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December 2017

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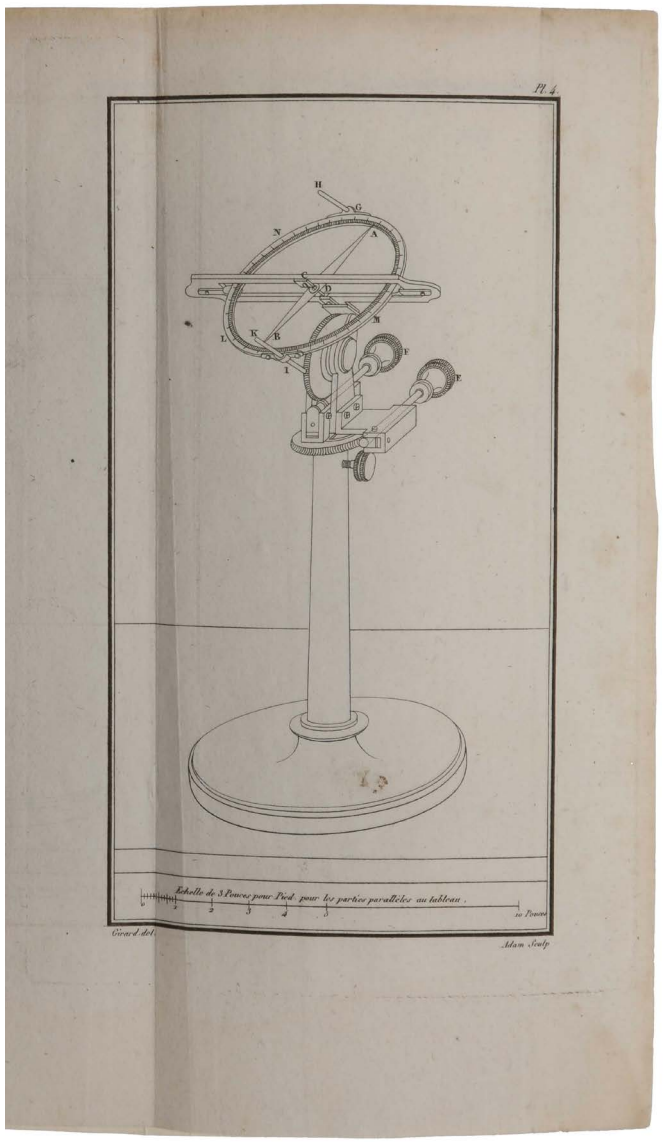
'ONE OF THE MOST BRILLIANT ACHIEVEMENTS IN SCIENCE' - JAMES CLERK MAXWELL

AMPÈRE, André Marie. *Mémoires sur l'action mutuelle de deux courans électriques, sur celle exise entre un courant électrique et un amiant ou le globe terrestre, et celle de deux aimans l'un sur l'autre.* Paris: l'Academie royale des Sciences, [1820].

$16,500

8vo (197 x 127 mm), pp. 68 with five folding engraved plates. Plain blue wrappers. A very clean and fresh copy.

First edition, very rare offprint issue, of Ampère's "first great memoir on electrodynamics" (DSB). 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 experimental investigation by which Ampére established the laws of the mechanical action between electric currents is one of the most brilliant achievements in science. The whole, theory and experiment, seems as if it had leaped, full grown and full armed, from the brain of the 'Newton of electricity.' It is perfect in form, and unassailable in accuracy, and it is summed up in a formula from which all the phenomena may be deduced, and which must always remain the cardinal formula of electro-dynamics."- Maxwell. "It was the discovery of electromagnetism by Hans Christian Oersted in the spring of 1820 which opened up a whole new world to Ampere and gave him the opportunity to show the full power of his



method of discovery. On 4 September 1820 François Arago reported Oersted's discovery to an astonished and skeptical meeting of the Académie des Sciences. Most of the members literally could not believe their ears; had not the great Coulomb proved to everyone's satisfaction in the 1780's that there could not be any interaction between electricity and magnetism? Ampere's credulity served him well here; he immediately accepted Oersted's discovery and turned his mind to it. On 18 September he read his first paper on the subject to the Académie; on 25 September and 9 October he continued the account of his discoveries. In these feverish weeks the science of electrodynamics was born" (DSB). Ampere's two papers appeared in the same volume of the *Annales de Chimie et de Physique*, but not consecutively (pp. 59-76 & 170-218). The present offprint contains both papers and is separately-paginated. OCLC lists four copies, none in US. ABPC/RBH lists five copies: Sotheby's New York, 13 December 2002, $9500 (inscribed); Christie's New York, 10 December 1999, $850; Christie's, New York, 29 October 1998, $9500 (Norman copy, inscribed); Doerling, 23 November 1983, $186 (disbound); Sotheby's, 30 October 1978, $1304 (Honeyman copy).

"There is some confusion over the precise nature of Ampère 's first discovery. In the published memoir, 'Mémoire sur 1'action mutuelle de deux courants électriques,' he leaped immediately from the existence of electromagnetism to the idea that currents traveling in circles through helices would act like magnets. This may have been suggested to him by consideration of terrestrial magnetism, in which circular currents seemed obvious. Ampère immediately applied his theory to the magnetism of the earth, and the genesis of electrodynamics may, indeed, have been as Ampère stated it. On the other hand, there is an account of the meetings of the Académie des Sciences at which Ampère spoke of his discoveries and presented a somewhat different order of discovery. It would appear that Oersted's discovery suggested to Ampere that two current-carrying wires might affect one another. It was this discovery that he announced to the Académie on 25 September. Since the pattern of magnetic force around a current-carrying wire was circular, it was no great step for Ampère the geometer to visualize the resultant force if the wire were coiled into a helix. The mutual attraction and repulsion of two helices was also announced to the Académie on 25 September. What Ampère had done was to present a new theory of magnetism as electricity in motion …

"Ampère's first great memoir on electrodynamics was almost completely phenomenological, in his sense of the term. In a series of classical and simple experiments, he provided the factual evidence for his contention that magnetism was electricity in motion. He concluded his memoir with nine points that bear repetition here, since they sum up his early work.

1. Two electric currents attract one another when they move parallel to one another in the same direction; they repel one another when they move parallel but in opposite directions.

2. It follows that when the metallic wires through which they pass can turn only in parallel planes, each of the two currents tends to swing the other into a position parallel to it and pointing in the same direction.

3.These attractions and repulsions are absolutely different from the attractions and repulsions of ordinary [static] electricity.

4. All the phenomena presented by the mutual action of an electric current and a magnet discovered by M. Oersted … are covered by the law of attraction and of repulsion of two electric currents that has just been enunciated, if one admits that a magnet is only a collection of electric currents produced by the action of the particles of steel upon one another analogous to that of the elements of a voltaic pile, and which exist in planes perpendicular to the line which joins the two poles of the magnet.

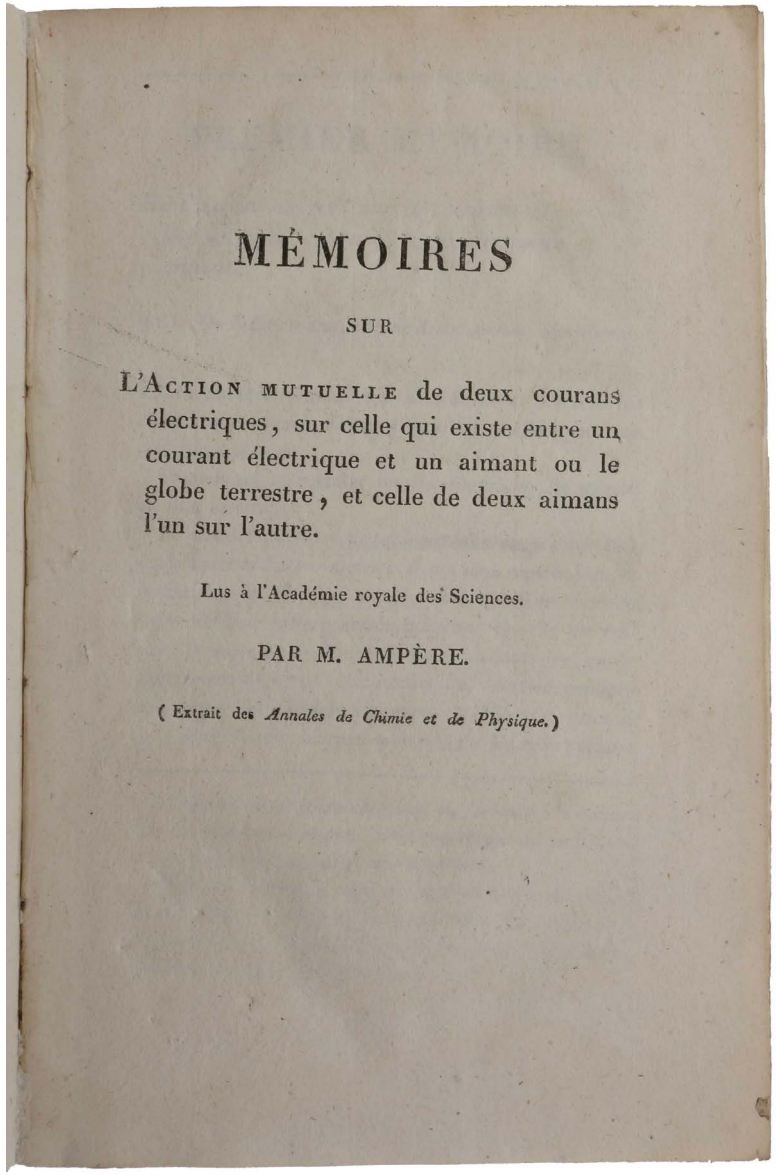
5. When a magnet is in the position that it tends to take by the action of the terrestrial globe, these currents move in a sense opposite to the apparent motion of the sun; when one places the magnet in the opposite position so that the poles directed toward the poles of the earth are the same [S to S and N to N, not south-seeking to S, etc.] the same currents are found in the same direction as the apparent motion of the sun.

6. The known observed effects of the action of two magnets on one another obey the same law.

7. The same is true of the force that the terrestrial globe exerts on a magnet, if one admits electric currents in planes perpendicular to the direction of the declination needle, moving from east to west, above this direction.

8. There is nothing more in one pole of a magnet than in the other; the sole difference between them is that one is to the left and the other is to the right of the electric currents which give the magnetic properties to the steel.

