792, and the work was started shortly afterward.

“Originally, the geodetic survey was to be entrusted to Méchain, Cassini, and Legendre. The latter two begged off, and Delambre—just made a member of the Academy—was appointed. It was decided that Delambre would be in charge of the survey from Dunkerque to Rodez, leaving the survey from Rodez to Barcelona in the hands of Méchain. An account of the labors and adventures of Méchain and Delambre is available in their joint publication, Basedu système métrique décimal (3 vols., Paris, 1806, 1807, 1810) …

“Delambre explains the inequality of the assigned distances (Méchain—170,000 toises from Rodez to Barcelona; Delambre—380,000 toises from Rodez to Dunkerque) as follows: “The reason for this unequal division was that the Spanish part was entirely new, whereas the remainder had already been measured twice; we were agreed that the former would provide many more difficulties.” Then he remarks, “We did not know that the greatest difficulties of all would be found at the very gates of Paris.” Méchain, the first to set out, on 25 June 1792, was arrested at his third observational site, at Essonne, by uneasy citizens who were convinced that his activities had some counterrevolutionary aspects. Only by constant explanation and good fortune was Méchain able to continue, and eventually to carry his survey into Spain. Delambre encountered similar difficulties; and, in addition, when he returned to Paris and had to leave again, he had to seek new passports as the government changed. It seems almost incredible that in time of revolution Delambre was able to continue his work as much as he did. In eight
months of 1792, however, he had established only four points of triangulation; but in 1793, despite delays in getting his passport, he made better progress. Then, in January 1794, he received an order from the Committee of Public Safety to stop all observations at once. On his return to Paris he learned that as of 23 December 1793 he had been removed from membership in the commission of weights and measures, along with Borda, Lavoisier, Laplace, Coulomb, and Bresson.

"Happily, the enterprise was revivified by the law of 18 Germinal an III (7 April 1795), and Delambre and Méchain were able to take up their old assignments, now under the title of Astronomes du Dépôt de la Guerre, serving under the head of that establishment, General Calon, a member of the Convention. Delambre thereupon set out for Orléans on 28 June 1795 and completed his assignment within four years.

"Delambre's task was not merely to make a series of correlated astronomical observations and terrestrial measurements; he had also to carry out extremely laborious calculations. The latter were made especially tedious by the need to convert the observations from the new centesimal units of angle-measure (used in Delambre's instruments) to the older units of degrees, on which all tables of logarithms and of trigonometric functions were then based …

"Méchain died in 1804, and it became Delambre's sole responsibility to complete the computations and to write up the final report. This constituted three volumes containing the history of the enterprise, the observations, and the calculations. The third volume was completed in 1810, some twenty years after the project was begun. When Delambre presented a copy of this work to Napoleon, the emperor responded, "Conquests pass and such works remain."  

"Delambre's results were put into the hands of a commission of French and foreign scientists, who then determined the unit of length which became the standard meter. Jean Joseph Fourier said that "no other application of science is to be compared with this as regards its character of exactness, utility, and magnitude." The newly constituted Institut de France designated this survey "the most important application of mathematical or physical science which had occurred within ten years" and in 1810 gave Delambre a prize for his share in the great work. The accuracy with which Delambre carried out his task may be seen in a comparison of two base lines: Perpignan and Mélin. Delambre measured both by direct methods. Then, making use of a network of triangulation, he used one to compute the other. According to Fourier, although the distance between the two bases is some 220 leagues, the results of calculation differed from the results of direct measurement by less than threethenths of a meter, less than one part in 36,000" (DSB, under Delambre).

PMM 260; Norman 1481 (lacking half-titles); En Français dans le Texte 212. Alder, The Measure of All Things, 2002.
THE INVENTION OF COORDINATE GEOMETRY

DESCARTES, René. Geometria, al Renato Des Cartes Anno 1637 Gallice edita; nunc autem cum notis Florimondi De Beaune, in curia Bloesensi consiliari regii, in linguam Latinam versa, & commentariis illustrata, operà atque studio Francisci al Schooten... Leyden: Jean Maire, 1649.

$12,500

4to (209 x 160 mm), pp. [xii], 336 [2, errata], title printed in red and black and with woodcut printer’s device, ornamental tailpiece at end, numerous diagrams in text. Contemporary interim boards, manuscript title to spine, lower part of spine very well repaired, entirely uncut. Cancelled library stamp at foot of final page of text.

First separate edition of Descartes’s magnum opus (DSB), the invention of coordinate geometry and one of the key texts in the history of mathematics – this is an exceptional copy, uncut in the original interim boards. The Geometry was originally published in French as the third part of the Discours de la Méthode; the French text was not issued separately until 1664. 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). It “rendered possible the later achievements of seventeenth-century mathematical physics” (M. B. Hall, Nature and nature’s laws (1970), p. 91). “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” (Serfati, p. 1). "Divided into three books, it opens with the claim that ‘Any problem in geometry can easily be reduced to such terms that a knowledge of the lengths of certain lines is sufficient for its construction.’ In this spirit, Book I is concerned with ‘Problems which can be constructed by the aid of circles and straight lines.’ The highlight of Book I is the solution, by algebraic means, of the problem, outstanding since the time of Euclid, of the four-line locus. Book II contains a little-used classification of curves, and Descartes’ method of drawing tangents. The final Book III deals with the solution of higher-order equations, as well as Descartes’ rule of signs” (Gjertsen, Newton Handbook, p. 170). It was through this Latin translation, with its extensive commentary by Frans van Schooten and Florimonde De Beaune, that Newton and other contemporary mathematicians acquired an understanding of Descartes’s work. It is also the most accessible edition for bibliophiles, the Discours now commanding a six-figure sum. We have never seen another copy uncut in original boards, and none is recorded on ABPC/RBH.

Descartes’ 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” both over Descartes’ previous work and the work of his contemporaries (Guicciardini, p. 38). 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” (Landmark Writings, pp. 13-14). 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.
Descartes acknowledged that the mathematical language in which the *Geometry* was written would inhibit many readers, inserting a special ‘notice’ at the beginning of the text: “Up to this point I have tried to make myself intelligible to everyone. However, for this treatise, I fear that only those who already know what is in geometry books will be able to read it. The reason is that they contain many truths that are very well demonstrated, and I therefore thought it would be superfluous to repeat them and, for that reason, I have taken the liberty of using them.’ The caution was appropriate. Only those who were trained in mathematics could understand what the problems were” (Clarke, *Descartes*, p. 151).

The editor and translator of this edition, Frans van Schooten (1615-60), first saw the *Géométrie* at Leiden, as Descartes had come there to supervise the printing of the *Discours*. “After the death of his father in 1645, Schooten took over his academic duties. He also worked on a Latin translation of Descartes’ *Géométrie*. Although Descartes was not completely satisfied with Schooten’s version (1649), it found a broad and receptive audience by virtue of its more carefully executed figures and its full commentary. It was from Schooten’s edition of the *Géométrie* that contemporary mathematicians lacking proficiency in French first learned Cartesian mathematics. In this mathematics they encountered a systematic presentation of the material, not the customary, more classificatory approach that essentially listed single propositions, for the most part in unconnected parallel. Further, in the Cartesian scheme the central position was occupied by algebra, which Descartes considered to be the only “precise form of mathematics”” (DSB, under Schooten). Schooten included in the present edition the ‘Notae breves’ of Florimonde De Beaune (1601-52), a French jurist and amateur mathematician, which contains what became known as ‘De Beaune’s problem’, the important problem of determining a curve from the properties of its tangent. De Beaune’s notes evidently pleased Descartes, who wrote to him on 20 February 1639: “J’ai admiré que vous ayez pu reconnaître des choses que je n’y ai mises qu’obscément comme en ce qui regarde la généralité de la méthode.”

“After 1649, the text became a long-lasting object of study for European mathematicians and a veritable bedside read for geometers, while the faithful Latin translation by the disciple van Schooten ensured its wide dissemination. The ample commentaries, painstakingly completed by De Beaune and much longer than the *Géométrie* itself, were indispensable in explaining Descartes’s ideas to his contemporaries, clarifying obscurities, reconstructing omitted calculations and also producing new constructions and loci … Leibniz, who knew the *Géométrie* in Paris no later than 1674, made full use of its methods in his arithmetic quadrature of the circle around 1674, his first discovery” (Serfati, pp. 17-18). "Newton possessed two copies of Descartes’ *Geometry*, both in Latin … Newton described his first contact with the work as: ‘in the year 1664 a little before Christmas … I bought Van Schooten’s *Miscellanies* and Descartes *Geometry* (having read this *Geometry* … above half a year before). He read it, according to Conduit, slowly, and with some difficulty, before eventually making himself ‘Master of the whole without having the least light or instruction from anybody.’ His copy of Van Schooten has survived” (Gijertsen, p. 170). Newton’s annotated copy of the 1659 re-issue is held by Trinity College, Cambridge (NQ.16.203).

DESCARTES, René.
THE SEVEN BRIDGES OF KÖNIGSBERG


$4,800


First edition of “one of Euler’s most famous papers—the Königsberg bridge problem. It is often cited as the earliest paper in both topology and graph theory” (Euler Archive, E53). The city of Königsberg is situated on the River Pregel, across which seven bridges had been built in Euler’s time, most of which connected to the island of Kneiphof; the problem asked whether it was possible to devise a route that would allow one to cross each of the bridges exactly once. “When reading Euler’s original proof, one discovers a relatively simple and easily understandable work of mathematics; however, it is not the actual proof but the intermediate steps that make this problem famous. Euler’s great innovation was in viewing the Königsberg bridge problem abstractly, by using lines and letters to represent the larger situation of landmasses and bridges. He used capital letters to represent landmasses, and lowercase letters to represent bridges. This was a completely new type of thinking for the time, and in his paper, Euler accidentally sparked a new branch of mathematics called graph theory, where a graph is simply a collection of vertices and edges. Today a path in a graph, which contains each edge of the graph once and only once, is called an Eulerian path, because of this problem. From the time Euler solved this problem to today, graph theory has become an important branch of mathematics, which guides the basis of our thinking about networks”
Although Euler felt that the Königsberg bridge problem was trivial, he was still intrigued by it, and believed it was related to Leibniz’s *geometria situs*, or geometry of position, although today Leibniz’s ideas are viewed as an anticipation of the subject of topology, whereas the bridge problem is one of graph theory. After Euler’s paper, graph theory developed rapidly with major contributions made by Augustin-Louis Cauchy, William Rowan Hamilton, Arthur Cayley, and Gustav Kirchhoff, among many others.

“In March 1736 Karl Ehler, the mayor of Dantzig (now Gdansk), a city eighty miles from Königsberg, imparted to Euler his thoughts on a recreational puzzle about the seven bridge of Königsberg. It was part of their ongoing correspondence, which covered such items as artillery, real and imaginary numbers, and the rectification of curves. Ehler called the bridges problem ‘an outstanding example of the calculus of position.’ Euler had already solved it. The city of Königsberg in East Prussia (now Kaliningrad in Russia) comprises four sections. At the center is an island in the Pregel River, and in Euler’s time seven bridges spanning the river connected the island with the other three sections. The question was whether someone could pass over the bridges in a connected walk, crossing each bridge once, and return to the same spot. The puzzle itself, unrelated to Euler’s mathematical research, was among several problems that he addressed only once. While Leibniz and Wolff posed problems of this type, Euler seems to have learned of them from Johann I Bernoulli. Finding the Königsberg Bridge Problem simple, Euler solved it negatively – not with mathematics but with reasoning alone. The article ‘Solutio problematis ad geometriam situs pertinentis’ (Solution of a problem relating to the geometry of position) gave his conclusion. Submitted the next year for volume 8 of the *Commentarii*, it was not published until 1741 in an issue containing thirteen mathematical articles – two by Daniel Bernoulli and eleven by Euler [see below].
“Solutio problematis’ contains no graphs but is considered the first work in graph theory. The requisite type of graphs to represent the possible Königsberg walk under the given conditions did not appear until the nineteenth century. Euler divided this paper into twenty-one numbered paragraphs. After paragraph 3 rejects as unworkable any attempt to solve the problem by checking all possible paths, the paper considers the transit entrances to land regions rather than the crossing of bridges” (Calinger, pp. 130-131). The four regions were denoted by the capital letters A, B, C, D (A being the island) and the seven bridges by lower case letters a, b, c, d, e, f, g: a & b connected A to B; c & d connected A to C; e connected A to B; f connected B to D and g connected C to D. “Euler noted that, if a region had an odd number of bridges (k), then the letter of that region must appear (k + 1)/2 times in the string of capital letters that represent the entire journey … Since five bridges connect the island to the city’s other regions, the frequency [of A in the tour] will be (5 + 1)/2 = 3. The frequency for B, C and D, there being three bridges, is (3 + 1)/2 = 2. The sum of these frequencies is nine [3 + 2 + 2 + 2], but the sum for a path crossing each of the seven bridges only once is eight [because each bridge separates two regions]; the Königsberg tour under the given conditions is impossible; the problem has no solution” (ibid.).

Of the other ten papers by Euler in this volume, the most significant is 'Theorematum quorundam ad numeros primos spectantium demonstratio' (pp. 141-146), in which Euler proves ‘Fermat’s Little Theorem.’ This was first stated the theorem in a letter dated October 18, 1640, to his friend and confidant Frénicle de Bessy. Fermat did not send Frénicle the proof, only writing ‘I would send you a demonstration of it, if I did not fear going on for too long.’ Euler was the first to publish a proof, in the present paper.

“Number theory continued to be Euler’s passion and a wellspring of challenging problems; its higher degrees of abstraction attracted him. Among the circle of scholars he met or corresponded regularly with were Goldbach and Krafft, both of whom particularly discussed number theory with him, and by 1736 Euler was inventing ways to prove its theorems by introducing the concepts, definitions, and methods required to complete these theorems; he assiduously tried to consolidate the methods.

"After beginning with studies of \(2^{p-1} - 1\), in 1736 Euler stated )without modern notation) Fermat’s Little Theorem: [if \(p\) denotes a prime number and \(a\) any whole number not divisible by \(p\), then in modern notation \(a^{p-1} - 1\) is divisible by \(p\). In 'Theorematum quorundam ad numeros primos spectantium demonstratio' (A proof of certain theorems regarding prime numbers), Euler gives his first of four proofs of the theorem, a clumsy additive one based on mathematical induction on the notation a … His proof employs the binomial expansion of \((1 + 1)^{p-1} - 1\), the subtraction of consecutive binomial coefficients and their divisibility, and an appropriate rearrangement of terms” (Calinger, p. 135).

The remaining nine Euler papers in the present volume are as follows (with comments based on the Euler Archive).

Methodus universalis serierum convergentium summas quam proxime inveniendi (Universal methods of series), pp. 3-9. Euler evaluates the first ten terms of \(\zeta(2) = 1/1^2 + 1/2^2 + \ldots + 1/10^2\) to be 1.549768 (probably in his head) and gives an expression for the error term. Then he finds the sum of the first million terms of the harmonic series

\[
1/1 + 1/2 + 1/3 + \ldots
\]
to be 14.392669.

Inventio summae cuiusque seriei ex dato termino generali (Finding the sum of any series from a given general term). Further studies on ζ(2), and an infinite series approximation to for the sum of the first \( n \) terms of the harmonic series.

Investigatio binarum curvarum, quorum arcus eidem abscissae respondentes summam algebraicam constituant (Investigation of pairs of curves whose arcs that correspond to the same abscissa constitute an algebraic sum).

De oscillationibus fili flexilis quotcunque pondusculis onusti (On the oscillations of a flexible wire weighted with arbitrarily many little weights).

Methodus computandi aequationem meridiei (A method for computing the equation of a meridian).

De constructione aequationum ope motus tractorii aliisque ad methodum tangentium inversam pertinentibus (On the construction of equations using dragged motion, and of other things pertinent to the inverse method of tangents).

Solutio problematum rectivicationem ellipsis requirentium (Solution of a problem requiring the rectification of an ellipse). "Euler starts with integrals of a certain form, which are really elliptical integrals, and derives second-order ordinary differential equations using the so-called "Modular equation" whose solution can be put back through the given integral. Then several geometric problems are solved, which cause special cases of derived differential equations to appear."

Methodus universalis series summandi ulterius promota (Universal method for summation of series, further developed). Euler considers power series of the form

\[ f(0) + f(1)x + f(2)x^2 + f(3)x^3 + ... \]

Curvarum maximi minive proprietate gaudientium inventio nova et facilis (New and easy method of finding curves enjoying a maximal or minimal property).

The volume also contains two papers by Daniel Bernoulli on hydrodynamics (pp. 99-112 & 113-127).

THE INTRODUCTION OF ANALYTICAL METHODS IN TO MECHANICS


$12,500

Two vols., 4to (253 x 190 mm), pp. [16] 1- 2 32 225-480 [i.e., 488]; [8], [1] 2-500, with engraved vignette on dedication leaf and thirty-two folding engraved plates. Contemporary calf, richly gilt spine with red and green spine labels, boards with some superficial wear, upper margin of title to volume 1 with a triangular piece (4 x 1 cm) torn and lost. Otherwise a very fine and clean set in unrestored condition.

First edition of “Euler’s famous work on mechanics in which he introduced the use of analytical methods instead of the geometrical methods of Newton and his followers” (Timoshenko, p. 29). Mechanica won the praise of many leading scientists of the time: Johann Bernoulli said of the work that “it does honour to Euler’s genius and acumen,” while Lagrange in his own Mécanique analytique acknowledges Euler’s mechanics to be “the first great work where Analysis has been applied to the science of motion.”

“In an introduction to the Mechanica Euler outlined a large program of studies embracing every branch of science. The distinguishing feature of Euler’s investigations in mechanics as compared to those of his predecessors is the systematic and successful application of analysis. Previously the methods of mechanics had been mostly synthetic and geometrical; they demanded too individual an approach to separate problems. Euler was the first to appreciate the importance of introducing
uniform analytic methods into mechanics, thus enabling the problems to be solved in a clear and direct way. Euler’s concept is manifest in both the introduction and the very title of the book, *Mechanica sive Motus Scientia analytice exposita*.

“This first large work on mechanics was devoted to the kinematics and dynamics of a point-mass. The first volume deals with the free motion of a point-mass in a vacuum and in a resisting medium; the section on the motion of a point-mass under a force directed to a fixed center is a brilliant analytical reformulation of the corresponding section of Newton’s Principia; it was sort of an introduction to Euler’s further works on celestial mechanics. In the second volume, Euler studied the constrained motion of a point-mass; he obtained three equations of motion in space by projecting forces on the axis of a moving trihedral of a trajectory described by a moving point, i.e. on the tangent, binormal and principal normal. Motion in the plane is considered analogously. In the chapter on the motion of a point on a given surface, Euler solved a number of problems on the differential geometry of surfaces and of the theory of geodesics” (DSB).

“This is Euler’s outline of a program of studies embracing every branch of science, involving a systematic application of analysis. [It] thus laid the foundations of analytical mechanics and was the first published work in which e appeared. In addition, these two volumes were the result of Euler’s consideration of the motion produced by forces acting on both free and constrained points.

“[The first] volume focuses on the kinematics and dynamics of a point-mass, introducing infinitely small bodies that can be considered to be points under certain assumptions. Euler focuses on single mass-points except for a few pages at the end of Chapter I, where he looks at the motion of one point relative to another moving point. He then looks at the nature of rest and uniform motion. In Chapter II, Euler states Newton’s second law of motion.
Throughout this volume, he considers the free motion of a point-mass in a vacuum and in a resisting medium so that all forces under consideration are known. Mathematically, acceleration is given to within an arbitrary multiplicand, and in each example he considers, the arguments of the force function are limited to position and speed. Thus, Euler devotes this volume to integrating particular second-order differential equations and to interpreting his results.

“For about half of this volume, Euler analyzes motion along straight lines. The remainder is mainly concerned with motion in a plane, with a few pages looking at motion along a skew curve. He introduces fixed rectangular Cartesian coordinates for the position of the mass-point but uses arc length as the independent variable to set up his differential equations of motion. He also resolves the enforced acceleration into components along the tangent and normal to the path. In three dimensions, he uses two orthogonal normals, one of which he forces to be parallel to a fixed plane.

“In [the second] volume, Euler considers the motion of a point-mass lying on a given curve or surface. He derives some differential equations of the geodesics governing the problem of free motion on a surface. In this way, he shows that the path of a mass-point that is free to move on a fixed surface is locally the shortest possible path between its initial and final points” (eulerarchive.org).

INSCRIBED BY GIBBS TO POINCARÉ


$17,500

8vo (223 x 148 mm), pp. [i-vii], viii-xviii, [1-3], 4-207, [1]. Original blue cloth, gilt-lettered spine, printed dust-jacket (slight wear to jacket). A fine copy.

First edition, inscribed presentation copy to the great French mathematician and mathematical physicist Henri Poincaré. "Of Gibbs [Einstein] wrote in 1918: '[His] book is … a masterpiece, even though it is hard to read and the main points are found between the lines'" (Pais, Subtle is the Lord (1983), p. 73). This book was "a major advance in statistical mechanics, the branch of science in which a purely mechanical view of natural phenomena is replaced by one combining mechanics with probability" (Norman). Gibbs' book was "a triumph of the rigorous axiomatic method, which placed him beside Clausius, Maxwell, and Boltzmann as one of the principal founders of statistical mechanics" (Mehra, p. 1786). "Albert Einstein – who independently developed his own version of statistical mechanics from 1902 to 1904, having no knowledge of Gibbs' work – remarked in 1910 'Had I been familiar with Gibbs' book at that time, I would not have published those papers at all, but would have limited myself to the discussion of just a few points'" (Inaba, p. 102). "Gibbs' book on statistical mechanics became an instant classic and has remained so for almost a century" (Mehra). In this book, Gibbs formulated statistical mechanics in terms of 'ensembles' of systems, which were collections of large numbers of copies of the system of interest, all identical except for their physical properties (volume, temperature, etc.). "In most of his elegant Principles
in Statistical Mechanics of 1902, [Gibbs] described the underlying mechanical system in a formal manner, by generalised coordinates subject to Hamilton’s equations … He introduced and systematically studied the three fundamental ensembles of statistical mechanics: the micro-canonical, the canonical, and the grand-canonical ensemble (in which the number of molecules may vary). He examined the relations between these three ensembles and their analogies with thermodynamic systems, including fluctuation formulas” (Buchwald & Fox, p. 784). “A year before his death, Einstein paid Gibbs the highest compliment. When asked who were the greatest men, the most powerful thinkers he had known, he replied ‘Lorentz’, and added ‘I never met Willard Gibbs; perhaps, had I done so, I might have placed him beside Lorentz’” (Pais, p. 73). According to Emilio Segre (From Falling Bodies to Radio Waves (1984), p. 250), “even Jules-Henri Poincaré found [Elementary Principles] difficult to digest” – the present copy is presumably the one Poincaré puzzled over. Although reasonably well represented in institutional collections, this is a very rare book on the market. ABPC/RBH lists only one other copy in the last 35 years (and that copy lacked the dust-jacket).

Provenance: Jules-Henri Poincaré (1854-1912), presentation inscription on front free endpaper: ‘M. J.-H. Poincaré with the respects of the author’. Poincaré was ‘one of the greatest mathematicians and mathematical physicists at the end of 19th century. He made a series of profound innovations in geometry, the theory of differential equations, electromagnetism, topology, and the philosophy of mathematics” (Britannica). Although Poincaré did not work directly on statistical mechanics, his work on the three-body problem in celestial mechanics had an important impact upon it. In 1890, he proved his ‘recurrence theorem’, according to which mechanical systems governed by Hamilton’s equations will, after a sufficiently long time, return to a state very close to the initial state. This theorem created serious difficulties for any mechanical explanation of the laws of thermodynamics, as it apparently contradicts the Second Law, which says that large dynamical systems evolve irreversibly towards states with higher entropy, so that if one starts with a low-entropy state, the system will never return to it.

"Josiah Willard Gibbs was born in 1839: his father was at that time a professor of sacred literature at Yale University. Gibbs graduated from Yale in 1858, after he had compiled a distinguished record as a student. His training in mathematics was good, mainly because of the presence of H. A. Newton on the faculty. Immediately after graduation he enrolled for advanced work in engineering and attained in 1863 the first doctorate in engineering given in the United States. After remaining at Yale as tutor until 1866, Gibbs journeyed to Europe for three years of study divided between Paris, Berlin, and Heidelberg. Not a great deal of information is preserved concerning his areas of concentration during these years, but it is clear that his main interests were theoretical science and mathematics rather than applied science. It is known that at this time he became acquainted with Möbius’ work in geometry, but probably not with the systems of Grassmann or Hamilton. Gibbs returned to New Haven in 1869 and two years later was made professor of mathematical physics at Yale, a position he held until his death [in 1903].

“His main scientific interests in his first year of teaching after his return seem to have been mechanics and optics. His interest in thermodynamics increased at this time, and his research in this area led to the publication of three papers, the last being his now classic ‘On the Equilibrium of Heterogeneous Substances’, published in 1876 and 1878 in volume III of the Transactions of the Connecticut Academy. This work of over three hundred pages was of immense importance. When scientists finally realized its scope and significance, they praised it as one of the greatest contributions of the century” (Crowe, p. 151).

“During the academic year 1889–1890 Gibbs announced ‘A short course on the a priori Deduction of Thermodynamic Principles from the Theory of Probabilities,’
a subject on which he lectured repeatedly during the 1890s” (DSB).

“Lord Rayleigh, writing on 5 June 1892 about an optical problem to Josiah Willard Gibbs in New Haven, Connecticut, concluded his letter as follows:

’Have you ever thought of bringing out a new edition of, or a treatise founded upon, your ‘Equilibrium of Heterogeneous Substances.’ The original version, though now attracting the attention it deserves, is too difficult and too condensed for most, I might say all, readers. The result is that, as has happened to myself, the idea is not grasped until the subject has come up in ones mind more or less independently. I am sure that there is no one who could write a book on Thermodynamics like yourself.’

“Gibbs replied on 27 June 1892.

’I thank you very much for your kind interest in my ‘Equilib. Het. Subst.’ I myself had come to the conclusion that the fault was that it was too long. I do not think that I had any sense of the value of time, of my own or others, when I wrote it. Just now I an trying to get ready for publication something on thermodynamics from the a priori point of view, or rather on ‘Statistical Mechanics’ of which the principal interest would be in its application to thermodynamics – in the line therefore of the work of Maxwell and Boltzmann. I do not know that I shall have anything particularly new in substance, but shall be contented if I can so choose my standpoint (as it seems to me possible) as to get a simpler view of the subject.’

“These lines doubtless indicate that the Statistical Mechanics of Gibbs was not conceived and written within nine or twelve months prior to his death, as seems to be the general impression, to some extent created by Gibbs’ own students.
An abstract entitled, 'The Fundamental Formulas of Statistical Mechanics, with Applications to Astronomy and Thermodynamics,' appeared in the Proceedings of the American Association for the Advancement of Science in 1884. Although it did not contain much information, it at least testified to the fact that Gibbs had been working on the problem of the statistical foundations of thermodynamics at the time. Moreover, detailed notes in Gibbs' own hand exist, some of them dated since 1892, extending over many years and dealing with the organization and details of the subject which Gibbs had undertaken to treat. Gibbs, who became a professor of mathematical physics at Yale in the year 1871, the same year in which Maxwell became Cavendish professor at Cambridge, read the scientific papers of his European colleagues from the very beginning. Thus it seems very probable that, starting already in 1873, Gibbs thought about the statistical foundations simultaneously with his writing of his papers on thermodynamics. It was also partially during these years that the important papers of Maxwell and Boltzmann on statistical mechanics were published.

There was much entirely new in what Gibbs was undertaking to write, not just a continuation of the 'line of Maxwell and Boltzmann' …

The immediate occasion to publish his formulation of the statistical approach top thermodynamics was provided by the request of the university administration to contribute a book to commemorate the bicentennial of Yale College in 1901 …

The first sketch of the book numbered nine chapters only: I. General Motions and Principles; II. Application to the Theory of Errors; III. Application to the Theory of Integration; IV. On Statistical Equilibrium and a Distribution in Phase Called Canonical Ensemble, Microcanonical Distribution; V. Maximum and Minimum Properties and Inequalities; VI. Effect of Time; VII. Various Processes; VIII. Thermodynamics; IX. Systems of Molecules.

In the final book, Chapters I, II and III were the same with respect to contents, but the mammoth Chapter IV was split into seven chapters, including: IV. On the Distribution-in-Phase called Canonical; V. Average Values in a Canonical Ensemble of Systems; VI. Extension-in-Configuration and Extension-in-Velocity; VII. Further Discussion of Averages in a Canonical Ensemble of Systems; VIII. On a Certain Important Function of the Energy of a System; IX. The Function \( \phi \) and the Canonical Distribution; X. On a Distribution in Phase Called Microcanonical in which all the Systems Have the Same Energy …

'The writing of the Elementary Principles seems to have proceeded very systematically and rather rapidly. Starting from the principal question of how the statistical equilibrium is to be described, and how the equilibrium can be reached in time, Gibbs developed section after section of his book, of which Boltzmann said: 'The task of systematizing this science, of compiling it into a large book, and of giving it a characteristic name, was executed by one of the greatest American scholars, and in regard to abstract thinking, purely theoretic investigation, perhaps the greatest, Willard Gibbs, the recently deceased professor of Yale University. He called this science statistical mechanics.'

Boltzmann and Maxwell had dealt with specific systems, those of more or less ideal gas molecules. Gibbs turned away from the restrictions imposed by specific systems. He imagined 'a great number of systems of the same nature, but differing in the configuration and velocities which they have at a given instant, and differing not merely infinitesimally, but it may be so as to embrace every conceivable combination of configurations and velocities.' Thus he developed the canonical ensemble, in which the equilibrium distribution depends in the most simple manner on the energy, namely 'the index of probability is a linear function of the energy,' the divisor of the energy being essentially the temperature.
“Having found the most natural, elegant or even ‘geometrical’ representation of the equilibrium situation, from which the results of Maxwell and Boltzmann, e.g., the famous energy-distribution of the latter, followed immediately, Gibbs went on to discuss the Second Law … Gibbs took great pains in defining and deriving properly the relations of equilibrium thermodynamics from his statistical mechanics.

“The sections on the approach to equilibrium, on the other hand, are largely of a qualitative character. The question of whether a system, which is in an arbitrary state, develops in a direction which one calls equilibrium. Gibbs treated the example of an incompressible fluid to which, in some initial far-from-equilibrium distribution, colored fluid is added. By stirring in any sense [i.e., direction] mixing will finally be achieved.

“To summarize, one might say that Gibbs succeeded very well in founding equilibrium thermodynamics on a statistical basis. But due to the great difficulties that existed at that time, and still do, he could not in the same way describe the gradual changes of the macroscopic properties in their approach to equilibrium …

“As usual, Gibbs sent copies of his book on the Elementary Principles in Statistical Mechanics to numerous colleagues and institutions. In his Scientific Correspondence, we find a few letters expressing warm thanks. Most of the distinguished recipients of Gibbs’ book wrote that they had not as yet had the opportunity of studying the book in detail, and in fact it is doubtful if many did so later on. However, two of them took a deeper interest; they were H. A. Lorentz and M. Planck …

“[Planck] said that Gibbs had given him great joy by sending the new work on statistical mechanics.

‘I need hardly mention that I shall study your book with the greatest interest, because as far as I can see from a perusal and from the preface, it deals with a question which seems to me to be of the greatest interest: the introduction of the methods of probability calculus into mechanics, independent of the application to thermodynamics. Because it is only along this path that we might hope once to gain a deeper insight into the laws of irreversible processes, which according to my conviction will move more and more into the forefront of theoretical interest.’

“Planck found the book so valuable that he persuaded his student and collaborator E. Zermelo to translate it. Planck also used Gibbs’ method to reformulate his quantum condition for the oscillator, shifting the emphasis from the quantum of energy to the quantum of action. In the Maxwell-Gibbs phase-space, which has two dimensions for a linear harmonic oscillator, the surfaces of constant energies are similar ellipses whose areas are measured in multiples of Planck’s quantum of action $h$. It was this from Gibbs’ Elementary Principles that Planck obtained a deeper insight into the phenomena of heat radiation” (Mehra).

“Later physicists regarded Gibbs’ theory as so general that its principles are still valid in quantum theory. It needed some adjustments, but its elementary features could be still mobilized, as a bridge that has been repaired can still serve to cross the river. In this sense we can say that Gibbs provided an infrastructure that made it possible to lead to quantum statistics” (Inaba, p. 104).

JOHN EVELYN’S UNIQUE COPY WITH THE PORTRAIT OF HARVEY


$49,500

4to (223 x 158 mm), pp. [xxviii], 301, [1], with etched portrait of Harvey probably by Richard Gaywood, engraved allegorical title-page, woodcut ornamental initials and headpieces (some light browning. T3 with marginal paper flaw, 2G4 with tiny marginal hole). Contemporary Parisian mottled calf for John Evelyn, the covers panelled in gilt with his cipher, spine gilt in compartments with red morocco label, all edges gilt, (rubbed, some flaking and loss of gilt due to action of the mottling acid, minor restoration). Brown half-morocco folding-case gilt, by Middleton.

John Evelyn’s copy, and possibly the unique copy containing the portrait of Harvey, of the first edition of “the most important book on [embryology] to appear during the seventeenth century” (Garrison-Morton). In this work, “he rejected the prevailing doctrine of the preformation of the fetus, and advanced the theory, radical for its time, of epigenesis, that all living beings derive from the ovum ‘by the gradual building up and aggregation of its parts’. Regarding Harvey’s theory of epigenesis, Thomas Henry Huxley (1825-95) claimed this should “give him an even greater claim to the veneration of posterity than his better known discovery of the circulation of the blood” (Keynes, Bibliography, p. 47). Harvey reported a wealth of observations on many aspects of reproduction in a wide variety of species. As representatives of vivipara, his attention was chiefly devoted to the deer,
while that for ovipara was the domestic fowl. For Harvey, all life develops from the egg. This is expressed on the frontispiece which depicts the supreme Roman god Jupiter [Jove] opening a large egg, inscribed with the fundamental dictum of embryology, ex (upper half of egg shell) ovo omnia (lower half of egg shell), which translates as, ‘from the egg everything,’ and from which the liberated animals and insects fly … An opponent of the theory of spontaneous generation, Harvey speculated that humans and other mammals must reproduce through the joining of an egg and sperm. No other theory was credible. By positing and demonstrating for viviparous animals the same mechanism of reproduction as that observed in oviparous animals, he thus initiated the search for the mammalian ovum” (Longo & Reynolds, p. 272). Harvey was persuaded to publish this work by his colleague Dr. George Ent, who wrote the preface addressed to the College of Physicians and saw the book through the press. “Ent reports in his dedication the conversations with Harvey in which he secured his consent to publication, and remarks at the end that ‘as our author writes a hand which no one without practice can easily read, I have taken some pains to prevent the printer committing any very grave blunders through this’” (Keynes, Bibliography, p. 46). The text comprises seventy-two ‘Exercises’ and extended chapters on parturition, the uterus, and conception. In Exercise 51 he formulates the theory of epigenesis, and his chapter ‘De partu’ is the first published essay on midwifery by an Englishman. The importance of Harvey’s text was immediately recognized, and it was reprinted three times in the year of its issue. The scarce portrait of Harvey inserted in the Evelyn copy was in fact intended to be published in this edition of De generatione animalium, as a letter to Evelyn from Dr. Jasper Needham quoted by Keynes (Life, p. 333) demonstrates: ‘Dr. Harvey’s picture is etcht by a friend of mine and should have been added to his work, but that resolution altered: however I’ll send you a proof with your book that you may bind it up with his book De Generatione. I’m sure ‘tis exactly like him, for I saw him sit for it.’ Keynes refers to the present copy (then in Christ Church College, Oxford) for the portrait.


“Harvey had for a great number of years experimented and recorded his observations on the development of the chick embryo and of other animals. There are many references to the subject in his writings on the heart and circulation of the blood … It is evident from references in the sixth Exercise of this work that this interest in the subject of generation had been initiated by his association with the great Fabricius when he was at Padua, 1598-1602” (Keynes, Bibliography, p. 46). “While Harvey’s general biological interests developed in Padua, there is no adequate evidence that he played an active part in Fabricius’s embryological studies. Nevertheless, he consciously based his own studies on Aristotle and Fabricius, the latter’s De formation ovi et pulli of 1621 being particularly important. Thus, it is probable that Harvey’s serious investigations of the embryology of the chick, which formed the basis of De generatione, began shortly after he had read Fabricius’s book.

“Evidence from De generatione suggests that Harvey was actively engaged in collecting materials relating to generation between 1625 and 1637. De motu cordis indicates that, by 1628, he had recognized the important theoretical implications of studies on generation and was already engaged on a major treatise on this subject.
"First, he was determined to resolve the contradictions in the various descriptions of the chick embryology. This in turn provided evidence for use in the wider problem of the nature of sexual generation throughout the animal kingdom. The technical and theoretical difficulties involved in this research would cause Harvey to progress slowly. However, it is quite possible that [Sir Thomas] Browne and [Sir Kenelm] Digby saw major sections of the work in about 1638 and this may have been the stimulus to their own embryological investigations.

"After 1638, Harvey probably continued modifying his work and compiling additional material relating to this inexhaustible subject. The study of insects and other invertebrates would have been a particularly demanding study. However, soon the continuity of his labour was interrupted, with the outbreak of the civil war in 1642. He vacated his Whitehall chambers; these were pillaged and many valuable manuscripts were lost. The loss of De insectis would have been a particularly severe blow to his studies on generation, for Harvey fully recognized the crucial importance of including invertebrates within the scope of his biological theories.

"Harvey's fortunes now depended on the fate of the King. Being forced to retire from London, he settled in Oxford, where, between 1642 and 1646, he was able to resume his embryological studies, having the assistance of George Bathurst and Sir Charles Scarburgh. However, in 1646, the King's failure to hold Oxford resulted in Harvey retiring from the royal service. He returned to London and, with the stability produced by the collapse of the royal cause, he was able to return to his studies. Thus, it is not surprising that the publication of De generatione was intimated in 1648 and again in 1649.

"It is quite possible that Harvey's final hesitations about publication were due to a desire to consolidate the principles announced in this ambitious work. The loss of many of his manuscripts was also a factor which would delay completion of
De generatione, for it destroyed irrevocably his plan for a complete systematic account of biology and medicine. He was too old and sick to regain the territory lost by the folly of war. Eventually, George Ent counteracted Harvey's pessimism and persuaded him to prepare the work for publication in the early months of 1651” (Webster, pp. 268-270).

"The finished book is a handsome quarto of over 300 pages; it has an allegorical frontispiece ... [which] provides a rather undistinguished figure of Jove seated on a pedestal with his eagle beside him ... The first intention was to add to the interest of Harvey's book by including a portrait of the author. He did in fact give sittings to an artist and an etching was made, but in the end this was not used. A few examples of the print have survived, and these present an image of an aged and unhappy-looking man. Not unnaturally this was not regarded with favour by Dr. Ent, or by the author's family, and it was set aside ...

"Harvey's book consists of seventy-two 'Exercises', or chapters, preceded by a long philosophical introduction discussing Aristotle's and Galen's views concerning generation, his own methods of attacking the problem, and how knowledge in general, and of generation in particular, may be acquired. The first thirteen Exercises describe the comparative anatomy of the reproductive organs of a number of animals, with an account of the physiology of reproduction. The twelfth to the twenty-fifth describe the day-to-day development of the chick in the egg. Exercises 26 to 62 discuss at length various theories and problems of generation. Some of the conclusions reached by Aristotle and Galen Harvey thought were erroneous and hasty, for 'like phantoms of darkness they suddenly vanish before the light of anatomical enquiry.' He adumbrated his newer and better method of ascertaining the truth by ocular investigation, not underestimating the labour involved, but pointing out the sweet compensation provided by the pleasure of discovery. The introduction is greatly lengthened by the discussion of how knowledge is acquired, beginning with Aristotle's insistence that all knowledge is gained primarily through the senses ...

"The sixty-second Exercise, headed 'That an egg is the Common Original of all Animals,' forms a loose link between Harvey's observations on hen's eggs and those on generation in viviparous animals and other classes. Towards the close of the chapter Harvey wrote: 'But hereafter when we treat of the Generation of Insects, and of Spontaneous Productions, we shall discover how each of them are either differenced among themselves, or else do agree.' This must have been written before the loss of his notes on insects in 1642, the passage being overlooked when the decision was made to print the book in spite of this serious omission. The sixty-third Exercise contains general remarks 'Of the Generation of Viviparous Animals'; the sixty-fourth to the seventy-second describe generation as seen in hinds and does. The last part of the book is almost a separate treatise on generation and obstetrics in mankind, and is not divided into Exercises" (Keynes, Life, pp. 333-336).

"In the early 1630s, Harvey had studied the herds of red and fallow deer that were kept in the parks of King Charles I. He dissected hinds that had recently mated, and to his surprise found nothing in the uterus: no semen, no menstrual blood, and most surprising of all, no egg. Studies of dogs and rabbits gave the same result. Harvey summed up his findings succinctly: 'Nothing at all can be found in the uterus after copulation for the space of several days.' This led him to conclude that 'the foetus does not arise either from the male or the female sperm emitted in coitus, nor from both of them mixed together, as the physicians think, nor from menstrual blood as being the substance, as Aristotle thought, and that something of the conception is not necessarily made immediately after coitus.' Harvey's failure to find semen or eggs after copulation led him to believe that both Galen's and Aristotle's views were wrong: whatever the male and female contributions
might be, there was no physical contact between them. Furthermore, however this indirect effect worked, it did not immediately lead to the appearance of an egg.

"Harvey groped with analogies to explain his findings, suggesting that semen had its effect through some immaterial ‘spirit’ or ‘contagion’, or perhaps like an odour, or a spark, or a bolt of lightning, or even a kind of magnetic effect. His vague conclusions were perfectly in keeping with his findings (no semen was observable in the uterus) and with contemporary knowledge about the apparently non-material transmission of disease. As to what the woman’s contribution might be, Harvey was at a loss. He dismissed Galen’s idea that there was a female ‘semen’ that was produced at orgasm, and in particular he opposed the suggestion that it involved female ‘ejaculation’: not all women ‘ejaculate’, he pointed out; those who do not can still both reach orgasm and be fertile; and he argued that the fluid involved has ‘a serous and watery consistency, like urine’, which he thought meant it was too thin to play the role of semen.

"Harvey made a spectacular mistake when he argued that the female ‘testicles’ (what we call ovaries) played no role in female generation. His dissection of female deer had shown no changes in the size or shape of the ‘testicles’ as the mating season progressed, so he had to conclude that ‘The so-called testicles, like things utterly unconcerned in generation, neither swell up nor vary in any wise from their wonted constitution either before or after coitus, nor gave any indication of being of any use either for coitus or for generation.’ This fitted in with the widespread assumption that the female ‘testicles’ were like male nipples – vestigial, functionless organs …

"Faced with this evidence – or, rather, lack of it – Harvey found himself driven into a corner. In the final section of his book, he speculated about what he called the ‘dark business’ of conception. Struck by his inability to find any physical trace
of the future embryo in his dissected deer, nor any sign of semen, Harvey grappled with the implications. ‘There is nothing which can be perceived in the uterus after coitus, and yet it is a necessity that something must be there to render the female fertile.’ His conclusion, which he clearly felt uncomfortable with, was the only one his evidence supported: ‘What imagination and appetite are to the brain,’ he wrote, ‘the same thing, or at least something analogous to it, is awakened in the uterus by coitus and from this proceeds the generation or procreation of the egg.’ In other words, the appearance of the egg was the product of a mysterious force: the power of the womb’ (Cobb, pp. 27-29).

“As one contemplates these singular narrations and other similar statements, one is reminded that even great men cannot escape from all the things that shackle common men … for Aristotle and Arantius had held that the fetusus of mammals were expelled by the contraction of the uterus, the abdominal muscles and the diaphragm. One scarcely cannot escape the conclusion that in this instance at least if not also in others, Harvey was misled by an opinion derived from a study of the chick and of insects” (Meyer, p. 130).

“We have few details of the techniques employed by Harvey in his researches on generation. His failures have been attributed to the limitations of the means at his disposal, particularly to lack of any form of microscope for observation of the initial stages of the development of the embryo in the hen’s egg and for the identification of the mammalian ovum and spermatozoon. The first use of a simple lens in embryology is attributed to Riolan the younger, Harvey’s contemporary and correspondent … That Harvey used a simple lens is evident from his references to the employment of a perspicilium, a variation of Riolan’s conspicilium … With regard to Harvey’s failure to identify the mammalian ovum, it must be remembered that, although blood corpuscles and spermatozoa were first seen with the help of a simple lens, the ovum was not in fact identified (by von Baer) until 1827” (Keynes, Life, pp. 339-342).

In spite of its length and difficulty (and the lack of diagrams), De generatione was an immediate bestseller, with four separate Latin editions appearing in 1651 (two in England, two in the Netherlands), followed by an English translation two years later.

The present copy was bound for John Evelyn, who was at the centre of the intellectual, social, political and ecclesiastical world of his day. Born into a substantial Surrey landowning family whose fortunes were founded in gunpowder manufacture, Evelyn came of age just as the Civil War began ‘in a conjunction of the greatest and most prodigious hazards that ever the youth of England saw’. To escape the disturbances, he embarked on a prolonged and formative period of travel in Italy and France. He visited the medical faculty of the University of Padua, where Harvey had studied, and in the Veneto he renewed his acquaintance with the famous art collector Thomas Howard, 21st Earl of Arundel. He finally came to rest in Paris in 1647 where he married the daughter of the English Resident, Sir Richard Browne, whose house was a centre for the exiled royalist community. This period abroad stimulated Evelyn’s wide-ranging intellectual interests. He embarked on an intensive programme of study, of which the evidence remains in his elaborate series of commonplace books, and began to build up his impressive private library: as he afterwards wrote, he always looked on a library ‘with the reverence of a temple’. By the time he returned to England in 1652 to take up residence at a house belonging to his wife’s family, Sayes Court at Deptford, he had made himself prodigiously learned, not only in classical literature but also in scientific and technical matters. He soon established himself as one of the foremost virtuosi of his day. The Restoration of Charles II in 1660 brought Evelyn a long wished-for opportunity to engage in public affairs. He became a founder member of the Royal Society. The King sought his company and commissioned him to write. But Evelyn never found ‘the fruitless, vicious and empty conversations’ of
the Restoration Court congenial. Samuel Pepys wrote of this many-faceted man, ‘a most excellent person he is, and must be allowed a little for conceitedness; but he may well be so, being a man so much above others’.

It seems that Evelyn did not know Harvey personally, but it is likely that Evelyn learned something of him from the Earl of Arundel, whom Harvey had accompanied on his continental trip in 1636. Evelyn must have seen Harvey’s stemma in the University of Padua’s arcade during his visit to Italy.

**INSCRIBED PRESENTATION OFFPRINT TO MICHEL-EUGÈNE CHEVREUL**


$28,500

Offprint from: *Philosophical Transactions of the Royal Society of London*, Vol. 132, Part II. 4to (298 x 229 mm), pp. [ii], [181]-214, with one folding engraved plate. Half-morocco gilt by Honnelaitre, with the mute original paper wrappers bound in. A fine copy.

First edition, extremely rare offprint, of this seminal early work of photography, the invention of the world’s first photocopying process, ‘cyanotype,’ later called ‘blue-printing’; this remained by far the most important reprographic process for more than a century after the publication of Herschel’s paper. This is an extraordinary presentation copy, inscribed by Herschel to the great French colour theorist Michel-Eugène Chevreul. “Photography in Prussian blue was discovered in 1842 by Sir John Herschel just three years after Louis Daguerre and Henry Talbot had announced their independent inventions of photography in silver, using metal and paper substrates, respectively. Their successes in finally securing silver photographs represented the fruition of an idea that had been gestating for more than a century in the minds and laboratories of many noted natural philosophers. In contrast, the birth of cyanotype came, literally and metaphorically, ‘out of the blue,’ to a single parent … As one of the leading physical scientists of his day, Herschel was driven by the urge to understand photochemical phenomena, and
to harness them as tools for probing the electromagnetic spectrum beyond the narrow optical limits imposed by human vision. Using light-sensitive coatings on paper, he sought to venture below the shortwave end of the visible spectrum, into the region of the ultra-violet or ‘actinic’ rays discovered in 1801 by Johann Ritter; and above the longwave visible limit, into the region of the infra-red or ‘thermic’ rays, which had been discovered in 1800 by his father, Sir William Herschel … There is no compelling evidence to suggest that he was in pursuit of commercially useful methods of reprography, unlike Talbot, whose clear aim was to multiply his photographic images in printer’s ink. It is therefore a happy irony that Herschel should have been responsible for inventing the first process for photocopying” (Ware, *Cyanotype: the history, science and art of photographic printing in Prussian blue* (1999), p. 11). “Only in 1872, one year after Herschel died, was the cyanotype revived, when the Paris-based Marion and Company renamed his invention ‘ferro-prussiate paper’ and began marketing it for the replication of architectural plans. (Previously, they had been copied by hand, which was expensive and prone to human error.) At the 1876 Philadelphia Centennial Exposition, the process reached American shores, where it finally met success as the blueprint, the first inexpensive means of duplicating documents. All that was required was a drawing traced on translucent paper. Pressed against a second sheet coated with Herschel’s chemical under glass, the drawing was exposed to sunlight, then washed in water. The blueprint paper recorded the drawing in reverse, black lines appearing white against a cyan background” (Keats, ‘The Blueprint,’ *Scientific American* 301 (2009), p. 90. ABPC/RBH list only a copy offered by Goldschmidt in 1936 (not a presentation copy). We have located only one other copy in commerce, offered by Ernst Weil in his Catalogue 7 (ca. 1946).

Provenance: Michel-Eugène Chevreul (inscribed by Herschel on title ‘M. Chevreul with the authors respects’ and on original front wrapper ‘M. Chevreul Membre de l’Inst[itut] &c &c. Paris’).

“As a scientist Sir John Herschel was naturally more interested in the theory of photography than in its practice. Photography is indebted to him for a wealth of ideas, but those who developed them often neglected to acknowledge their originator. Herschel was of a retiring disposition and never pushed forward his claims; indeed, we marvel at the restraint with which he bore the incorrect behaviour of Talbot who, thwarted in his desire for public acclaim, hastened to the Patent Office with more than one idea which Herschel had freely published.

“Herschel’s photographic researches are concentrated within the first few years after the discovery of photography, and the genius and energy which he displayed were overwhelming. For him, it would have been an easy matter to invent a photographic process earlier had he felt, like Niépce, any urge to do so, or had he believed that it would facilitate his work, as Daguerre and Talbot and Reade did. As far back as 1819 Herschel discovered the property of the hyposulphites as solvents for silver salts, whereas ignorance of this fact had proved the stumbling-block to other investigators in photography for a long time. Herschel’s scientific knowledge was indeed so great that on merely receiving a note, on 22 January 1839, from Captain (later Admiral) Beaufort telling him the bare fact of Daguerre’s discovery, ‘a variety of processes at once presented themselves,’ and only a week later Herschel succeeded in producing his first photograph” (Gernsheim & Gernsheim, *The History of Photography 1685-1914* (1969), p. 95). Herschel learned of Talbot’s competing process just a few days after Daguerre’s.

“In January of 1839, stimulated by Talbot’s announcement of his invention of photogenic drawing, Herschel took up the study of photographic phenomena. Within a week he had solved the problem of silver fixation. In contrast to Talbot’s single-minded pursuit of the silver image, however, Herschel soon began to widen his investigations in the search for other viable photographic systems … To the enduring benefit of the embryonic science of photography, the spring of 1840 was
remarkably brilliant. Herschel had already initiated a new series of exposure tests of ‘vegetable colours’ using extracts of the juices of plants and flowers, but this work was interrupted in March by the removal of the family home from Slough in Buckinghamshire to Hawkhurst in Kent. Once resettled, Herschel resumed his experiments in August, but by then he frequently found the sun to be ‘pale’ or ‘desultory’, requiring long exposures for these very insensitive processes. He pursued them nonetheless, during the very poor summer of 1841 that followed ...

In the early spring of 1842, Herschel suspended his tests of plant colours in favour of further broadening his search for new photosensitive substances; his attention transferred from these rather evanescent organic dyes (now appropriately called anthocyanins) to deeply-coloured inorganic compounds” (Ware, p. 23).

“Early in 1842, the electro-chemist Alfred Smee sent Herschel a quantity of the bright red compound called potassium ferricyanide. While testing the sensitivity of this substance under the light of the spectrum, Herschel noted that it acted with much the same sensitivity as guaiacum, and when thrown into water, it became a deep Prussian blue. Smee suggested two further compounds, Ammonio Citrate and Ammonio Tartrate of Iron, and by June of 1842, Herschel had developed both the Chrysotype, named for its use of gold ‘to bring about the dormant picture,’ and the Cyanotype, his most practical and enduring process” (Hannavy, Encyclopedia of Nineteenth-Century Photography, p. 655).

“The 15th of June 1842 was the day on which Herschel’s long and important paper, entitled ‘On the Action of the Rays of the Solar Spectrum on Vegetable Colours, and on some new Photographic Processes’ was accepted for publication in the Philosophical Transactions of the Royal Society. Part of this paper was read before the Society on 16 June, but it was not to appear in print until September. It will be convenient to refer to this seminal work as ‘the 1842 Paper’. It was here that Herschel first publicly described the making of prints in Prussian blue from
potassium ferricyanide alone, as follows:

‘202. A beautiful example of such deoxidising action on a non-argentine compound has lately occurred to me in the examination of that interesting salt, the ferrosesquicyanuret of potassium, described by Mr. Smee in the Philosophical Magazine, No.109, September 1840, and which he has shown how to manufacture in abundance and purity by voltaic action on the common, or yellow ferrocyanuret.

203. Paper simply washed with a solution of this salt is highly sensitive to the action of light. Prussian blue is deposited. After half an hour or an hour's exposure to sunshine, a very beautiful negative photograph is the result’ …

“It is clear from the 1842 Paper that, by June, Herschel had also made cyanotypes (but not yet named them thus) by exposing a mixture of ammonium ferric citrate and potassium ferricyanide – the recipe that has endured as the standard practice until the present day:

‘206. If in lieu of the perchloride of iron, we substitute a solution of that curious salt the ammonio-citrate of iron, the photographic effects are among the most various and remarkable that have yet offered themselves to our notice in this novel and fertile field of inquiry. The two solutions mix without causing any precipitate, and produce a liquid of a brown colour, which washed over paper is green (being strongly dichromatic). If this be done under the prism, the action of the spectrum is almost instantaneous, and most intense. A copious and richly coloured deposit of Prussian blue is formed over the blue, violet and extra-spectral rays’ …

“Prussian blue printing was totally eclipsed during the intervening month of July by Herschel's endeavours - again employing the marvellously versatile ammonium ferric citrate - to make pictures in mercury and gold, whose striking beauty captivated his undivided attention … The following week in August, however, saw Herschel's return to printing in Prussian blue, with fresh endeavours to refine and perfect the processes. His chemical logic suggested that a complementary, positive-working system could be achieved by employing, not the ferricyanide, but the commonplace ferrocyanide of potassium, in conjunction with ammonium ferric citrate … By the end of August he had accumulated sufficient new results to justify adding a substantial postscript to his 1842 Paper, which was still awaiting publication by the Royal Society. It is in this postscript, dated 29 August 1842, that the name ‘cyanotype’ appears in print for the first time” (Ware, ‘John Herschel's Cyanotype: invention or discovery?’).

“Herschel's experiments on photographic subjects came to a halt in 1843, victims of his astronomical writing and public duties. But his interest in photography never ceased. Anna Atkins, a close friend of the Herschel family, immediately took up the cyanotype in her self-publishing effort in Botany. Julia Margaret Cameron declared that Sir John was ‘her first teacher’ and immortalized him in a series of portraits. In 1845 Herschel published his final contribution to photographic research, an observation of what he called 'epipolic dispersion' (nos. 46 & 47). George Gabriel Stokes would later rename this phenomenon 'fluorescence', the study of which led directly to radiation photography of all types” (Hannavy, p. 655).

"At first, Herschel's invention was only taken up by a small elite of amateur botanists for the purposes of plant illustration. The most notable achievement was that of Anna Atkins who, during two decades from 1843, produced her now famous and highly-treasured album of botanical photographs [Photographs of British Algae: cyanotype impressions]. But in the Great Exhibition of 1851, the cyanotype process was represented by just one minor specimen, among a multitude of exhibits illustrating the burgeoning art-science of photography. This
must be taken as a sign of the insignificant status accorded to cyanotype at the time. It continued in disuse for a further 20 years. Following Herschel’s death in 1872, the ‘re-invention’ of cyanotype by entrepreneurs of a more commercial turn of mind than the inventor, exploited its potential as a reprographic medium for the first time. The re-styles ‘ferroprussiate’ process also found some use among photographers as a cheap and easy option for proofing negatives, but its major market was for copying the plans in every drawing office …

“In Britain, the cyanotype has suffered an almost total aesthetic boycott by photographic artists, connoisseurs, and curators until the last decade or two. By contrast, one can point to huge archives of cyanotypes where the utility of the process was the paramount consideration. The commercial success of the cyanotype process was owed, not to its pictorial use, but its reprographic applications. These have endowed our language with a new word: ‘blueprint,’ a word that has now taken on an expanded and more abstract meaning which endures long after the process it described became obsolete. The era of the blueprint as a copying process was heralded by the manufacture of cheap, sensitized paper in huge quantities. By the turn of the century its use for copying engineering and architectural plans had become universal in drawing offices. In 1918, a 30-foot roll of cyanotype paper a yard wide could be purchased for as little as 1s. 6d. This was just as well, because consumption must have been enormous: the plans for a battleship, for instance, consumed 11,000 square feet of the material. Although it faced two or three competing processes, the blueprint held sway for 80 years as the foremost industrial reproduction process, and was only finally displaced in the mid-1950s in the UK, first by the diazo print medium, and then by the invention of electrophotography, which enabled photocopying by entirely dry methods. It is interesting to note that commercial production and use of blueprint paper, though dwindling, was still significant in 1972 in the USA, and production of the paper persists there still, albeit on a very reduced scale” (Ware, pp. 12-14).
John Frederick William Herschel (1792-1871) was born at Observatory House in Slough, where his father, the Astronomer Royal Sir William Herschel, discovered the planet Uranus. “As a result, Observatory House was a scientific landmark and it was visited throughout John Herschel’s childhood by royalty, gentry and scientists from all parts of the world. Growing up in such a household and under the influence of his renowned father and aunt, the astronomer Caroline Lucretia Herschel, it is hardly surprising that John Herschel acquired his own fame in astronomical and mathematical subjects. But, as he wrote to his wife Margaret in 1841, ’Light was my first love.’ And it was through this lifelong interest in the properties and vagaries of light that he came to photography” (Hannavy, p. 653).

“Herschel played important roles in major scientific organizations—including the Royal Society, the Astronomical Society (later the Royal Astronomical Society), and the British Association for the Advancement of Science—became a member of dozens of others throughout Europe, and served as an advisor to numerous national committees and large-scale, data-gathering projects, such as the mapping of Earth’s magnetic field and the collecting of meteorological data” (DSB).

Michel-Eugène Chevreul (1786-1889) spent the first two decades of his working life as an organic chemist. In 1824, “soon after his appointment that year as director of dyeing at the tapestry works in Gobelin, he received complaints about the lack of vigour in tapestry colours. He found that the problem was not chemical in nature but optical. His lengthy investigation into the optical mixing of colours led to his finding several types of contrast of colour and tone and a formulation of the law of simultaneous contrast: colours mutually influence one another when juxtaposed, each imposing its own complementary colour on the other. The fruit of his colour studies was De la loi du contraste simultané des couleurs (1839), his most influential book. He provided many examples of how juxtaposed colours can enhance or diminish each other’s intensity, and he described many ways to produce desired colour effects, such as with massed monochromatic dots. To represent colours by definite standards, he brought together all of the colours of the visible spectrum, relating them to each other in a circular system, and he also produced scales of thousands of tints. He applied his findings to Gobelin tapestries and textiles, wallpaper, horticulture, mapmaking, colour printing, mosaics, and painting. Indeed, he ‘wrote the book’ for artists, designers, and decorators. His book, with its English and German translations, became the most widely used colour manual of the 19th century” (Britannica).

Boni, Photographic Literature, p. 91; Poggendorff I, 1090.
A wonderfully revealing letter, in which Herschel (1792-1871) accepts the results of Lyell's seminal 1858 paper on volcano formation, *On the Structure of Lavas which have consolidated on Steep Slopes: with Remarks on the Mode of Origin of Mount Etna and on the Theory of 'Craters of Elevation',' and adds his own observations on the implausibility of the craters of elevation theory. Lyell advocated the modern view that volcanoes are built up gradually from lava solidifying after multiple eruptions, possibly from different points. The craters of elevation theory, put forward in 1825 by Leopold von Buch, proposed that volcanoes formed like a bubble on Earth's crust which is then subject to upheaval to form a mountain; only later does magma sometimes emerge from the underground, causing a volcanic eruption. Over a period of 30 years Lyell visited Etna multiple times and gathered data from around the world. Through the 1840s his own gradualist theory of volcano formation was attacked by such luminaries as Alexander von Humboldt and even Charles Darwin, but by the 1850s the craters of elevation theory was the last remaining challenge to the acceptance of his famous 'uniformitarian' theory, first developed
in the 1820s and expounded at length in his masterpiece, *Principles of Geology*. In the 1858 Etna paper, Lyell “dissected every assumption, every misconstruction of evidence and every element of reasoning in the theory of craters of elevation to bring the whole preposterous edifice crashing to the ground” (Wilson, in Blundell & Scott, p. 31). Given the earlier doubts of his contemporaries, the present letter must have delighted Lyell. While geology was not one of the primary concerns of the great polymath Herschel, he had engaged with Lyell’s uniformitarianism more than twenty years earlier: Charles Babbage had published a lengthy letter from Herschel to Lyell in *The Ninth Bridgewater Treatise* in which Herschel had given his support to Lyell’s overarching theory, and discussed his own theories of volcano formation closely related to those in the present letter. Herschel letters with significant scientific content are rare on the market. The letter reads:

*My dear Sir Charles Lyell*

*I am very glad indeed to receive from yourself the account of the results of your researches on Etna of which I have already heard a great deal & of the interest attending to them. I have never held a very firm faith in the doctrine of ‘Craters of Elevation’ which always appeared to me not only gigantesque but open to the obvious query how it could have happened that such huge air bubbles should have been gas-tight during the time required for their elevation and cooling – and you seem to have given the theory its coup de grace.*

*Lady H. desires me to add her kind regards to Lady Lyell in which I may join.*

“Leopold von Buch had put forward the ‘craters of elevation’ theory in 1825 to explain the origin of large bowl-shaped depressions associated with volcanoes, such as the Caldera of Palma in the Canary Islands, and the Gulf of Santorin in the Grecian archipelago. Buch postulated that the Caldera of Palma and similar bowl-shaped valleys had been formed by a sudden, explosive upheaval within the volcano, elevating accumulated layers of lava and leaving a large circular crater at the site of the explosion. The theory was intended to explain both the great circular valley at the center of a volcanic mountain and the fact that on all sides the sheets of lava sloped away from the central valley.

“In 1830 in the first volume of the *Principles*, Lyell objected to the theory, noting that it was not founded on any comparable effect produced by a modern volcano or earthquake. All modern volcanic cones and craters were produced by volcanic eruptions. None resulted in a truncated cone with a great cavity in the center. Buch’s craters of elevation always occurred in the midst of extinct volcanoes. The theory required that volcanic rocks must first accumulate in horizontal beds to a depth of several thousand feet, an accumulation that could only occur in the vicinity of a volcanic vent. Then by a sudden explosion the beds were heaved up thousands of feet. Lyell objected that ‘instead of being shattered, contorted, and thrown into the utmost disorder, [the beds] have acquired that gentle inclination, and that regular and symmetrical arrangement, which characterize the flanks of a large cone of eruption, like Etna!’” (Wilson, *Lyell in America*, p. 314).

“In October 1857 Lyell revisited southern Italy and Sicily, where he had not been since 1828, to make a fresh study of the structure of Vesuvius and Etna. In 1853–1854, in his study of Madeira and the Canaries, made with Hartung, Lyell had become convinced that the evidence on which Leopold von Buch had founded his theory of craters of elevation was completely fallacious. According to Buch’s theory, the volcanic rocks forming such islands as Tenerife and Palma had been formed originally as horizontal sheets of lava which had later been upheaved in a great convulsion to create the cones and craters of their modern volcanoes. Buch believed that liquid lavas could not solidify to form sheets of solid rock on the steep slopes where they now occurred. Therefore, they must have been solidified
in a horizontal position and later upheaved. In 1834 Élie de Beaumont, in a paper on Mount Etna, argued that the solid beds of lava in the Val del Bove, inclined at angles of 28° and more, resembled portions of modern lavas that flowed over ground almost level or inclined at no more than 3°. Geologists who accepted Élie de Beaumont's conclusions were forced to believe that all modern volcanoes had acquired their conical form by later upheaval of their beds of lava.

“In 1853–1854, on Madeira and Palma, Lyell had seen modern lavas forming sheets of solid rock and inclined at angles of 15° to 20° but showing no sign of any disturbance in position. In 1855 Hartung had also observed on Lanzarote in the Canaries a solid basaltic lava on a slope of 30°. In 1857 Lyell found on Etna lavas which had solidified on steep slopes of from 15° to 40° in inclination. The lavas formed continuous sheets of rock alternating with layers of loose scoriae above and beneath them. Lyell reported these results to the Royal Society on 10 June 1858, and in September and October 1858 he returned to Etna to make a more thorough study of its structure. He found that Etna had neither a linear axis nor a single center of upheaval. Instead, he found two earlier centers of eruption in the Val del Bove. From each center the beds of lava sloped away in all directions, but beds of lava arising from Etna's modern center of eruption had flowed over and buried those from the earlier centers. Lyell considered this to be decisive evidence against the crater-of-elevation theory because 'although one cone of eruption may envelope and bury another cone of eruption, it is impossible for a cone of upheaval to mantle round and overwhelm another cone of upheaval so as to reduce the whole mass to one conical mountain.' He concluded that the conical form of Etna, as of all volcanoes, was entirely the result of the long-continued process of volcanic eruption” (DSB).

A volcanic eruption in the Mediterranean Sea, which led to the formation of the island of Ferdinandea, later confirmed Lyell's observations and made it clear that volcanic mountains do not form like a bubble, but rather grow over time.

John Herschel is today best known as an astronomer, mathematician, physicist, and one of the founders of photography, but he had a life-long interest in geology. In a long letter to Lyell written on February 20, 1836, and reproduced in Babbage's Ninth Bridgewater Treatise (pp. 202-213), Herschel refers to Lyell's Principles as "one of those productions which work a complete revolution in their subject, by altering entirely the point of view in which it must thence-forward be contemplated" (p. 203). He goes on to ask, "Has it ever occurred to you to speculate on the probable effect of the transfer of pressure from one part to another of the earth's surface by the degradation of existing and the formation of new continents – on the fluid or semi-fluid matter beneath the outer crust?" (p. 204). Herschel admits that "It has always been my greatest difficulty in Geology to find a primum mobile for the volcano, taken as a general, not a local phenomenon" (p. 205). In the remainder of the letter Herschel sets out a detailed and closely argued theory for the origin of volcanoes. What influence this might have had on Lyell is uncertain – Herschel's theory is not identical to Lyell's – but it is clear that Herschel realized just as Lyell did the importance of volcanoes in understanding fundamental geological processes.