9. Although Volta has proven that the two electricities, positive and negative, of the two ends of the pile attract and repel one another according to the same laws as the two electricities produced by means known before him, he has not by that demonstrated completely the identity of the fluids made manifest by the pile and by friction; this identity was proven, as much as a physical truth can be proven, when he showed that two bodies, one electrified by the contact of [two] metals, and the other by friction, acted upon each other in all circumstances as though both had been electrified by the pile or by the common electric machine [electrostatic generator]. The same kind of proof is applicable here to the identity of attractions and repulsions of electric currents and magnets.



MÉMOIRES

SUR

L'ACTION MUTUELLE de deux courans électriques, sur celle qui existe entre un courant électrique et un aimant ou le globe terrestre, et celle de deux aimans l'un sur l'autre.

Lus à l'Académie royale des Sciences.

PAR M. AMPÈRE.

(Extrait des *Annales de Chimie et de Physique.*)

"Here Ampère only hinted at the noumenal background. Like most Continental physicists, he felt that electrical phenomena could be explained only by two fluids and, as he pointed out in the paper, a current therefore had to consist of the positive fluid going in one direction and the negative fluid going in the other through the wire. His experiments had proved to him that this contrary motion of the two electrical fluids led to unique forces of attraction and repulsion in current-carrying wires, and his first paper was intended to describe these forces in qualitative terms. There was one problem: how could this explanation be extended to permanent magnets? The answer appeared deceptively simple: if magnetism were only electricity in motion, then there must be currents of electricity in ordinary bar magnets.

"Once again Ampère's extraordinary willingness to frame ad hoc hypotheses is evident. Volta had suggested that the contact of two dissimilar metals would give rise to a current if the metals were connected by a fluid conductor. Ampère simply assumed that the contact of the molecules of iron in a bar magnet would give rise to a similar current. A magnet could, therefore, be viewed as a series of voltaic piles in which electrical currents moved concentrically around the axis of the magnet. Almost immediately, Ampère's friend Augustin Fresnel, the creator of the wave theory of light, pointed out that this hypothesis simply would not do. Iron was not a very good conductor of the electrical fluids and there should, therefore, be some heat generated if Ampère's views were correct. Magnets are not noticeably hotter than their surroundings and Ampère, when faced with this fact, had to abandon his noumenal explanation.

"It was Fresnel who provided Ampère with a way out. Fresnel wrote in a note to Ampère that since nothing was known about the physics of molecules, why not assume currents of electricity around each molecule. Then, if these molecules could be aligned, the resultant of the molecular currents would be precisely the concentric currents required. Ampere immediately adopted his friend's suggestion, and the electrodynamic molecule was born. It is, however, a peculiar molecule. In some mysterious fashion, a molecule of iron decomposed the luminiferous ether that pervaded both space and matter into the two electrical fluids, its constituent elements. This decomposition took place within the molecule; the two electrical fluids poured out the top, flowed around the molecule, and reentered at the bottom. The net effect was that of a single fluid circling the molecule. These molecules, when aligned by the action of another magnet, formed a permanent magnet. Ampere did not say why molecules should act in this way; for him it was enough that his electrodynamic model provided a noumenal foundation for electrodynamic phenomena" (DSB).

Dibner, *Heralds of Science* 62; Norman 43; Sparrow, *Milestones of Science* 8: Wheeler Gift 763a.

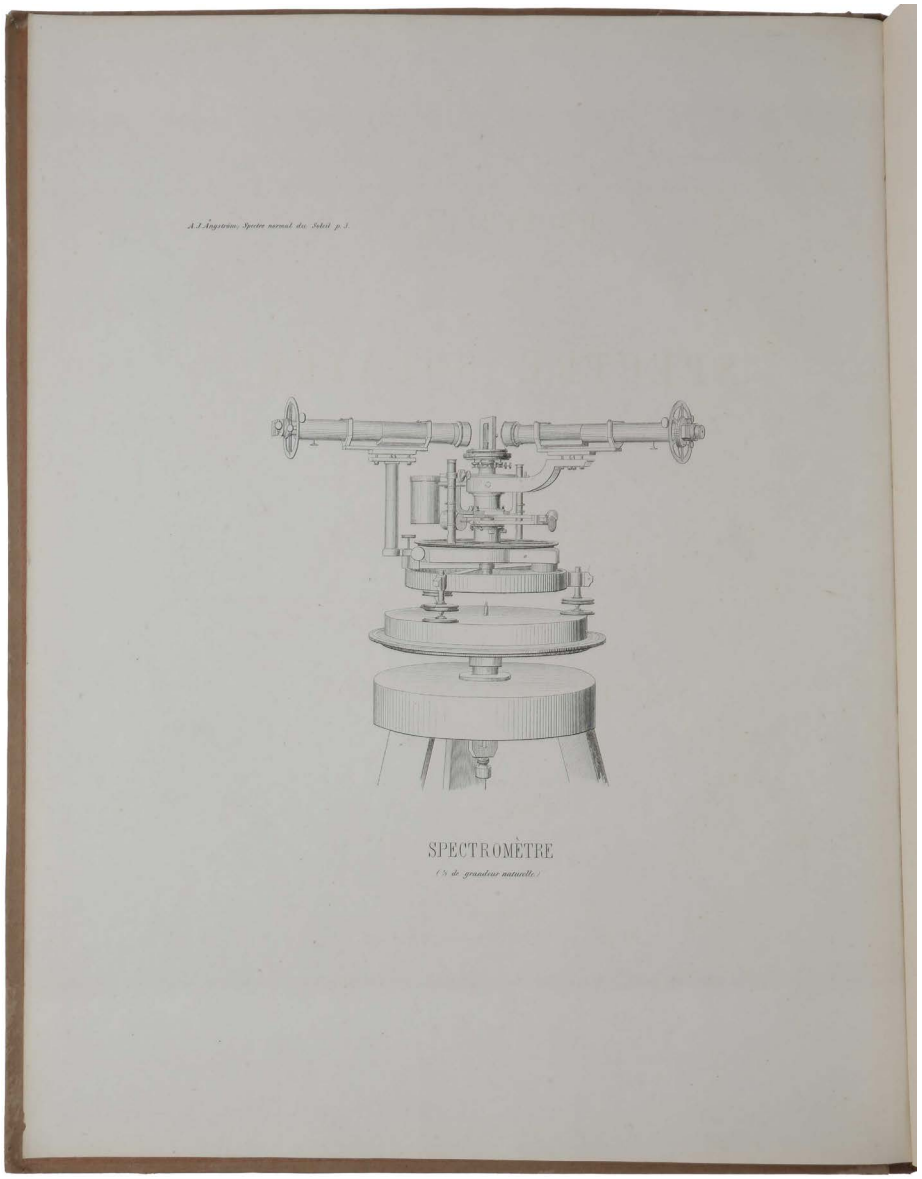
A FOUNDING WORK OF SPECTROSCOPY

ÅNGSTRÖM, Anders Jonas. *Recherches sur le Spectre Solaire. [With:] Spectre normal du soleil. Atlas.* Uppsala: W. Schultz, 1868.

$8,000

Text: Large 4to (310 x 240 mm), pp. [iv], 42, xv, [1], original printed boards with cloth spine, lithographed frontispiece showing Ångström's spectrometer; Atlas: Oblong folio (370 x 510 mm), [ii], with six plates by Robert Thalén (1827-1905). Original brown printed wrappers, with some wear and fraying to the edges.

First edition, rare in unrestored original printed wrappers, of one of the founding works of spectroscopy in which Ångström demonstrated the presence of hydrogen and a number of other elements in the sun; the atlas contains his great map of the solar spectrum. "After 1861 Ångström intensively studied the spectrum of the sun, noticing the presence of hydrogen in the solar atmosphere and confirming the probable existence there of a number of other elements. In 1868 he published the monumental *Recherches sur le Spectre Solaire*, which contained an atlas of the solar spectrum with measurements of the wavelengths of approximately a thousand lines determined by the use of diffraction gratings. Ångström expressed his results in units of one ten-millionth of a millimetre – a unit of length that has been named the ångström unit in his honor. In order to have a precise basis for the new science of spectroscopy, accepted standards were needed. In 1861 Kirchhoff made a map of the solar spectrum and labeled lines with the corresponding scale readings of his own prismatic instrument. These rapidly became the almost universally accepted manner of designating spectral lines, but they were inconvenient because each observer had to correlate his



own readings with those of the arbitrary Kirchhoff scale. Ångström's wavelength measurements provided a more precise and convenient reference and, after 1868, became a competing authoritative standard" (DSB).

"Anders Ångström (1814-1874) was an astronomical observer, physicist, and a pioneer in spectroscopy. His father Johan was a clergyman in the Lutheran church of Sweden. Ångström and his two brothers, Johan and Carl, all received higher education. Carl became a professor of mining technology; Johan became a physician and well-known botanist. Ångström studied at Uppsala University, and in 1839 he became a docent in physics there. As the professor in physics was a fairly young man, and as there were no other academic positions in physics other than the professorship, Ångström switched to astronomy, where there was a position as astronomical observer at the university.

"During the 1840s and 1850s Ångström worked as astronomical observer and acting professor of both astronomy and physics at Uppsala University. He did research in various fields during these years, for example in geomagnetism and the heat conduction of metals.

"By the time he was appointed regular professor of physics, in 1858, Ångström had already published one of his two most famous contributions to the new scientific field of spectroscopy. The paper *Optical Researches* was published in Swedish in 1853 and in English and German two tears later. In it Ångström presented, in an unsystematic fashion, a number of experimental results concerning the absorption of light from electrical sparks in gases. He also made theoretical interpretations indicating, among other things, that gases absorb light of the same wavelengths that they emit when heated, and suggesting, somewhat obliquely, that the Fraunhofer lines could be explained in this way.

"During the priority disputes that followed Gustav Kirchhoff's publication of the law of absorption and the explanation of the Fraunhofer lines around 1860, Ångström and his collaborator at Uppsala University, Robert Thalén, vigorously defended the Swede's priority. Their claims were to some extent recognized also in Britain when the Royal Society elected Ångström foreign member in 1870 and awarded him the Rumford Medal two years later. These honors were also given in recognition of Ångström's other important spectroscopic work, an atlas of the solar spectrum published in 1868. Much of the painstaking work that went into the atlas of the Fraunhofer lines (identified by wavelengths, which led to the designation Ångström being used for the unit of length 10-10 m) had been carried out by Thalén, though Ångström appeared as sole author of the work. During the 1860s and 1870s Ångström and Thalén carried out a great number of spectroscopic measurements, not only on the Fraunhofer lines but also on the wavelengths of emission spectra of many substances.

"During these decades and into the early 1880s, Ångström and Thalén dominated European spectroscopy. A measure of their influence is the publication of lists of spectroscopic data for the elements carried out by the British Association for the Advancement of Science [BAAS] in the mid-1880s. Of 67 elements, measurements by Ångström and Thalén (mostly by the latter) were given for 60; no other spectroscopists came close to that figure. Ångström 's atlas was used as standard reference by the BAAS, though it was soon to be superseded by the photographic atlas of Henry Rowland.

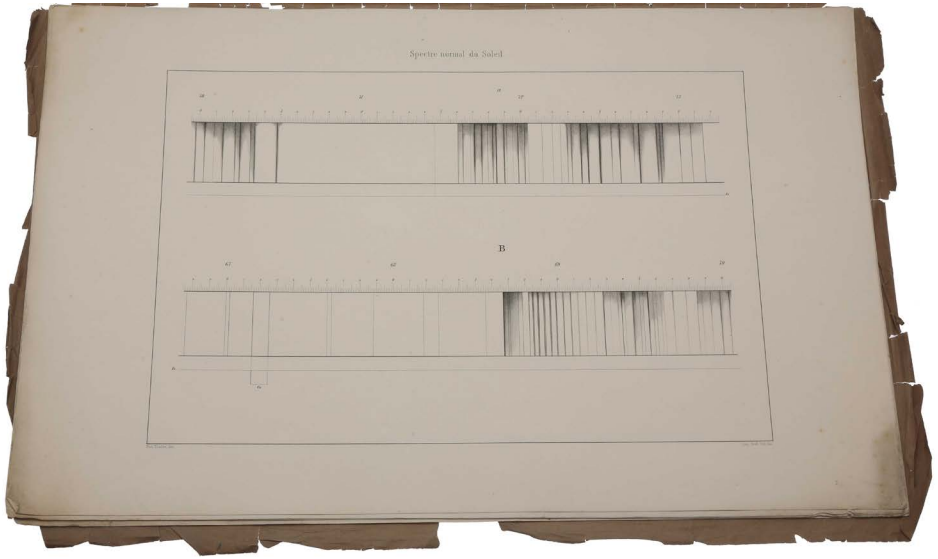
"Ångström became a member of the Royal Swedish Academy of Sciences in 1850, of the Prussian Academy of Sciences in 1867, of the Royal Society in 1870, and of the French Academy of Sciences in 1873. He was elected a member of several other Swedish and foreign scientific societies as well.

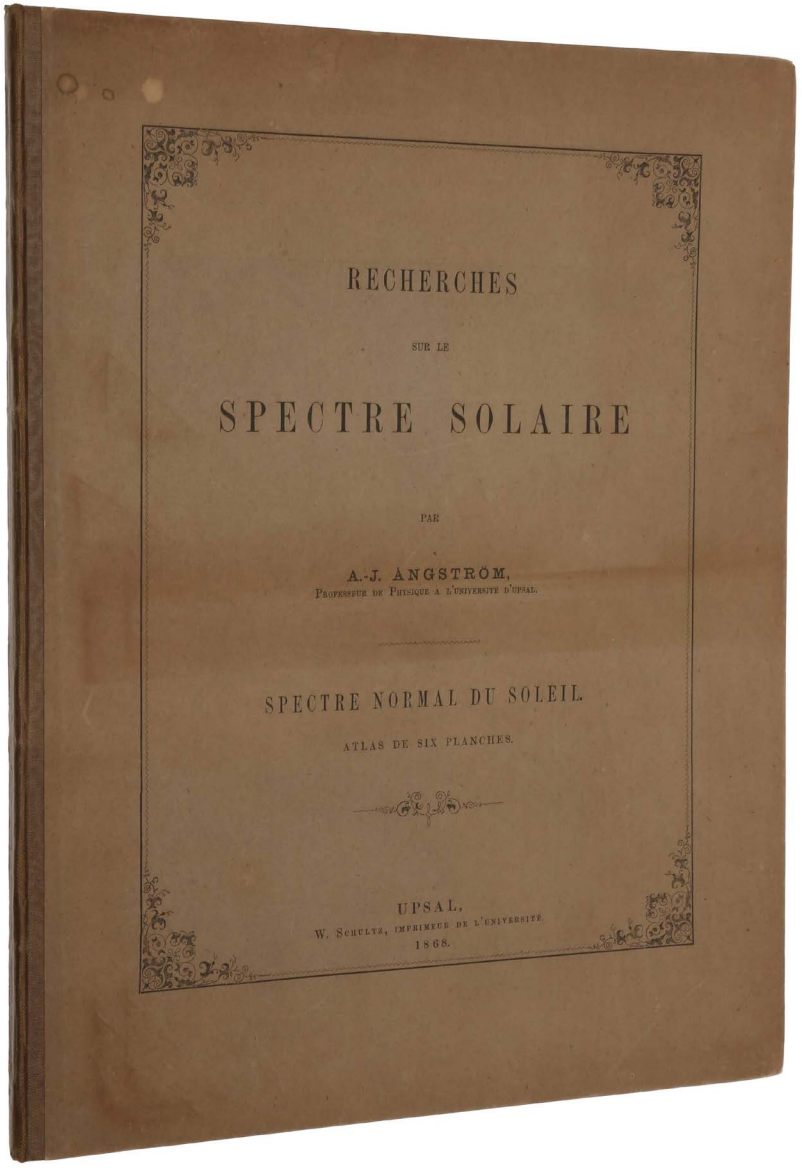
"In 1845 Ångström married Augusta Bedoire, and they had four children, two

of whom survived to adulthood. Their son Knut became a professor of physics at Uppsala University, succeeding his father's successor Robert Thalén in 1896. Their daughter Anna married Carl Gustaf Lundquist, a student of her father's, who in 1875 succeeded Thalén as professor of theoretical physics. There were additional family ties between the Ångströms and other scientific families at Uppsala. Hence, Anders Ångström was a founder not only of the science of spectroscopy but also of a scientific dynasty" (Biographical Encyclopedia of Astronomers).

Some copies have a further two plates showing the ultraviolet spectrum, not present in the Norman, Gedeon, Green and Honeyman copies. They were possibly added later.

Norman 56; Honeyman 96.





FIRST EDITION OF SPINOZA'S ETHICS

B. d. S. [SPINOZA, Benedictus de]. *Opera Posthuma. Quorum series post Praefationem exhibetur.* [Amsterdam: Jan Rieuwertsz], 1667.

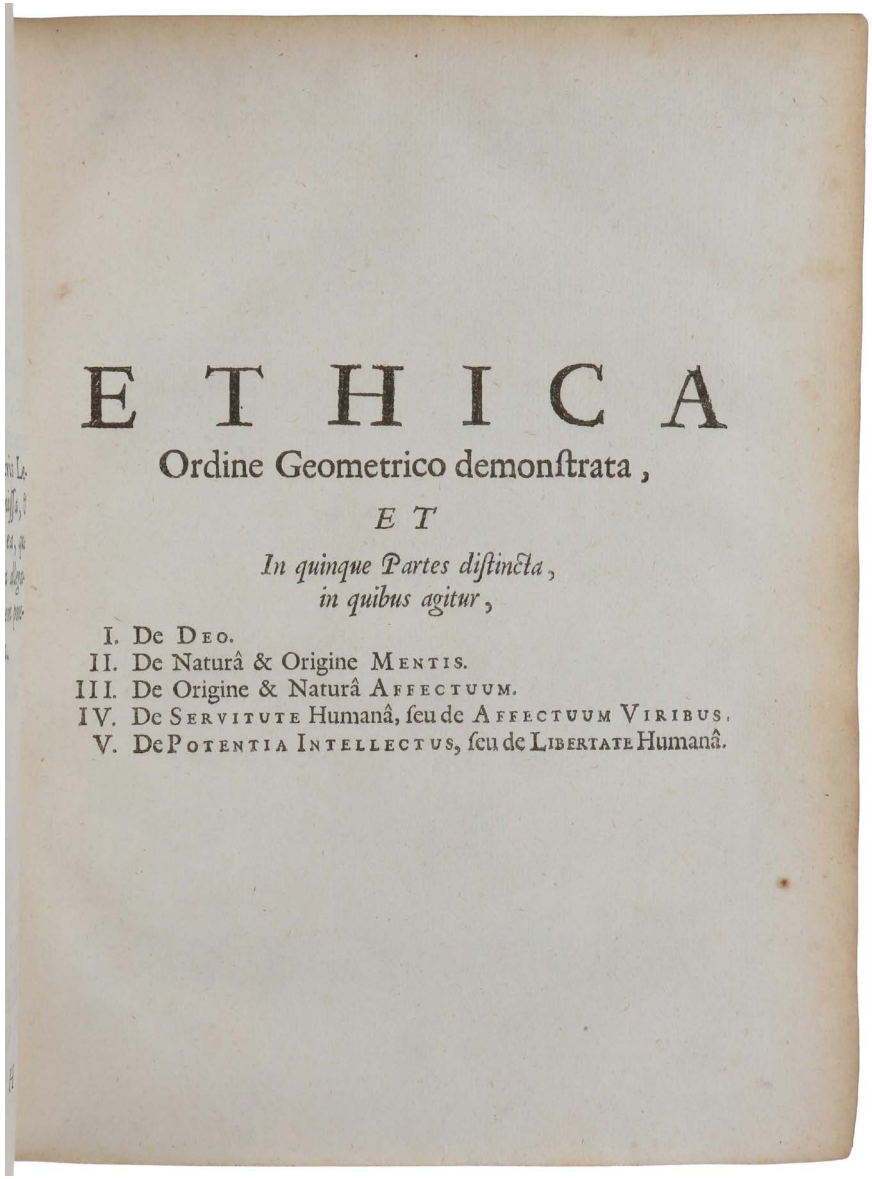
$17,500

4to (201 x 155 mm), pp. [40], 614, [34], 112, [8], with woodcut vignette on title. Contemporary vellum, handwritten title to spine. A very fine and fresh copy with no restoration at all. Rare in such good condition.

First edition, and a very fine copy, of Spinoza's Opera Posthuma which "have served, then and since, with the *Tractatus Theologico-Politicus*, to immortalize his name" (PMM 153). The first work in the volume is "Spinoza's one indisputable masterpiece, the *Ethics*" (Bennett).

The first and "principal work in the *Opera Posthuma* is Spinoza's *Ethics*, in which Spinoza bridged the Cartesian duality of body and spirit by maintaining that the universe, including God, constitutes a unified infinite and all-inclusive 'Substance,' of which corporeality and spirituality were merely attributes – a unity expressed in the controversial phrase 'Deus sive Natura' (God or Nature). *Ethics* is thus considered the first systematic exposition of pantheism, the philosophy in which God is identified with the entire universe" (Norman 1988).