Ten weeks after Herschel wrote this letter, on July 18, Charles Darwin, who by no means always agreed with Lyell's theories, wrote to Lyell to thank him for sending an abstract of his Etna paper, adding “It seems to me a very grand contribution to our volcanic knowledge. Certainly I never expected to see E. de B's [Élie de Beaumont] theory of slopes so completely upset. He must have picked out favourable cases for measurement. And such an array of facts he gives! You have scotched, and will see die, I think, the Crater of Elevation theory. But what vitality there is in a plausible theory!”

Wilson, 'Lyell: the man and his times,’ pp. 21-37 in: Lyell: the Past is the Key to the Present (Blundell & Scott, eds.), 1998.
If Leader of Swedenia's ships always appeared to me the most
excessive and near to the sea our journey now is calmer and more
happier that sent hope and wonder should have been
prosperous during the time expected for their Cleveland
and casting - and your,
return to home unless the
hurry of camp de Passe.
Lucky you, then, he so did
her-kind behalf to Lucy.
PMM 205 - THE MEASUREMENT OF LIGHT

LAMBERT, Johann Heinrich. *Photometria sive de mensura et gradibus luminis, colorum et umbræ*. Augsburg: Christoph Peter Detlefsen for the widow of Eberhard Klett, 1760.

$68,000

8vo (171 x 108 mm), fine contemporary German blind-tooled calf, all edges gilt, traces of clasps, hinges and capitals with scifull leather restoration, text and plates with light uniform browning (as is usually the case with this work), pp [xvi] 547 [13: index] and 8 engraved folding plates, a very fine copy.

First edition, and a remarkably fine copy, of this cornerstone of modern optics, with applications which touch on astronomy and photography; this is one of the rarest of modern science books of this stature. “It established a complete system of photometric quantities and principles; using them to measure the optical properties of materials, quantify aspects of vision, and calculate illumination” (Wikipedia, accessed 13/05/19). Lambert’s discoveries “are of fundamental importance in astronomy, photography and visual research generally … Both Kepler and Huygens had investigated the intensity of light, and the first photometer had been constructed by Pierre Bouguer (1698-1758); but the foundation of the science of photometry – the exact scientific measurement of light – was laid by Lambert’s ‘Photometry’ … In the *Photometria* he described his photometer and propounded the law of the absorption of light named after him. He investigated the principles and properties of light, of light passing through transparent media, light reflected from opaque surfaces, physiological optics, the scattering of light
passing through transparent media, the comparative luminosity of the heavenly bodies and the relative intensities of coloured lights and shadows” (PMM). “In his famous Photometria sive de mensure et gradibus luminis, colorum et umbrae (Augsburg, 1760), Lambert laid the foundation for this branch of physics … [he] carried out his experiments with few and primitive instruments, but his conclusions resulted in laws that bear his name. The exponential decrease of the light in a beam passing through an absorbing medium of uniform transparency is often named Lambert’s law of absorption, although Bouguer discovered it earlier. Lambert’s cosine law states that the brightness of a diffusely radiating plane surface is proportional to the cosine of the angle formed by the line of sight and the normal to the surface. Such a diffusely radiating surface does therefore appear equally bright when observed at different angles, since the apparent size of the surface also is proportional to the cosine of the said angle” (DSB). ABPC/RBH record the sale of four copies in the last 30 years (Christie’s, November 23, 2011, lot 66, £27,500 = $43,118; Christie’s NY, June 16, 1998, lot 591, £32,200 (Norman copy); Sotheby’s, March 14, 1996, lot 229, £24,150 = $36,899 (Madsen copy); Christie’s NY, April 22, 1994, lot 38, $24,150 (Horblit copy)). OCLC lists copies in US at Brown, Harvard Medical School and Oklahoma.

“Photometria was the first work to accurately identify most fundamental photometric concepts, to assemble them into a coherent system of photometric quantities, to define these quantities with a precision sufficient for mathematical statement, and to build from them a system of photometric principles. These concepts, quantities, and principles are still in use today.

“Lambert began with two simple axioms: light travels in a straight line in a uniform medium and rays that cross do not interact. Like Kepler before him, he recognized that ‘laws’ of photometry are simply consequences and follow directly from these two assumptions. In this way Photometria demonstrated (rather than assumed) that

1. Illuminance varies inversely as the square of the distance from a point source of light.
2. Illuminance on a surface varies as the cosine of the incidence angle measured from the surface perpendicular.
3. Light decays exponentially in an absorbing medium.

“In addition, Lambert postulated a surface that emits light (either as a source or by reflection) in a way such that the density of emitted light (luminous intensity) varies as the cosine of the angle measured from the surface perpendicular. In the case of a reflecting surface, this form of emission is assumed to be the case, regardless of the light’s incident direction. Such surfaces are now referred to as ‘Perfectly Diffuse’ or ‘Lambertian’.

“Lambert demonstrated these principles in the only way available at the time: by contriving often ingenious optical arrangements that could make two immediately adjacent luminous fields appear equally bright (something that could only be determined by visual observation), when two physical quantities that produced the two fields were unequal by some specific amount (things that could be directly measured, such as angle or distance). In this way, Lambert quantified purely visual properties (such as luminous power, illumination, transparency, reflectivity) by relating them to physical parameters (such as distance, angle, radiant power, and color). Today, this is known as ‘visual photometry.’ Lambert was among the first to accompany experimental measurements with estimates of uncertainties based on a theory of errors and what he experimentally determined as the limits of visual assessment.

“Although previous workers had pronounced photometric laws 1 and 3, Lambert established the second and added the concept of perfectly diffuse surfaces. But more importantly, as Anding pointed out in his German translation of Photometria
Lambert had incomparably clearer ideas about photometry and with them established a complete system of photometric quantities. Based on the three laws of photometry and the supposition of perfectly diffuse surfaces, *Photometria* developed and demonstrated the following:

1. **Just noticeable differences.** In the first section of *Photometria*, Lambert established and demonstrated the laws of photometry. He did this with visual photometry and to establish the uncertainties involved, described its approximate limits by determining how small a brightness difference the visual system could determine.

2. **Reflectance and transmittance of glass and other common materials.** Using visual photometry, Lambert presented the results of many experimental determinations of specular and diffuse reflectance, as well as the transmittance of panes of glass and lenses. Among the most ingenious experiments he conducted was that to determine the reflectance of the interior surface of a pane of glass.

3. **Luminous radiative transfer between surface.** Assuming diffuse surfaces and the three laws of photometry, Lambert used Calculus to find the transfer of light between surfaces of various sizes, shapes, and orientations. He originated the concept of the per-unit transfer of flux between surfaces and in *Photometria* showed the closed form for many double, triple, and quadruple integrals which gave the equations for many different geometric arrangements of surfaces. Today, these fundamental quantities are called View factors, Shape Factors, or Configuration Factors and are used in radiative heat transfer and in computer graphics.

4. **Brightness and pupil size.** Lambert measured his own pupil diameter by viewing it in a mirror. He measured the change in diameter as he viewed a larger or smaller part of a candle flame. This is the first known attempt to quantify pupillary light reflex.
5. Atmospheric refraction and absorption. Using the laws of photometry and a great deal of geometry, Lambert calculated the times and depths of twilight.

6. Astronomic photometry. Assuming that the planets had diffusely reflective surfaces, Lambert attempted to determine the amount of their reflectance, given their relative brightness and known distance from the sun. A century later, Zöllner studied Photometria and picked up where Lambert left off, and initiated the field of astrophysics.

7. Demonstration of additive color mixing and colorimetry. Lambert was the first to record the results of additive color mixing. By simultaneous transmission and reflection from a pane of glass, he superimposed the images of two different colored patches of paper and noted the resulting additive color.

8. Daylighting calculations. Assuming the sky was a luminous dome, Lambert calculated the illumination by skylight through a window, and the light occluded and interreflected by walls and partitions.

"Lambert's book is also mathematical. Though he knew that the physical nature of light was unknown (it would be 150 years before the wave-particle duality was established) he was certain that light's interaction with materials and its effect on vision could be quantified. Mathematics was for Lambert not only indispensable for this quantification but also the indisputable sign of rigor. He used linear algebra and calculus extensively with a matter-of-fact confidence that was uncommon in optical works of the time. On this basis, Photometria is certainly uncharacteristic of mid-18th century works.

"Lambert's book is fundamentally experimental. The forty experiments described in Photometria were conducted by Lambert between 1755 and 1760, after he decided to write a treatise on light measurement. His interest in acquiring experimental data spanned several fields: optics, thermometry, pyrometry, hydrometry, and magnetics. This interest in experimental data and its analysis, so evident in Photometria, is also present in other articles and books Lambert produced. For his optics work, extremely limited equipment sufficed: a few panes of glass, convex and concave lenses, mirrors, prisms, paper and cardboard, pigments, candles and the means to measure distances and angles.

"Lambert began conducting photometric experiments in 1755 and by August 1757 had enough material to begin writing. From the references in Photometria and the catalogue of his library auctioned after his death, it is clear that Lambert consulted the optical works of Newton, Bouguer, Euler, Huygens, Smith, and Kästner. He finished Photometria in Augsburg in February 1760 and the printer had the book available by June 1760.

"Maria Jakobina Klett (1709–1795) was owner of Eberhard Klett Verlag, one of the most important Augsburg 'Protestant publishers.' She published many technical books, including Lambert's Photometria, and 10 of his other works. Klett used Christoph Peter Detleffsen (1731–1774) to print Photometria. Its first and only printing was evidently small, and within 10 years copies were difficult to obtain. In Joseph Priestley's survey of optics of 1772 [The History and Present State of Discoveries relating to Vision, Light, and Colours], 'Lambert's Photometrie' appears in the list of books not yet procured. Priestley makes a specific reference to Photometria; that it was an important book but unprocurable.

"Photometria presented significant advances and it was, perhaps, for that very reason that its appearance was greeted with general indifference. The central optical question in the middle of the 18th century was: what is the nature of light?
Lambert work was not related to this issue at all and so *Photometria* received no immediate systematic evaluation, and was not incorporated into the mainstream of optical science. The first appraisal of *Photometria* appeared in 1776 in Georg Klügel's German translation of Priestley’s 1772 survey of optics. An elaborate reworking and annotation appeared in 1777. *Photometria* was not seriously evaluated and utilized until nearly a century after its publication, when the science of astronomy and the commerce of gas lighting had need for photometry. Fifty years after that, *Illuminating Engineering* took up Lambert's results as the basis for lighting calculations that accompanied the great expanse of lighting early in the 20th century. Fifty years after that, computer graphics took up Lambert's results as the basis for radiosity calculations required to produce architectural renderings. *Photometria* had significant, though long delayed influence on technology and commerce once the industrial revolution was well underway, and is the reason that it was one of book listed in *Printing and the Mind of Man*” (Wikipedia).

“Johann Heinrich Lambert was born on 26 August 1728 in Mühlhausen (today Mulhouse, France). Mühlhausen was at that time associated to Switzerland. Lambert received six years of formal education from the municipality but had to leave school to help his father, a tailor, when he was 12 years old. However, Lambert never stopped learning though he did not attend any formal school afterwards. He studied French, Latin and Mathematics largely on his own. He became an assistant to the city clerk of Mühlhausen, J. H. Reber, then a bookkeeper to an industrialist and finally in 1746 a secretary to Prof. J. R. Iselin in Basel. In this position he gained access to the knowledge of physics and mathematics of his time. In 1748 he obtained a position in Chur as a private tutor to a grandson of Count Peter von Salis. At the court of von Salis, Lambert could finally pursue his research on physics and optics. He travelled with his pupil through Europe, meeting many eminent scientists and continuously pursuing his research. He
became a member of the ‘physikalisch-mathematische Gesellschaft’ of Basel in 1754. From 1759 Lambert travelled on his own through Europe. During that time he published his early masterpiece, the *Photometria*.

“Lambert was living in rather poor conditions, though he received some support from the academies he was a member of. After long deliberations and in spite of Lambert’s eccentric character he became a member of the Royal Academy of Berlin in 1765. Finally Lambert had a secure post and he started researching and publishing on diverse topics of his interest. In this time of high productivity he proved that π and e are irrational, wrote about philosophy, studied non-additive probabilities and made contributions to hyperbolic functions and to cartography. Lambert died in Berlin on 25 September 1777” (Hulliger, pp. 2-3).

NON-EUCLIDEAN GEOMETRY

LAMBERT, Johann Heinrich. Theorie der Parallellinien. Leipzig: [np], 1786.

$12,500

8vo (203 x 116 mm). Contained in: Leipziger Magazin für reine und angewandte Mathematik, which was a relatively minor and short-lived (1786–1789) mathematical journal published by Johann III Bernoulli and Carl Friedrich Hindenburg. Lambert’s paper is pp. 137-164 and pp. 325-358 of the 1st volume (1786) and is accompanied by 2 engraved plates. Offered here is a very fine copy of the relevant volume ([2], 556 pp. and 8 plates) bound in contemporary German boards with richly gilt spine – a beautiful and unmarked copy.

First edition, very rare, and a copy with excellent provenance, of one of the most important works on non-Euclidean geometry preceding those of Bolyai and Lobachevsky half a century later. Lambert derived several fundamental results in this subject, and “no one else came so close to the truth without actually discovering non-Euclidean geometry” (Boyer, History of Mathematics, p. 504). “The memoir Theorie der Parallellinien (Theory of parallel lines) by Johann Heinrich Lambert (1727-1777), written probably in 1766, is a masterpiece of mathematical literature, and its author is one of the most outstanding minds of all times” (Papadopoulos & Théret). “In the introductory part of his treatise Lambert wrote: ‘This work deals with the difficulty encountered in the very beginnings of geometry and which, from the time of Euclid, has been a source of discomfort for those who do not just blindly follow the teachings of others but look for a basis for their convictions and do not wish to give up the least bit of the rigor found in most proofs. This difficulty immediately confronts every reader of Euclid’s Elements, for
it is concealed not in his propositions but in the axioms with which he preface the first book” (Rosenfeld, A History of Non-Euclidean Geometry, p. 99). This difficulty was the question of whether Euclid’s ‘Parallel Postulate’ – that through any given point not on a given straight line one can draw exactly one straight line that is parallel to (i.e., which does not intersect) the given line – could be deduced from the other axioms of Euclidean geometry. Girolamo Saccheri, in his Euclides ab omni naevo vindicatus (1733), had deduced many interesting consequences of denying the parallel postulate, but had ultimately concluded, erroneously, that denying it led to a contradiction. Lambert was the first to realize “that Euclid’s Parallel Postulate cannot be proved from the other Euclidean postulates and that it is possible to build a logically consistent system satisfying the other postulates but explicitly rejecting the Parallel Postulate” (Parkinson, Breakthroughs, 1766 & 1786). OCLC lists no copies in US; no copies on ABPC/RBH.

Provenance: Max Steck (1907-71), German-Swiss mathematician and mathematical historian (bookplate on front paste-down). Steck was the editor of Johann Heinrich Lambert: Schriften zur Perspektive (Berlin, 1943), which contains a Bibliographia Lambertiana (reprinted separately, Hildesheim, 1970).

Lambert became interested in the parallel postulate after having heard of Georg Simon Klügel’s dissertation Conatum praecipuorum theoriarum parallelarum demonstrandi from 1763, in which he had shown the flaws of all proofs so far of the parallel postulate. This inspired Lambert to take up the subject himself. Like Saccheri’s Euclides vindicatus, “Lambert wrote his Theorie der Parallellinien in an attempt to prove, by contradiction, the parallel postulate. He deduced remarkable consequences from the negation of that postulate. These consequences make his memoir one of the closest (probably the closest) text to hyperbolic geometry, among those that preceded the writings of Lobachevsky, Bolyai and Gauss. We recall by the way that hyperbolic geometry was acknowledged by the mathematical community as a sound geometry only around the year 1866, that is, one hundred years after Lambert wrote his memoir.

“To give the reader a feeling of the wealth of ideas developed in Lambert’s memoir, let us review some of the statements of hyperbolic geometry that it contains. Under the negation of Euclid’s parallel postulate, and if all the other postulates are untouched, the following properties hold:

1. The angle sum in an arbitrary triangle is less than 180°.

2. The area of triangles is proportional to angle defect, that is, the difference between 180° and the angle sum.

3. There exist two coplanar disjoint lines having a common perpendicular and which diverge from each other on both sides of the perpendicular.

4. Given two coplanar lines \(d_1\) and \(d_2\) having a common perpendicular, if we elevate in the same plane a perpendicular \(d_3\) to \(d_1\) at a point which is far enough from the foot of the common perpendicular, then \(d_3\) does not meet \(d_2\).

5. Suppose we start from a given point in a plane the construction of a regular polygon, putting side by side segments having the same length and making at the junctions equal angles having ascertain value between 0 and 180°. Then, the set of vertices of these polygons is not necessarily on a circle. Equivalently, the perpendicular bisectors of the segment do not necessarily intersect.

6. There exist canonical measures for length and area.

“Property (6) may need some comments. There are several ways of seeing the
existence of such a canonical measure. For instance, we know that in hyperbolic
gometry, there exists a unique equilateral triangle which has a given angle which
we can choose in advance (provided it is between 0 and 60°). This establishes a
bijection [one-to-one correspondence] between the set of angles between 0 and
60° and the set of lengths. We know that there is a canonical measure for angles
(we take the total angle at each point to be equal to four right angles.) From the
above bijection, we deduce a canonical measure for length. This fact is discussed
by Lambert in §80 of his memoir. Several years after Lambert, Gauss noticed the
same fact. In a letter to his friend Gerling, dated April 11, 1816 (cf. C. F. Gauss,
Werke, Vol. VIII, p. 168), he writes: "It would have been desirable that Euclidean
gometry be not true, because we would have an a priori universal measure. We
could use the side of an equilateral triangle with angles 59°59'59,9999" as a unit
of length". We note by the way that there is also a canonical measure of lengths in
spherical geometry, and in fact, a natural distance in this geometry is the so-called
'angular distance'.

"It also follows from Lambert's memoir that in some precise sense there are
exactly three geometries, and that these geometries correspond to the fact
that in some (equivalently, in any) triangle the angle sum is respectively equal,
greater than, or less than two right angles. This observation by Lambert is at
the basis of the analysis that he made of the quadrilaterals that are known as
Lambert quadrilaterals, or Ibn al-Haytham–Lambert quadrilaterals. These are the
triangular quadrilaterals (that is, quadrilaterals having three right angles), and
Lambert studied them systematically, considering successively the cases where
the fourth angle is obtuse, right or acute. It is fair to note here that Lambert was
not the first to make such an analysis in the investigation of the parallel problem,
and we mention the works of Gerolamo Saccheri (1667-1773) and, before him,
suggested by Lambert's and his predecessors' analysis correspond to constant
zero, positive or negative curvature respectively, but of course Lambert and his predecessors did not have this notion of curvature. The interpretation of the three geometries in terms of curvature was given one century after Lambert’s work, by Beltrami.

“Another important general property on which Lambert made several comments and which he used thoroughly in his memoir is the following: there exist strong analogies between statements in the three geometries (Euclidean, spherical and hyperbolic), with the consequence that some of the statements in the three geometries may be treated in a unified manner. More precisely, he noticed that there exist propositions that are formally identical in the three geometries up to inverting some inequalities or making them equalities. A well-known example is the fact that in Euclidean (respectively spherical, hyperbolic) geometry, the angle sum of triangles is equal to (respectively greater than, smaller than) two right angles … There exist several statements of the same type, in which one passes from one geometry to the other by inverting certain inequalities. Euclidean geometry appears in this setting as the frontier geometry between spherical and Euclidean geometries. In relation with this, Lambert noticed that certain formulae of hyperbolic geometry can be obtained by replacing, in certain formulae of spherical geometry, distances by the same distances multiplied by the imaginary number $\sqrt{-1}$, and by keeping angles untouched. One well-known example is the following: Take, as a model of spherical geometry, the sphere of radius $r$ (or curvature $\frac{1}{r^2}$). Recall that the area of a spherical triangle is equal to

$$r^2(\alpha + \beta + \gamma - \pi),$$

where $\alpha$, $\beta$, $\gamma$ are the angles (in radians). This result is attributed to Albert Girard (1595-1632), who stated it in his Invention nouvelle en algebra (1629). If instead of $r$ we take an imaginary radius $\sqrt{-1}r$, we obtain, as a formula for area,

$$-r^2(\alpha + \beta + \gamma - \pi) = r^2(\pi - \alpha - \beta - \gamma),$$

which is precisely the area of a triangle of angles $\alpha$, $\beta$, $\gamma$ in the hyperbolic space of constant curvature $-1/r^2$. Lambert declares at the occasion of a closely related idea that ‘we should almost conclude that the third hypothesis occurs on an imaginary radius’” (Papadopoulos & Théret).

“Lambert’s memoir is divided into three parts: §1 to 11, §12 to 26, and §27 to 88. The central ideas are the following.

“In the first part, the author recalls the problem of parallels, presenting Euclid’s eleventh axiom, and the position it occupies among the propositions and the other axioms of the Elements. He mentions several difficulties presented by this axiom, quoting commentaries and attempts at proofs by his predecessors. Lambert, who was a fervent reader of classical literature, certainly knew the works of the Greek commentators and their successors on the parallel problem. Furthermore, he was aware of Klügel’s dissertation, written in 1763, which contains a description of 28 attempts to prove the parallel axiom. In particular, Lambert knew about Saccheri’s work. It is also good to note that Lambert had probably no intention to publish his manuscript in the state it reached us, which explains the fact that certain historical references (in particular to Saccheri) are missing in that manuscript.

“In the second part, Lambert presents some propositions of neutral geometry, that is, the geometry based on the Euclidean axioms from which the parallel axiom has been deleted. One reason for which he works out these propositions is that he thinks that they may be used to prove the parallel axiom.

“The third part is the most important part of the memoir. Lambert presents his
own approach to prove the parallel axiom. He develops a theory based on the
negation of that axiom, hoping that it will lead to a contradiction” (ibid.).

After Lambert's death in 1777, the Berlin Academy bought Lambert's Nachlass, his
unpublished manuscripts, notes and correspondence, on the recommendation of
the Swiss mathematician Johann Georg Sulzer (1720-79). Following Sulzer's death
two years later, the task of editing the Nachlass was taken over by Johann III Bernoulli
(1744-1807). The Academy sold Bernoulli the Nachlass on the condition that he
made a large portion of it available to the public. In 1782 Bernoulli inserted a note
on the Nachlass in the widely read journals Allerneueste Mannigfaltigkeiten and
Teutscher Merkur. He also published a posthumous manuscript of Lambert in the
Mémoires of the Berlin Academy, he edited the first volume of Lambert’s Logische
und Philosophische Abhandlungen, and published the first volume of Lambert’s
Deutscher gelehrter Briefwechsel. In subsequent years, Bernoulli published a second
volume of the Logische und Philosophische Abhandlungen and four more volumes
of the Deutscher gelehrter Briefwechsel (1781-87). Bernoulli published Lambert's
manuscripts on mathematics and physics in the journals that his friend Carl
Friedrich Hindenburg (1741-1808) edited, the short-lived Leipziger Magazin für
reine und angewandte Mathematik (1786-89) and Archiv für reine und angewandte
Mathematik (1795-99). Finally Bernoulli sold the Nachlass to the Duke of Gotha,
in whose library it was rediscovered in the early 20th century by Karl Bopp.

"Johann Heinrich Lambert was born on 26 August 1728 in Mühlhausen (today
Mulhouse, France). Mühlhausen was at that time associated to Switzerland.
Lambert received six years of formal education from the municipality but had to
leave school to help his father, a tailor, when he was 12 years old. However, Lambert
never stopped learning though he did not attend any formal school afterwards.
He studied French, Latin and Mathematics largely on his own. He became an
assistant to the city clerk of Mühlhausen, J. H. Reber, then a bookkeeper to an
industrialist and finally in 1746 a secretary to Prof. J. R. Iselin in Basel. In this
position he gained access to the knowledge of physics and mathematics of his
time. In 1748 he obtained a position in Chur as a private tutor to a grandson of
Count Peter von Salis. At the court of von Salis, Lambert could finally pursue
his research on physics and optics. He travelled with his pupil through Europe,
meeting many eminent scientists and continuously pursuing his research. He
became a member of the 'physikalisch-mathematische Gesellschaft' of Basel in
1754. From 1759 Lambert travelled on his own through Europe. During that time
he published his early masterpiece, the Photometria.

"Lambert was living in rather poor conditions, though he received some support
from the academies he was a member of. After long deliberations and in spite
of Lambert's eccentric character he became a member of the Royal Academy of
Berlin in 1765. Finally Lambert had a secure post and he started researching and
publishing on diverse topics of his interest. In this time of high productivity he
proved that π and e are irrational, wrote about philosophy, studied non-additive
probabilities and made contributions to hyperbolic functions and to cartography.
Lambert died in Berlin on 25 September 1777” (Hulliger, pp. 2-3).

Somerville, Bibliography of non-Euclidean geometry, p. 11; Somerville, Elements
of non-Euclidean Geometry, pp. 13-15; Gray, Worlds Out of Nothing, pp. 82-84; Klein,
Society for History of Mathematics 36 (2014), pp. 129-155. Lambert's treatise was
reprinted in Engel and Stäckel's Die Theorie der Parallellinien von Euklid bis auf
Society 14 (2003), pp. 4-10.
LAMBERT, Johann. Parallelinien.

$9,500

8vo (230 x 151 mm), pp. [ii], [3], 4-254. Twentieth-century half-calf with original blue printed wrappers bound in (front wrapper with Japanese tissue repairs to a few minor marginal defects, rear wrapper with ink stain).

First edition, first offprint issue, extremely rare, of Le Verrier’s mathematical prediction of the existence of Neptune, “undeniably one of the major scientific events of the nineteenth century” (Lequeux, p. 22). This issue precedes both the journal appearance in Connaissance des Temps and the second offprint issue, in both of which ‘Uranus’ in the title was changed to ‘le planète Herschel’ (and a related footnote was added). “Neptune, whose existence was visually confirmed in 1846, was the first planet to be discovered by mathematical rather than observational means. The discovery of Neptune not only represents the greatest triumph for Newton’s gravitational theory since the return of Halley’s Comet in 1758, but it also marks the point at which mathematics and theory, rather than observation, began to take the lead in astronomical research … The discovery of Neptune resulted from the need to develop a theory explaining the motion of the solar system’s seventh planet, Uranus, the movements of which could not be completely accounted for by the gravitational effects of Jupiter and Saturn. Several astronomers since
the planet’s discovery in 1781 had suggested that the perturbations in Uranus’s orbit could be caused by an as yet unknown trans-Uranian planet. However, the complex mathematics required for proving this hypothesis was so daunting that no one had attempted the task … Le Verrier had begun his own work on the Uranus problem in the summer of 1845, encouraged by François Arago, who by then had become France’s leading astronomer. On November 19, 1845 Le Verrier published his first brief paper on the subject in the Comptes rendus de l’Académie des sciences, following it with three more equally brief papers published on June 1, August 31 and October 5, 1846. These short papers, totaling only 34 pages, were preliminary to the full and detailed account Le Verrier gave of his results in [the present work]; on p. 5 of that work Le Verrier referred to the Comptes rendus papers as ‘publications partielles’” (historyofinformation.com). Le Verrier communicated the result of his investigations to several astronomers who had powerful instruments at their disposal. Among them was J. G. Galle, at the Berlin observatory, who was notified by Le Verrier on 23 September. Two days later he wrote to Le Verrier, announcing that he had observed the planet within 1° of Le Verrier’s predicted position. “During the time that Le Verrier was conducting his research on the movements of Uranus, the English astronomer J. C. Adams was independently arriving at the same conclusions, which he communicated to the Astronomer Royal, George Biddell Airy. Adams’s paper remained unpublished until 1847” (Norman 1343). OCLC lists only the BNF copy of this first offprint issue (and nine copies of the second issue); only one other copy of this first issue in auction records (Sotheby’s 1983).

“In his celebrated treatise on celestial mechanics, Pierre Simon de Laplace had developed mathematical expressions for the mutual perturbations exerted by the planets as a result of their gravitational attraction. Using these expressions, one could carry out numerical calculations to produce tables of the positions of the planets over time. The responsibility for doing so was claimed by the Bureau of Longitudes, headed by Laplace himself, though the work of actually performing these backbreaking calculations was distributed among several astronomers at the Bureau, including Delambre, Alexis Bouvard, and Burckhardt. Bouvard, Laplace’s student, was assigned the most thankless task. In 1821, he began the laborious calculation of tables predicting the movements of the three giant planets: Jupiter, Saturn, and Uranus. The calculation of the tables of Jupiter and Saturn proved to be relatively straightforward. Uranus, however, proved to be highly intractable. Even after taking into account the perturbations exerted by the other planets, Bouvard could not derive a set of orbital elements that would successfully account for the movements of Uranus during the entire period over which it had been observed …

“Resigned to defeat, Bouvard wrote in the introduction of his Tables of Uranus in 1821 that it would remain the task of future investigators to determine whence arose the difficulty in reconciling these two data sets: whether the failure of the observations before 1781 to fit the tables was due to the inaccuracy of the older observations or whether they might depend on ‘some foreign and unperceived source of disturbance acting upon the planet’ … It seems, then, that Alexis Bouvard himself had been the first to speculate that the anomalous motion of Uranus could be occasioned by the gravitational action of a new planète troublante (disturbing planet) … Following Alexis Bouvard’s death in 1843, his nephew Eugène was charged by the Bureau of Longitudes to work on new tables of the planets. He submitted his results to the Academy of Sciences on September 1, 1845, but they were never published. By then he had come to regard the discrepancies between observation and theory as irreconcilable without adding another factor, and personally found ‘entirely plausible the idea suggested by my uncle that another planet was perturbing Uranus.’

“Arago evidently hoped that the problem of Uranus would be taken up at the Paris Observatory, but he lacked confidence in Eugène Bouvard, whose measurements
at the eclipse expedition of 1842 had been of poor quality. Since there was no one else at the observatory he deemed capable of tackling such a difficult problem, he turned to Le Verrier (1811-77). He had great faith in Le Verrier’s mathematical abilities, and so, at Arago’s request, Le Verrier abandoned the investigation of comets in which he was then involved and devoted himself to Uranus …

“Le Verrier scrupulously examined all the available observations up until 1845, notably those made recently at the Paris Observatory, which Arago put in his hands, and which were of excellent quality; and also those made at Greenwich which were sent by the director, Airy. He also examined carefully Alexis Bouvard’s calculations (he seems not to have considered those of his nephew, Eugène). He discovered that certain terms had been neglected unjustifiably, and he also turned up several outright errors, which required him to redo parts of the calculation. Next he undertook to determine the actual location of the perturbing planet.

“The problem was entirely novel: hitherto, the position of each planet was determined by taking into account the perturbations of the others whose positions were known by observation. In the present case, it was a matter of determining the position of a planet about which one knew nothing except the perturbations that it exerted on the other planets. In mathematics, this is called an inverse problem. It is both difficult and complex, because there are many unknowns to be determined. Le Verrier simplified the problem from the outset by supposing as known the distance of the planet from the Sun and the inclination of its orbit. He wrote on 1 June 1846:

‘It would be natural to suppose that the new body is situated at twice the distance of Uranus from the Sun, even if the following considerations didn’t make it almost certain. First, it is obvious that the sought-after planet cannot come too close to Uranus [since then its perturbations would have been very evident]. However,
it is also difficult to place it as far off, say, as three times the distance of Uranus, for then we should have to give it an excessively large mass. But then its great distance both from Saturn and Uranus would mean that it would disturb each of these two planets in comparable degree, and it would not be possible to explain the irregularities of Uranus without at the same time introducing very sensible perturbations of Saturn, of which however there exist no trace.

We might add that since the orbits of Jupiter, Saturn, and Uranus all have a very small inclination to the ecliptic, it is reasonable to suppose, as a first approximation, that the same must apply to the sought-after planet.

"By such legerdemain, Le Verrier had reduced the number of unknowns by two: he assumed the semi-major axis of the orbit, a quantity that would have been particularly difficult to determine otherwise, and the inclination of the orbit. Nevertheless, there remained more than enough other unknowns, in part because the orbital elements of Uranus were themselves poorly determined owing to the lack of any solution fitting all the observations … Seeing this, Le Verrier was obliged to determine simultaneously both the orbital elements of Uranus and those of the new planet. This is a problem with 12 unknowns. However, as we have seen, Le Verrier had already settled on two for the unknown planet, and using the same reasoning he settled on the same ones for Uranus: the semi-major axis and the orbital inclination. With this simplification, there remained eight unknowns in the orbital elements, to which he added a ninth, the mass of the perturbing planet … Le Verrier affirmed, in his presentation to the Academy of sciences on 1 June 1846:

'I demonstrate that all the observations of the planet [Uranus] can be represented with the exactitude they deserve … I conclude also that one can effectively model the irregularities of Uranus's movements by the action of a new planet placed at a distance of twice that of Uranus from the Sun; and what is just as important, that one can arrive at the solution in only one way. To say that the problem is susceptible to only one solution, I mean that there are not two regions in the sky in which one can choose to place the planet in a given epoch (such as, for instance, 1 January 1847). Within this unique region, we can limit the object's position within certain bounds.'

"Next Le Verrier indicates within 10° the possible positions occupied by the perturbing planet for 1 January 1847. The uncertainty was still considerable, and Le Verrier added that he could do no better at the time of his presentation, since the work for which he had just presented an abstract to the Academy 'must be considered a rough draft or outline of a new theory, which [was] only in the initial stages.' The orbital elements he calculated were provisional, but he hoped to extend his labors to provide more precise results …

"Despite Le Verrier's seeming confidence, skepticism still reigned in certain quarters. Thus Airy wrote on 26 June to Le Verrier to ask for further clarifications, at the same time sending him additional Greenwich observations. Le Verrier thanked Airy for his assistance, and responded to Airy's specific questions. He even proposed to communicate the orbital elements of the perturbing planet, if Airy were at all inclined to search for it. Airy was very impressed by Le Verrier's confidence. Though his skepticism was completely overcome, he declined Le Verrier's offer, for reasons that remain rather mysterious even today.

"Despite the novelty of the problem and the great mathematical difficulties involved, Le Verrier needed only 3 months to specify the orbital elements of the perturbing planet, guess at its mass, and even provide an order of magnitude estimate of the apparent diameter it would present in the telescope … On August 31 1846, Le Verrier presented a paper to the Academy of Sciences, containing the elements
of the planet and the place where it ought to be found. He then wrote to several foreign astronomers in an effort to enlist a powerful instrument in the search. Sadly, there were at the time no suitable instruments at the Paris Observatory itself. Furthermore, the observatory did not then have at its disposal any good maps of this part of the sky. Despite all that Arago and Le Verrier between them had done, the planet would not, and indeed could not, be discovered in Paris.

"Among the foreign astronomers contacted by Le Verrier was Johann Gottfried Galle, of the Berlin observatory. Le Verrier wrote to him on 18 September. The letter reached Berlin on 23 September; that night Galle, after seeking and receiving permission from the observatory’s director, Johann Franz Encke, and being assisted by a graduate student from Copenhagen, Heinrich Louis d'Arrest, quickly discovered the planet. On 25 September, Galle wrote to Le Verrier (in French; the latter did not know German): ‘Monsieur, the planet whose position you had indicated really exists. On the very day I received your letter I found an eighth magnitude star, which did not appear in the excellent chart Hora XXI (drawn up by Dr. Carl Bremiker) from the collection of celestial charts published by the Academy of Berlin. The observation of the next night clinched the matter: here was indeed the planet we were looking for. Encke and I found with the great refractor of Fraunhofer (with an objective 9 1/2 inches [23 cm] in diameter) that in brightness it was comparable to a ninth magnitude star’ …

"Shortly after the announcement of the discovery, the planet was viewed in Paris by Le Verrier himself, as well as by several other astronomers, including Otto Struve and his father Wilhelm at the Pulkova Observatory near Saint Petersburg, by Emil Plantamour in Geneva, by Carl Ludwig von Littrow in Vienna, by John Russell Hind and James Challis in England, and by Carl Friedrich Gauss in Göttingen, etc. Many wrote to congratulate him, notably Otto Struve and Father Angelo Secchi at the Jesuit Collegio Romano in Rome …

“Though a torrent of salutations rained down on Le Verrier, those of his own colleagues meant the most to him. He became famous overnight, and received countless honors: Officer of the Legion of Honor (though he had only been a Chevalier for 4 months), assistant member of the Bureau of Longitudes, chair of celestial mechanics in the faculty of sciences in Paris – the latter was specifically created for him in honor of his achievement. King Louis-Philippe named him preceptor of astronomy for his grandson, Louis-Philippe d’Orléans. The Royal Society of London awarded him the prestigious Copley Medal, the very same that William Herschel had won for the discovery of Uranus, and inscribed him among its foreign members. Many other learned societies followed suit …

"After the discovery of the new planet, it was necessary to agree on a name for it. Normally, the astronomer who makes the discovery offers a proposal, and a learned scientific society votes on its appropriateness … Since Le Verrier was considered to be the true discoverer of the planet – as Arago put it so poetically, he had discovered it ‘at the tip of his pen’ – it was Le Verrier’s prerogative to name the planet. Indeed, it seems to have been Le Verrier himself who first proposed Neptune, asserting, moreover, to his correspondents that the Bureau of Longitudes had already selected this name … But now Le Verrier, having first proposed Neptune, seems to have had second thoughts. Unaccountably, he resigned the task of choosing the planet’s name to Arago. Arago, in turn, promptly proposed a different name – ‘Le Verrier’ …

"Le Verrier was evidently highly satisfied with Arago’s proposal. Moreover, he now attempted to regularize the situation by using for Uranus the name Herschel, a name which had hitherto been used only sporadically:
'In my subsequent publications, I will consider it a strict duty to make disappear completely the name Uranus, and to only refer to the planet using the name HERSCHEL. I sorely regret that my already published writings do not permit me to follow the determination that I shall religiously observe henceforth.'

Nevertheless, the name 'Le Verrier' would encounter more and more fierce opposition, and finally the name Neptune would be adopted" (Lequeux).

Le Verrier’s initial announcement of his prediction precipitated a priority dispute with the Cambridge mathematical astronomer John Couch Adams. "Adams began his investigation of Uranus in 1843, and in 1845 sent his calculations and observations to the Astronomer Royal, George Biddell Airy, who failed to recognise the importance of the paper. In 1846, Urbain Jean Joseph Le Verrier published his own research and reached the same conclusion, leading to the immediate identification of Neptune by J.G. Galle. Only then was Adams’ work published, leading to a bitter dispute over priority” (Norman 7). But this standard account, in which Adams’ prediction preceded Le Verrier’s (although it was published later), has been brought into question by contemporary documents rediscovered in 1999. "In contrast to the traditional story of Adams’s wonderful prediction that went shamefully ignored by British astronomers, the real Adams appears to have been rather vague, and his predictions for the planet kept changing. At no point did he have the confidence to say, in effect, as Le Verrier did, ‘point your telescope here and you will find it.’ Instead, Adams’s predictions ranged over as much as 20° of sky, throwing British searchers at the Cambridge University Observatory on a six-week wild goose chase hunting the planet during the summer of 1846. The actual discovery of Neptune took Galle just a half hour at the telescope in Berlin (the planet was only 1° from Le Verrier’s predicted position). Afterward the British – and especially Airy – got together a carefully digested and heavily selected version of events. Adams’s vacillation and mathematical scrupulosity were hushed up and covered over, and only his early, preliminary result – which proved to be more accurate than his later ones – was made public. The result was a remarkable British takeover … Le Verrier protested at the time, but in vain. He never had an opportunity to read all the documents in the possession of the British, so he was forced to accept their version of Adams’s priority in the calculations” (Sheehan). For further details on the dispute, see Lequeux, pp. 44-49 and Kollerstrom, ‘Recovering the Neptune files’, Astronomy & Geophysics 44 (2003), pp. 5.23–5.24.

The detailed account of Le Verrier’s calculations contained in the present offprint was published three times in quick succession. It appeared in the Connaissance des Temps ou des Mouvements Célestes, a l’Usage des Astronomes et des Navigateurs, pour l’An 1849, Publiée par le Bureau des Longitudes (Additions, pp. 3-254), the French analogue of the British Nautical Almanac, under the title ‘Recherches sur les mouvements de la planète Herschel (dite Uranus)’, the article being dated 5 October 1846 at the end; the journal imprint gives its date of publication as November 1846. Two offprints of this article were published, which can be distinguished from the journal issue by the presence of the publisher’s imprint and the phrase ‘Extrait de la Connaissance des Temps pour 1849’ on p. 254. The second offprint issue differs from the version offered here in its title, ‘Recherches sur les mouvements de la planète Herschel’ (note that the phrase ‘dite Uranus’ has been omitted), and by the presence of the note referred to above stating Le Verrier’s intention to use the name Herschel instead of Uranus: this note is printed at the foot of p. 3 of the second offprint issue, and also appears in the journal issue, but not in the offprint offered here. This clearly indicates the following order of publication:
1. The offprint offered here with title ‘Recherches sur les mouvements d’Uranus’;

2. The publication in *Connaissance des Temps* with title ‘Recherches sur les mouvements de la planète Herschel (dite Uranus)’;

3. The second offprint with title ‘Recherches sur les mouvements de la planète Herschel’.

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First edition of Leibniz’s invention of the differential calculus. “Leibniz was an almost universal genius whose place in the history of mathematics depends on his being an independent inventor of the infinitesimal calculus and on his contributions to combinatorial analysis which foreshadowed the development of modern mathematical analysis … The *Acta Eruditorum* was established in imitation of the French *Journal des Scavans* in Berlin in 1682 and Leibniz was a frequent contributor. Another German mathematician (E.W. Tschirnhausen), having published in it his paper on quadratures, based on researches that Leibniz had communicated to him, Leibniz at last decided in 1684 to present to the world the more abstruse parts of his own work on the calculus. His epoch-making papers give rules of calculation without proof for rates of variation of functions and for drawing tangents to curves … The infinitesimal calculus originated in the 17th century with the researches of Kepler, Cavalieri, Torrecelli, Fermat and Barrow, but the two independent inventors of the subject, as we understand it today, were Newton and Leibniz … Although both Newton and Leibniz developed similar
ideas, Leibniz devised a superior symbolism and his notation is now an essential feature in all presentation of the subject … With the calculus a new era began in mathematics, and the development of mathematical physics since the 17th century would not have been possible without the aid of this powerful technique” (PMM). Leibniz “applied his new method to the solution of the cubic parabola and the inverse methods of tangents and many problems left unsolved by Descartes” (Dibner). “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 Lipsiensi susunt 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).

These two volumes of Acta Eruditorum contain the following additional papers by Leibniz: ‘De dimensionibus figurarum inveniendis’ (III, pp. 233-6) (Ravier 88); ‘Demonstrationes novae de resistentia solidorum’ (III, pp. 319-25 and Tab. IX) (Ravier 89); ‘Meditatione de cognitione, verite et Ideis’ (III, pp. 537-42) (Ravier 91); ‘Additio ad schedam in Actis proxime antecedentis Maii pa. 233 editam, De dimensionibus curvilineorum’ (III, pp. 585-7) (Ravier 92); ‘Demonstratio geometrica regulae apud staicos receptae de momentis gravium’ (IV, pp. 501-5) (Ravier 93).

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.
LEIBNIZ, Gottfried Wilhelm.

MENSIS OCTOBRI A. M. DC. LXXXIV.

NOVA METHODIS PRO MAXIMIS ET MINIMIS, REGEMQUE TANGENTIS, QUE NEE SIFACERIS, NEC IRREDUNDANTES QUASQUE SVRTUR, EB, & SINGULARI PRO LIBUS CALULAB

TAB. XII.

...
ONE OF THE MOST IMPORTANT PAPERS IN THE HISTORY OF COMPUTING


$45,000

Pp. 666-731 and one folding table, in: Scientific Memoirs 3 (1843). 8vo, pp. vi, 734, with 10 plates and one folding table. Contemporary red half-calf and marbled boards, spine lettered in gilt (extremities rubbed), old inscription at head of title (title with a couple of small stains).

First edition, journal issue, of the best contemporary description of Babbage’s Analytical Engine, the first programmable (mechanical) computer. It is a translation by Lovelace of a report by Menabrea of a series of lectures given by Babbage while he was in Turin. At Babbage’s suggestion, Lovelace added seven explanatory notes; as a result, the translation is three times as long as the original. Two of these notes are essentially programs for the Analytical Engine; their inclusion has given rise to the claim that Lovelace was the first computer programmer. “In the fall of 1841, after eight years of work, Babbage described his landmark Analytical Engine at a seminar in Turin. Although the Engine was never constructed, there is no doubt that in conception and design, it embodied all of the essential elements of what is recognized today as a general-purpose digital computer. L.F. Menabrea, an Italian military engineer who attended the seminar, reported the presentation the following year in an obscure Swiss serial, and Babbage urged Ada Lovelace...”
to translate the report into English. In fact, Lovelace undertook a far larger task: adding to her translation a series of important explanatory ‘Notes’ substantially longer than Menabrea’s article” (Grolier Extraordinary Women, p. 122). The collaboration “between Byron’s celebrity daughter and Babbage is one of the more unusual in the history of science … Ada’s translation of Menabrea’s paper, with its lengthy explanatory notes, represents the most complete contemporary account in English of the intended design and operation of the first programmable digital computer. Babbage considered this paper a complete summary of the mathematical aspects of the machine, proving ‘that the whole of the development and operations of Analysis are now capable of being executed by machinery.’ As part of his contribution to the project, Babbage supplied Ada with algorithms for the solution of various problems. These he had worked out years ago, except for one involving Bernoulli numbers, which was new. Ada illustrated these algorithms in her notes in the form of charts detailing the stepwise sequence of events as the hypothetical machine would progress through a string of instructions input from punched cards” (Swade, p. 165). These procedures, and the procedures published in the original edition of Menabrea’s paper, were the first published examples of computer ‘programs.’ Ada also expanded upon Babbage’s general views of the Analytical Engine as a symbol-manipulating device rather than a mere processor of numbers. She brought to the project a fine sense of style that resulted in the frequently quoted analogy, ‘We may say most aptly that the Analytical Engine weaves algebraic patterns just as the Jacquard-loom weaves flowers and leaves.’ She suggested that … ‘Many persons who are not conversant with mathematical studies, imagine that because the business of the engine is to give its results in numerical notation, the nature of its processes must consequently be arithmetical and numerical, rather than algebraical and analytical. This is an error. The engine can arrange and combine its numerical quantities exactly as if they were letters or any other general symbols; and in fact it might bring out its results in algebraical notation, were provisions made accordingly’ (p. 713)” (OOC). Lady Lovelace signed these notes ‘A.A.L.,’ masking her class and gender in deference to the conventions of the time. 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 was not ready and declined. “In 1840 a similar meeting was arranged in Turin. By then Babbage did feel ready, and accepted the invitation from [Giovanni] Plana (1781-1864) to present the Analytical Engine before the assembled philosophers of Italy … In the middle of August 1840, Babbage left England …

“Babbage had persuaded his friend Professor MacCullagh of Dublin to abandon a climbing trip in the Tyrol to join him at the Turin meeting. There in Babbage’s apartments for several mornings met Plana, Menabrea, Mosotti, MacCullagh, Plantamour, and other mathematicians and engineers of Italy. Babbage had taken with him drawings, models and sheets of his mechanical notations to help explain the principles and mode of operation of the Analytical Engine. The discussions in Turin were the only public presentation before a group of competent scientists during Babbage’s lifetime of those extraordinary forebears of the modern digital computer. It is an eternal disgrace that no comparable opportunity was ever offered to Babbage in his own country …

“The problems of understanding the principles of the Analytical Engines were by no means straightforward even for the assembly of formidable scientific talents which gathered in Babbage’s apartments in Turin. The difficulty lay not as much in detail but rather in the basic concepts. Those men would certainly have been
familiar with the use of punched cards in the Jacquard loom, and it may reasonably be assumed that the models would have been sufficient to explain the mechanical operation in so far as Babbage deemed necessary. Mosotti, for example, admitted the power of the mechanism to handle the relations of arithmetic, and even of algebraic relations, but he had great difficulty in comprehending how a machine could handle general conditional operations: that is to say what the machine does if its course of action must be determined by results arising from its own previous calculations. By a series of particular examples, Babbage gradually led his audience to understand and accept the general principles of his engine. In particular, he explained how the machine could, as a result of its own calculations, advance or back the operation cards, which controlled the sequence of operations of the Engine, by any required number of steps. This was perhaps the crucial point: only one example of conditional operations within the Engine, it was a big step in the direction of the stored program, so familiar today to the tens of millions of people who use electronic computers.

“In explaining the Engines Babbage was forced to put his thoughts into ordinary language; and, as discussion proceeded his own ideas crystallized and developed. At first Plana had intended to make notes of the discussions so that he could prepare a description of the principles of the Engines. But Plana was old, his letters of the time are in a shaky hand, and the task fell upon a young mathematician called Menabrea, later to be Prime Minister of the newly united Italy. It is interesting to reflect that no one remotely approaching Menabrea in scientific competence has ever been Prime Minister of Britain …

“Babbage’s primary object in attending the Turin meeting had been to secure understanding and recognition for the Analytical Engine. He hoped that Plana would make a brief formal report on the Engine to the Academy of Turin and that Menabrea would soon complete his article. Babbage sent him further
explanations to complement the notes he had made during Babbage’s exposition and the discussions in Turin. Babbage had certainly little hope of government comprehension or support in England but he was determined not to miss the slightest opportunity of securing recognition for his Engines.

“He set down his own thoughts in a letter written at about this time to Angelo Sismoda, whom he had often seen during the Turin meeting: ‘The discovery of the Analytical Engine is so much in advance of my own country, and I fear even of the age, that it is very important for its success that the fact should not rest upon my unsupported testimony. I therefore selected the meeting at Turin as the time of its publication, partly from the celebrity of its academy and partly from my high estimation of Plana, and I hoped that a report on the principles on which it is formed would have been already made to the Royal Academy.’ But Plana was old and ill: no report was forthcoming …

“Babbage returned from the sunny hills and valleys of Tuscany where he had basked in Ducal warmth and the approbation of philosophers to a chilly climate in England. He sent further explanations to Menabrea who in turn entirely rewrote the article. On 27 January 1842 Menabrea wrote to Babbage from Turin: ‘Je donnerai la dernière main à 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).

“Babbage’s friend Charles Wheatstone commissioned Ada Lovelace to translate Menabrea’s paper into English. She then augmented the paper with notes, which were added to the translation. Ada Lovelace spent the better part of a year doing this, assisted with input from Babbage. These notes, which are more extensive than Menabrea’s paper, were then published in the September 1843 edition of Taylor’s Scientific Memoirs under the initialism AAL.

“Ada Lovelace’s notes were labelled alphabetically from A to G. In note G, she describes an algorithm for the Analytical Engine to compute Bernoulli numbers. It is considered to be the first published algorithm ever specifically tailored for implementation on a computer, and Ada Lovelace has often been cited as the first computer programmer for this reason. The engine was never completed so her program was never tested … The engine has now been recognised as an early model for a computer and her notes as a description of a computer and software.

“In her notes, Lovelace emphasised the difference between the Analytical Engine and previous calculating machines, particularly its ability to be programmed to solve problems of any complexity. She realised the potential of the device extended far beyond mere number crunching. In her notes, she wrote:

‘[The Analytical Engine] might act upon other things besides number, were objects found whose mutual fundamental relations could be expressed by those of the abstract science of operations, and which should be also susceptible of adaptations to the action of the operating notation and mechanism of the engine … Supposing, for instance, that the fundamental relations of pitched sounds in the science of harmony and of musical composition were susceptible of such expression and adaptations, the engine might compose elaborate and scientific pieces of music of any degree of complexity or extent.’

“This analysis was an important development from previous ideas about the capabilities of computing devices and anticipated the implications of modern computing one hundred years before they were realised. Walter Isaacson [Fortune, 18 September 2014] ascribes Lovelace’s insight regarding the application of computing to any process based on logical symbols to an observation about
textiles: ‘When she saw some mechanical looms that used punch-cards to direct the weaving of beautiful patterns, it reminded her of how Babbage’s engine used punched cards to make calculations’ …

“According to the historian of computing and Babbage specialist Doron Swade:

‘Ada saw something that Babbage in some sense failed to see. In Babbage’s world his engines were bound by number … What Lovelace saw—what Ada Byron saw—was that number could represent entities other than quantity. So once you had a machine for manipulating numbers, if those numbers represented other things, letters, musical notes, then the machine could manipulate symbols of which number was one instance, according to rules. It is this fundamental transition from a machine which is a number cruncher to a machine for manipulating symbols according to rules that is the fundamental transition from calculation to computation—to general-purpose computation—and looking back from the present high ground of modern computing, if we are looking and sifting history for that transition, then that transition was made explicitly by Ada in that 1843 paper.’


‘All but one of the programs cited in her notes had been prepared by Babbage from three to seven years earlier. The exception was prepared by Babbage for her, although she did detect a ‘bug’ in it. Not only is there no evidence that Ada ever prepared a program for the Analytical Engine, but her correspondence with
Babbage shows that she did not have the knowledge to do so (p. 89) …

“Eugene Eric Kim and Betty Alexandra Toole ['Ada and the First Computer,' *Scientific American* 280 (1999), p. 76] consider it ‘incorrect’ to regard Lovelace as the first computer programmer, as Babbage wrote the initial programs for his Analytical Engine, although the majority were never published. Bromley notes several dozen sample programs prepared by Babbage between 1837 and 1840, all substantially predating Lovelace’s notes. Dorothy K. Stein ['Lady Lovelace’s Notes: Technical Text and Cultural Context,' *Victorian Studies* 28 (1984), pp. 33–67] regards Lovelace’s notes as ‘more a reflection of the mathematical uncertainty of the author, the political purposes of the inventor, and, above all, of the social and cultural context in which it was written, than a blueprint for a scientific development’ (p. 34)” (Wikipedia, accessed May 15, 2019).

Diagram for the computation by the Engine of the Numbers of Bernoulli. See Note G. (page 722 et seq.)

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<th>Statement of Results</th>
<th>Working Variables</th>
<th>Remainder Variables</th>
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Here follows a repetition of Operational ideas up to twenty-three.
PMM 206 - CREATION OF PATHOLOGICAL ANATOMY


$28,500

Two volumes, large folio (390 x 245 mm), both volumes entirely untouched in their original state from the printer; uncut and unpressed in carta rustica, internally fine and clean with only occasional light spotting, an exceptional set. Engraved frontispiece portrait by Jean Renard after Giovanni Volpato, pp. [i-viii] ix-xcvi, [1-2] 3-298 [2]; [1-2] 3-452. Highly rare in such fine condition. Custom half-leather box.

First edition, first issue, an exceptional copy, completely untouched in the original printer’s interim-boards, of “one of the most important [works] in the history of medicine” (Garrison & Morton). “After Antonio Benivieni (1443-1502), Giovanni Battista Morgagni is considered the founder of pathological anatomy. His ‘De sedibus’, regarded as one of the most important books in the history of medicine, established a new era in medical research” (Norman). “Morgagni’s contribution to the understanding of disease may well rank with the contributions of Vesalius in anatomy and Harvey in physiology” (Heirs of Hippocrates). “On the basis of direct examination and records of some 700 post mortem dissections, he advanced the procedure of basing diagnosis, prognosis and treatment on a detailed and comprehensive knowledge of the anatomical conditions of common diseases. In the above volumes, some of the cases are given with a precision and details hardly
surpassed in medical history. His proposal was a shift of emphasis from the traditional ‘nature’ of a disease to its anatomical ‘seat’. It combined the approach of anatomist and pathologist, making their special knowledge available to the diagnostician” (Dibner). Due to this work, “Morgagni may thus be considered to be the founder of pathological anatomy” (DSB). “Rudolf Virchow epitomized Giovanni Battista Morgagni’s influence on the development of modern medicine when he wrote, ‘The full consequences of what he worked out were harvested in London and Paris, in Vienna and in Berlin. And thus we can say that, beginning with Morgagni and resulting from his work, the dogmatism of the old schools was completely shattered, and that with him the new medicine begins.’ This ‘new medicine’ began with the publication of Morgagni’s masterpiece known as De Sedibus et Causis Morborum per Anatomen Indagatis Libri Quinque or ‘The Seats and Causes of Disease Investigated by Anatomy in Five Books’ … In addition to Vesalius’ Fabrica and Harvey’s De Motu Cordis, De Sedibus was the final vinculum by which the old medicine was to be buried perpetually” (Ventura, p. 792).

“Morgagni’s most important work … is his ‘De sedibus et causis morborum per anatomen indagatis’ of 1761. This book grew out of a concept of Malpighi, which Morgagni then developed into a major work. The concept may be stated simply as the notion that the organism can be considered as a mechanical complex. Life therefore represents the sum of the harmonious operation of organic machines, of which many of the most delicate and minute are discernible, hidden within the recesses of the organs, only through microscopic examination.

“Like inorganic machines, organic machines are subject to deterioration and breakdowns that impair their operation. Such failures occur at the most minute levels, but, given the limits of technique and instrumentation, it is possible to investigate them only at the macroscopic level, by examining organic lesions on the dissecting table. These breakdowns give rise to functional impairments that produce disharmony in the economy of the organism; their clinical manifestations are proportional to their location and nature.

“This thesis is implicit in the very title ‘De sedibus et causis morborum per anatomen indagatis’. In this book Morgagni reasons that a breakdown at some point of the mechanical complex of the organism must be both the seat and cause of a disease or, rather, of its clinical manifestations, which may be conceived of as functional impairments and investigated anatomically. Morgagni’s conception of etiology also takes into account what he called ‘external’ causes, including environmental and psychological factors, among them the occupational ones suggested to Morgagni by Ramazzini.

“The parallels that exist between anatomical lesion and clinical symptom served Morgagni as the basis for his ‘historiae anatomico-medicae’, the case studies from which he constructed the ‘De sedibus’ … the special merit of Morgagni’s work lies in its synthesis of case materials with the insights provided by his own anatomical investigations. In his book Morgagni made careful evaluations of anatomic medical histories drawn exhaustively from the existing literature. In addition, he describes a great number of previously unpublished cases, including both those that he had himself observed in sixty years of anatomical investigation and those collected by his immediate predecessors, especially Valsalva, whose posthumous papers Morgagni meticulously edited and commented upon. The case histories collected in the ‘De sedibus’ therefore represent the work of an entire school of anatomists, beginning with Malpighi, then extending through his pupils Valsalva and Albertini to Morgagni himself” (DSB).

“The number of pathologic observations described by Morgagni, many of them for the first time, is enormous. His observations are included in the De Sedibus et Causis Morborum per Anatomen Indagatis Libri Quinque or The Seats and Causes
of Disease Investigated by Anatomy in Five Books, which was published in 1761, when Morgagni was 79 years old. Prior to the publication of De Sedibus, the first attempt to correlate premortem symptoms with postmortem findings was described in a book named the Sepulchretum Sive Anatomica Practica. Theophilus Bonetus published this treatise first in 1679, and an enlarged second edition appeared in 1700. It included almost 3,000 cases in which clinical histories were correlated with autopsy reports and commentary. There were several deficiencies in the Sepulchretum, which made the work virtually useless to scholars. These included misquotations, misinterpretations, inaccurate observations, and the lack of a proper index. The idea for De Sedibus was generated in 1740, while Morgagni was involved in a discussion of the deficiencies of Theophilus Bonetus' encyclopedic compilation. At the time, Morgagni had agreed to write a series of letters to a young friend, which were to resolve the various questions that were unsatisfactorily answered by Bonetus. These letters would be written in the course of the years on the basis of Morgagni's own personal observations at the autopsy table. It took 20 years to complete the task and the resultant 70 letters, included in 5 books, represent the core of De Sedibus. Each book dealt with a different category: (1) Diseases of the Head, (2) Diseases of the Thorax, (3) Diseases of the Abdomen, (4) Diseases of a General Nature and Disease requiring Surgical Treatment, and (5) Supplement.

"It is important to emphasize that Morgagni's correlation between symptoms and structural organ changes removed pathology from the anatomical museum halls to the realm of the practicing physician. Morgagni devoted several letters of the De Sedibus to study of the diseased heart, in which he accurately described the principal cardiac lesions which he found after the death of the patients. He included a description of angina pectoris, he suggested that dyspnea and asthma were the result of diseases of the heart, and he also suggested a relationship between syphilis and aneurysm. He described the rupture of the heart, vegetative
endocarditis, pericardial effusion, adhesions, and calcifications. In addition, he described cyanotic congenital cardiac defects. Perhaps Morgagni's best classical descriptions included mitral stenosis, heart block, calcareous stenosis of the aortic valve with regurgitation, coronary sclerosis, and aneurysm of the aorta. Few passages of some of these descriptions are important to illustrate the significance of Morgagni's contributions to cardiology.

The ninth letter describes heart block:

‘he was in his sixty-eight year, of a habit moderately fat … when he was first seiz'd with the epilepsy, which left behind in the greatest slowness of pulse, and in a like manner a coldness of the body … the disorder often returned.’

“This clinical narration was to become the Stokes-Adams syndrome, when these two physicians from the Irish school correlated the slow pulse with heart disease.

“In the 24th letter, he writes:

‘the pulse has been weak and small, but not intermittent, when on account of an incarcerated hernia … he was brought to the hospital at Padua . . . whether the pulse had been in that state before this disorder came on, or whether it was rather brought on by this disease, joint with an inflammation of the intestines, to such a degree, that a speedy death prevented any method of cure to be attempted … As I examined the internal surface of the heart, the left coronary artery appeared to have been changed into a bony canal from its very origin to the extent of many fingers breadth, where it embraces the greater part of the base. And part of that very long branch, also, which it sends down upon the anterior surface of the heart, was already become bony to so great space as could be covered by three fingers place transversely.’

“This letter clearly describes coronary artery disease due to atherosclerosis and perhaps its association with sudden death.

“The 26th letter named ‘Treats of sudden death, from a disorder of the sanguiferous vessels, specially those lie in the thorax’ narrates a patient with an aortic aneurism. He writes:

‘A man who had too much given to the exercise of tennis and the abuse of wine, was in consequence of both these irregularities, seized with a pain in the right arm, and soon after of the left, joined with a fever. After these there appeared a tumour on the upper part of the sternum, like a large boil: by which appearance some vulgar surgeon being deceived, and either not having at all observed, or used to bring these tumors to suppuration; and these applications were of the most violent kind. As the tumour still increased, other applied emollient medicines, from which it seemed to them to be diminished; … only soon recovered its former magnitude, but even was, plainly, seen to increase every day … when the patient came into the Hospital of Incurables, at Bologna . . . it was equal in size to a quince; and what was much worse, it began to exude blood in one place … he was ordered to keep himself still and to think seriously and piously of his departure from this mortal life, which was near at hand, and inevitable … this really happened on the day following, from the vast profusion of blood that had been foretold, though not soon expected by the patient … and there was a large aneurism, into which the anterior part of the curvature of the aorta itself being expanded, and partly consumed the upper part of the sternum, the extremities of the clavicles which lie upon it … and where the bones had been consumed or affected with caries, there not the least traces of the coats of the artery remained … the deplorable exit of this man teaches in the first place, how much care ought to be taken in the beginning, that an internal aneurism may obtain to increase: and in the second place, if, either by ignorance of the persons who attempt their
cure, or the disobedience of the patient, or only by the force of the disorder itself, they do at length increase."

“This observation exemplifies the reason why De Sedibus was such an important vinculum to the foundation of modern medicine. One can first witness the meticulous clinical description of a disease process the correlation with anatomo-pathologic findings and second, the judicious interpretation of the findings, and finally the attempt to describe a prognosis and a therapeutic strategy. Morgagni’s ability to integrate and synthesize information was paramount to accomplish progress in medicine, either in the diagnosis or the treatment of diseases” (Ventura, pp. 793-4).

“Giovanni Battista Morgagni was born on February 25, 1682, in Forli, a small town 35 miles southeast of Bologna, Italy. He was a precocious student, already manifesting in his teenage years an intense interest in such diverse subjects as poetry, philosophy, and medicine. Throughout his life, he was to maintain his interest in philosophy and literature along with history and archaeology. This interest generated many papers on archaeological findings in the vicinity of Ravenna and Forli; letters to Lancisi on ‘The Manner of Cleopatra’s Death’; and commentaries on Celsus, Sammonicus, and Varro. At the age of sixteen he went to Bologna to study medicine and philosophy, soon coming under the patronage of Antonio Maria Valsalva, the great anatomist who had been a pupil of Malpighi. Upon receiving his degree with distinction in 1701, Morgagni became Valsalva’s assistant for six years. During that time he published his first work on anatomy, Adversaria Anatomica Prima, which was presented before the Academia Inquietorum of which he had just been elected president. He left Bologna for a postgraduate study in anatomy at Padua and Venice, and upon completion of these studies he left academia to return to Forli to become a practicing physician. Soon
he became a successful practitioner and married Paola Verazeri, the daughter of a noble family of Forli. Together they raised twelve daughters and three sons, eight of the girls becoming nuns and one of the boys entering the priesthood. According to Dr. Nuland’s description of Morgagni, he was a tall, robust person with an engaging personality. His peers and students admired him not only for his scientific achievements but also for his nobility of character. Dr. Nuland [The New Medicine, the Anatomical Concept of Giovanni Morgagni, 1988] writes:

‘His years were characterized by regularity of habits and consistency of devotion to his scientific work, to his large family, and to the religious principles that guided both his search for the truth and the stability of his spirit. As one reads the description of his personality that have come down to us, the image that emerges is that of a serene scholar, much admired by his students of many nationalities and by his friends, among whom were included several of the most powerful figures of the day, such as Pope Benedict XIV and the Holy Roman Emperor Joseph II. He enjoyed warm professional relationships with some of the great medical thinkers of his time, including Hermann Boerhaave of Leyden, Albrecht von Haller of Berne, Johann Meckel of Gottingen, and Richard Mead of London, a group whose spectrum of interests reflected Morgagni’s own interests, ranging from education to research to the care of the sick.’

“In 1711, he was appointed professor of practical medicine at the University of Padua, and four years later, the University authorities, on the advice of Lancisi, appointed him professor of anatomy. In an address delivered after receiving this appointment he remarked that he was overwhelmed by the thought of holding the same chair that had been filled by, among others, Vesalius and Falloppio. He soon became a popular teacher, attracting not only Italian students but also foreigners, particularly Germans, who came in large numbers to attend
his lectures and demonstrations. The second volume of *Adversaria Anatomica* appeared in 1717, and his *Adversaria Anatomica Omnia* in 1719. These works established his reputation as an anatomist, a scholar of great intellectual capacity, and a master of Latin prose. It was, however, *De Sedibus et Causis Morborum per Anatomen Indagatis*, a book published in 1761, when Morgagni was 79 years old, that inscribed his name among the greatest in the history of medicine. He has justly received the title of the 'Father of Pathology.' It would be a mistake, however, to consider Morgagni solely a pathologist. He also carried out many physiologic experiments and was active as a practitioner of medicine and a clinical consultant. In 1935, for the first time, his *Consulti Medici* was published in Bologna by Enrico Benassi, who found 12 large volumes of unpublished manuscript written by Morgagni in the library at Parma. These consultations, 100 in number, record Morgagni’s advice on the diagnosis and treatment of patients referred by other physicians or often advice based on the physician’s letters regarding patients he did not see personally. These consultations reveal Morgagni as a clear and cogent reasoner. He died at age 89, in the house to which he had brought his family, at 3003 Via S. Massimo. A memorial plaque may still be seen bearing the sentence ‘Giamb. Morgagni, after founding pathological anatomy, died here on Dec. 6, 1771’” (*ibid.*, pp. 792-3).