"Baruch (or Benedictus) Spinoza is one of the most important philosophers - and certainly the most radical - of the early modern period. His thought combines a commitment to Cartesian metaphysical and epistemological principles with elements from ancient Stoicism and medieval Jewish rationalism into a



ETHICA

Ordine Geometrico demonſtrata,

ET

In quinque Partes diſtincta, in quibus agitur,

I. De Deo.
II. De Naturâ & Origine Mentis.
III. De Origine & Naturâ Affectuum.
IV. De Servitute Humanâ, ſeu de Affectuum Viribus.
V. De Potentia Intellectus, ſeu de Libertate Humanâ.

nonetheless highly original system. His extremely naturalistic views on God, the world, the human being and knowledge serve to ground a moral philosophy centered on the control of the passions leading to virtue and happiness. They also lay the foundations for a strongly democratic political thought and a deep critique of the pretensions of Scripture and sectarian religion. Of all the philosophers of the seventeenth-century, perhaps none have more relevance today than Spinoza" (*Stanford Encyclopedia of Philosophy*).

"Born in Amsterdam to a distinguished family of Sephardic exiles from Spain, Spinoza (1632-77) early absorbed all the theological and philosophical knowledge that the rabbis of his community were able to impart. Latin he learnt from an eccentric physician of materialistic tendencies, which brought him into contact with Giordano Bruno and Descartes. From this followed his break with Jewish orthodoxy, and the excommunication imposed upon him on 27 July 1656. From then on Spinoza, adopting the Latin form Benedict of his birth name Baruch, led a wandering life. Like all his Jewish contemporaries, he had learnt a handicraft: the grinding of lenses. In this, as in the theory of optics, he showed great ability. His lenses were in considerable demand, and his skill brought him into contact with Huygens and Leibniz: a tract on the rainbow, long thought to be lost, was published as recently as 1862. Thus Spinoza was able to support himself as the guest of a friend, a member of the Collegiants, an Armenian religious community, in the country outside Amsterdam out of reach of his late co-religionists, and to devote himself to concentrated thought and study. There he found himself the centre of a small philosophical club, which, originally meeting to study Cartesian philosophy, eventually parted company with Descartes; it was for them, in all probability, that Spinoza wrote his 'Ethics'" (PMM).

"[M]ost likely in the spring of 1662, Spinoza took up his pen to begin what would be his philosophical masterpiece, the 'Ethics' (*Ethica*) … [I]n essence, a treatise on "God, man and His Well-Being," the 'Ethics' was an attempt to provide a fuller, clearer, and more systematic layout in "the geometric style" for his grand metaphysical and moral project. He worked on it steadily for a number of years, through his move to Voorburg in 1663 and on into the summer of 1665. He envisioned at this point a three-part work, and seems to have had a fairly substantial draft in hand by June 1665. He felt confident enough of what he had written so far to allow a select few to read it, and there were Latin and even Dutch (translated by Pieter Balling) copies of the manuscript circulating among his friends. We do not know how close to a final product Spinoza considered this draft of the 'Ethics' when he put it aside, probably in the fall of 1665 ... At the time he probably saw it as mostly complete but in need of polishing. It would be a good number of years, though, before Spinoza returned to his metaphysical-moral treatise to put the finishing touches on it, which included significant additions and revisions, no doubt in the light of further reading and reflection" (Nadler, *Spinoza's Ethics. An Introduction*, p. 15).

"In 1675 he contemplated publishing his 'Ethics,' but baseless rumours, later idly repeated by Hume, of his atheism, decided him against it. On 20 February 1677 he died of consumption and his funeral was attended by a devoted and distinguished gathering" (PMM).

Immediately after his death, his friends arranged the publication of his Ethics, together with his other unpublished writings, in *Opera Posthuma*. It was edited by one of Spinoza's closest friends, Jarig Jelles. The *Ethics* is followed in the volume by four other works:

Tractatus de intellectus emendatione, a preliminary work to the *Ethics*, "written probably before Spinoza was thirty years old, is important not only historically, as showing how gradually and consecutively what he had to tell the world was revealed to him, but also for its own intrinsic worth" (Hale-White (translator),

Tractatus de intellectus emendation (1895), p. 2). "The *Tractatus* is an attempt to formulate a philosophical method that would allow the mind to form the clear and distinct ideas that are necessary for its perfection. It contains, in addition, reflection upon the various kinds of knowledge, an extended treatment of definition, and a lengthy analysis of the nature and causes of doubt" (*Wikipedia*).

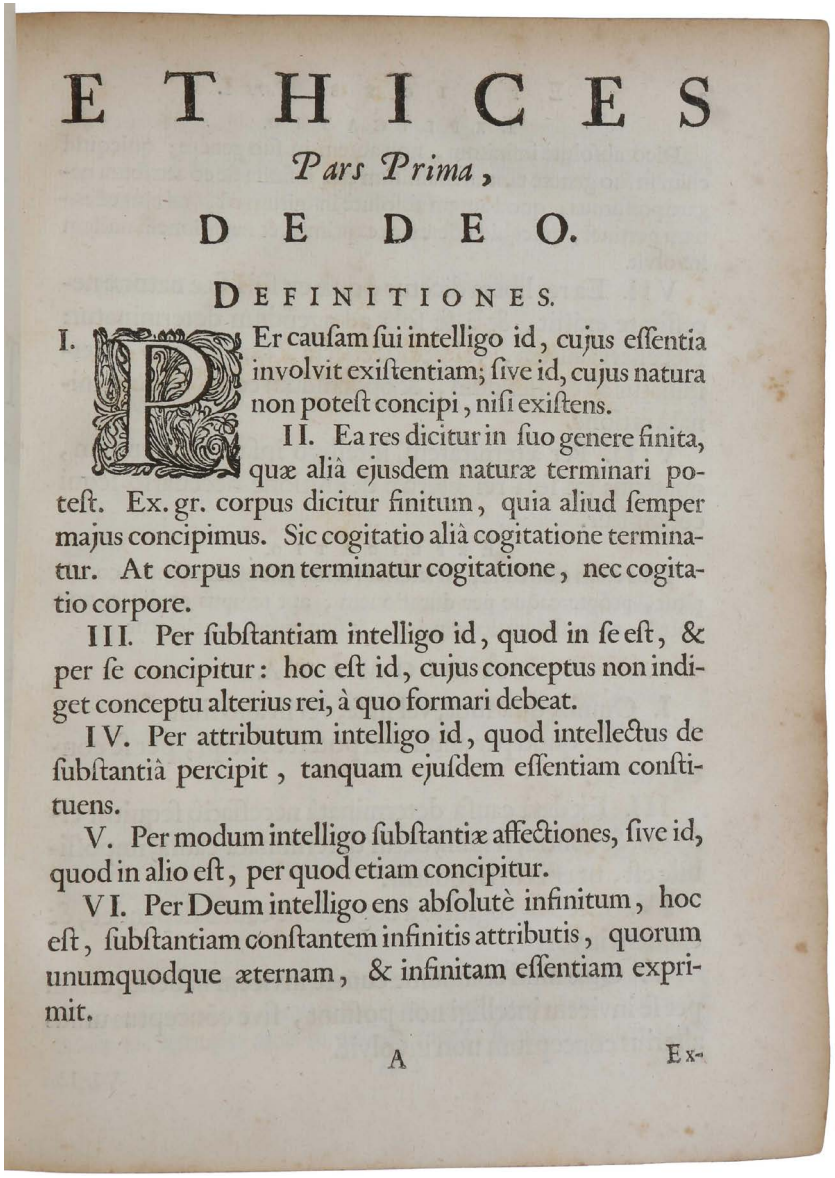
The unfinished *Tractatus politicus* "is a fitting sequel to the *Ethics*. Whereas the *Ethics* reveals the path to individual freedom, the *Tractatus politicus* reveals the extent to which individual freedom depends on civil institutions. We should not be surprised to find Spinoza to be civic-minded. From his earliest writings, he claims that he is concerned not just to perfect his own nature but also "to form a society of the kind that is desirable, so that as many as possible may attain [a flourishing life] as easily and surely as possible." The *Tractatus politicus* may be seen as Spinoza's attempt to articulate some of the conditions for the possibility of such a society" (*Stanford Encyclopedia of Philosophy*).

A collection of 74 *Epistolae*, letters from and to Spinoza. "The letters are an invaluable source of information about Spinoza's life, his network of friends and acquaintances, and his works. The reason for writing the *Tractatus Theologico-Politicus* is explained in Ep. 30, and in many letters Spinoza responds to criticisms or enquiries about his views on religion … The correspondence also reflects how Spinoza's contemporaries worried about the ethical and religious implications of his philosophy, and documents the variety of subjects that were discussed under the heading of philosophy: planets (Ep. 26), hydrostatics (Ep. 41), nitre (Ep. 6, 13), probability calculus (Ep. 38). Spinoza's expertise in lens-grinding is apparent in discussions of lenses, telescopes, optics and dioptrics (Ep. 26, 32, 36, 39, 40, 46)" (*The Bloomsbury Companion to Spinoza*, p. 359).

"The fifth and final work in Spinoza's *Opera Posthuma* (with its own title page,

pagination, and errata) is a Grammar of the Hebrew Language, *Compendium Grammaticus Lingua Hebraeae*. Spinoza was one of the first to subject the Bible to critical analysis but demanded that such analysis be rooted in a thorough understanding of the Hebrew language. Then, and only then, Spinoza states, may one turn to 'the life, the conduct and the pursuits of the author of each book ... [and] the fate of each book: how it was first received, into whose hands it fell, how many different versions there were of it, by whose advice it was received into the Canon, and how all the books now universally accepted as sacred, were united into a single whole'" (jewishvirtuallibrary.org).

See PMM 153; Brunet V, 492; Caillet 10309; Kingma & Offenberg 24; Norman 1988; Van der Linde 22 (apparently lacking the separate half-titles for Ethica and *Compendium Grammatices Linguæ Hebrææ*, which are present in our copy); Wolf Collection 378.



ETHICES

Pars Prima,

DE DEO.

DEFINITIONES.

I. PEr causam sui intelligo id, cujus essentia involvit existentiam; sive id, cujus natura non potest concipi, nisi existens.

II. Ea res dicitur in suo genere finita, quæ aliâ ejusdem naturæ terminari potest. Ex. gr. corpus dicitur finitum, quia aliud semper majus concipimus. Sic cogitatio aliâ cogitatione terminatur. At corpus non terminatur cogitatione, nec cogitatio corpore.