“[Morgagni’s] work was … developed by Baillie, who classified organic lesions as types (1793); Auenbrugger and Laënnec, who recognized organic lesions in the living subject (1761 and 1819, respectively); Bichat, who found the pathological site to be in the tissue, rather than the organ (1800); and Virchow, who traced the pathology from the tissue to the cell (1858)” (DSB).

“The first issue of ‘De sedibus’ had the title page of Volume I printed in red and black. In a second issue, also of 1761, the title page was printed entirely in black, and there exists at least one copy of the first edition with imprint, Venice: Ex Typographia Remondiniana, 1762. The work was reprinted in Naples in 1762 and at Padua in 1765, as well as 1769, a German translation in 1771-76, a French translation in 1821-24, and an Italian version in 1823-29” (Haskell Norman in Grolier/Medicine).
NEWTON’S DISCOVERY OF CALCULUS


$85,000

4to (231 x 160 mm), pp. [14], 101, [1], with two folding plates. Woodcut initials, historiated intaglio head- and tail-pieces (some of the latter woodcuts), engraved allegorical vignette on title by Nutting incorporating a portrait of Newton as the source of light. The plates are etched tables on double leaves entitled: ‘Tabula curvarum simpliciorum quae cum ellipsi et hyperbola comparari possunt’ and ‘Residuum tabulæ curvarum simpliciorum quae cum ellipsi et hyperbola comparari possunt’; both are signed: ‘Iohan. Senex sculpt.’ ‘Tractatus de quadratura curvarum’ and ‘Enumeratio linearum tertii ordinis’ have separate half-titles. The author’s name is given in the ‘Praefatio editoris’, which is signed W. Jones. Contemporary panelled calf, red lettering-piece on spine. A fine copy without any restoration. Rare in such good condition.

First edition of the third of Newton’s great works on physics and mathematics, following Principia (1687) and Opticks (1704), and certainly the rarest of the three. This is a very fine copy in untouched contemporary English calf. This work contains ‘De Analysi per Aequationes Numero Terminorum Infinitas,’ written in 1669 and published here for the first time, containing Newton’s theory of infinite series; ‘Methodis differentialis,’ a treatise on interpolation written in 1676 and published here for the first time, the basis of the calculus of finite differences; two treatises, ‘De quadratura curvarum’ and ‘Enumeratio linearum tertii ordinis,’ first published in the Opticks but written in 1693 and 1695; the ‘Epistola prior’
and ‘Epistola posterior,’ first published in vol. III of John Wallis’ *Opera* (1699), a letter from Newton to Collins, written November 8, 1676, and one to Wallis dated August 27, 1692. Newton described *De analysi* “to Oldenburg as ‘a compendium of the method of these [infinite] series, in which I let it be known that, from straight lines given, the areas and lengths of all the curves and the surfaces and volumes of all the solids [formed] could be determined, and conversely with these [taken as] given the straight lines could be determined, and I illustrated the method there outlined by several series.’ Despite the use of the words ‘method of series’ rather than ‘method of fluxions’ (in the letter quoted Newton made no open reference of ‘fluxions’ at all), it is obvious from the inversion (lines to areas, areas to lines) that differentiation and integration, that is, the method of fluxions, is in question” (Hall, pp. 16-17). “Modern workers, Duncan Fraser noted in 1927, had only just struggled up to the level reached by Newton in 1676 [in ‘Methodus differentialis’]. Whiteside was equally impressed by the work, claiming that ‘During the years 1675-76 … Newton laid down the … modern elementary theory of interpolation by finite differences but … diffidently kept back his insights and discoveries therein for nearly forty years more’ (Papers, IV, pp. 7-8)” (Gjertsen, p. 357). Newton’s decision to allow the publication of the 1669 tract at this time was heavily influenced by the on-going priority dispute with Leibniz over the invention of calculus. “The work [*Analysis per quantitatum series …*] also contained a Preface drafted, no doubt, with Newton’s assistance. It contained no mention of Leibniz. It did, however, contain the claim that Newton had ‘Deduced the quadrature of the circle, hyperbola, and certain other curves by means of infinite series … and that he did so in 1665; then he devised a method of finding the same series by division and extraction of roots, which he made general the following year’” (ibid., p. 18). For good measure, the tract was included in its entirety the following year in *Commercium epistolicum*, the official report on the priority dispute, largely drafted by Newton himself.

“*De analysi*, the work which established Newton’s reputation outside the walls of Trinity College, was first heard of in a letter from Barrow to Collins dated 20 June 1669. ‘A friend of mine,’ Barrow wrote, ‘brought me the other day some papers, wherein he hath sett downe methods of calculating the dimension of magnitudes like that of Mr Mercator concerning the Hyperbola; but very Generall; as also of resolving equations’ (Correspondence, I, p. 13). The manuscript, with Newton’s permission, was sent to Collins on June 31. The author’s name was revealed to Collins on 20 August, when Barrow wrote that the author was ‘Mr Newton, a fellow of our College, and very young … but of an extraordinary genius and proficiency in these things’ (ibid., pp. 14-15).

“Not only was Collins the first outside Cambridge to see important work of Newton; he had also, although inadvertently, provoked the work. In the early months of 1669 he had sent Barrow a copy of Mercator’s *Logarithmotechnia* (1668), a work which contained the series for log(1 + x). Barrow was aware that Newton had worked out for himself a general method for infinite series some two years before. Mercator’s book warned Barrow, and through him Newton, that others were working along similar lines. Newton’s reaction was to write, probably in a few summer days of 1669, his treatise *De analysi* which showed, by its generality, how far ahead he was of all other rivals.

“Collins, like Barrow, had no difficulty in recognising the originality and power of Newton’s technique and, consequently, brought up the question of publication. An appendix to Barrow’s forthcoming optical lectures seemed a suitable place. Newton revealed, however, for the first time, his ability to frustrate even such skilled and persistent suitors as Collins. Immediate publication was rejected out of hand; thereafter Newton deployed a variety of excuses: a need to revise the work, a desire to add further material, the pressures of other business and, as a last resort when demands became too pressing, he simply failed to reply. As a result
De analysi remained, with a good deal more of Newton’s early mathematical work, unpublished for half a century.

“Newton’s reluctance to publish did not prevent Collins from copying and distributing the work. One copy was found by Jones in 1709 and is now to be seen in the Royal Society. Another copy was sent to John Wallis, at some point passed to David Gregory, and is at present in the Gregory papers at St. Andrew’s. Others who heard from Collins of Newton’s work were James Gregory, de Sluse and, above all, Leibniz. In October 1676 Leibniz visited London, saw Collins, and was allowed to read De analysi. He took thirteen printed pages of notes, an event construed by Newton as undoubted evidence of Leibniz’s reliance upon the discoveries of others in his mathematical development.

“The work, Newton began, would present a general method ‘for measuring the quantity of curves by an infinite series of terms.’ To this end, three rules were formulated” (Gjertsen, pp. 149-150).

Rule 1 stated that the area under a curve (in modern terms, the integral) of the form $y = x^{m/n}$, where $m$ and $n$ are positive whole numbers, is $nx^{(m+n)/n}/(m+n)$.

Rule 2 stated that the area under a sum of different curves $y = X_1 + X_2$ is the sum of the areas under the individual curves $y = X_1$ and $y = X_2$ (the integral of a sum is the sum of the integrals).

Proofs were offered for both of these rules.

“In the third rule, which took up the bulk of the work, Newton considered cases where ‘the value of y, or any of its terms’ were so compounded as to require a reduction into more simple terms. This was done variously by division, by
the extraction of roots, and by the resolution of affected equations [equations in which \( y \) is only implicitly defined in terms of \( x \), such as by a polynomial equation involving both \( x \) and \( y \)]. Thus, if the curve was a hyperbola, and the equation was \( y = 1/(1 + x^2) \), Newton began by dividing 1 by \( 1 + x^2 \) which yielded:

\[
y = 1 - x^2 + x^4 - x^6 + x^8 - \ldots
\]

Rule 2 was applied at this point and the area of the hyperbola was seen to be equal to:

\[
x - 1/3 x^3 + 1/5 x^5 - 1/7 x^7 + 1/9 x^9 - \ldots
\]

The series, Newton noted, was an infinite one and therefore carried on indefinitely. What, then, of the area of the hyperbola? No matter, Newton somewhat complacently responded, as ‘a few of the initial terms are exact enough for any use.’

“There was more in *De analysi*, historians have noted, than the manipulation of infinite series. When at the conclusion of the paper Newton set out his proof of Rule 1, he revealed at the same time details of his method of fluxions. The proof required the use of ‘infinitely small’ areas (later to be called ‘moments’). It was, Boyer has noted, ‘the first time in the history of mathematics that an area was found through the inverse of what we call differentiation’, and thus made Newton ‘the effective inventor of the calculus’, for his ability ‘to exploit the inverse relationship between slope and area through his new infinite analysis.’

“Newton’s failure to establish his priority at this point by following the advice of Barrow and Collins would later involve him, and many others, in much distress and in considerable polemical effort” (Gjertsen, pp.151-152).

Born the son of a Welsh farmer, William Jones (1675-1749) earned his living teaching mathematics. One of his pupils later became the Earl of Macclesfield, who in due course took Jones upon his staff at Shirburn Castle, Oxfordshire. Jones was known to Newton through the publication of his *Synopsis palmariorum matheseos* (1706), a work which introduced the symbol \( \pi \) to denote the ratio of the circumference to the diameter of a circle.

“In 1708, [Jones] had obtained the papers of John Collins, including the original correspondence in which Barrow had discussed Newton’s mathematical work for the first time outside Cambridge. Also among Collins’ manuscripts were copies that had been made of a number of unpublished papers. One of these was an anonymous version of ‘De analysi’. From the correspondence now in his possession, Jones was quickly able to identify Newton as the author of this essay and he began to make preparations for its publication.

“Jones was perhaps fortunate in the moment of his acquisition of Collins’ archive. It is hard to imagine Newton wishing to collaborate on an edition of his juvenilia at any time before the middle of the first decade of the eighteenth century. Then, however, it started to become increasingly important to him to find clearly dated evidence of his work on infinitesimals during the 1660s. Only by doing this could he establish a significant interval between the moment of his own invention of the calculus and Leibniz’s discoveries. The prospect of access to the letters in which he had first described his mathematical activities must have seemed like a godsend. In particular, the correspondence with Collins about Newton’s planned additions to Mercator’s translation of Kinckhuysen [*Algebra Ofte Stelkonst*, 1661, an introduction to algebra] indicated that he had already reached beyond the mathematical competence of his Continental counterparts. Letters from Barrow and Collins testified to the extent of Newton’s abilities even before he had read Mercator’s *Logarithmotechnia* (1668), a book that had considerably
extended contemporary knowledge of infinite series. It was true that Newton’s youthful letters expressed modesty and reservations about the nature of his own discoveries at this point. But the slightly later tract, ‘De analysi’, which Newton had planned to revise for publication in the early 1670s, suggested a more confident claim to the originality of his thinking. Moreover, as Newton almost certainly realised, Collins had allowed Leibniz sight of his copy of the manuscript when the young German natural philosopher visited London in October 1676.

“Newton communicated the autograph copy of ‘De analysi’ to Jones for use in the preparation of his edition. He also gave permission for Jones to include two other early mathematical papers, ‘Enumeratio linearum tertii ordines’ and ‘Methodus differentialis’ in his work. These dated in origin from the late 1660s and early 1670s, as notes in Newton’s ‘Waste Book’ and elsewhere indicate. They bore signs, however, of much more recent revision. This was even more true of the fourth essay that Jones edited, ‘De quadratura curvarum’, in which Newton’s full mastery of the dynamic nature of his calculus and of the peculiar notation that expressed it was made clear. Newton composed this work in the early 1690s, not in the 1660s, as he had hinted when he had published it as an appendix to the Opticks in 1704 …

“In his introduction to the edition, Jones quoted extensively from the correspondence that he had collected to prove Newton’s priority in the invention of the calculus. In about 1712, he placed many of the originals at Newton’s disposal. Some of these, together with both Collins’ copy and the autograph of ‘De analysi’, Newton later deposited in the Royal Society. Most of the earliest letters, however, entered the Macclesfield Collection [and are now in Cambridge University Library]. As a result of his efforts, Jones was elected a Fellow of the Royal Society in 1712” (Footprints, pp. 78-80).
“The [Methodis differentialis] began, as did so many of Newton’s mathematical papers, with a query. A certain John Smith working on a table of square and higher roots, at the suggestion of John Collins, sought Newton’s advice in 1675 on ways to reduce the immense computational labours involved. Newton advised Smith to pursue methods of interpolation and, more importantly, began to consider himself how such methods could be generalised. The Methodis, at its most general, sought to show how ‘Given some number of terms of any series whatever arranged at given intervals, to find any intermediate term you will with close approximation’ ([Papers VIII, p. 251]). Something of this work also emerged in Principia in Book III, lemma V, where it is demonstrated how ‘To find a curved line of the parabolic kind which shall pass through any given number of points.’

De quadratura curvarum was the last of Newton’s major treatises on calculus to be composed, in 1691, but the first to be published, as an appendix to the Opticks (1704). It was in this work that Newton developed the method of fluxions in terms of the ‘prime and ultimate ratios’, an early version of the theory of limits, first met with in Principia. "The work is significant in a number of ways. At the level of notation the manuscript of 1691-2 saw for the first time the use of Newton’s dotted fluxional notation … Also used was a capital Q to stand for the process of quadrature, rather than the summation sign ∫ adopted by Leibniz in his published work. On a more substantive issue, De quadratura contained the first published statement of the binomial theorem, discovered by Newton some forty years before.

“The text of De quadratura, in its published form, is in two parts. In the first part Newton, in the manner of De analysi, demonstrated how infinite series could be deployed to determine the quadrature and rectification of curves. In the second part he returned to the topic of fluxions, discussed at greater length in his then unpublished De methodis [eventually published as The method of fluxions and infinite series in 1736]” (Gjertsen, p. 579).

In the final treatise in this collection, Enumeratio linearum tertii ordinis, composed around 1695 and first published in the Opticks, Newton sought to classify cubic curves, in a manner analogous to the classification of quadratic curves (conics) into ellipses, parabolas and hyperbolas (and some degenerate cases). Newton identified 72 species of cubic curves, mostly classified in terms of the properties of their diameters and asymptotes. There are, in fact, 78 species: four were added by James Stirling in his Lineae tertii ordinis Newtonianae (1717), and the remaining two by François Nicole and Nicolas Bernoulli in the 1730s. Newton also stated a general theorem according to which all cubic curves can be obtained as ‘shadows’ cast by one particular species of cubic, in a manner analogous to that in which every conic section can be obtained as a shadow of a circle. This was proved by Nicole and Alexis-Claude Clairaut in 1731. “In some ways the Enumeratio is the most original of Newton’s mathematical work. It had no predecessors, met with no rivals claiming to have anticipated the results, or few even who acknowledged its results. No mention of the work appeared in the Philosophical Transactions before 1715, while Bernoulli, writing to Leibniz in 1714, commented ‘I have not yet been able to bring myself to examine this matter, since I do not willingly embroil myself with intricacies of that sort, utterly useless as they are indeed’ ([Papers, VII, p. 572])” (Gjertsen, p. 187).
THE ‘MOTHER OF MODERN ALGEBRA’


$2,800

*Offprint from: Mathematische Annalen, Band 83, Heft 1/2. 8vo (231 x 158 mm), pp. [1, blank], [24], 25-66. Original printed wrappers, a very fine copy.*

First edition, very rare offprint, of one of the most important works of “the most significant creative mathematical genius thus far produced since the higher education of women began” (Einstein, obituary in The New York Times, May 5, 1935). In the same year, but before she died, Norbert Wiener wrote: “Miss Noether is … the greatest woman mathematician who has ever lived; and the greatest woman scientist of any sort now living, and a scholar at least on the plane of Madame Curie.” “The prominent algebraist Irving Kaplansky called Emmy Noether the ‘mother of modern algebra.’ The equally prominent Saunders MacLane asserted that ‘abstract algebra,’ as a conscious discipline, starts with Noether’s 1921 paper ‘Ideal Theory in Rings’ [the offered paper]. Hermann Weyl claimed that she ‘changed the face of algebra by her work’” (Kleiner, p. 91). “During the period from 1920 to 1926, she attracted numerous mathematicians and students – she was the doctoral advisor for ten – to her research program and she became a leader in the development of modern abstract algebra” (Grolier).

A ring is an algebraic object which shares some, but not all, of the properties of the integers (whole numbers): it has addition and multiplication (and the result of these operations does not depend on the order in which they are performed), there are 0 and 1, but division is not usually possible and indeed the product of non-zero elements can be zero. The integers form the simplest example of a ring,
but many other examples arise from geometry and number theory, as well as other areas of mathematics. In the present paper Noether extends to the general setting of a ring some well-known properties of the factorization of integers into products of prime numbers. It turns out that this cannot usually be done with the elements of the ring – rather it is the ‘ideals’ of the ring which enjoy good factorization properties (an ideal is a subset of the ring with certain properties – see below). “Formulating geometry and number theory in the language of rings is currently a massive mathematical operation, and Noether’s work is a turning point in that endeavour” (Gray, p. 295). No copies located on OCLC or in auction records.

According to Emmy Noether’s student and successor Bartel van der Waerden, “the essence of Noether’s mathematical credo is contained in the following maxim: ‘All relations between numbers, functions and operations become perspicuous, capable of generalization, and truly fruitful after being detached from specific examples, and traced back to conceptual connections.’ We identify these ideas with the abstract, axiomatic approach in mathematics. They sound commonplace to us. But they were not so in Noether’s time. In fact, they are commonplace today in considerable part because of her work.

“Algebra in the nineteenth century was concrete by our standards. It was connected in one way or another with real or complex numbers. For example, some of the great contributors to algebra in the nineteenth century, mathematicians whose works shaped the algebra of the twentieth century, were Gauss, Galois, Jordan, Kronecker, Dedekind, and Hilbert. Their algebraic works dealt with quadratic forms, cyclotomy, field extensions, permutation groups, ideals in rings of integers of algebraic number fields, and invariant theory. All of these works were related in one way or another to real or complex numbers.

“Moreover, even these important works in algebra were viewed in the nineteenth century, in the overall mathematical scheme, as secondary. The primary mathematical fields in that century were analysis (complex analysis, differential equations, real analysis), and geometry (projective, non-euclidean, differential, and algebraic). But after the work of Noether and others in the 1920s, algebra became central in mathematics …

“Noether contributed to the following major areas of algebra: invariant theory (1907–1919), commutative algebra (1920–1929), non-commutative algebra and representation theory (1927–1933), and applications of non-commutative algebra to problems in commutative algebra (1932–1935). [‘Commutative’ here means that the order in which any two elements of the algebra are multiplied has no effect on the result.] … The two major sources of commutative algebra are algebraic geometry and algebraic number theory. Emmy Noether’s two seminal papers of 1921 and 1927 on the subject can be traced, respectively, to these two sources. In these papers, entitled, respectively, ‘Ideal Theory in Rings’ and ‘Abstract Development of Ideal Theory in Algebraic Number Fields and Function Fields,’ she broke fundamentally new ground, originating ‘a new and epoch-making style of thinking in algebra’ (Weyl)” (Kleiner, pp. 91-94).

“[A] fundamental concept which she highlighted in the 1921 paper was that of a ring. This concept, too, did not originate with her. Dedekind (in 1871) introduced it as a subset of the complex numbers closed under addition, subtraction, and multiplication, and called it an ‘order.’ Hilbert, in his famous Report on Number Theory (Zahlbericht) of 1897, coined the term ‘ring,’ but only in the context of rings of integers of algebraic number fields. Fraenkel (in 1914) gave essentially the modern definition of ring, but postulated two extraneous conditions. Noether in her 1921 paper gave the definition in current use” (Kleiner, p. 95).
"As she stated in opening [the offered] paper, ‘the aim of the present work is to translate the factorization theorems of the rational integer numbers and of the ideals in algebraic number fields into ideals of arbitrary integral domains and domains of general rings.’ Drawing on Fraenkel’s formulation of a ring, Noether made the key observation that the abstract notion of ideals in rings could be seen not only to lay at the heart of prior work on factorization in the context of algebraic number fields and of polynomials (in the work of Hilbert, Francis Macaulay (1862-1937), and Emmanuel Lasker (1868-1941)) but also to free that work from its reliance on the underlying field of either real or complex numbers. In her paper she built up that theory – not surprisingly in the context of commutative rings given her motivating examples – in structural terms.

“Noether began by presenting what are essentially the modern axioms for a (commutative) ring … With the notion of a ring thus established, Noether proceeded to define an ideal in terms somewhat different from those employed by Dedekind in 1871. [A subset of a ring is an ideal if the sum and difference of any two elements of the ideal is still in the ideal, and the product of any element of the ideal by any element of the ring is in the ideal.] … Since, for Noether, ideals in general rings were to play the role of factors in the setting of the rational numbers as well as of ideals, in Dedekind’s sense, she needed to define a general notion of divisibility [by ideals] … Another key ingredient to Noether’s approach was the fact that ideals were finitely generated … This assumption allowed her to immediately prove one of the hallmarks of her approach to ring theory, namely, the so-called ascending chain condition on ideals, or, as she termed it, the ‘theorem on finite chains’ … Noether used this structural notion – based on clearly and crisply articulated axioms – to show how to decompose ideals and, in so doing, to establish the factorization theories of her predecessors and contemporaries in the general context of commutative rings” (Katz & Parshall, pp. 441-2).
"Through her ground-breaking papers in which that concept [of a ring] played an essential role, and of which the 1921 paper was an important first, she brought it into prominence as a central notion of algebra. It immediately began to serve as the starting point for much of abstract algebra, taking its rightful place alongside the concepts of group and field, already reasonably well established at that time.

"Noether also began to develop in the 1921 paper a general theory of ideals for commutative rings. Notions of prime, primary, and irreducible ideal, of intersection and product of ideals, of congruence modulo an ideal—in short, much of the machinery of ideal theory, appears here. Toward the end of the paper she defined the concept of module over a non-commutative ring and showed that some of the earlier decomposition results for ideals carry over to submodules …

"The concepts she introduced, the results she obtained, and the mode of thinking she promoted, have become part of our mathematical culture … A number of mathematicians and historians of mathematics have spoken of the ‘algebraization of mathematics’ in the twentieth century. Witness the terminological penetration of algebra into such fields as algebraic geometry, algebraic topology, algebraic number theory, algebraic logic, topological algebra, Banach algebras, von Neumann algebras, Lie groups, and normed rings. Noether's influence is evident directly in several of these fields and indirectly in others" (Kleiner, pp. 95-100).

"Emmy Noether (1882-1935) had come to mathematics at a time when women were officially debarred from attending university in Germany. Daughter of the noted algebraic geometer and professor at the University of Erlangen, Max Noether (1844-1921), Emmy Noether had grown up among mathematicians and in a university setting but had initially pursued an educational course deemed culturally suitable for one of her sex, that is, she qualified herself to teach modern languages in ‘institutions for the education and instruction of females.’ This vocational preparation aside, Noether studied mathematics – from 1900 to 1902 and then in the winter semester of 1904-1904 – as an auditor and at the discretion of selected professors at her hometown University of Erlangen and at Göttingen University, respectively. At the time, there was no other way for a German woman to attend courses at either institution. Things changed at the University of Erlangen in 1904 and throughout the country four years later, when women were officially allowed to attend and earn degrees from German universities. Noether enrolled at Erlangen in 1904 and earned a doctorate there under the invariant theorist, Paul Gordan, in December of 1907.

"Her degree in hand, however, Noether had nowhere to go within the German system. Even as holders of the doctoral degree, women were allowed neither to hold faculty positions nor even to lecture. Noether thus remained in Erlangen until 1915 as an unofficial assistant to her father, while pursuing her own research agenda. In 1915 and at the invitation of her former professors, Felix Klein and David Hilbert, she finally had the opportunity to make the move to Göttingen. There, she lectured in courses, although in physics, officially listed under Hilbert’s name, beginning in the winter semester of 1916-1917, while Hilbert and Klein fought with the university authorities to allow her to obtain the Habilitation or credentials that would allow her to lecture in her own right. They won that final battle in 1919 in a Germany defeated in World War I and under the new parliamentary representative democratic government of the Weimar Republic that had replaced the old imperial regime" (Katz & Parshall, pp. 438-9).

After the War, “Emmy Noether’s attention shifted to abstract algebra, and by the end of the 1920s she had drawn a group of almost equally gifted young mathematicians around her, Emil Artin and Bartel van der Waerden among them, jocularly called the Noether boys. It is they who turned her way of thinking into ‘Modern Algebra,’ while she did fundamental work on commutative and non-commutative algebra.
"In 1933 the Nazis came to power in Germany and set about barring Jews from the civil service and the universities. Emmy Noether was a Jew, and one with well-known left-wing sympathies, but she was able to get a position at the women’s university Bryn Mawr, in Pennsylvania, USA. She died there on 14 April 1935 from a post-operative infection" (Gray, p. 290).

"Noether is best known for her contributions to the development of the then-new field of abstract algebra, as well as ring theory. But one of the reasons Hilbert pushed to bring Noether to Göttingen was the hope that her expertise on invariant theory could be brought to bear on Albert Einstein’s fledgling theory of general relativity, which seemed to violate conservation of energy.

"Noether did not disappoint, devising a theorem that has become a fundamental tool of modern theoretical physics. One of its consequences is that if a physical system behaves the same regardless of its spatial orientation, the system’s angular momentum is conserved. Noether’s theorem applies to any system with a continuous symmetry. When Einstein read Noether’s work on invariants, he wrote to Hilbert: “I’m impressed that such things can be understood in such a general way. The old guard at Göttingen should take some lessons from Miss Noether. She seems to know her stuff” (APS News, vol. 22, no. 3, March 2013). Nobel Laureate Frank Wilczek of MIT has written that Noether’s theorem “has been a guiding star to 20th and 21st century physics” (quoted by Emily Conover, Science News, June 12, 2018).

THE FOUNDING PAPER OF QUANTUM CHEMISTRY

PAULING, Linus. The Nature of the Chemical Bond. Application of Results Obtained from the Quantum Mechanics and from a Theory of Paramagnetic Susceptibility to the Structure of Molecules. [Easton: Mack Printing Co., 1931].

$15,000


First edition, extremely rare offprint, signed by Pauling, of this landmark paper, the birth of the ‘valence bond theory’ of molecular structure and the beginning of the application of quantum mechanics to chemistry. This is “arguably Pauling’s most important contribution to science [and] it was the contribution of which Pauling himself was most proud” (Goertzel & Goertzel, Linus Pauling (1995), pp. 70-71). As students in the mid-1920s, Pauling and his colleagues worked under the prevailing theory that atoms formed molecules through rudimentary ‘hook-and-eye’ bonds conceptually similar to the types of devices used by recreational fishermen to connect their boats to the back of towing vehicles. Pauling shattered these now-archaic assumptions by applying the new quantum physics to the understanding of molecular architecture. Introducing concepts such as valency and the hybrid orbital, Pauling posited a revolutionary set of theories in which chemical bonds were created through the exchange of energy between atoms. Almost instantly the hook and eye approach was cast into oblivion – Pauling had drafted the new blueprint for modern structural chemistry. “New Scientist” magazine once characterized Pauling as one of ‘the 20 greatest scientists of all
time, on a par with Newton, Darwin, and Einstein.' Pauling has also been called one of the two greatest scientists of the 20th century (the other being Einstein) and the greatest chemist since Antoine-Laurent Lavoisier, the 18th-century founder of modern chemistry” (American Chemical Society). Pauling is the only person in history to win two unshared Nobel Prizes – the 1954 chemistry prize “for his research into the nature of the chemical bond and its application to the elucidation of the structure of complex substances,” and the Nobel Peace Prize in 1962. ABPC/RBH lists one other copy, also inscribed (Sotheby’s, 15 June, 2006, lot 116, $6600). OCLC lists only one copy in North America, in the Ava Helen and Linus Pauling Archives at Oregon State University (scarc.library.oregonstate.edu/coll/pauling/bond/papers/1931p.3-28.html).


"In the fall of 1927, a newly hired professor – tall and energetic, with a beautiful young wife and an abundance of self confidence – arrived at the California Institute of Technology near Los Angeles. His name was Linus Pauling. He came fresh from Europe, where he had spent more than a year on a Guggenheim Fellowship participating in a scientific revolution. He did not know it, but he was about to start another at Caltech. During the next twelve years he would reshape the study of chemistry, lay the groundwork for molecular biology, write one of the most important books in scientific history and define the nature of the chemical bond. In 1954 he would win a Nobel Prize for his work …

"Although trained as a chemist, he had spent his time in Europe studying theoretical physics — a passion that ran so deep he had seriously considered switching from chemistry to physics. The science he learned in Münich, Copenhagen and Zürich was a new approach to the field called quantum physics. Pauling learned about it directly from its discoverers Niels Bohr, Erwin Schrödinger, Werner Heisenberg and Wolfgang Pauli, and from its greatest teacher, Arnold Sommerfeld. Schrödinger’s approach, based on the physics of waves, especially interested Pauling because he saw that it might throw new light on questions he had pondered since he was an undergraduate: What forces held atoms together to form molecules? How did those forces give the molecules particular shapes and qualities? …

"Linus Pauling intended to solve these mysteries by applying the new physics he had learned in Europe. He was following the lead of one of his scientific heroes: the legendary, cigar-chomping head of chemistry at Berkeley, Gilbert Newton Lewis. In the early 1920s, Lewis published an idea about the bonds between atoms that he had developed with General Electric researcher Irving Langmuir. They theorized that an element’s valence arose naturally from its atomic structure. Atoms, it was known, consisted of a positively charged nucleus surrounded by negatively charged electrons. Lewis and Langmuir hypothesized that atoms were most stable when the electrons orbited the nucleus in shells containing eight at a time (except for the atom’s innermost shell, which contained two electrons). According to the Lewis and Langmuir model, if an atom had seven electrons in an outer shell, it would tend toward collecting an eighth for maximum stability. One way to do this was to combine with another atom that had one extra electron in its outer shell. The two atoms would ‘share’ an electron, creating a more stable product. The resulting ‘shared electron bond’ tied the atoms together into a molecule …

"Pauling was intrigued by the Lewis and Langmuir model, but he knew that it was too simple to explain a number of laboratory observations about real molecules. In addition, he learned in Europe that the sort of atomic structure Lewis and Langmuir used in their model – one in which electrons orbited the nucleus like little planets – was in the process of being discarded. The new quantum physics was bringing to light an entirely new, paradoxical and exciting view of the atom.
And it was on the foundation of this new science that Pauling intended to build a new understanding of the chemical bond …

“When Pauling arrived in Münich on his Guggenheim Fellowship in 1925, Sommerfeld’s institute was abuzz with news of a radically new approach toward understanding the atom that had been proposed by one of Sommerfeld’s former students, a young physicist named Werner Heisenberg … But then, just as the Paulings were settling into a tiny Münich apartment, a seemingly new, very different approach was presented by one of Heisenberg’s critics, the Austrian physicist Erwin Schrödinger. The two competing theories were the subject of heated debate during the entire time Pauling was in Europe. But he quickly decided which one appealed to him most.

“Heisenberg’s purely mathematical approach to the structure of the atom – based on a difficult set of matrix calculations – yielded results that matched the bewildering array of new observations physicists were making about the properties of simple atoms. But for a chemist like Pauling, trained to view atoms and molecules as real things with particular sizes and shapes, pure mathematics was unsatisfactory. He preferred Schrödinger’s theory. The old picture of electrons circling the atomic nucleus like little planets did not fit the new data physicists were gathering. But unlike Heisenberg’s purely mathematical approach, Schrödinger proposed a new theory that replaced orbiting electrons with an image more like standing waves around the nucleus – waves like those found in a plucked guitar string or the head of a beaten drum. By applying an existing mathematics of wave functions to atomic questions, Schrödinger was able to create equations that matched the properties of simple atoms.

“It became clear during the months of Pauling’s stay in Europe that Schrödinger’s and Heisenberg’s ideas were not two different realities but two different
mathematical methods for arriving at the same atomic reality. Ultimately they became joined under a new name: quantum mechanics. Researchers, it seemed, could pick whichever method was easiest to use for a particular problem. Pauling preferred the wave approach not only because the mathematics was somewhat easier for him but also, he said, because it contained ‘at least a trace of physical picture behind the mathematics.’

“Linus Pauling returned to America in 1927 fired with the inspiration of the new quantum mechanics. He was one of the first Americans to understand the importance of the European revolution in physics, and one of the first to apply its lessons to the field of chemistry” (scarl.library.oregonstate.edu/coll/pauling/bond/index.html).

“Pauling was building on a body of earlier work regarding the use of quantum physics to study chemical structure, most notably a 1927 paper by Walter Heitler and Fritz London. Also, parts of his work were independently duplicated by J. C. Slater, thus leading to the name HLSP (Heitler-London-Slater-Pauling) theory, which Pauling used in his own writing to refer to the valence bond theory. While Heitler and London’s contributions are frequently highlighted in European writing on the subject, Americans, particularly chemists, tend to associate the theory primarily with Pauling. But the truth is that, while Heitler and London’s work was important, Pauling was the first to devise a systematic method for applying quantum-mechanical concepts to complex molecules. It was Pauling’s ideas that made the crucial link between the atomic and molecular realms.

“It is perhaps difficult for the nonscientist to appreciate the magnitude of this achievement. Even a molecule as ‘simple’ as methane, considered as a system of elementary particles, was far too complex to be analyzed mathematically using the equations of quantum physics. One might say that deriving the behavior of a molecule by quantum physics is like deriving the behavior of a group of people from a knowledge of the personalities of the individual people. In both cases, certain rough predictions can be made easily, but gaining detailed understanding is very difficult, because many subtle interactions are at play.