III. Per substantiam intelligo id, quod in se est, & per se concipitur: hoc est id, cujus conceptus non indiget conceptu alterius rei, à quo formari debeat.

IV. Per attributum intelligo id, quod intellectus de substantiâ percipit, tanquam ejusdem essentiam constituens.

V. Per modum intelligo substantiæ affectiones, sive id, quod in alio est, per quod etiam concipitur.

VI. Per Deum intelligo ens absolutè infinitum, hoc est, substantiam constantem infinitis attributis, quorum unumquodque æternam, & infinitam essentiam exprimit.

A Ex-

'THE MOST REMARKABLE GENERAL WORK IN THEORETICAL AND APPLIED MECHANICS'

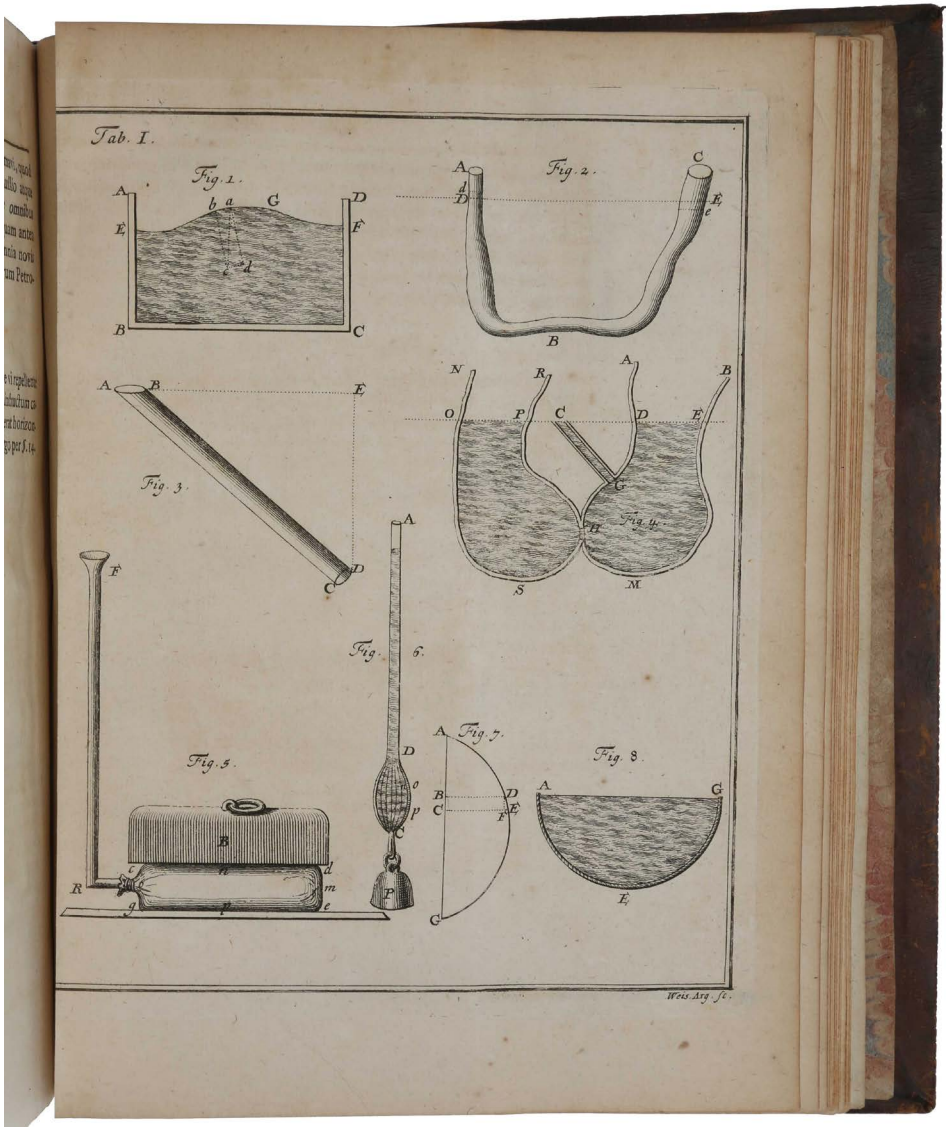
BERNOULLI, Daniel. *Hydrodynamica, sive De Viribus et Motibus Fluidorum Commentarii. Opus Academicum.* Strasbourg: Johann Reinhold Dulsseker, 1738.

$13,500

4to (256 x 205 mm), pp [viii] 304, title in red and black, with engraved vignette and 12 folding engraved plates by Johann Martin Weiss, contemporary calf, gilt spine label, upper capital with restoration.

First edition of Bernoulli's epochal work on fluid dynamics and the kinetic theory of gases, containing the famous 'Bernoulli equation' for fluid flow. "Besides introducing the first hydraulic theory of fluid flow, this book is the most remarkable general work in theoretical and applied mechanics written in the pre-Lagrangian period of the 18th century, based on a deep physical understanding of mechanical phenomena and presenting many new ideas … Bernoulli's treatise was to influence the entire development of mechanics and, especially, of applied mechanics, for at least a century" (*Landmark Writings*, pp. 131-2). "In 1738 Bernoulli published Hydrodynamica. In this treatise, which was far in advance of his time in many ways, is his famous equation governing the flow of fluids in terms of speed, pressure, and potential energy, upon which much modern technology is based, especially aerodynamics" (DSB).

"In this book Bernoulli presented the earliest adequate theory of motion of



an incompressible fluid in tubes (vessels) and fluid outflow through orifices, introducing the notion of hydrodynamic pressure. However, the treatise is not restricted to theoretical hydraulics. In the subsequent sections, he opens up new branches of physics and mechanics. He develops the first model of the kinetic theory of gases, approaches the principle of conservation of energy, establishes a foundation for the analysis of efficiency of machines, and develops a theory of hydroreactive (water-jet) ship propulsion, including a solution of the first problem of motion of a variable-mass system.

"*Hydrodynamica* contains many profound remarks on the physical background of a wide range of mechanical effects, and its study remains most edifying also to the modern reader … However, many of his advanced ideas were far ahead of his time and met an adequate understanding only later. In the 19th century, J.-V. Poncelet called Bernoulli's treatise 'the immortal *Hydrodynamica*' in 1845, and Paul Du Bois-Reymond referred to 'the enormous wealth of ideas which assures this work one of the first places in the literature of Mathematical Physics of all ages' in 1859.

"*Hydrodynamica* is founded mainly on the principle of conservation of 'living forces' (that is, kinetic energy). Bernoulli preferred to use this principle not in its traditional form, received with hostility by Newtonians, but in Christiaan Huygens's formulation that Bernoulli named the principle of equality between the actual descent and potential ascent: 'If any number of weights begin to move in any way by the force of their own gravity, the velocities of the individual weights will be everywhere such that the products of the squares of these velocities multiplied by the appropriate masses, gathered together, are proportional to the vertical height through which the centre of gravity of the composite of the bodies descends, multiplied by the masses of all of them'.

"As to hydraulics proper, Bernoulli's considers only quasi-one-dimensional fluid motion, reducing any flow to this case by means of the hypothesis of plane sections: he does not distinguish between tubes and vessels. The principle of conservation of living forces was used for studying fluid flow by Bernoulli and also by Leonhard Euler. Coincidence of their results presented independently in the Petersburg Academy of Sciences in 1727 forced Euler to change his scientific plans and to leave this field for his elder colleague. When Bernoulli developed his work, besides studying many special cases of flow, he achieved two new fundamental results. He succeeded in explaining the nature and determining the value of the hydrodynamic pressure of moving fluids on the wall of tubes and he discovered the principal role of losses of living forces in the fluid flow, especially at sudden changes of the flow cross-sections. The former gave an instrument to engineers for calculation of tube strength and the latter served, in addition, a step to the general principle of conservation of energy. Bernoulli concluded also the sharp discussion of many years on the impact and reaction of emitting jets, giving the final solution of the problem" (*Landmark Writings*, pp. 132-3).

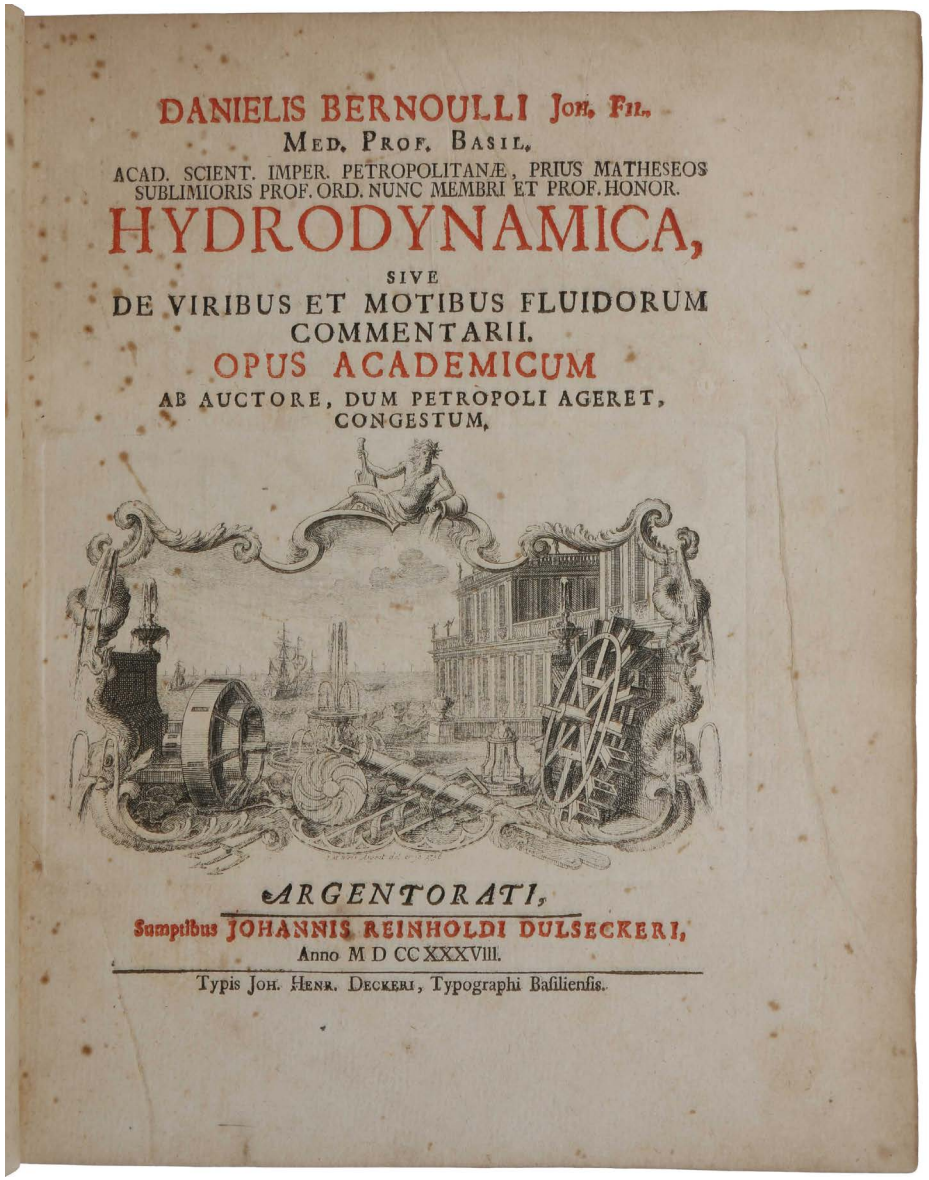
Bernoulli composed the first version of this work at St. Petersburg in the early 1730s; its forthcoming publication was announced in the September 1734 issue of the journal Mercure Suisse. The following December Bernoulli, now in Basel, wrote to Euler: 'My *Hydrodynamica* is now really being printed by Mr. Dulsecker, and he gives me, besides 30 copies, even 100 thalers of royalty'. However, the actual printing of the book seems to have begun only in 1737, and it finally appeared at the end of April or the beginning of May 1738. In May, Bernoulli sent the first copies of his treatise to St. Petersburg and asked Euler for his comments, but the parcel was lost on the way and it was not until spring 1739 that Euler saw a copy of the book and warmly congratulated its author: "I have read through your incomparable Treatise with full attention and have drawn immense gain from it. I congratulate you, Sir, from all my heart on the felicitous execution of such a difficult and obscure topic, as well as on the immortal fame thus gained. The

entire execution of the project deserves all conceivable attention, and all the more so as it is not accessible to rigorous mathematics, but demands the help of several important physical principles, which you have known to employ to indescribable advantage."

In contrast to Euler's appreciation, *Hydrodynamica* became the focus of a bitter dispute between Daniel and his father Johann I. Although the details are disputed, it seems that Johann saw a copy of Daniel's book and used it to compose his own work on the subject, *Hydraulica*, keeping this secret from his son. Johann published *Hydraulica* at the beginning of 1743 in his *Opera Omnia*, adding the subtitle now for the first time disclosed and directly shown from purely mathematical foundations, 1732. Daniel was incensed by Johann's pretence that his *Hydraulica* had been composed some six years before his own work had been published, and the resulting rift between father and son was never completely healed.

"Daniel Bernoulli (1700-1782) was the second son of Johann Bernoulli, who first taught him mathematics. After studying philosophy, logic, and medicine at the universities of Heidelberg, Strasbourg, and Basel, he received an M.D. degree (1721). In 1723–24 he wrote *Exercitationes quaedam Mathematicae* on differential equations and the physics of flowing water, which won him a position at the influential Academy of Sciences in St. Petersburg, Russia. Bernoulli lectured there until 1732 in medicine, mechanics, and physics, and he researched the properties of vibrating and rotating bodies and contributed to probability theory. In that same year he returned to the University of Basel to accept the post in anatomy and botany. By then he was widely esteemed by scholars and also admired by the public throughout Europe.

"Between 1725 and 1749 Daniel won 10 prizes from the Paris Academy of Sciences for work on astronomy, gravity, tides, magnetism, ocean currents, and the



DANIELIS BERNOULLI JOH. FIL.
MED. PROF. BASIL.
ACAD. SCIENT. IMPER. PETROPOLITANÆ, PRIUS MATHESEOS SUBLIMIORIS PROF. ORD. NUNC MEMBRI ET PROF. HONOR.

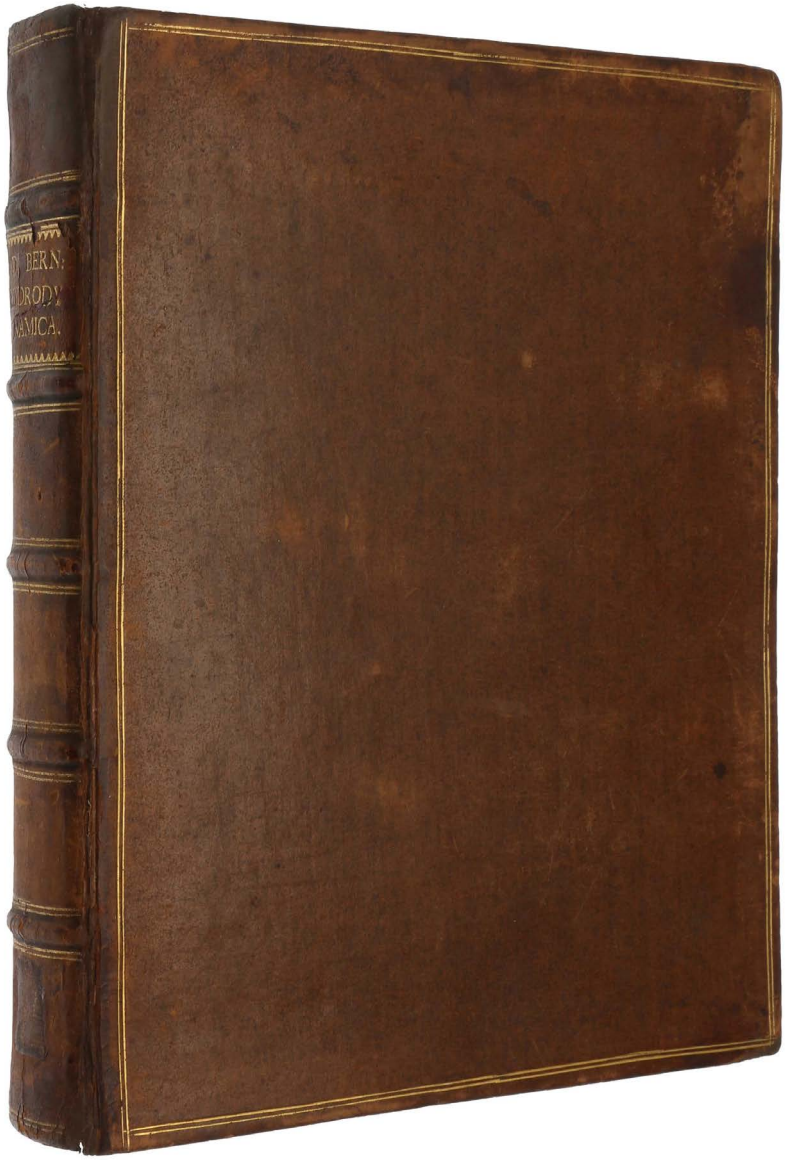
HYDRODYNAMICA,

SIVE
DE VIRIBUS ET MOTIBUS FLUIDORUM COMMENTARII.
OPUS ACADEMICUM
AB AUCTORE, DUM PETROPOLI AGERET, CONGESTUM.

ARGENTORATI,
Sumptibus JOHANNIS REINHOLDI DULSECKERI,
Anno M D CC XXXVIII.
Typis JOH. HENR. DECKERI, Typographi Basilienſis.

behaviour of ships at sea. He also made substantial contributions in probability. He shared the 1735 prize for work on planetary orbits with his father, who, it is said, threw him out of the house for thus obtaining a prize he felt should be his alone. Daniel's prizewinning papers reflected his success on the research frontiers of science and his ability to set forth clearly before an interested public the scientific problems of the day. In 1732 he accepted a post in botany and anatomy at Basel; in 1743, one in physiology; and in 1750, one in physics" (*Britannica*).

Barchas 175; Norman 215; Parkinson pp. 155-6; Roberts and Trent pp. 34-5. Mikhailov, 'Daniel Bernoulli, Hydrodynamica (1738),' Chapter 9 in *Landmark Writings in Western Mathematics* 1640-1940, Grattan-Guinness (ed.), 2005. For a detailed analysis of the work, see Truesdell, 'Rational fluid mechanics, 1687-1765,' in Euler *Opera Omnia*, Ser. 2, Vol. 12 (1954), pp. vii–cxxv (especially pp. xxiii–xxxviii).



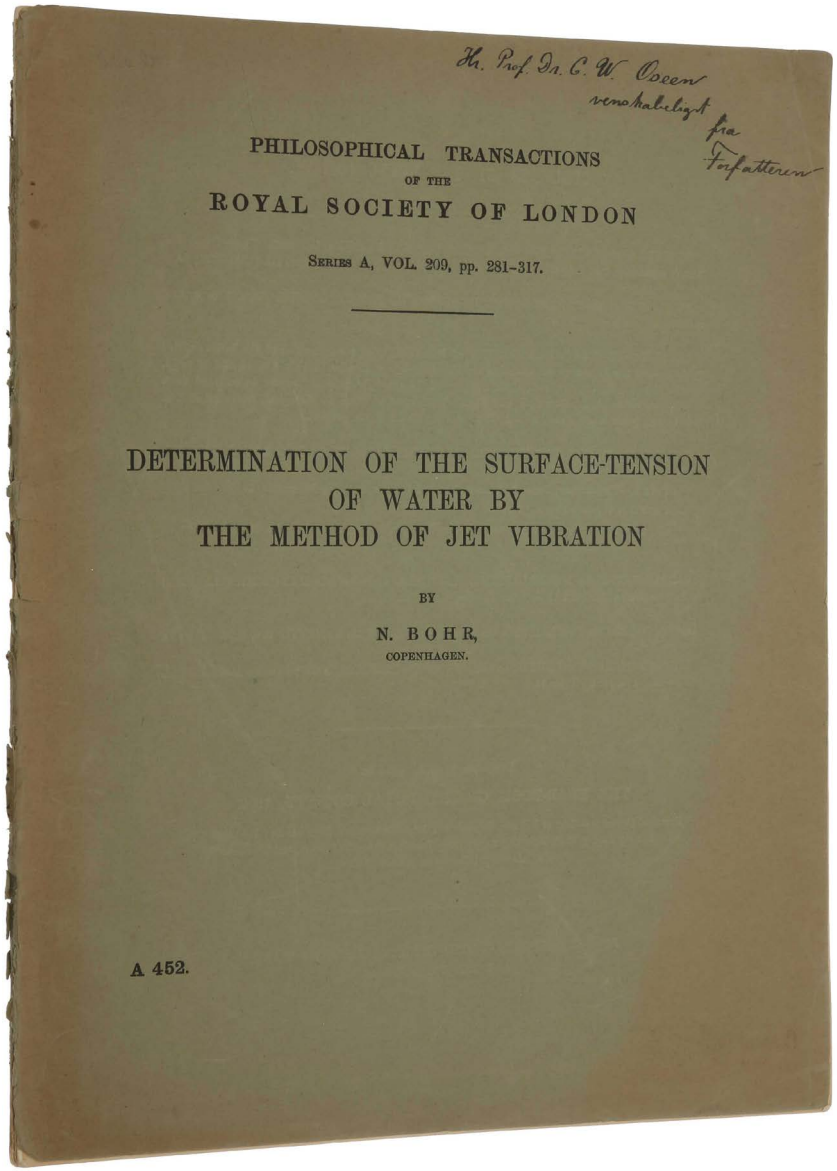
INSCRIBED PRESENTATION COPY OF BOHR'S FIRST SCIENTIFIC WORK

BOHR, Niels. *Determination of the Surface-Tension of Water by the Method of Jet Vibration.* London: The Royal Society, 1909.