“Today one can obtain rather good results about molecular structure from computer simulations, but in the 1920s and 1930s computers did not exist, and one had to rely entirely on human ingenuity and mathematical tables. Pauling’s theory of the chemical bond consisted of six rules, three of which followed fairly directly from the mathematics of quantum theory as applied to hydrogen, helium, and lithium atoms, and three of which were pure inspiration. Each of these rules was stated in mathematical form. It is possible, however, to express the meaning of the rules in ordinary language, although much of the precision is lost.

“The first three rules, roughly speaking, are as follows: First, electron pair bonds are formed by the interaction of two unpaired electrons, one on each of the two bonding atoms. Second, the spins of the electrons must be opposed when the bonds are formed, so that they do not contribute to the magnetic properties of the substance. And third, the two electrons that form a shared pair cannot take part in forming additional pairs. These rules systematized the understanding of chemical bonding that was emerging from the rapidly developing quantum theory.

“The next three rules were fundamentally novel; they may represent Pauling’s greatest stroke of genius. They exemplify, more than any other single discovery, the extraordinary chemical intuition that, in one area after another, led Pauling to simple and elegant explanations of extremely complex phenomena. The rules were justified, in the 1931 paper, by sketchy mathematical and qualitative arguments; their real justification lies in the numerous chemical structures that have been correctly inferred from them.
“The fourth rule states that the most important terms in the equations for the electron-pair bond are those involving only one quantum wave function from each atom. This is a mathematical approximation of the type often made by physicists: one ignores interactions that are ‘small’ in magnitude in order to derive tractable equations. The trick is always to make the right approximation – not to overlook the important points. Pauling’s fourth rule was inspired by the nineteenth-century idea of valence; it was ‘right’ in the sense that the interactions that it ignored were in many cases insignificant.

“Pauling’s fifth rule was the greatest innovation: it states that, generally speaking, stronger bonds are formed by orbitals that overlap more with orbitals of the other atom. So, if there are two orbitals competing for a bond with a certain atom, the winner will be the one that overlaps more with that atom. In addition, the direction of the bond formed by an orbital will tend to be the same as the direction that the orbital is concentrated in. There was nothing in the old idea of valence to suggest this fifth rule, because the idea of ‘overlap’ was a new one, a corollary of the idea of a quantum wave function. However, despite its lack of precedents, the rule smacks of common sense. Greater overlap makes a stronger bond, and the bond is in the direction of the orbital that is bonding – these are very natural, intuitive conclusions.

“Finally, the sixth rule states that, between two orbitals concentrated in approximately the same direction, the stronger bonds will be formed by the orbital closer to the nucleus of the atom, which corresponds to a lower energy level for the atom. This rule is mathematically similar to the fifth rule but tends to have fewer direct applications.

“The fifth rule implies … the hybridization of orbitals – the ability of bonding, in itself, to affect the form taken by the orbitals of an atom. If one wished to wax poetic, one might say that atoms reach out to each other, distorting the quantum wave functions of their electrons in precisely the most effective way to ‘grab’ each other. In this way atoms join together to make molecules, the basic elements of matter. This is a natural, intuitive idea, almost visceral in its simplicity, and it cuts through the complexity of interacting quantum wave functions in a most remarkable way.

“This work culminated in a series of important papers, beginning with ‘The Nature of the Chemical Bond’ in 1931 [the offered paper], and is described in detail in Pauling’s book The Nature of the Chemical Bond (1939). The pivotal concept in the theory is the highly technical, highly innovative idea of the hybridization of orbitals, based on the concept of resonance among different electrons.

“The basic idea for this paper came to Pauling in a flash of insight, after many days of struggling with complex mathematical models:

‘Finally, in December 1930, one day I thought of a way to get around the mathematical difficulties. A simplification which made it very easy to get the results. And I was so excited and happy, I think I stayed up all night making, writing out, solving the equations which were so simple that I could solve them in a few minutes. Solve one equation and get the answer and solve another equation … I just kept getting more and more euphorious as time went by, and it didn’t take me long to write a long paper about the nature of the chemical bond. That was a great experience.’

“The 1931 paper combined chemistry and physics to an unprecedented extent. And, years later, Pauling remembered believing that it would have the journal
editor, Arthur Beckett Lamb, ‘buffaloed … [He] thought, ‘What referee shall I send this paper to? It has to be somebody who has a good knowledge of chemistry … but also has a thorough understanding of quantum mechanics, and I can't think of anybody of that sort,' anybody who might be said to be my peer. He [thought], 'Well, past experience has shown that this author knows what he's writing about, so I'll just go ahead and publish the paper.'

“‘The paper appeared just seven weeks after it was submitted. The original 1931 article was followed within the next three years by many other articles refining and developing Pauling’s model of the chemical bond and applying it to numerous other substances. Pauling’s achievement was quickly recognized by the scientific establishment in the United States. Late in the spring of 1931, he was selected to receive the Langmuir Prize from the American Chemical Society. He was the first recipient of this award, which was intended to honor the most promising young research chemist in the United States. Overnight, Pauling became a celebrity. His office and his home were invaded by reporters. Caltech and Pasadena were proud of the accomplishments of one of their favorite sons. The wire services spread the news throughout the country and abroad. The New York Times and Herald Tribune, the Christian Science Monitor, The Nation, and the Portland Oregonian were among the publications that spoke of Pauling as ‘the rising star who may yet win the Nobel Prize.’ They were quoting the president of the American Chemical Society.

“‘The New York Times told how Albert Einstein, while visiting the Pasadena campus in 1931, asked many questions of Pauling at a seminar, confessed his lack of understanding of the chemical bond, and apologized for taking so much of the speaker’s time. The Portland Oregonian speculated that if only ten men in the world could understand Einstein's theory of relativity, there must be even fewer who could understand Pauling's work. This was not actually true of Pauling's or Einstein's work, of course. Pauling's work was readily understood by other specialists in the field of molecular chemistry. In fact, unlike many other leading scientists, Pauling had a great gift for making his ideas intelligible and did so frequently in lectures” (Goertzel & Goertzel, pp. 72-77).
The best bond eigenfunction which can be obtained from \( s, \beta \) and \( d \) is
\[
\frac{1}{2} \sqrt{3} s + \frac{1}{2} \sqrt{3} \beta + \frac{\sqrt{3}}{2} d
\]
and has a strength of 3. The best two equivalent bond eigenfunctions involving one \( d \) eigenfunction
\[
\frac{1}{2} \sqrt{3} s + \frac{1}{2} \sqrt{3} \beta + \frac{\sqrt{3}}{2} d \quad \text{and} \quad \frac{1}{2} \sqrt{3} s - \frac{1}{2} \sqrt{3} \beta + \frac{\sqrt{3}}{2} d
\]
are oppositely directed and have a strength of 2.95.

The atoms of the transition elements, for which \( d \) eigenfunctions need to be considered, are of such a size as usually to have a coordination number of 4 or 6, so that four or six equivalent bond eigenfunctions are here of especial interest. If there is available only one \( d \) eigenfunction to be combined with an \( s \) and three \( \beta \) eigenfunctions, then no more than five bond eigenfunctions can be formed. One may have the maximum strength 3, in which case the others are weak; or two may be strong and three weak; but such a single \( d \) eigenfunction no more than four strong bonds can be formed, and these lie in a plane. The fifth bond is necessarily weak. The four equivalent bond eigenfunctions formed from \( s, \beta \) and one \( d \) eigenfunction are
\[
\begin{align*}
\psi_1 &= \frac{1}{2} s + \frac{1}{2} \beta + \frac{\sqrt{3}}{2} d \\
\psi_2 &= \frac{1}{2} s - \frac{1}{2} \beta + \frac{\sqrt{3}}{2} d \\
\psi_3 &= \frac{1}{2} s + \frac{1}{2} \beta - \frac{\sqrt{3}}{2} d \\
\psi_4 &= \frac{1}{2} s - \frac{1}{2} \beta - \frac{\sqrt{3}}{2} d
\end{align*}
\]

(12)

The polar graph of
\[
\frac{1}{2} x + \sqrt{3} y, \quad \text{in the} \quad xy \text{plane,} \quad \cos \theta = \sqrt{3} \cos 2 \phi
\]
in the \( xy \) plane, representing one of the four equivalent \( d \beta ^4 \) bond eigenfunctions. The directions of the maxima of the four are represented by arrows.

Fig. 8.—Polar graph of
\[
\frac{1}{2} x + \sqrt{3} y, \quad \cos \theta = \sqrt{3} \cos 2 \phi
\]
in the \( xy \) plane, representing one of the four equivalent \( d \beta ^4 \) bond eigenfunctions. The directions of the maxima of the four are represented by arrows.

The equivalent tetrahedral bond eigenfunctions
\[
\begin{align*}
\phi_{1a} &= \frac{1}{4} s + \frac{\sqrt{3}}{4} \beta + \frac{\sqrt{6}}{4} d \\
\phi_{2a} &= \frac{1}{4} s + \frac{\sqrt{3}}{4} \beta - \frac{\sqrt{6}}{4} d \\
\phi_{3a} &= \frac{1}{4} s - \frac{\sqrt{3}}{4} \beta + \frac{\sqrt{6}}{4} d \\
\phi_{4a} &= \frac{1}{4} s - \frac{\sqrt{3}}{4} \beta - \frac{\sqrt{6}}{4} d
\end{align*}
\]

have a strength of 2.950, nearly equal to the maximum 3. These leave only two pure \( d \) eigenfunctions behind, however, the others being part \( d \) and part \( \beta \). Thus we conclude that if there are three \( d \) eigenfunctions available, a transition group element forming four electron-pair bonds will direct them toward tetrahedral corners. Examples of such bonds are provided by \( \text{CrO}_4^{2-} \), \( \text{MoO}_4^{2-} \), etc. Only when one \( d \) eigenfunction above is available will the four bonds lie in a plane. In compounds of bivalent
THE DISCOVERY OF
THE FINITE SPEED OF LIGHT


$14,500

4to (221 x 162 mm), pp. 233-236 in: Journal des Scavans, no. XX, 7 December 1676. 44 issues of the Journal des Scavans bound in one volume, pp. [ii, title page to 1676 volume], 161-172 (no. [XIV], 5 April 1666), 491-502 (1666, no. XLII), 12 (17 December 1674), 49-60 (1675, no. V), 157-168 (1675, no. XIV), 181-192 (1675, no. XVI), 217-240 (1675, nos. XIX & XX), 48 (1676, nos. I-IV), 85-96 (1676, no. VIII), 121-132 (1676, no. XI), 145-156 (1676, no. XIII), 181-240 (1676, nos. XVI-XX), 253-264 (1676, no. XXII), 72 (1677, nos. I-VI), 85-228, 205-264 (1677, nos. VIII-XXIV, the volume misnumbered), with two folding engraved plates and numerous engraved and woodcut illustrations in text (some full-page). Contemporary calf, spine richly gilt in compartments with red lettering-piece, red speckled edges (ends of joints split, foot of spine and one corner worn), a few leaves folded in to avoid cropping, a couple of diagrams just shaved, but still a very good large copy (the Rømer article with good margins and no cropping).

First edition, very rare, of the discovery that light is not instantaneously propagated, as Aristotle, Kepler and Descartes had maintained. “The timing of the eclipses of the satellites [of Jupiter] varies systematically with the relative positions of the earth, sun, and Jupiter; and Rømer notices that these variations can be accounted for by the varying distance of travel of the light rays. His calculations both require and, for the remaining doubters, help substantiate the Copernican theory of the
earth's revolution around the sun” (Parkinson). Rømer's discovery was made possible by the investigation of the motions of the satellites of Jupiter by Giovanni Domenico Cassini (1625-1712) which began at Bologna in 1666 and culminated in his *Ephemerides Bononiensis Mediceorum siderum* (1668). Following his move to Paris in 1669, Cassini continued observing Jupiter's satellites; one member of Cassini's staff there was Ole Rømer (1644-1710), brought from Denmark by Jean Picard in 1671. "In 1675 Cassini found an inequality [irregularity] in the motion of Jupiter's innermost moon (now called Io) which was strongly correlated with the distance between Jupiter and the Earth. Cassini initially hypothesized that this inequality was due to a finite speed of light, but then he rejected this notion. Rømer adopted Cassini's stepchild, and predicted in September 1676 that the eclipse of Io that was to occur on 9 November of that year would take place 10 minutes later than the time that would be predicted if the 'equation of light' was ignored. His paper delivered to the Académie Royale des Sciences on 22 November 1676 was summarized in the issue of 7 December of the *Journal des Sçavans* (Van Helden, p. 138). Although Rømer did not actually calculate a value for the velocity of light, Van Helden has shown that the data available to him would have led Rømer to a value of at least 135,000 km/sec (the currently accepted value is 299,792 km/sec). Rømer's historic paper is here found in a composite volume, assembled and bound by a contemporary reader, comprising 44 individual issues of the *Journal des Sçavans* from the period April 1666 to December 1677 (each issue carries its own imprint at the foot of the last page). The choice of issues probably indicates an interest in astronomy, as the collection contains a substantial number of other important astronomical papers, including ten by Cassini discussing comets, sunspots, Jupiter's moons, a transit of Mercury, and other topics. The most notable is probably Cassini's discovery of a gap in the rings of Saturn, now called the 'Cassini division' ('Observations nouvelles de M. Cassini, touchant le Globe & l'Anneau de Saturne,' 1 March 1677, pp. 56-60). The original Paris printing of the *Journal des Sçavans* is rare on the market, unlike the common Amsterdam reprint.

"Rømer came from a family of small merchants. In 1662 he was sent to the University of Copenhagen, where he studied with both Thomas Bartholin, professor of medicine, and his brother Erasmus Bartholin, a physician who was better known for his discovery of double refraction of light in Iceland spar. He lived in Erasmus Bartholin's house and studied astronomy and mathematics under his direction; Bartholin was so impressed with Rømer's abilities that he entrusted him with the editing of the unpublished manuscripts of Tycho Brahe.

"In 1671 Erasmus Bartholin was visited by Jean Picard, who had been sent by the Académie des Sciences to measure precisely the position of Tycho Brahe's observatory, the Uraniborg, on the island of Hven [so that Tycho's observations could be referred to the coordinates of Paris]. In September of that year Bartholin and Rømer accompanied Picard to Hven, where, in order to re-determine the longitude of the observatory, they made observations of a series of eclipses of the first satellite of Jupiter, while G. D. Cassini carried out the same work in Paris. When Picard returned to Paris he took with him a notebook containing eight months' observations, the original manuscripts of Tycho Brahe's observational works, and Rømer, whom he had persuaded to work there under the auspices of the Academy …"
ephemeris, without notable success, and the task was assigned to the astronomers of the new Paris observatory by Colbert. G. D. Cassini and his nephew Maraldi discovered the first large inequality in the periodic times of the minima, that caused by the eccentricity of the orbit of Jupiter around the sun; their second discovery, announced by Cassini in August 1675, was more interesting, since the inequality seemed to depend on the position of the earth relative to Jupiter.

“Cassini considered, but discarded, the idea that the fluctuation of periodic times might be caused by the finite speed of light; it remained to Rømer to demonstrate that such was indeed the case. With rare exceptions, previous astronomers, both ancient and more recent – including Aristotle, Kepler, and Descartes – had held that light propagated itself instantaneously. Galileo, on the other hand, was not only convinced of its finite velocity, but also designed an experiment (although not an adequate one) by which the speed of light might be measured. These divergent views were discussed among the Paris academicians, and were well known to Rømer.

“In his observational work Rømer noticed that the eclipses of Io occurred at longer intervals as the earth receded from Jupiter, but happened in closer sequence as the earth and that planet came closer together. Beginning from the point at which the earth and Jupiter were closest to each other, Rømer tried to predict the time of occurrence of an eclipse of Io at a later date, when the earth and Jupiter had drawn further apart. In September 1676 he announced to the members of the Academy that the eclipse predicted for 9 November of that year would be ten minutes later than the calculations made from previous eclipses would indicate. Observations confirmed his hypothesis, and Rømer correctly interpreted this phenomenon as being the result of the finite velocity of light. He was thereupon able to report to the Academy that the speed of light was such as to take twenty-two minutes for light to cross the full diameter of the annual orbit of the earth; in other terms, that the light from the sun would reach earth in eleven minutes (a time interval now
measured to be about eight minutes and twenty seconds)” (DSB).

“Rømer’s idea was accepted with enthusiasm by Christiaan Huygens (1629-1695), who had temporarily left Paris for the Netherlands in June 1676 and discovered them through the excellent English translation (by Halley?) of the *Journal des Scavans* paper, which was published on 25 July 1677 in the *Philosophical Transactions of the Royal Society*. Actually, Huygens needed a finite velocity for light in order to account for reflection and refraction in his undulatory theory, and he was very pleased with Rømer’s theory. In his *Traité de la Lumièr...
velocity, even though the recent improvement in knowledge of the length of the astronomical unit made this feasible.

“Rømer, accordingly, makes little effort to determine the velocity of light. In the *Journal des Sçavans* for 7 December 1676, he ‘demonstrates that for a distance of about 3000 leagues, which is the approximate size of the diameter of the Earth, light requires less than a second of time … This is because of the 22 [minutes that it requires] for the whole interval … double that from here to the Sun’” (Taton & Wilson, pp. 153-4).

In 1681, Rømer returned to Denmark and was appointed professor of astronomy at the University of Copenhagen. He was active as an observer, both at the University Observatory at Rundetårn, the ‘Round Tower’, and in his home, using improved instruments of his own construction. Unfortunately, his observations have not survived: they were lost in the great Copenhagen Fire of 1728. However, a former assistant (and later an astronomer in his own right), Peder Horrebow (1679-1764), loyally described and wrote about Rømer’s observations. In Rømer’s position as royal mathematician, he introduced the first national system for weights and measures in Denmark in May 1683. In 1700, Rømer persuaded the king to introduce the Gregorian calendar in Denmark-Norway – something Tycho Brahe had argued for in vain a hundred years earlier. Rømer also developed one of the first temperature scales. Fahrenheit visited him in 1708 and improved on the Rømer scale, the result being the familiar Fahrenheit temperature scale still in use today.

Rømer’s article appeared in the Amsterdam reprint of the *Journal des Sçavans* in 1683 (pp. 276-9, without the famous illustration) and in 1730 in the *Histoire et Memoires de l’Academie Royale des Sciences*, Tome X (pp. 575-7). The *Journal des Sçavans* was the earliest academic journal to appear, preceding the *Philosophical Transactions of the Royal Society of London* by two months. The first issue was
published at Paris on 5 January, 1665. It was edited by Denis de Sallo, Sieur de la Coudraye, at the instigation of Louis XIV’s first minister, Colbert, whose objective was the state control of new knowledge. The Journal des Scavans ran into trouble almost immediately; de Sallo created enemies with his hard-hitting reviews. But perhaps more importantly for what followed, he openly supported the Gallican movement which was in favour of a French church independent of Rome. This antagonized the Church in general and the Jesuits in particular, and the papal nuncio used his influence to have the journal suspended after the thirteenth number on 30 March, 1665. Its need was however recognised and it was reinstated by the end of the year under a new editor, the Abbé Gallois for the years 1666-1674, the Abbé La Roque for 1674-1687, and a long sequence of other editors, though de Sallo remained for many years the moving force in the background. De Sallo promised that “We shall make known the experiments in Physics and Chemistry; which may serve to explain the effects of Nature, the new discoveries made in Arts & Sciences, such as the machines and the useful or curious inventions that can help Mathematics: observations of the Sky, of the Meteors, & what new things Anatomy may find in animals,” although for much of the 17th century the Journal published only a few original ‘scientific’ articles, the majority being book reviews, obituaries and decisions made by the religious and secular courts, as well as censorship pronouncements. Issues appeared weekly in the first couple of years of publication, and approximately fortnightly thereafter.


Cassini articles:

1676:

Eclipses des Satellites de Jupiter dans les derniers mois de l’année 1676, proposées par Monsieur Cassini de l’Académie Royale des Sciences pour la determination exacte des Longitudes des lieux où ells seront observées, p. 192


Description du mouvement qu’a fait une tache dans le Soleil sur la fin de Novembre dernier 1676, p. 239

Balance arithmetique, sa description et son usage pour connaitre les nombres par les poids, pp. 253-255

1677:

Suite des observations faites a l’Observatoire Royal, touchant la Tache qui a paru dans les Soleil, le mois d’Octobre, de Novembre & Decembre derniers, pp. 8-10

Observations nouvelles de M. Cassini, touchant le Globe & l’Anneau de Saturne, pp. 56-60
Nouvelle théorie de la Lune, de M. Cassini, pp. 117-120

Théorie de la Comète qui a paru aux mois d’Avril et de Mai 1677, tirée des observations des plus célèbres astronomes, p. 214-216

Vérification de la Periode de la Revolution de Jupiter autour de son Axe par les Observations nouvelles de Monsieur Cassini, pp. 214-216

Reflexions de M. Cassini sur les observations de Mercure dans le Soleil, pp. 247-248 (see preceding article by Galles, pp. 241-246)
PMM 380 - THE DISCOVERY OF X-RAYS


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First editions, first issues, and fine copies, of the rare offprints of Röntgen's discovery of X-rays, the most important contribution to medical diagnosis in a century, and a key to modern physics. "While performing experiments with a Crookes vacuum tube, a type of cathode-ray tube, Röntgen observed that some agent produced in the tube was causing barium platinocyanide crystals to fluoresce. Upon investigation he found that the fluorescence was caused by unknown rays (which he named 'X-rays') originating from the spot where cathode rays hit the glass wall of the vacuum tube. He announced his discovery in the present paper, which described the rays' photographic properties and their amazing ability to penetrate all substances, even living flesh. Although he was unable to determine the true physical nature of the rays, Röntgen was certain that he had discovered something entirely new, a belief soon confirmed by the work of other scientists such as Becquerel, Laue and the Curies. For his discovery, Röntgen was awarded
the Nobel Prize in physics for 1901” (Norman 1841). “Röntgen's second paper on X-rays reported his latest findings: that X-rays render air conductive (a phenomenon already recognized), and that the target of the rays does not have to be simultaneously the anode of the cathode-ray tube. He described a scale for measuring X-ray intensity, along with other innovations in equipment designed for the optimal production of X-rays” (Norman 1842). “Their importance in surgery, medicine and metallurgy is well known. Incomparably the most important aspect of Röntgen's experiments, however, is his discovery of matter in a new form, which has completely revolutionized the study of chemistry and physics. Laue and the Braggs have used the X-rays to show us the atomic structure of crystals. Moseley has reconstructed the periodic table of the elements. Becquerel was directly inspired by Röntgen’s results to the investigation that discovered radioactivity. Finally J.J. Thomson enunciated the electron theory as a result of investigating the nature of the X-rays” (PMM). “The discovery by Professor Röntgen of a new kind of radiation from a highly exhausted tube through which an electric discharge is passing has aroused an amount of interest unprecedented in the history of physical science” (J.J. Thomson, ‘On cathode rays,’ Report of the Sixty-sixth Meeting of British Association for the Advancement of Science, 1896). “It was this separate printing [of the first paper], and the following four additional printings in five issues, that were primarily responsible for the rapid dissemination of the news of Röntgen's discovery” (Klickstein, Röntgen, p. 62).

"On Friday evening, 8 November 1895, Wilhelm Röntgen remained long hours in his laboratory and was late for dinner − so the story goes. He had been kept by a most puzzling observation he made while repeating some of Heinrich Hertz's and Philipp Lenard's recent experiments on cathode rays.

"His apparatus was very simple and standard; it consisted of a Ruhmkorff spark coil with a mercury interrupter and a Hittorf discharge tube. That evening, in preparing for his next experiment, he had carefully covered the tube with black cardboard and drawn the curtains of the windows. He hoped to be able to detect some fluorescence coming from the tube with a fluorescent screen made of a sheet of paper painted with barium platinocyanide. That screen, which he intended to bring close to the tube later on, was lying on the table at some distance. Röntgen wanted to test the tightness of the black shield around the tube. He operated the switch of the Ruhmkorff spark coil, producing high-voltage pulses of cathode rays and looked for any stray light coming from the glass tube. He then happened to notice out of the corner of his eye a faint glimmer towards the end of his experimentation table. He switched off the coil, the glimmer disappeared. He switched the coil back on, the glimmer reappeared. He repeated the operation several times, the glimmer was still there. He looked for its source and found that it came from the fluorescent screen.

"In the interview he granted in March 1896 to H. J. Dam, a London-based American reporter for the American magazine, McClures, Röntgen was asked: 'What did you think?' His answer was: 'I did not think, I investigated. I assumed that the effect must have come from the tube since its character indicated that it could come from nowhere else'. Röntgen found that the intensity of the fluorescence increased significantly as he brought the screen close to the discharge tube. More baffling, the propagation of this ‘radiation’ was not hampered if he put a piece of cardboard between the screen and the tube, or other objects such as a pack of cards, a thick book or a wooden board two or three centimetres thick. Then he moved the screen farther and farther away, even as far as two metres, and, his eyes being well accustomed to obscurity, he could still see the very faint glimmer. As an added fortunate circumstance, according to H. H. Seliger, Röntgen being colour-blind, his eyes had enhanced sensitivity in the dark.
“After dinner, Röntgen went back down to his laboratory and repeated his experiment, now putting various sheets of materials such as aluminium, copper, lead or platinum in front of the screen. Only lead and platinum absorbed the radiation completely, and lead glass was found to be more absorbing than ordinary glass. Röntgen held a small lead disk in front of the screen and was very surprised to see not only the shadow of the disk, but also the shadow of the bones of his own hand! He also found that photographic plates were sensitive to this unknown radiation.

“In the days that followed, Röntgen told no one of his startling observations, neither his assistants nor his wife. He was morose and abstracted, according to his wife, and often ate and even slept in his laboratory. The discovery was so astounding, so unbelievable, that he would not disclose it before he had fully convinced himself of its reality by repeated observations and had determined the properties of this new radiation.

“In the same interview for McClures Magazine mentioned above, he said: ‘It seemed at first a new kind of light. It was clearly something new, something unrecorded. ’‘Is it light?’ ’No, it can neither be reflected nor refracted. ’‘Is it electricity?’ ’Not in any form known. ’‘What is it?’ ’I do not know. Having discovered the existence of a new kind of rays, I of course began to investigate what they could do. ’Indeed, being a careful experimenter, he made in the following seven weeks very systematic studies of the properties of the new rays, X-Strahlen, as he called them. During all that period, he remained uncommunicative, but, shortly before Christmas, he invited his wife to his laboratory and showed her his work. He even took a radiograph of her hand. The results of his investigations are recorded in the preliminary report he handed to the president of the Würzburg Physikalisch-medicinische Gesellschaft on 28 December [the offered paper]. On account of its outstanding importance, the President of the Society agreed that the report should
be printed at once, even though it had not been presented orally at a meeting …

“Röntgen’s first communication, written in a precise and matter-of-fact way, reveals what a thorough and meticulous investigation he made of the properties of the new rays.

1. Many other bodies besides barium platinocyanide exhibit fluorescence when submitted to the action of X-rays: calcium sulphide, uranium glass, Iceland spar, rock-salt, etc.

2. X-rays pass through all bodies, as shown by Lenard for cathode rays. Röntgen compared the attenuation of X-rays through various materials. For instance, the radiographs of a hand showed that bones were more absorbing than flesh. Generally speaking, the absorption of X-rays increases with the density and the thickness of the bodies. Röntgen made quantitative estimates and found roughly the same attenuation for metallic foils of platinum, lead, zinc, and aluminium, 0.018 mm, 0.050 mm, 0.100 mm, and 3.500 mm thick, respectively. He also checked the increase of absorption with thickness by means of photographs taken through tin foils of gradually increasing thicknesses.

3. X-rays are not deflected by a prism. Röntgen used water and carbon disulphide in mica prisms of 30°, and prisms of ebonite and aluminium, but found no effect. There was no refraction by lenses either, and this ‘shows that the velocity of X-rays is the same in all bodies.’

4. X-rays are diffused by turbid media, like light. Likewise, no conclusive reflection of X-rays by a mirror was observed. After these negative observations, Röntgen thought that maybe, nevertheless, ‘the geometrical arrangement of the molecules might affect the action of a body upon the X-rays for instance according to the orientation of the surface of an Iceland spar plate with respect to its [optical] axis,’ but the experiments with quartz and Iceland spar on this point also lead to a negative result.

7. Despite all his efforts, Röntgen could not find any interference effects. He attributed this negative result to the very feeble intensity of the X-rays. Laue noted that he was right in this, since, having shone X-rays on quartz and calcite crystals, he would have observed interference fringes if the intensity had been higher. But Röntgen told him that in any case he would never have imagined interference effects to be like those seen by Friedrich and Knipping!

8. X-rays are much less absorbed than cathode rays, and unlike them, are not deflected by magnets. They are a different kind of radiation.

9. The intensity of the rays decreases as the inverse square of the distance between the discharge tube and the screen.

10. X-rays cast regular shadows, as shown by many photographs of shadows of various objects, as well as by pinhole photographs. This indicates a rectilinear propagation, hence the term ‘rays’.

“In conclusion, Röntgen noted that ‘a kind of relationship between the new rays and light appears to exist’ and suggested tentatively ‘Should not the new rays be ascribed to longitudinal waves in the aether?’

“There were no illustrations in the report, but Röntgen made copies of nine of the most important radiographs, such as a set of weights in a closed wooden box, a piece of metal whose lack of homogeneity was revealed by the X-rays, and a wooden door with lead paint, the most striking and extraordinary one being, of
course, the radiograph of a hand showing the bones. He mailed them on New Year's Day 1896, together with preprints of his paper, to ninety leading physicists in Germany, Austria, France, and England. One of the recipients was F. Exner, the Director of the Institute of Physics at Vienna University, whom he knew from his younger days at the Polytechnic Institute in Zürich. Professor Exner showed the report and the photographs to some friends, among whom was E. Larcher. Larcher's father happened to be the editor of the journal *Die Presse* in Vienna. As a good journalist, he immediately felt the importance of Röntgen's discovery and wrote without waiting an article which made the front page of that journal on Sunday, 5 January 1896, under the headline 'Eine sensationelle Entdeckung' (a sensational discovery). This was indeed sensational news. They were cabled immediately by foreign correspondents to their home journals, and, from then on, they spread round the world with the speed of lightning. The discovery was reported next day in the dailies, on 6 January in the *Frankfurter Zeitung* and in the London *Daily Chronicle*, on the 7th in the *Standard*, on the 13th in the Paris *Le Matin*, on the 16th in the *New York Times*, and on the 31st in the *Sydney Telegraph*. The professional journals followed suit immediately, the *Electrical Engineer*, New York, on 8 January, under the title 'Electrical photography through solid matter', the *Electrician*, London, on the 10th, the *Lancet*, London, on the 11th, and the *British Journal of Medicine* on the 18th, with a note by the English physicist, A. Schuster, one of the recipients of Röntgen's mailings. It was announced at the French Academy of Sciences on 20 January. An English translation of Röntgen's communication was published in *Nature*, London, on 23 January, along with short articles by A. A. Campbell Swinton and A. Schuster, and in *Science* (USA) on 14 February. A French translation appeared in *L'Eclairage electrique* on 8 February. The imagination of the general public was naturally inflamed and it is no surprise, in that Victorian age, that some advertisements appeared for 'X-ray proof underclothing – especially for the sensitive woman'...
In the two months that followed his first communication, Röntgen worked very hard to continue the study of the properties of X-rays, not letting himself be distracted by all the honors which were bestowed on him and the many unwelcome visitors. During that period, he concentrated on two points, which had been briefly mentioned in the first report, and which are described in his second communication:

1. The first point is the property of X-rays to discharge electrified bodies. In order to be able to observe this phenomenon in a space that is completely protected. He ‘had a chamber made of zinc plates soldered together, which was airtight and large enough to contain himself and his apparatus.’ He found that ‘electrified bodies in air, charged positively or negatively, conductors or insulators, are discharged when X-rays fall on them.’ With his customary meticulousness he detailed the conditions under which this property appears, and recognized that it is due to a change in the air, namely air is ionized by the passage of X-rays. He recognized the effect, but did not name it.

2. The second point was that X-rays could be produced in many materials other than glass, for instance if the beam of cathode rays fell on a plate of aluminum or platinum. Röntgen found that the greatest intensity was obtained with platinum. For that he used ‘a discharge apparatus in which the cathode is a concave mirror of aluminium and the anode a plate of platinum at the centre of curvature of the mirror,’ a usual set up at the time’ (Authier, Early Days of X-ray Crystallography, pp. 52-60).

After holidaying in Italy with his wife in March 1896, Röntgen continued his study of the properties of X-rays, recording his observations in his third communication, ‘Weitere Beobachtungen über die Eigenschaften der X-Strahlen.

Dritte Mittheilung’ (published in 1897 in the Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin). He showed that any matter, when submitted to X-rays, itself emits X-rays, and that a body hit by cathode rays emits X-rays equally in all directions; he further investigated the transparency of various substances to X-rays; and he failed to demonstrate diffraction of X-rays.

‘Röntgen was showered with honours, invitations, and prizes, the most prestigious one being the very first Nobel Prize in Physics, awarded in 1901, but, being shy of nature, he declined many other invitations to speak again in public. He did not even give a lecture after receiving his Nobel Prize. The Prince Regent of Bavaria bestowed on him the Royal Bavarian Order of the Crown, which entitled the recipient to be called von. Rontgen accepted the decoration, but declined the nobility. He did not take any patent, and gave his discovery to the world without deriving any personal profit from it’ (ibid., p. 59).

There were five separate printings, in six issues, of the offprint of the first communication in the space of two months. The first issue offered here has wrappers but no title page and is dated ‘Ende 1895.’ No offprints of the third communication are known. Röntgen published no further work on X-rays after these three communications.
Ueber eine neue Art von Strahlen.

(Fortsetzung)

Da meine Arbeit auf mehrere Wochen unterbrochen werden muss, gestatte ich mir im Folgenden einige neue Resultate schon jetzt mitzuteilen.