$5,500

Offprint from: Philosophical Transactions of the Royal Society of London, Series A, Vol. 209. 4to (305 x 231 mm), pp. 281-317, [1]. Original printed wrappers (wrappers detached, spine slightly damaged).

First edition, offprint issue, author's inscribed presentation copy, of Bohr's first published scientific paper. "His first research project, a precision measurement of the surface tension of water by the observation of a regularly vibrating jet, was completed in 1906, when he was still a student, and it won him the gold medal from the Academy of Sciences. It is a mature piece of work, remarkable for the care and thoroughness with which both the experimental and theoretical parts of the problem were handled" (DSB). In 1879 Lord Rayleigh had proposed a method for determining the surface tension of liquids. "His idea was this. When a liquid jet with a non-circular cross-section emerges from a cylindrical tube, its surface vibrates. Rayleigh showed that from the velocity and cross-section of the jet and the wavelengths of its surface vibrations one can determine the surface tension of the liquid. He had not, however, performed quantitative experiments to implement this method. The problem posed by the Academy was to do just that. The question was purely experimental. Bohr, however, included in his work essential improvements on Rayleigh's theory by taking into account the influence of the liquid's viscosity and of the ambient air, and by extending the earlier theory


Hr. Prof. Dr. C. W. Oseen
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PHILOSOPHICAL TRANSACTIONS
OF THE
ROYAL SOCIETY OF LONDON

SERIES A, VOL. 209, pp. 281–317.

DETERMINATION OF THE SURFACE-TENSION
OF WATER BY
THE METHOD OF JET VIBRATION

BY

N. BOHR,
COPENHAGEN.

A 452.

from infinitesimal to arbitrarily large vibration amplitudes … The prize problem had been announced in February 1905. The deadline for submission was 30 October 1906. Bohr spent most of the intervening time working intensely on the problem, doing the experimental part in his father's laboratory … He was his own glass blower, preparing long tubes with elliptical cross-section so as to produce an elliptical jet with mean radius less than a millimeter. He examined every tube under a microscope and accepted only those with uniform elliptical cross-section. The jet had to be maintained under stable conditions over long periods (it should not rapidly break up into drops) and at constant temperature. The jet velocity was accurately determined by cutting the jet at a given position at two different times, measuring the time interval and, photographically, the length of the segment cut out. Bohr analyzed the vibration amplitudes of the liquid used, tap-water, by photographic observation in nearly monochromatic light. In order to avoid perturbing vibrations due to passing traffic, many observations he made were at night" (Pais, Niels Bohr's Times, pp. 101-2). ABPC/RBH list only two copies of this offprint: Bruun Rasmussen, 2014, inscribed to Einar Bülmann, €2625; and the Plotnick copy, Christie's, 2002, inscribed to S. Weber, $7170, previously sold by Swann, 1995, $375.

Provenance: Carl Wilhelm Oseen (1879-1944) (inscription in Danish on upper wrapper in Bohr's hand). Oseen was a theoretical physicist in Uppsala and Director of the Nobel Institute for Theoretical Physics in Stockholm. "The Bohr brothers had first met Oseen in the summer of 1911 at a congress of Scandinavian mathematicians in Copenhagen. The Swedish physicist, who had read Niels Bohr's dissertation [offered here] with interest (as a Swede he had no difficulties with the Danish language), was among the first to recognize the genius of his colleague in Copenhagen" (Kragh, *Niels Bohr and the Quantum Atom*, p. 45). Nevertheless, Oseen could be critical. "In a letter to Bohr of 11 November 1913 in which he congratulated Bohr on his second paper in the trilogy ['On the constitution of atoms and molecules'], [Oseen wrote that] Bohr had developed his theory 'beyond the region of hypotheses and theories and into that of truth itself'. Praise apart, Oseen was curious to know 'how the Maxwell – Lorentz theory should be modified to allow for the existence of an atom of your type'. Oseen (in contrast to Bohr) continued to worry about the problem, and in a detailed analysis in Physikalische Zeitschrift he reached the following, unequivocal conclusion: 'Bohr's atom model can in no way be reconciled with the fundamental assumptions of Lorentz's electron theory. We have to make our choice between these two theories. One of them may be correct, but not both of them'" (*ibid*., p. 125). Nevertheless, Oseen was full of admiration for Bohr's theory. In his presentation speech at the award of the 1925 Nobel Prize to James Franck and Gustav Hertz, Oseen "emphasized that although the experiments of Franck and Hertz, were valuable by themselves, 'even more important at the present time is the general finding that Bohr's hypotheses concerning the different states of the atom and the connexion between these states and radiation, have been shown to agree completely with reality'" (ibid., p. 146). As a full professor of a Swedish university, Oseen had the right to nominate Nobel Prize winners, and it was Oseen who nominated Albert Einstein for the Nobel Prize in 1921, for Einstein's work on the photoelectric effect. Einstein was finally awarded the prize for 1921 when Oseen repeated the nomination in 1922.

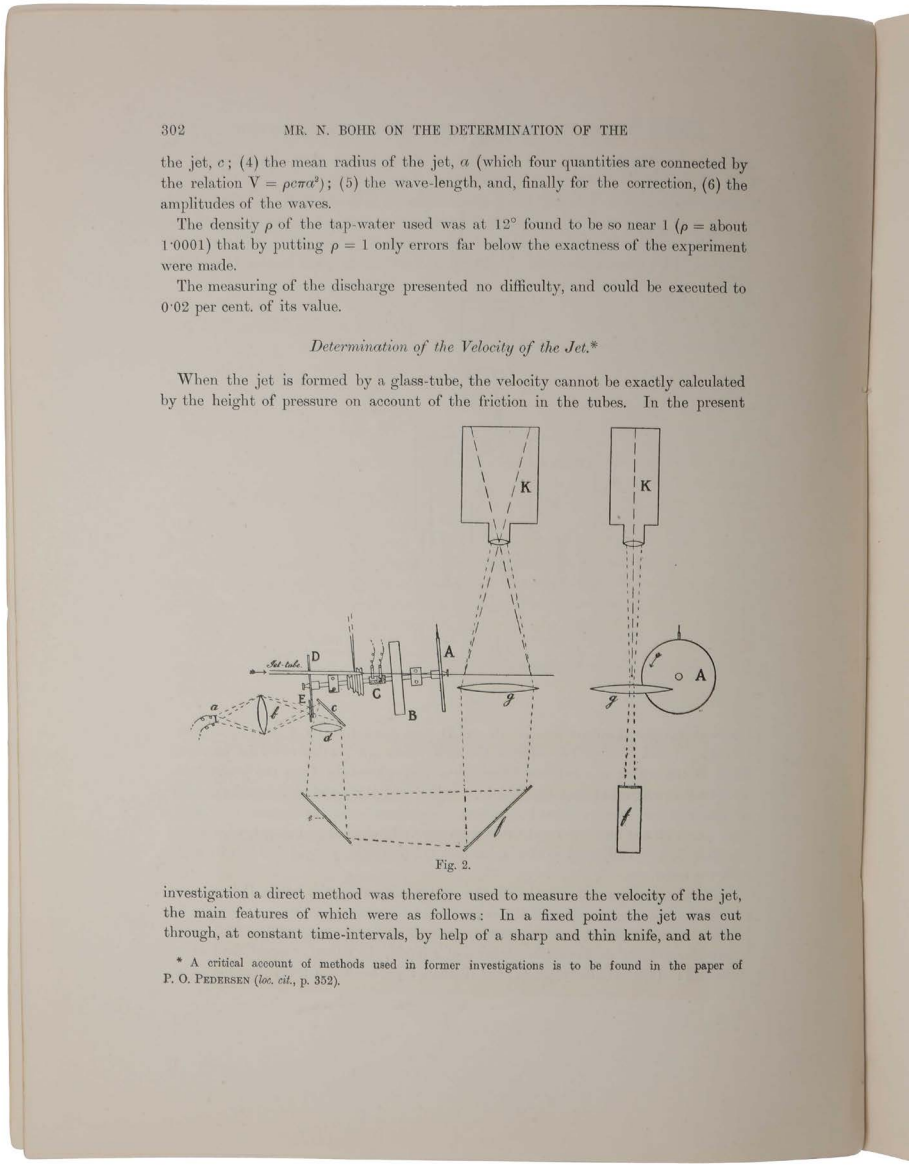
"Niels Bohr (1885-1962) entered the University of Copenhagen in the autumn of 1903 and immediately began the study of physics, with mathematics, astronomy and chemistry as secondary subjects … With few lectures to attend, he began already as a young student to carry out original research in physics. The field of his earliest work, surface tension and surface waves on liquids, was determined more by the interest of his teachers than by his own choice …

"In those days the Royal Danish Academy of Sciences and Letters awarded each year gold or silver medals for monographs on topics specified by the Academy two years previously. Among the prize problems announced in February 1905

was one for physics which read as follows: 'In the Proceedings of the Royal Society XXIX, 1879, Lord Rayleigh has developed the theory of the vibrations which a liquid jet carries out about the cylindric shape when it is somehow made to take another cross-section. From the theory, as well as from the experiments, which Lord Rayleigh has performed on these vibrations, it appears that they could serve to determine the surface tension of a liquid. The Academy, therefore, offers its gold medal for a more detailed investigation of the vibrations of liquid jets with special reference to the application mentioned. It is desired that the investigation be extended to a fairly large number of liquids. The results are to be compared with those previously found in other ways.'