Solche lassen sich wohl nur dann erhalten, wenn man die Beobachtungen in einem Raum anstellt, der nicht nur vollständig gegen die von der Vakuumröhre, den Zuleitungsleitern, dem Induktionsapparat etc. ausgehenden elektrostatischen Kräfte geschützt ist, sondern der auch gegen Luft abgeschlossen ist, welche aus der Nähe des Entladungsapparates kommt.

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First edition, very rare, of the first statement and proof that the general equation of degree five or more cannot be solved algebraically. This is a remarkable *author’s presentation copy*, uncut in the publisher’s printed wrappers. “One of the most fascinating results in the realm of algebra – indeed in all of mathematics – is the theorem that the general polynomial of degree ≥ 5 is not solvable by radicals. Its discovery at the very end of the 18th century went counter to the belief and expectations of mathematical scholars; it came as a great surprise and was naturally met with scepticism … this revolutionary idea was not accepted without a great deal of resistance” (Ayoub, p. 253). An exception was the great French mathematician Augustin-Louis Cauchy, who wrote to Ruffini in 1821: “Your memoir on the general resolution of equations is a work that has always seemed to me worthy of the attention of mathematicians and one that, in my opinion, demonstrates completely the impossibility of solving algebraically equations of higher than the fourth degree.” In Ruffini’s arguments one can now see the beginnings of modern group theory. “Ruffini’s methods began with the relations that Lagrange had discovered between solutions of third- and fourth-
degree equations and permutations of three and four elements, and Ruffini's development of this starting point contributed effectively to the transition from classical to abstract algebra and to the theory of permutation groups. This theory is distinguished from classical algebra by its greater generality: it operates not with numbers or figures, as in traditional mathematics, but with indefinite entities, on which logical operations are performed" (DSB). "Ruffini is the first to introduce the notion of the order of an element, conjugacy, the cycle decomposition of elements of permutation groups and the notions of primitive and imprimitive. He proved some remarkable theorems [in group theory]" (MacTutor). Ruffini's proof did, in fact, have a gap which was filled in 1824 by Niels Henrik Abel (although Abel's proof also had a gap), and the insolvability of quintic equations is now known as the Ruffini-Abel theorem. This is a very rare book on the market in any form (ABPC/RBH list only a single copy) and we have never before seen nor heard of a copy in publisher's wrappers, or a presentation copy.

Provenance: Author's presentation copy (“dono dell’autore” written on the front fly-leaf of both volumes).

The method of solving quadratic equations was known to the Baghdad mathematician and astronomer Al-Khwarizmi (c. 780-850), and the formula involving square roots is now taught to every student in high school. Similar formulas for solving cubic and quartic equations were not found until the 16th century, by Scipione del Ferro (1465-1525), Lodovico Ferrari (1522-60), and Niccolò Tartaglia (1506-59), and were first published by Girolamo Cardano (1501-76) in his Ars magna (1545). These formulas expressed the solutions in terms of ‘radicals,’ i.e., expressions involving rational functions (ratios of polynomials) of the coefficients of the equation and their square-, cube-, and higher roots. The search for a similar formula for quintic equations proved fruitless. “For two centuries thereafter, the resolution of the enigma was regarded as one of the most important problems of algebra and occupied the attention of the leading mathematicians of this epoch” (Ayoub, p. 257).

The most important work on the problem preceding Ruffini’s was Lagrange’s remarkable memoir ‘Réflexions sur la résolution algébrique des equations, published in the Nouveaux Mémoires de l’Académie Royale des Sciences et Belles-Lettres de Berlin in 1770-71. Katz & Parshall (p. 298) remark that “his introduction of the notion of permutations proved crucial to the ultimate proof that there was no algebraic solution of a fifth-degree polynomial equation,” but Lagrange himself still believed that a solution of the quintic would be found. "He concludes with this statement: ‘There, if I am not mistaken, are the true principles of the resolution of equations, and the most appropriate analysis which leads to solutions; all reduces, as we see, to a type of calculus of combinations by which we find results which we might expect a priori. It would be pertinent to make application to equations of the fifth and higher degrees whose solution is, up to the present, unknown: but this application requires a large number of combinations whose success is, however, very doubtful. We hope to return to this question at another time and we are content here in having given the fundamentals of a theory which appears to us new and general.’ So in spite of past failures in solving the quintic, Lagrange still harbors the hope that a careful analysis of his method will achieve the goal.

“Did no one suspect that the solution of the quintic was impossible? Apparently not until 1799 when Ruffini published his book on the theory of equations: ‘General theory of equations in which it is shown that the algebraic solution of the general equation of degree greater than 4 is impossible.’ Parenthetically, we note that in the same year the young Carl Friedrich Gauss (1777-1855) wrote in his dissertation (in which he proved the fundamental theorem of algebra) as follows: ‘After the labors of many geometers left little hope of ever arriving at the resolution of the general equation algebraically, it appears more and more likely
that the resolution is impossible and contradictory … Perhaps it will not be so difficult to prove, with all rigor, the impossibility for the fifth degree. I shall set forth my investigations of this at greater length in another place. Here it is enough to say that the general solution of equations understood in this sense [i.e., by radicals] is far from certain and this assumption [i.e., that any equation is solvable by radicals] has no validity at the present time. Gauss published nothing more on the subject.

“Ruffini begins the introduction of his book as follows: ‘The algebraic solution of general equations of degree greater than 4 is always impossible. Behold a very important theorem which I believe I am able to assert (if I do not err); to present the proof of it is the main reason for publishing this volume. The immortal Lagrange, with his sublime reflections, has provided the basis of my proof’” (Ayoub, pp. 262-3).

Ruffini’s proof has been presented in modern dress by Ayoub and cannot be described in detail here. It is now viewed as being correct but with a gap: Ruffini assumed that the radicals that arise in the course of solving the equation are rational functions of the roots, and this assumption requires proof which Ruffini did not provide. This gap was filled 25 years later by Abel (1802-29). Abel’s proof also had a gap, which was filled in 1849 by Leopold Königsberger.

In the course of proving his remarkable theorem, Ruffini laid the foundations of group theory, which is now of central importance both in mathematics and in physics (symmetry groups). “Ruffini went beyond the mere recognition that there exists a connection between the solvability of algebraic equations and permutations. In his work the theory of permutations no longer plays the role of a mere computing device but is rather a structural component of solvability theory.
“He begins with Lagrange's program of systematic investigation of permutations from the point of view of their effect on algebraic functions of \( n \) variables (a permutation can fix or change such a function) … he prefaces his treatment of fifth- and sixth-degree equations in chapter 13 ('Riflessioni intorno all soluzione generale delle equazioni') with a classification of permutations. Apart from a different terminology, the modern permutation group concept appears in this chapter with full clarity. Not only is Ruffini – like Lagrange – concerned with permutations that leave a rational function of the roots invariant; he deals also with the totality of such permutations and their properties. He calls such a set of permutations ‘permutazione.’ Thus Ruffini’s ‘permutazione’ coincides with what Cauchy later called a ‘system of conjugate substitutions’ and Galois called a (permutation) ‘group.’

“It is remarkable that Ruffini used consistently the fact that his ‘permutazione’ is closed, both in connection with the composition of the permutations reproducing the function, and even for the purpose of classifying groups in connection with the question of generators of the ‘permutazione’ …

“Ruffini calls the number \( p \) of permutations that leave invariant a given function of \( n \) roots of an equation the ‘degree of equality’ (grado di uguaglianza). Thus this concept coincides with that of the ‘order’ of a (permutation) group … Ruffini goes on to determine the value of \( p \) for all groups that occur in connection with five quantities, the roots of a quintic … In terms of content, this investigation comes down to an (almost) complete determination of all subgroups of the symmetric group \( S_5 \). In this way Ruffini obtains the main result, formulated in article 275, to the effect that \( p \) can never be 15, 30, or 40, that is, that there are no (rational) functions of five quantities that take on 8, 4, or 3 different values when these quantities are permuted in all possible ways. On the basis of this correctly-proved group-theoretic result, Ruffini gives a proof – with some gaps – of the unsolvability of the general quintic in radicals” (Wussing, pp. 82-3).

“What reception was accorded this remarkable discovery? In about 1801 Ruffini sent a copy of his ‘Teoria’ to Lagrange but received no response. Shortly thereafter, he wrote: ‘Because of the uncertainty that you may have received my book, I send you another copy. If I have erred in any proof, or if I have said something which I believed new, and which is in reality not new, finally if I have written a useless book, I pray you point it out to me sincerely.’ Lagrange did not reply.

"Again in 1802 he wrote to Lagrange: ‘No one has more right … to receive the book which I take the liberty of sending to you … In writing this book, I had principally in mind to give a proof of the impossibility of solving equations of degree higher then 4.’

“Pietro Paoli, professor of analysis at Pisa, wrote in September 1799 with a certain chauvinism: ‘I read with much pleasure your book … and recommended greatly the most important theorem which excludes the possibility of solving equations of degree greater than 4. I rejoice exceedingly with you and with our Italy, which has seen a theory born and perfected and to which other nations have contributed little.’ [It must be remembered that Lagrange was born in Turin and was considered Italian by Italians, though he had come of a French family.]

"In 1803, Ruffini published a paper entitled ‘On the solvability of equations of degree greater than 4.’ This was written at the urging of his friend Pietro Abbati (1768-1842). Ruffini wrote: ‘In the present memoir, I shall try to prove the same proposition [insolvability of the quintic] with, I hope, less abstruse reasoning and with complete rigor.’

“Gian-Francesco Malfatti (1731-1807) raised certain objections...
which suggest that he did not understand the proof clearly … In 1806 Ruffini published yet another proof with no visible reaction, and in 1813 he published a paper 'Reflections on the solution of general algebraic equations.' In the introduction, he expresses his disappointment, if not pique, at the reception accorded his work” (Ayoub, p. 269).

"A further impulse to seek appraisal of his work came from a publication by Jean Baptiste Joseph Delambre (1749-1822). This was a report to 'His Majesty the Emperor and King' called 'Historical report on the progress of the mathematical sciences since 1789.' In it Delambre says 'Ruffini proposes to prove that it is impossible …' Ruffini replied: 'I not only proposed to prove but in reality did prove … and I had in mind presenting the proof to the institute to have it examined and to have the institute pronounce on its validity.' Ruffini was informed that Lagrange, Legendre and Lacroix had been appointed to a committee to examine his memoir. He was told, however, that 'if a thing is not of importance, no notice is taken of it and Lagrange himself 'with his coolness' found little in it worthy of attention.'

"Ruffini wrote again to Delambre asking about the status of his paper, and noted that the Italian minister had spoken to Lagrange who told the minister that because of the character and manner of expression, he had understood nothing and no longer wished to undertake the reading of his memoir. Ruffini asked Delambre to speak to Lagrange and if the latter did not want to read it, Delambre was to appoint a new board of examiners.

"As it turns out, Lagrange, who was old at the time, reported to Gaultier de Claubry that he had read Ruffini's memoir, had found it good but, since it treated of a difficult matter and since Ruffini had not given sufficient proof of certain
things which he claimed, Lagrange did not want to create excitement among the mathematicians of the institute and, therefore, did not want to publish his approval.

"Ruffini also sent his memoir to the Royal Society in London. The reply said that the Society itself does not give official approval of any work but reported that those who had read it were quite satisfied that he had proved what he claimed to prove.

"His greatest advocate, however, was no less a person than A. L. Cauchy (1789-1857). Cauchy found in Ruffini’s work a veritable gold mine. In the years 1813-1815, Cauchy wrote a lengthy paper on the theory of permutation groups generalizing some of Ruffini's results. This paper was assessed by a committee of the French Academy of Sciences and this committee mentions Ruffini by name.

"Cauchy acknowledged his indebtedness to Ruffini in a letter dated 1821 about 6 months before Ruffini’s death: ‘… your memoir on the general resolution of equations is a work which has always seemed to me worthy of the attention of mathematicians and which, in my judgment, proves completely the insolvability of the general equation of degree > 4. If I have not discussed it in my course, it is because this course was directed at the students of the École Royale Polytechnique and I could not deviate too much from the syllabus … In another memoir which I read last year to the Academy of Sciences, I cited your work and reminded the audience that your proofs establish the impossibility of solving equations algebraically … I add moreover, that your work on the insolvability is precisely the title of a lecture which I gave to several members of the academy …’

"In view of the endorsement of the Royal Society (admittedly vague) and of the strong approbation of Cauchy, why then did Ruffini’s work not receive general acceptance?

"It is difficult to say. Lagrange’s comment to Claubry leads us to surmise that the mathematical community was not ready to accept so revolutionary an idea: that a polynomial could not be solved by radicals. Then, too, the method of permutations was too exotic and, it must be conceded, Ruffini's early account is not easy to follow. There was the correct but unproved assertion on accessory irrationalities, but this objection was never seriously raised until much later” (ibid., pp. 270-272).

The proof of the impossibility of an algebraic solution of the general equation of a degree higher than the fourth is given in Part I of the Riflessioni; Part II treats of the impossibility of solution by the aid of certain transcendental expressions which, among others, include trigonometric and logarithmic functions. Although no decisive results were obtained, Ruffini was again ahead of his time, solutions in terms of transcendental functions being considered by many later authors: the first such solution of the general quintic equation, in terms of elliptic functions, was given by Gauss’s student Eisenstein in 1845.

"Ruffini was born in 1765 in the small town of Valentano. His father was a physician and the family moved to Reggio near the city of Modena. As a child he was of a mystical temperament and appeared destined for the priesthood but chose instead to study medicine and mathematics. He entered the University of Modena in 1783 and received degrees in philosophy, medicine and surgery in 1788 and shortly thereafter was appointed to a chair in analysis at Modena. He was elected representative from the district of Parma and lived in Milan for several years.
"These were years of political upheaval; the influence of the French Revolution was strongly felt, and although he did not embrace the newly imported ideas, he was not doctrinaire in his adherence to traditional ideology. On the other hand, for refusing to swear a civil oath, he was deprived of his chair in Modena and this was not restored until 1799. Meanwhile, he was practicing medicine and pursuing his mathematical researches. It was during this period of enforced idleness (comparatively speaking) that he wrote his first work – the theory of equations in which the theorem on the impossibility was proved.

"He was a devoted physician; on his rounds from hospital to hospital his mind constantly turned to mathematics and in the evenings he would record his thoughts of the day. Indeed his mind was constantly occupied with mathematics as testified by his scribblings on the letters he received and in the margins of various papers.

"He was elected to the newly founded Academy of Sciences, the so called ‘Society of the Forty’, and was a very active member.

"In 1817-1818, there was an epidemic of typhoid fever and Ruffini unsparingly tended to the sick. He came down with the fever in 1818 and was in danger of losing his life. On the basis of his experience, he wrote on contagious typhoid. He was also interested in religion and philosophy and wrote on the definition of life as well as a discourse refuting Laplace’s mechanistic theory of moral phenomena.

"He was a spiritual and religious man who was modest to the point of being self-effacing. He was once offered a chair in mathematics at the University of Padua but he refused it because of his reluctance to leave the many families in his medical practice.

"Admired by his colleagues and loved by his patients, he died in 1822 at the age of 57" (ibid., pp. 275-276).

RUFFINI, Paolo.

\[ \begin{align*}
q - \delta, \text{ ec., e di più i due prodotti} \\
(p - \mu)(p - \nu)(p - \pi)(p - \gamma) \ldots.
\end{align*} \]

sono entrambi reali, e positivi (N:\textsuperscript{a} 359). Dunque gli altri due prodotti
\( (p - \beta)(p - \gamma)(p - \delta) \ldots \)
\( (q - \mu)(q - \nu)(q - \pi) \ldots \)
\( (q - \beta)(q - \gamma)(q - \delta) \ldots \)
sono uguali, e pertanto ciascuno di \( p - \alpha > 0 \),
\( q - \mu < 0 \), i terzi
\( (p - \alpha)(p - \beta)(p - \gamma)(p - \delta) \ldots \)
\( (p - \mu)(p - \nu)(p - \pi) \ldots \)
\( (q - \mu)(q - \nu)(q - \pi) \ldots \)
\( (q - \beta)(q - \gamma)(q - \delta) \ldots \)
saranno di segno contrario. Ma questi ultimi due prodotti non sono, se non se è, che diversa il primo membro della \( (D) \) per la sostituzione delle quantità \( p, q \) in luogo della \( x \). Dunque ec.

381. Se la differenza tra le due quantità \( p \)
\( q \) sia minore della differenza fra due qualsiasi dei le radici della \( (D) \), come se \( p - q = \frac{1}{V\kappa} \) (N:\textsuperscript{a} 379); e se collocando in \( (D) \) in luogo della \( x \) tali quantità, ne vengano due risultati di segno contrario; lo dico, che tra esse \( p, q \) c’è sempre una sola delle radici della data.

Che fra le \( p, q \) c’è solo una radice della \( (D) \), questo lo supponiamo dal (N:\textsuperscript{a} 381); che poi non ne esista che una sola, ciò si deduce facilmente, poiché se ne c'è...
SCHRÖDINGER’S CAT


$15,000

In: Die Naturwissenschaften, vol. 23, issues 48 (November 29), 49 (December 6), & 50 (December 13), 1935, pp. 807-12; 823-28; 844-49. Large 8vo (271 x 196 mm), 23. jahrgang, heft 48 (pp. 807-12); heft 49 (pp. 823-28); heft 50 (pp. 844-49), the three entire issues offered here in the original printed wrappers. This journal is normally found as a bound volume or otherwise the issues have been extracted. The present issues are in their original state and completely unmarked and unrepaired. Very rare in such fine condition.

First edition, journal issues, very rare in the original printed wrappers, of the papers in which Schrödinger gave his definitive views on the nature of quantum mechanics, illustrating them with one of the most famous thought experiments in the history of physics, ‘Schrödinger’s cat.’ “A cat is locked in a steel box with a small amount of a radioactive substance such that after one hour there is an equal probability of one atom either decaying or not decaying. If the atom decays, a device smashes a vial of poisonous gas, killing the cat. However, until the box is opened and the atom’s wave function collapses, the atom’s wave function is in a superposition of two states: decay and non-decay. Thus, the cat is in a superposition of two states: alive and dead. Schrödinger thought this outcome ‘quite ridiculous,’ and when and how the fate of the cat is determined has been a subject of much debate among physicists” (Britannica). “From the late 1930s to the early 1960s the thought experiment was little mentioned, except sometimes as a classroom anecdote. For instance, Columbia professor and Nobel laureate T. D.
Lee would tell the tale to his students to illustrate the strange nature of quantum collapse … Renowned Harvard philosopher Hilary Putnam – who learned about the conundrum from physicist colleagues, was one of the first scholars outside the world of physics to analyze and discuss Schrödinger’s thought experiment. He described its implications in his classic paper ‘A philosopher looks at quantum mechanics’. When the paper was mentioned the same year in a Scientific American book review, the term ‘Schrödinger’s cat’ entered the realm of popular science. Over the decades that followed, it crept into culture as a symbol of ambiguity and has been mentioned in stories, essays and verse” (Halpern, *Einstein’s Dice and Schrödinger’s Cat*, 2015). ABPC/RBH list only one copy of this three-part paper in the original printed wrappers (Sotheby’s, June 18, 2002, lot 95, $2390).

"Motivated by the EPR paper [A. Einstein, B. Podolsky & N. Rosen, ‘Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?’, *Physical Review*, vol. 47 (1935), pp. 777-780], Schrödinger published in 1935 a three-part essay in Die Naturwissenschaften on ‘The present situation in quantum mechanics.’ He said he did not know whether to call it a ‘report’ or a ‘general confession’. It is written in a sardonic style, which suggests that he found the ‘present situation’ to be less than satisfactory.

"He first explained in detail how physics, on the basis of experimental data, creates models, which are presentations of natural objects idealized or simplified so that mathematical analysis can be applied to them. The deductions from such analysis are then tested by experiments, the results of which may lead to refinement or even drastic alteration of the model. The model can be described in terms of certain specifications/ For example, the Rutherford model of the hydrogen atom consists of two mass points, and the specifications might be the two times three coordinates of these mass points, and their two times three components of momentum. Such specifications are often called variables … In classical physics one can define a state of the model by giving the values of the twelve specification variables. In quantum mechanics, however, not all the variables can be simultaneously specified. If one measures exact values for the position coordinates, one can determine nothing about the values of the six momentum components. This situation is a result of the Heisenberg uncertainty relation, which is derived directly from the operators for position \(q\) and momentum \(p\) do not commute. It is, however, possible to measure values of \(q\) and \(p\) that fall within certain ranges in accordance with the uncertainty relation, so that one can speak of the specification variables of the model as being washed out or blurred.

"Nevertheless the wave function \(\psi\) defines the state of the model unequivocally. It constitutes a complete catalog of the probabilities of finding any specified result for a measurement made upon the physical system for which the model was designed …

"In Section 5 of his paper, Schrödinger asks ‘are the variables really blurred?’ He points out that the classical description with its sharp values for the variables can be replaced by the \(\psi\)-function as long as the blurring is restricted to atomic dimensions which escape our direct control. But when the uncertainty includes visible and tangible things, the expression ‘blurring’ becomes simply wrong.

‘One can even construct quite burlesque cases. A cat is shut up in a steel chamber, together with the following diabolical apparatus (which one must keep out of the direct clutches of the cat): in a Geiger tube there is a tiny mass of radioactive substance, so little that in the course of an hour perhaps one atom of it disintegrates, but also with equal probability not even one; if it does happen, the counter responds and through a relay activates a hammer that shatters a little flask of prussic acid. If one has left this entire system to itself for an hour, then one will say to oneself that the cat is still living, if in that time no atom has disintegrated. The
first atomic disintegration would have poisoned it. The $\psi$-function of the whole system would express this situation by having the living and the dead cat mixed or smeared out (pardon the expression) in equal parts.

'It is typical of such cases that an uncertainty originally restricted to the atomic domain has become transformed into a macroscopic uncertainty, which can then be resolved through direct observation. This inhibits us from accepting in a naïve way a ‘blurred model’ as an image of reality … There is a difference between a shaky or not sharply focused photograph and a photograph of clouds or fogbanks.'

"This conclusion has been called ‘the principle of state distinction’: states of a macroscopic system which could be told apart by a macroscopic observation are distinct from each other whether observed or not. Only a few commentators on the cat paradox, the most notable being Eugene Wigner and John Neumann, have defended the uncompromising idealist position that the cat is neither alive nor dead until a human observer has looked into the box and recorded the fact in a human consciousness. It might be, of course, that the cat itself has a consciousness quite adequate to complete the experiment and resolve the probabilities by passing from a superposition of two states to a single state. Even without an animal consciousness, the experiment would be decided as soon as the atomic disintegration activated the Geiger counter. In the state $\psi_A + \psi_B$, the waves $\psi_A$ and $\psi_B$ must represent solutions of the time-dependent Schrödinger equation for the macroscopic system, including the cat. The system cannot be effectively isolated from perturbations by the rest of the universe. Thus formulation of the quantum mechanical problem becomes impossible, and it is meaningless to talk about a superposition of states, $\psi(\text{live}) + \psi(\text{dead})$. The cat paradox, however, was useful as an antidote to the view that the wavefunction refers not to a physical model but to human knowledge about an object, and perhaps even more importantly, it served to emphasize the principle of state distinction.
“A delayed choice experiment that is meaningful can, however, in principle be carried out with neutrons, in which two alternate paths, A and B, are possible for a neutron passing through an interferometer. The superposition of states $\psi_A + \psi_B$ can be demonstrated by the interference pattern produced. At the end of the experiment, the determination of which path was taken corresponds to opening the box and looking at the cat. Such a determination, of course, prevents the interference effect” (Moore, pp. 306-8).

Die gegenwärtige Situation in der Quantenmechanik

Von E. Schrödinger, Oxford.

Inhaltsübersicht.

1. Voraussetzungen
2. Das Problem
4. Die Entscheidung: die neue Quantenmechanik.

1. Die Ausgangsannahmen in der Quantenmechanik

2. Der Beweis


Die entscheidende Überlegung in der Quantenmechanik ist, dass es unmöglich ist, eine bestimmte Quantenzustand zu bestimmen, ohne dass der Quantenzustand sich verändert.

Der zentrale Punkt ist, dass die Quantenzustände unbestimmbar sind, und dass dies zu einem Widerspruch führt, wenn man versucht, eine bestimmte Messung vorzunehmen.

Die Quantenmechanik ist eine Theorie der Wahrnehmung, und nicht eine Theorie der Realität, was bedeutet, dass sie nicht eine Aussage über die Realität macht, sondern nur über die Wahrnehmung.

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Die Quantenmechanik ist eine Theorie der Wahrnehmung, und nicht eine Theorie der Realität, was bedeutet, dass sie nicht eine Aussage über die Realität macht, sondern nur über die Wahrnehmung.
A GEM OF THE MATHEMATICAL LITERATURE


$15,000

Three parts in one volume, 4to (227 x 167 mm), pp. [ii], [2, blank], [1-2] 3-243 [1], [1-2], 3-115 (i.e. 151), [1], half-title, part I title with imprint, section titles to parts II & III, dedication to Grand Duke Ferdinand II de’ Medici, part III separately signed & paginated with separate dedication to Prince Leopold de’ Medici, imprimatur leaf at end, numerous small woodcut diagrams, one full-page engraving, letterpress tables. Original interim limp boards, uncut, without any restoration.

First edition of the only book by Torricelli published in his lifetime, and an outstanding copy, uncut in original boards. The Opera geometrica contains his most important works on mathematics and physics. It diffused and considerably advanced the new geometry of indivisibles begun by Cavalieri, and (in De motu gravium) continued the study of the parabolic motion of projectiles begun by Galileo. “As Torricelli acquired increasing familiarity with the method of indivisibles, he reached the point of surpassing the master – as Cavalieri himself said” (DSB). “Torricelli far outdid his master Cavalieri in the flexibility and perspicuity of his use of the method of indivisibles in making new discoveries. One of the novel results which pleased him greatly was the determination, in
1641 [and included in the present work, p. 115 of *De dimensione parabolae*], that
the volume of an infinitely long solid, obtained by revolving about its asymptote a
portion of the equilateral hyperbola, was finite … Torricelli’s proof is interesting in
that it makes use of the idea of cylindrical indivisibles, whereas those of Cavalieri
had invariably been plane” (Boyer, pp. 125-6). This is “a gem of the mathematical
literature of the time” (DSB). In the *Opera geometrica*, Torricelli “worked out the
consequences of laws of falling (also in parabolic ballistics) formulated by Galileo,
and verified them by a number of ingenious experiments. In a particular section
[pp. 200-201] Galileo’s laws are applied to liquids flowing from apertures in vessels,
and what is known as Torricelli’s law [that the efflux velocity of a jet of liquid
spurting from a small hole at the bottom of a vessel is equal to that which a single
drop of the liquid would have if it could fall freely in a vacuum from the level of the
top of the liquid] was stated and experimentally proved” (Biographical Dictionary
of Scientists). On the basis of this result, “Ernst Mach proclaimed Torricelli the
founder of hydrodynamics” (DSB). The final section of the *Opera* also contains
Torricelli’s study of the cycloid, the curve traced out by a point on the circumference
of a circle rolling along a straight line; this was the first printed work on the
cycloid. “By the Method of Indivisibles he demonstrated its area to be triple that
of the revolving circle, and published his solution. The same quadrature had been
effected a few years earlier by Roberval in France, but his solution was not known
to the Italians” (Cajori, p. 172) – and Roberval’s proof was not published until
1693. In *De motu gravium*, Torricelli also develops an original method of drawing
tangents to curves. “Amongst all those who contributed to the development of
infinitesimal processes before Newton and Leibniz, Torricelli exhibited most
clearly the link between the two operations now known as differentiation and
integration” (Baron, p. 185). The only other uncut copy in original boards listed
by ABPC/RBH is the Norman copy (Sotheby’s, June 15, 1998, lot 822, $19,550).

“Mathematical research occupied Torricelli’s entire life. During his youth he
had studied the classics of Greek geometry, which dealt with infinitesimal
questions by the method of progressive elimination. But since the beginning of
the seventeenth century the classical method had often been replaced by more
intuitive processes; the first examples were given by Kepler, who in determining
areas and volumes abandoned Archimedean methods in favor of more expeditious
processes differing from problem to problem and hence difficult to imitate. After
many years of meditation, Cavalieri, in his geometry of indivisibles (1635), drew
attention to an organic process, toward which Roberval, Fermat, and Descartes
had been moving almost in the same year; the coincidence shows that the time
was ripe for new geometrical approaches.

“The new geometry considered every plane figure as being formed by an infinity
of chords intercepted within the figure by a system of parallel straight lines;
every chord was then considered as a rectangle of infinitesimal thickness—the
indivisible, according to the term introduced by Galileo. From the assumed or
verified relations between the indivisibles it was possible to deduce the relations
between the totalities through Cavalieri’s principle, which may be stated as follows:
Given two plane figures comprised between parallel straight lines, if all the straight
lines parallel thereto determine in the two figures segments having a constant
relation, then the areas of the two figures also have the same relation. The principle
is easily extended to solid figures. In essence Cavalieri’s geometry, the first step
toward infinitesimal calculus, replaced the potential mathematical infinity and
infinitesimal of the Greek geometricians with the present infinity and infinitesimal.

“After overcoming his initial mistrust of the new method, Torricelli used it as
a heuristic instrument for the discovery of new propositions, which he then
demonstrated by the classical methods. The promiscuous use of the two
methods—that of indivisibles for discovery and the Archimedean process for
demonstration—is very frequent in the *Opera geometrica*. The first part of *De sphaera et solidis sphaeralibus*, compiled around 1641, studies figures arising through rotation of a regular polygon inscribed in or circumscribed about a circle around one of its axes of symmetry ... he classifies such rotation solids into six kinds, studies their properties, and presents some new propositions and new metrical relations for the round bodies of elementary geometry. The second section of the volume deals with the motion of projectiles ... In the third section, apart from giving twenty demonstrations of Archimedes’ theorem on squaring the parabola, Torricelli shows that the area comprised between the cycloid and its base is equal to three times the area of the generating circle. As an appendix to this part of the work there is a study of the volume generated by a plane area animated by a helicoid motion round an axis of its plane, with the demonstration that it equals the volume generated by the area in a complete rotation round the same axis. Torricelli applies this elegant theorem to various problems and in particular to the surface of a screw with a square thread, which he shows to be equal to a convenient part of a paraboloid with one pitch.

“[Torricelli] extended [Cavalieri’s] theory by using curved indivisibles, based on the following fundamental concept: In order to allow comparison of two plane figures, the first is cut by a system of curves and the second by a system of parallel straight lines; if each curved indivisible of the first is equal to the corresponding indivisible of the second, the two figures are equal in area. The simplest example is given by comparison of a circle divided into infinitesimal concentric rings with a triangle (having the rectified circumference as base and the radius as height) divided into infinitesimal strips parallel to the base. From the equality of the rings to the corresponding strips it is concluded that the area of the circle is equal to the area of the triangle.
“The principle is also extended to solid figures. Torricelli gave the most brilliant application of it in 1641 by proving a new theorem, a gem of the mathematical literature of the time. The theorem, published in *Opera geometrica*, is as follows: 

[take a point \( x = a \) (> 0) on the hyperbola \( xy = 1 \), and rotate the part of the hyperbola with \( 0 < x \leq a \) and \( y > 0 \) around the \( y \)-axis]. Although such area is infinite in size, the solid it generates by rotating round the asymptote [\( y \)-axis], although unlimited in extent, nevertheless has a finite volume, calculated by Torricelli as \( \pi/a \). Torricelli’s proof, greatly admired by Cavalieri and imitated by Fermat, consists in supposing the solid generated by rotation to be composed of an infinite number of cylindrical surfaces of axis \( x \), all having an equal lateral area, all placed in biunivocal correspondence with the sections of a suitable cylinder, and all equal to the surfaces of that cylinder: the principle of curved indivisibles allows the conclusion that the volume of this cylinder is equal to the volume of the solid generated by rotation of the section of the hyperbola considered … Still using curved indivisibles, Torricelli found, among other things, the volume of the solid limited by two plane surfaces and by any lateral surface, in particular the volume of barrels. In 1643 the results were communicated to Fermat, Descartes, and Roberval, who found them very elegant and correct …

“Torricelli made other important contributions to mathematics during his studies of mechanics. In *De motu gravium* he continued the study of the parabolic motion of projectiles, begun by Galileo, and observed that if the acceleratory force were to cease at any point of the trajectory, the projectile would move in the direction of the tangent to the trajectory. He made use of this observation, earning Galileo’s congratulations, to draw the tangent at a point of the Archimedean spiral, or the cycloid, considering the curves as described by a point endowed with two simultaneous motions. In unpublished notes the question is thoroughly studied in more general treatment. A point is considered that is endowed with two simultaneous motions, one uniform and the other varying, directed along two straight lines perpendicular to each other. After constructing the curve for distance as a function of time, Torricelli shows that the tangent at any point of the curve forms with the time axis an angle the tangent of which measures the speed of that moving object at the point. In substance this recognizes the inverse character of the operations of integration and differentiation, which from the fundamental theorem of the calculus, published in 1670 by Isaac Barrow, who among his predecessors mentioned Galileo, Cavalieri, and Torricelli. But not even Barrow understood the importance of the theorem, which was first demonstrated by Newton …

“In *De motu gravium* Torricelli seeks to demonstrate Galileo’s principle regarding equal velocities of free fall of weights along inclined planes of equal height. He bases his demonstration on another principle, now called Torricelli’s principle but known to Galileo, according to which a rigid system of a number of bodies can move spontaneously on the earth’s surface only if its center of gravity descends. After applying the principle to movement through chords of a circle and parabola, Torricelli turns to the motion of projectiles and, generalizing Galileo’s doctrine, considers launching at any oblique angle—whereas Galileo had considered horizontal launching only. He demonstrates in general from Galileo’s incidental observation that if at any point of the trajectory a projectile is relaunched in the opposite direction at a speed equal to that which it had at such point, the projectile will follow the same trajectory in the reverse direction. The proposition is equivalent to saying that dynamic phenomena are reversible—that the time of Galileo’s mechanics is ordered but without direction. Among the many theorems of external ballistics, Torricelli shows that the parabolas corresponding to a given initial speed and to different inclinations are all tangents to the same parabola (known as the safety parabola or Torricelli’s parabola, the first example of an envelope curve of a family of curves).
“The treatise concludes with five numerical tables. The first four are trigonometric tables giving the values of \( \sin 2\alpha \), \( \sin^2 \alpha \), \( \tan \frac{\alpha}{2} \), and \( \sin \alpha \), respectively, for every degree between 0° and 90°; with these tables, when the initial speed and angle of fire are known, all the other elements characteristic of the trajectory can be calculated. The fifth table gives the angle of inclination, when the distance to which the projectile is to be launched and the maximum range of the weapon are known. In the final analysis these are firing tables, the practical value of which is emphasized by the description of their use in Italian, easier than Latin for artillerymen to understand. Italian is also the language used for the concluding description of a new square that made it easier for gunners to calculate elevation of the weapon.

“The treatise also refers to the movement of water in a paragraph so important that Ernst Mach proclaimed Torricelli the founder of hydrodynamics. Torricelli’s aim was to determine the efflux velocity of a jet of liquid spurting from a small orifice in the bottom of a receptacle. Through experiment he had noted that if the liquid was made to spurt upward, the jet reached a height less than the level of the liquid. He supposed, therefore, that if all the resistances to motion were nil, the jet would reach the level of the liquid. From this hypothesis, equivalent to a conservation principle, he deduced the theorem that bears his name: The velocity of the jet at the point of efflux is equal to that which a single drop of the liquid would have if it could fall freely in a vacuum from the level of the top of the liquid at the orifice of efflux. Torricelli also showed that if the hole is made in a wall of the receptacle, the jet of fluid will be parabolic in form; he then ended the paragraph with interesting observations on the breaking of the fluid stream into drops and on the effects of air resistance. Torricelli’s skill in hydraulics was so well known to his contemporaries that he was approached for advice on freeing the Val di Chiana from stagnant waters, and he suggested the method of reclamation by filling.
Torricelli (1608-47) "attended the mathematics and philosophy courses of the Jesuit school at Faenza, showing such outstanding aptitude that his uncle was persuaded to send him to Rome for further education at the school run by Benedetto Castelli, a member of his order who was a mathematician and hydraulic engineer, and a former pupil of Galileo’s. Castelli took a great liking to the youth, realized his exceptional genius, and engaged him as his secretary … In 1641 Torricelli was again in Rome; he had asked Castelli and other mathematicians for their opinions of a treatise on motion that amplified the doctrine on the motion of projectiles that Galileo had expounded in the third day of the Discorsi e dimostrazioni matematiche intorno a due nuove scienze . . . (Leiden, 1638). Castelli considered the work excellent; told Galileo about it; and in April 1641, on his way from Rome to Venice through Pisa and Florence, after appointing Torricelli to give lectures in his absence, submitted the manuscript to Galileo, proposing that the latter should accept Torricelli as assistant in drawing up the two ‘days’ he was thinking of adding to the Discorsi. Galileo agreed and invited Torricelli to join him at Arcetri. But Castelli’s delay in returning to Rome and the death of Torricelli’s mother, who had moved to Rome with her other children, compelled Torricelli to postpone his arrival at Arcetri until 10 October 1641. He took up residence in Galileo’s house, where Vincenzo Viviani was already living, and stayed there in close friendship with Galileo until the latter’s death on 8 January 1642. While Torricelli was preparing to return to Rome, Grand Duke Ferdinando II of Tuscany, at Andrea Arrighetti’s suggestion, appointed him mathematician and philosopher, the post left vacant by Galileo, with a good salary and lodging in the Medici palace. Torricelli remained in Florence until his death; these years, the happiest of his life, were filled with the greatest scientific activity …

"In 1644 Torricelli’s only work to be published during his lifetime appeared, the Grand Duke having assumed all printing costs … The fame that Torricelli acquired as a geometer increased his correspondence with Italian scientists and with a number of French scholars (Carcavi, Mersenne, F. Du Verdus, Roberval), to whom he was introduced by F. Niceron, whom he met while in Rome. The correspondence was the means of communicating Torricelli’s greatest scientific discoveries but also the occasion for fierce arguments on priority, which were common during that century. There were particularly serious polemics with Roberval over the priority of discovery of certain properties of the cycloid, including quadrature, center of gravity, and measurement of the solid generated by its rotation round the base. In order to defend his rights, Torricelli formed the intention of publishing all his correspondence with the French mathematicians, … but while he was engaged in this work he died” (DSB).

"On p. 9 of the preface [of Opera geometrica] the author says that the book was published at the behest (‘non suasit, sed iussit’) of Andrea Arrighetti of Florence. Arrighetti (1592-1672) was a pupil of Benedetto Castelli and held important office in the Tuscan state including that of buildings supervisor. It was he who was responsible for looking after Torricelli in Florence after Galileo’s death. There is also mention of a sharp-eyed student of Archimedes, Antonio Nardi of Arezzo, ‘to whom, and to whose learned conversations, I owe whatever there is of geometry in this work (‘scriptura’). Nardi again was one of Galileo’s pupils along with Magiotti and Torricelli, and indeed he wrote to Galileo about his work on Archimedes” (Macclesfield).

FIRST PROPOSAL TO USE MULTI-STAGE SPACE ROCKETS


$3,800

8vo (217 x 153mm), pp. [3], 4-38, with a full-page portrait of the author. Original printed wrappers, spine ends slightly worn, light uniform browning due to the quality of the paper. A very good copy.

First edition, very rare, of this extremely important sequel to Tsiolkovsky’s multi-part work of 1903-14 in which he first set out the basic principles of the construction of space rockets. In the present work, he made the crucial proposal to construct multi-stage rockets, which are used in all modern space programmes.

“His calculations proved that building a rocket with separate stages, each of which would be jettisoned as it finished consuming its propellants, would allow a payload to be accelerated indefinitely” (russianspaceweb.com). “Sergei Korolev, Chief Designer of the Russian space programme, commented that Tsiolkovsky’s theory of multi-stage rockets – ‘rocket trains’ – to all intents and purposes opened the path for humanity to get into space” (blog.sciencemuseum.org.uk/russias-19th-century-cosmic-pioneers/). Tsiolkovsky (1857-1935) began research into rocket propulsion and the theoretical and practical aspects of space travel in 1896, formulating many of the basic principles that govern space flight today, such as “his now widely known formula establishing the analytical dependence between the velocity of a rocket at a given moment, the velocity of the expulsion of gas particles from the nozzle of the engine, the mass of the rocket, and the mass of the
expended explosive material … Tsiolkovsky contributed to the recently established mechanics of bodies of changing mass. He evolved a theory of rocket flight taking into account the change of mass while in motion; he suggested the concept of gas-driven rudders for guiding a rocket in vacuum; and he determined the coefficient of a rocket’s practical operation. From 1903 to 1917 Tsiolkovsky offered several plans for constructing rocket ships. He considered such questions as guiding a rocket in a vacuum, the use of a fuel component to cool the combustion chamber walls, and the application of refractory elements … Tsiolkovsky’s advanced ideas did not find acceptance. He was met with indifference and disbelief, and many considered this autodidact to be a rootless dreamer. Having received neither material nor moral support, Tsiolkovsky was left to his own resources. ‘It has been difficult for me,’ he wrote with bitterness, ‘to work alone for many years under unfavourable conditions and not even to see the possibility of hope or assistance’” (DSB). “Tsiolkovsky pushed back the frontiers of human knowledge, and his idea of using the rocket for the exploration of space is only now, in our own time [i.e. 1954], beginning to be fully appreciated. He was the father of the theory of long-range liquid-fuelled rockets and the founder of a rigorously scientific theory of inter-planetary travel” (Collected Works, NASA, 1954, Vol. II, p. 3). OCLC lists five copies. No copy at auction in the last 35 years.

“At the close of the 19th century scientific and technical research into rocketry was resumed in Russia by Tsiolkovsky who created many original types of rockets. His long-range rockets and liquid-fuel space rockets were an important new step in the development of the rocket. Prior to Tsiolkovsky’s works only gunpowder rocket motors (solid-fuel rockets) were studied and offered for use. The introduction of liquid fuel (the fuel proper and an oxidizer) facilitated the construction of an original liquid-fuel rocket motor with thin walls cooled by the fuel (or oxidizer), light and dependable. Such type of motor was the only one acceptable for large rockets.
"The first type of long-range rocket was described in Tsiolkovsky's *Investigating Space with Rocket Devices*, published in 1903. The rocket was an elongated metal chamber resembling in shape a barrage balloon, a dirigible, or an immense fish-sound. 'Picture to yourselves,' Tsiolkovsky wrote, 'an apparatus consisting of an elongated metal chamber (the shape offering the least resistance) equipped with lighting, oxygen, carbon-dioxide absorber, miasma and other excretion absorbers, designed to house not only various physical instruments but the man who is to direct it ... The chamber is provided with a large store of substances which, on being mixed, produce an explosive. These substances explode in a correct proportion and at equal intervals at a regulated point, from which in the form of heated gases they flow along widening tubes (just like a speaking trumpet or a wind instrument). In a narrow part of the tube the explosives mix, producing condensed and heated gases. At the other, wide, end of the tube the gases, rarefied and, consequently, cooled, escape through the nozzle with a very high relative velocity ..." 

"Tsiolkovsky discovered and studied in detail the equation of the rocket motion with constant exhaust velocity and arrived at a very important mathematical result known as 'the Tsiolkovksy formula' ... It follows from Tsiolkovsky's formula for maximum velocity that: a. the greater the exhaust velocity, the greater the velocity of the rocket at the end of its powered flight. If the jet velocity is doubled the velocity of the rocket also increases two-fold; b. the velocity of the rocket at the end of its powered flight increases with the ratio of the initial weight of the rocket to that at the end of combustion. But the dependence here is more complicated and is formulated in the following proposition of Tsiolkovsky: 'When the mass of the rocket plus the mass of the explosives of the rocket motor increase in the geometrical proportion, the velocity of the rocket increases in the arithmetical proportion' ... On the basis of his formula, Tsiolkovsky proved that with exhaust velocities of the order of 5 km/sec the rocket's velocity would be high enough for interplanetary flight ..."
"After a detailed investigation of the rocket’s rectilinear motion and of exhaust velocities obtained from different fuels, Tsiolkovsky saw that reaching high cosmic velocities is an exceedingly difficult engineering problem. In 1929 Tsiolkovsky proposed an original solution for utilizing industrial fuels of the day in order to obtain cosmic velocities. He proposed ‘rocket-trains’, or step-rockets, and elaborated their mathematical theory. ‘A rocket-train,’ Tsiolkovsky wrote, ‘is a system of several uniform reaction devices, moving first along a track, then in the air, then beyond the atmosphere, and lastly, somewhere in space among the planets or suns. But only part of the system reaches space, while the rest, not possessing the necessary velocity, fall back to the Earth. If a single-stage rocket is to attain cosmic velocity it must carry an immense store of fuel. Thus, to reach the first cosmic velocity, 5km/sec, the weight of the fuel must exceed that of the whole rocket (payload included) by at least 4 times. This will present considerable difficulties. The stage principle, on the other hand, enables us either to obtain high cosmic velocities, or to employ comparatively small amounts of propellant components …

“Tsiolkovsky supplied no drawings to his works on the step-rocket, but from the descriptions given it is clear that he envisaged two types of step-rockets. The first was like a railway train with the locomotive pushing the carriages from behind. Let us imagine three rockets linked together one after another. Such a system is pushed first by the lowest rocket (the booster) with the first-stage motor operating. When its fuel is expended, the rocket is discarded and falls to Earth, while the motor of the second stage, the lowest of the two left, carries on work. As soon as its fuel is exhausted, this rocket, too, is discarded and the last rocket, already possessing a sufficiently high velocity, uses for further motion the thrust of its own motor … It has been calculated that with an exhaust velocity of 3,660m/sec the initial weight of a five-step lunar rocket with a payload of 5kg will be only 3,770kg, with the initial thrust of 18,000kg. The maximum velocity of the fifth stage will then be 10,400m/sec at the height of 900 kilometres. With the assistance of the Moon’s gravity, this velocity will be sufficient for the payload to reach the Moon.

“The second type of step-rocket Tsiolkovsky called a ‘rocket-squadron’. This is several, for instance 8, rockets, moving parallel to one another and fastened together like logs in a raft. All the rocket motors work simultaneously at the start. When each of the eight rockets has spent half its fuel, four rockets (for instance, two on the right and two on the left) fill the half-empty tanks of the other four rockets with their remaining fuel and detach themselves from the ‘squadron’. The remaining four rockets, their tanks full, continue their flight. When these have spent half of their fuel, two of them (one on the right and one on the left) fill the tanks of their neighbours with the remaining fuel and also detach themselves from the two that proceed on their way. Finally, one of the two remaining rockets pours its remaining fuel into the half-empty tank of the one which is to reach the goal and leaves it. The advantage of the ‘squadron’ type is that here all the rockets are of equal build and weight. Refuelling in flight is a difficult, but a wholly practicable engineering feat” (Kosmodemyansky, Konstantin Tsiolkovsky. His Life and Work, 1956).

“Konstantin Eduardovich Tsiolkovsky was born Sept. 17, 1857, in Izhevskoye, Russia. He was the son of a Polish deportee to Siberia. At age ten he nearly became deaf from scarlet fever and had to quit school. He refused to be handicapped by his deafness and continued his education on his own at home. His family recognized his thirst for knowledge and sent him to Moscow to attend college. He was accomplished in both science and mathematics and became a teacher at Kaluga, Russia. Even as a teacher, Tsiolkovsky found time to learn. He read Jules Verne's stories of space travel and began to write science fiction stories himself. He introduced elements of science and technology into his stories, such as the problem of controlling a rocket as it moved between
gravitational fields. Gradually Tsiolkovsky moved from writing science fiction to writing theoretical papers on topics such as gyroscopes, escape velocities, the principle of action and reaction, and the use of liquid propellant rockets …

“Tsiolkovsky is remembered for believing in the dominance of humanity throughout space, also known as anthropocosmism. He had grand ideas about space industrialization and the exploitation of its resources. Tsiolkovsky has been honored since his death in 1935. A far side moon crater is named in his honor. In 1989 he was invested in the International Aerospace Hall of Fame. The Konstantin E. Tsiolkovsky State Museum of the History of Cosmonautics in Kaluga, Russia, keeps the importance of his theoretical work before the public. In Russia, Konstantin Tsiolkovsky is called ‘the father of theoretical and applied cosmonautics.’ Although the Romanian Oberth and the American Goddard conducted similar research and arrived at comparable conclusions, there is no evidence that each knew details of the other’s work. Therefore, all three of these scientists share the title of Father of Rocketry” (nasa.gov).
AN EXCEPTIONAL COPY OF THE RARE ‘VENESECTION LETTER’

VESALIUS, Andreas. Epistola docens venam axillarem dextrī cubītī j in dolore lateralī secandam: et melancholicum succum ex venae porto ramis ad sedem pertinentibus purgari. Basel: [Robert Winter, 1539 (colophon)].

$225,000

Three works bound in one volume, 4to (208 x 139 mm). I. GUENTHER, Johann. Anatomicarvm institvtionvm ex Galeni sententia libri IIII ... Basel: [colophon: Robert Winter, June 1539]: pp. [xii], 231, [21]; II. VESALIUS: pp. 66, [2, colophon], including one full-page woodcut on p. 41 after a drawing by Vesalius; III. FUCHS, Leonhart. L. F. ... libri IIII., difficilium aliquot questionum, et hodie passim controversarum explicationes continet ... aucti et recogniti. Basel: [colophon: Robert Winter, September 1540]: pp. [xxviii], 230 (pp. 211/212 blank), [6]. Contemporary blind-stamped pigskin over wooden boards with two metal clasps, monogram and date ‘MVZP 1567’ stamped on upper cover (a little rubbed). A very fine copy, completely untouched.

First edition, and a truly wonderful copy in a dated contemporary binding, of Vesalius’s ‘venesection letter,’ one of his rarest works, embodying what may be the earliest approach to an area of medicine which may be called scientific in the modern sense. This is a fine copy, complete with the final leaf (the Cushing and Waller copies both lack it); it is almost never found in a contemporary binding as here. This copy is doubly interesting for preserving its original context – that of a 16th-century physician’s compendium of texts which attempted to condense and survey the most important elements of contemporary medical knowledge in
a single volume: Vesalius’ work is here accompanied by two other medical works from the same press, published within a year of Vesalius’ *Epistola*. The letter on venesection “was written for Nicolas Flourens, physician to Charles V, who had queried Vesalius regarding the notes on the azygos vein in *Tabulae anatomicae sex* [published by Vesalius in 1538]; Flourens wished to know what relation the vein had to the question of bloodletting in cases of pleurisy and pneumonia. Vesalius’ letter advocated the new ‘classical’ method of letting blood near the site of the affliction, a method arousing great controversy among the medical community as it was directly opposed to the traditional ‘reulsive’ bleeding taught by the Arabic authorities. Although the classical method was derived from a more accurate reading of Hippocrates and Galen ... the importance of Vesalius’ defense of it lies in the authority he gave to his own knowledge of the structure of the venous system – an important step in his movement away from traditional anatomical concepts” (Norman). “In this letter we perceive the first steps in the slow and gradual loosening of traditional bonds whence eventually emerged the principle that the validity of a hypothesis rests solely upon facts established by observation. Here Vesalius asks a first tentative question ‘whether the method of an anatomy could corroborate speculation’; a question not without moment in a day when principles based solely upon the power of the intellect were enshrined as truth … Vesalius’s fame rests upon his anatomical contributions, but he was as fully concerned with the problem of practical medicine … The venesection letter strongly suggests that it was Vesalius’s preoccupation with such clinical problems which provided the insight that enabled him to shake off the dead hand of Galen’s pronouncements and make the production of the *Fabrica* possible” (Saunders & O’Malley, pp. 5-6). “Out of the venesection controversy came as a purely incidental finding the discovery of the venous valves … which in the consciousness of Harvey was to provide the key to unlocking the door to the circulation” (ibid., p. 20). The *Anatomicarum institutionum* is the only quarto edition (third overall) of the great textbook of Johann Guenther [Winter] of Andernach, who taught Vesalius anatomy at Paris. The first edition, published (in 8vo) at Basel in 1536, contains the first mention of Vesalius in print; a second edition (16mo), revised by Vesalius himself, was published at Venice in 1538. “Of all the many commentaries on Galen’s innumerable works that followed rapidly on the heels of one another during the late Renaissance, few proved more popular than Guenther’s manual of four books” (Cushing, p. 44). The final work in the volume is the first edition of Fuchs’ pharmacological treatise ‘Four books on some difficult questions’, which gives a commentary on the indications and dosages of prescriptions of Ibn Sina (Avicenna) and of Masawaih al-Mardini (Mesue the Younger), and praises the work of Galen. ABPC/RBH list only two copies of the venesection letter since 1929, both in modern bindings: the Norman copy (Christie’s New York, 18 March 1998, lot 212, $33,350) and the Blondelet copy (Sotheby’s Paris, 31 May 2016, lot 50, €65,000).

“In 1538 Vesalius visited Matteo Corti, professor of medicine in Bologna, and discussed the problems of therapy by venesection. Differences of opinion between the two men seem to have been the impulse behind Vesalius’ next book, *Epistola docens venam axillarem dextri cubiti in dolore laterali secundam* (Basel, 1539), written in support of the revived classical procedure first advocated in a posthumous publication (1525) of the Parisian physician Pierre Brissot. In this procedure blood was drawn from a site near the location of the ailment, in contrast to the Muslim and medieval practice of drawing blood from a distant part of the body. As the title of his book indicates, Vesalius sought to locate the precise site for venesection in pleurisy within the framework of the classical method. The real significance of the book lay in Vesalius’ attempt to support his arguments by the location and continuity of the venous system rather than by an appeal to earlier authority. Despite his own still faulty knowledge, his method may be called scientific in relation to that of others; certainly it was nontraditional and required that his opponents resort to the same method if they wished to reply effectively.
With this novel approach to the problem of venesection Vesalius posed the then striking hypothesis that anatomical dissection might be used to test speculation. Here too he declared clearly, on the basis of vivisection, that cardiac systole was synchronous with arterial expansion and for the first time mentioned his initial efforts in the preparation of the anatomical monograph that was ultimately to take shape as *De humani corporis fabrica*” (DSB).

“Since remote antiquity, venesection had occupied a unique and important position in the minds of physicians as the sheet anchor of therapeutics. In the sixteenth century the subject had become one of violent and bitter controversy. The humanists in clearing away the rubbish of Arabian compilations and scholastic commentary had exposed how far current practice had deviated from the teachings of Hippocrates and Galen. Armed with the new learning they sought not only to defend the purified classics against the onslaughts of the Arabists, but with subtle dialectic each attempted to uphold the rightness of his textual criticism. Barren and sterile though this controversy may have been, nonetheless it was to every physician, anxious for the welfare of his patient, a subject of very real importance. Impelled by such motives and employing the familiar tools of a scholastic tradition, Vesalius enters the fray.

“Hitherto, every argument rested upon acceptance of the humoral doctrine and every measure directed toward the practice of phlebotomy depended upon the opinion of Galen for its anatomical interpretation. There is, however, no part of Galen’s anatomy more vulnerable and unsatisfactory than his description of the venous system. Vesalius, while fully accepting the philosophical basis of his heritage, introduces into the debate a new element, the findings of direct observation. These observations are, as he advises us, no isolated discovery, but the outcome of repeated dissections, and they enable him to challenge with growing confidence the infallibility of the Prince of Physicians. The emancipation
of Vesalius begins with the venesection letter … How significant the subject of blood-letting was in his liberation can still further be judged by the attention devoted to it again and again in both the *Fabrica* and in the second part of the China Root Letter directed against the attacks of his old master Sylvius …

“As Vesalius concerns himself with venesection in pleurisy, it is of particular importance to examine the opinion of Hippocrates in this respect. Pleurisy is an epidemic disease and one of the ‘acute affections’, but venesection is to be employed only when the pain is above the diaphragm, an admonition of such importance that it is repeated a little later in the same work. This restriction in the use of venesection is somewhat puzzling. It would seem to revolve around the question of the exact meaning of the term employed by Hippocrates which has been rendered by the classical Latin authors as *dolor lateralis*. It has been assumed by both sixteenth-century and modern writers that the disease so described is pleurisy or some allied pulmonary disease. There can be little doubt that pleurisy is, even in classical times, usually implied by this phrase. Vesalius, however, interprets the expression, and we believe correctly, as a general one to be taken literally as ‘pain in the side,’ in which case pleurisy is but one of several diseases covered by the term, and the restriction is logical in light of theoretical considerations of the humoral pathology. In this view, Hippocrates’ teaching was that one should let blood in pleurisy but not in other forms of *dolor lateralis* occurring below the diaphragm. The interpretation of *dolor lateralis* as pleurisy alone gave rise in later times to great confusion as to the rationale of its treatment …

“In the post-Galenical period, the venesection argument waxed and waned … until we meet with the Arab practice in which bleeding was generally, if not exclusively, revulsive at a site chosen as remote as possible from the seat of the affection. The Arab practice was the standard from the mediaeval period until the sixteenth century when it was first definitely opposed as a result of the development of Greek studies and the new and accurate translation of Hippocrates and Galen … It remained for Pierre Brissot, a physician of Paris, to enunciate and support the Hippocratic and Galenical procedure by actual practice, an epidemic of ‘pleurisy’ in 1514 providing the opportunity. The results were, in his opinion, so brilliant that in the following year he felt called upon to make a public pronouncement, and thus began one of the most violent, acrimonious and extensive medical controversies whose repercussions extended into the seventeenth century. He condemned as Arab nonsense, the prevailing practice of slowly bleeding drop by drop from the region most distant from the site of the affection, a practice which had reached such heights of absurdity that it was thought sufficient to express a drop of blood from the big toe of the opposite side. He maintained that for bleeding to be effective, a sensible quantity of blood must be removed and since pleurisy existed in a region drained by the vena cava, it made no difference whether the right or left side was selected …

“Although Brissot obtained a few influential supporters from among his teachers at Paris, his views were received with general antagonism, but the controversy remained meanwhile a purely local affair. The explosion began some years later in Portugal whither Brissot had migrated. An epidemic of ‘pleurisy’ at Evora in 1518 once again presented him with the opportunity of applying his principles with such success as to invite the jealousy of the royal physician, Denis, who attacked the new-fangled method in a bitter polemic. Owing to his premature death, Brissot’s reply was not issued until 1525 but with its posthumous publication, the medical world promptly split into the two major factions to which Vesalius constantly refers …

“The approach of Vesalius to the controversy is unique. In general terms an adherent of the Brissot party, he stands with the champions of the purified classics but his position inflexibly rests on the secure ground of factual observation.
Confident in his knowledge of the true arrangement of the azygos system, upon which the whole rationale of the place of venesection in the treatment of pleurisy rests, he is willing to go further and to promulgate on anatomical grounds his own aphorism. For the first time, the infallibility of Galen in anatomical matters is challenged.

“It is unnecessary to trace any further the tortuous windings of the venesection controversy and to the extravagant excesses of blood-letting which climaxed its decay as a therapeutic method in the eighteenth century. It should be observed, however, that it was Vesalius’s insistence on the significance of the azygos vein in phlebotomy which led to the discovery of the venous valves.

“Despite much discussion, the entire question relating to this epochal discovery has become somewhat beclouded and confused. Yet the story is clear enough if we keep in mind the circumstances surrounding the statements of the various writers of the time and their relationship to the burning problem of phlebotomy.

“The Galenical physiology and the notions based upon it naturally engendered an abnormal preoccupation with the venous system, and with the publication of the Venesection Letter and further Vesalian studies this preoccupation had become greatly intensified. One need only examine an anatomical work appearing after the middle of the sixteenth century to observe the disproportionate treatment given to the venous over the arterial system – a complete reversal from what obtains in the modern textbook – to appreciate how deeply concerned the physician was with its every detail.

“The issuance of the present work had now entirely changed the complexion of the controversy. Up to the year 1539 every participant had marshaled his
arguments from the pronouncements of authorities or from empirical observations on the outcome of illness, but thereafter, if he was to attack the Vesalian thesis effectively, he must adopt the new objective method of dissection. At our distance we are apt to forget that venesection was the major practical therapeutic measure evolved from the universally held humoural doctrine. Its effective exploitation depended upon knowledge of the venous system, presumed to be correct; hence, the great cogency of the Vesalian argument. From his repeated references to this earlier work in his later writings, Vesalius, no less than his contemporaries, was fully cognizant with the strength of his position. The physician, therefore, if he was to remain in the mainstream of what was to him logical and rational medicine, perforce was left by the new doctrine no option but the standard, and now ineffectual, use of polemical abuse, or he must take up the scalpel and stain his hands in the cadaver. Thus out of the venesection controversy came as a purely incidental finding the discovery of the venous valves. Their real significance could not, of course, be appreciated, because the focus of attention was on the arrangement of the veins and not on such apparently trivial details. But the question of their existence determined a Paduan tradition and so left a minor puzzle which in the consciousness of Harvey was to provide the key to unlocking the door to the circulation” (Saunders & O’Malley, pp. 5-20).

“Two other matters of some importance, although unrelated to the thesis of the Venesection Letter, deserve attention. First, the observation that cardiac systole is synchronous with arterial expansion. Vesalius had given this some consideration the year before in his edition of Giunter’s Institutiones anatomicae, or, as he stated in his current work. ‘I quietly expressed my doubts regarding that theory about which all physicians are very positive, whether the arteries and heart beat in the same way as the pulse.’ Now he presented his opinion more boldly:

“When the heart is contracted it diffuses [vital] spirit into the aorta and blood into the pulmonary artery; this motion of the heart is systole. When, however, the ventricles of the heart are dilated, the heart receives air from the pulmonary vein and blood for the vena cava; this motion is properly the diastole of the heart. When the arteries are dilated, I believe that they are filled with vital spirit from the heart which they distribute throughout the body. However, when they are contracted I consider it obvious that the sooty vapors are expelled. Hence the motions of the heart and arteries are contradictory and contrary.”

“Such observation was apparently the result of vivisection, such as Vesalius may have first observed under Giunter in Paris, or similar studies which … Vesalius asserted he had originated in Padua: “To some degree this can be proved during vivisection if one hand is placed upon the artery lying on the sacrum and the other grasps the whole of the intact heart.”

“As in his previous writings at Padua, so here Vesalius gives general as well as more specific promise of things to come:

“I shall omit for the present the movements of the head as well as the muscles and nerves which at my modest suggestion students do well to study. Indeed, with the favor of the gods, I shall discuss this matter more fully at another time … With regard to the rest of my studies there is little to say at present. I have now almost completed two illustrations of the nerves; in the first, the seven pairs of cranial nerves have been drawn, and in the other all the small branches of the dorsal marrow. I feel that these must be held back until I have produced illustrations of the muscles and of all the internal parts.

“This year I tried a plan by which these things might be accomplished during
the dissections, but it was unsuccessful with such a large group of spectators. If bodies were available here as they sometimes are elsewhere, not for long would the students lack such a useful work, especially since many distinguished men are constantly urging me to it … besides others, Marcantonio Genua, our distinguished professor of philosophy … has strongly urged me to the task … If bodies become available and Joannes Stephanus, the distinguished contemporary artist, does not refuse his services, I shall certainly undertake that task.”

“The closing part of this statement, referring to the artist Joannes Spephanus, has led to considerable controversy in attempts to identify the artists of the Fabrica …

“The Venesection Letter concludes with the words: ‘Padua, from the house of the sons of the most illustrious Count Gabriel of Ortembourg, 1 January 1539.’ It is likely that these sons of the Count of Ortembourg, of lower Austria, with whom Vesalius seems to have been living, were students at the university, although probably not of medicine … how long Vesalius lived with these young men is unknown, but probably it was until the end of the academic year 1538-39” (O’Malley, pp. 96-7).

“The young Vesalius (1514-64) received his elementary education in Brussels and matriculated at the University of Louvain in February 1530 to pursue the arts course, the necessary prerequisite for entrance into a professional school … Since at this time the medical school of Louvain had little repute, Vesalius chose to carry on his medical studies at the more illustrious faculty of the University of Paris, matriculating there probably in September 1533, where he studied with Guenther of Andernach (1505-74), Jacobus Sylvius (Jacques Dubois), and Jean Ferne. Guenther, who in his Institutions anatomicae (1536) spoke very favorably of his student, and Sylvius, an arch-Galenist and later an enemy of Vesalius, each
in his own way directed the young man toward anatomical research. Since they were both supporters of the Galenic tradition, it was natural that their student, although he acquired skill in the technique of dissection, remained under the influence of Galenic concepts of anatomy” (DSB).

“The [offered] treatise was written by Guenther as a dissection manual for the use of his students … Although Guenther himself seldom, if ever, participated directly in dissections, he permitted his students to do so, and in the Institutiones anatomicae he credited Vesalius with a discovery concerning the spermatic ducts – the first published reference to the young Vesalius. ‘These spermatic vessels, which are attached to the back by slender fibres, are extended downward, and as they approach the iliac region arteries are joined to them which arise very differently than the veins from the vena cava. I believe that no anatomist hitherto has mentioned this, let alone noticed it. Recently we discovered them after long investigation of the parts and through the skill of Andreas Vesalius, son of the Emperor’s apothecary – a young man, by Hercules, of great promise, possessing an extraordinary knowledge of medicine, learned in both languages, and very skilled in dissection of bodies’ (tr. O’Malley, pp. 55-56). Reflecting traditional practice in an age when cadavers were not readily available and refrigeration was unknown, Guenther advocated dissecting the perishable internal organs first and then the other parts of the body, an order from which Vesalius was later to dissent in De humani corporis fabrica.

“Vesalius’ redaction of Guenther’s treatise (Venice 1538) was one of his earlier published works, following the publication of his thesis, Paraphrasis in nonum librum Rhazae (Louvain 1537, reprinted Basel 1537), and his Tabulae anatomicae (Venice 1538). In the dedication to Johannes Armenterianus, professor of medicine at Louvain, Vesalius explained that his objective was to present Guenther’s work in a corrected and authoritative form. Expressing admiration for the merits of his teacher, he attributed the errors to the haste with which the text had originally been printed” (Norman).

The ‘Four books on some difficult questions’ of Leonhardt Fuchs (1501-66) is a heavily revised version of his Apologiae tres from 1538. It is part of a debate on the preferences for ancient Greek and Latin sources on one side and Arabic sources on the other. In the first three books, Fuchs refutes the views of Guillaume Dupuis, Sébastien de Monteux and Jérémie de Dryvere, respectively; the fourth book provides ‘Explicationes aliquot paradoxorum.’ Fuchs is best known for his great herbal, De historia stirpium, published two years after the present work. The ‘Four books’ contain “interesting points, as on the printing of his herbal, on mumification of the Hebrews and the use of bitumen for it found in the Near East, on sugar and sugar-derivatives, on blood and its nature, etc.” (Weil, Cat. 28).

“In attempting to reform medicine, Fuchs emphasized the importance of relying upon the ancient Greek authorities rather than upon later authors, just as Protestant leaders emphasized the importance of the Bible, rather than later authors and traditions, as the source of Christianity. He was active in the movement to publish new and more accurate editions of the Greek texts and the Latin translations based upon them. One of the editors of a Greek edition of Galen’s works (Basel, 1538), he translated both Hippocratic and Galenic medical texts and also the pharmaceutical work of Nicolaus Myrepus Alexandrinus.

“Fuchs’s reforming zeal led him into many controversies. Sometimes, as in his castigations of the hack writer Walther Hermann Ryff, his barbs were clearly deserved; but sometimes he seemed to side with Greek authors merely because they were ancient and their critics were less venerable. His bias nevertheless
suited the polemical atmosphere of his times, and he became a very successful author” (DSB).