"Solutions were to be submitted anonymously by October 30, 1906. Although most winners of these prizes had been mature scholars, the 19-year-old Niels Bohr decided to try his hand on the problem. He carried out the experimental work in the physiological laboratory of the University of Copenhagen, of which his father, Professor Christian Bohr, was the director. His father had to put pressure on him to terminate the experiments and cease making new and time-consuming corrections to the theory. The paper was written at his grandmother's estate, Naerumgaard, a few miles north of Copenhagen. The fair copy was handwritten by Niels Bohr's two years younger brother, Harald. Consisting of 114 pages and 19 figures, it was submitted on the very day of the deadline …

"On November 2, 1906, Bohr submitted an eleven-page addendum with the following note: 'It is respectfully requested that the enclosed addendum, which, owing to an accident in the copying, was not submitted with the main essay, may be appended to the paper with the motto βγδ which was submitted as physics prize essay.' That there was difficulty in meeting the deadline is indicated by the fact that this addendum was handwritten in part by Harald, in part by Niels himself, and in part by their mother …



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the jet, c; (4) the mean radius of the jet, a (which four quantities are connected by the relation $V = \rho c \pi a^2$); (5) the wave-length, and, finally for the correction, (6) the amplitudes of the waves.

The density ρ of the tap-water used was at 12° found to be so near 1 (ρ = about 1·0001) that by putting $\rho = 1$ only errors far below the exactness of the experiment were made.

The measuring of the discharge presented no difficulty, and could be executed to 0·02 per cent. of its value.

*Determination of the Velocity of the Jet.**

When the jet is formed by a glass-tube, the velocity cannot be exactly calculated by the height of pressure on account of the friction in the tubes. In the present

Fig. 2.

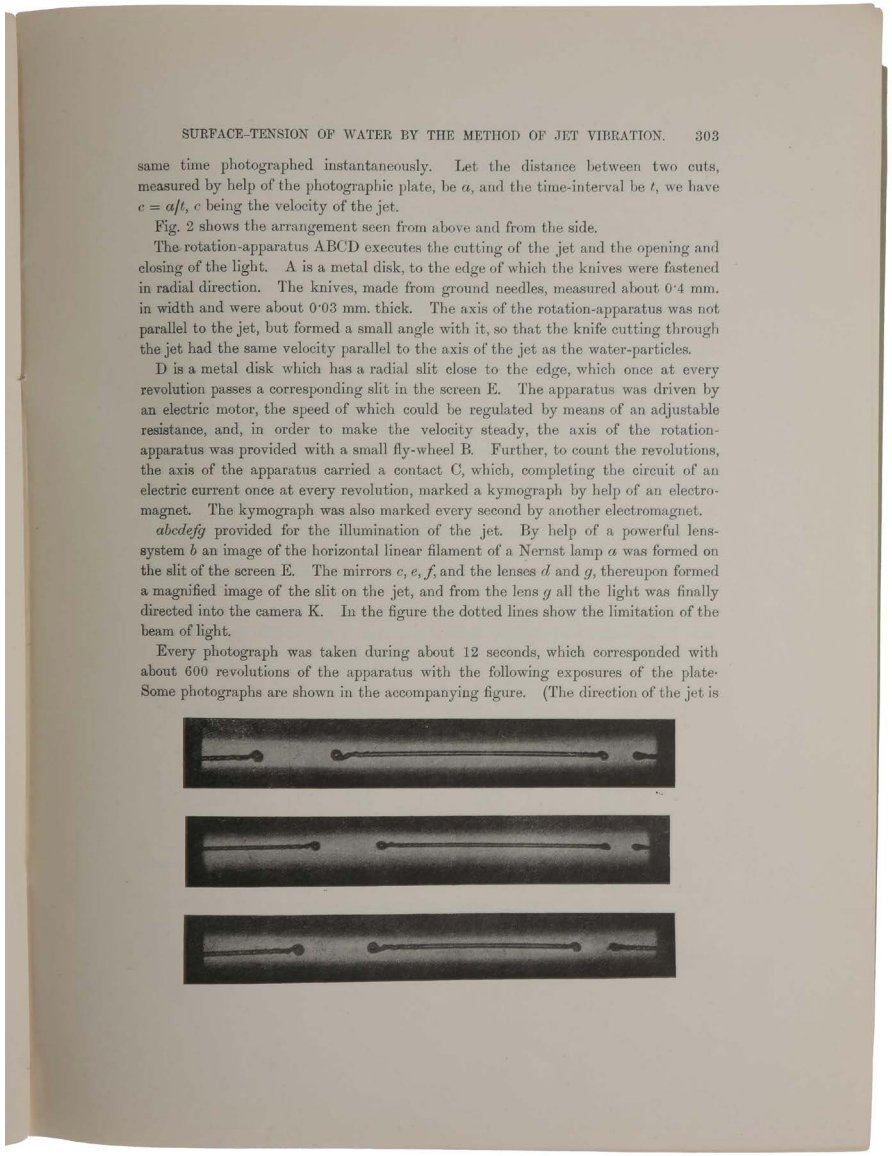
investigation a direct method was therefore used to measure the velocity of the jet, the main features of which were as follows: In a fixed point the jet was cut through, at constant time-intervals, by help of a sharp and thin knife, and at the

* A critical account of methods used in former investigations is to be found in the paper of P. O. Pedersen (*loc. cit.*, p. 352).

"The physics prize essays were judged by Professors C. Christiansen and K. Prytz. Their report to the Royal Academy reads: 'In answer to the Physics Prize Problem set by the Society for 1905, in which a detailed investigation of the vibrations of liquid jets was requested, two essays have been submitted … The author of the other essay, who designated himself by the mark βγδ [i.e., Bohr], has only managed to investigate the surface tension of water, on account of the experimental arrangement used. On the other hand, he has carried out a very extensive investigation of the conditions in the water jet. To produce a sufficiently long, regular, undivided and untwisted jet, the author let the water flow out through a long narrow glass tube whose orifice was made elliptic to bring about the vibratory motion. The jet was examined at a distance of about 25cm from the orifice to permit the viscosity to smooth out regularities in the motion. By selecting from a large number of prepared tubes, a few were found which gave the jet a symmetrical shape with respect to two mutually perpendicular planes through the axis. This symmetry was tested by an optical method which was also used to measure the wavelength. The amplitude of the vibration was found by measurement on an enlarged photograph of the jet.

"To determine the constant of capillarity, the author measured the amount of water flowing out in a given time, the velocity of the jet, and the wavelength. The velocity was found by a clever method which consists in cutting through the jet at a given place at two instants separated by a short time interval and measuring the length of the segment cut out and the corresponding time (ca. 1/50 s). The length of the segment cut out was found by photographing the cut jet by instantaneous illumination. The method gave very good results which could be checked by varying the time interval.

"To measure the wavelength, the aforementioned optical method was applied.



SURFACE-TENSION OF WATER BY THE METHOD OF JET VIBRATION. 303

same time photographed instantaneously. Let the distance between two cuts, measured by help of the photographic plate, be a, and the time-interval be t, we have $c = a/t$, c being the velocity of the jet.

Fig. 2 shows the arrangement seen from above and from the side.

The rotation-apparatus ABCD executes the cutting of the jet and the opening and closing of the light. A is a metal disk, to the edge of which the knives were fastened in radial direction. The knives, made from ground needles, measured about 0·4 mm. in width and were about 0·03 mm. thick. The axis of the rotation-apparatus was not parallel to the jet, but formed a small angle with it, so that the knife cutting through the jet had the same velocity parallel to the axis of the jet as the water-particles.

D is a metal disk which has a radial slit close to the edge, which once at every revolution passes a corresponding slit in the screen E. The apparatus was driven by an electric motor, the speed of which could be regulated by means of an adjustable resistance, and, in order to make the velocity steady, the axis of the rotation-apparatus was provided with a small fly-wheel B. Further, to count the revolutions, the axis of the apparatus carried a contact C, which, completing the circuit of an electric current once at every revolution, marked a kymograph by help of an electro-magnet. The kymograph was also marked every second by another electromagnet.

abcdefg provided for the illumination of the jet. By help of a powerful lens-system b an image of the horizontal linear filament of a Nernst lamp a was formed on the slit of the screen E. The mirrors c, e, f, and the lenses d and g, thereupon formed a magnified image of the slit on the jet, and from the lens g all the light was finally directed into the camera K. In the figure the dotted lines show the limitation of the beam of light.

Every photograph was taken during about 12 seconds, which corresponded with about 600 revolutions of the apparatus with the following exposures of the plate. Some photographs are shown in the accompanying figure. (The direction of the jet is

It consists in reflecting a light source at the surface of the jet and finding the positions on the jet where the tangent planes are parallel to the axis.

"The performance of a single determination by the author's method requires continued work through many hours. Hence, the jet must be maintained for a long time and under very constant conditions. This long time limits the applicability of the method in the case of liquids which change under exposure to air, and requires a comparatively large amount of liquid.

"When the problem was formulated, it was thought that the theory given by Lord Rayleigh should form the basis for the investigation. However, this theory gives only the first approximation. The author of this essay has remedied this by extending the theory so as to take into account viscosity and amplitudes that are not infinitely small. It is obvious that these investigations are of great interest for judging the value of the method and finding under what conditions it may be expected to yield the best results.

"This work does not solve the problem as completely as the former, in that it has studied only a single liquid, water. On the other hand, its author has earned so great merit by carrying the solution further at other points that we feel we must recommend that also this essay be awarded the Society's gold medal …

"After receiving the gold medal, Bohr carried out additional measurements of the surface tension of water. At the same time he was occupied with the considerable task of preparing his work for publication. In the latter part of 1908 he submitted a paper entitled 'Determination of the Surface-Tension of Water by the Method of Jet Vibration' to the Royal Society of London. This paper is not a simple translation of the prize essay but deviated from the latter at a number of points …

"On June 9, 1909 he sent a reprint of the paper to his brother Harald, then in Gōttingen. The paper appeared in the Philosophical Transactions of the Royal Society. Of all the papers published by Bohr, this is unique, not only in being his earliest publication, but also in being the only paper in which he reports experimental work carried out by himself.

"The addendum submitted three days after the main part of the prize essay was an investigation of the influence of the finite amplitude upon waves on the surface of deep water, progressing without change in shape under the influence of gravity and surface tension" (Collected Works, Vol. 1, pp. 3-11). It was first published in Bohr's *Collected Works* (in English translation).

The other entry for the prize, which also won the gold medal, was by Pio Pedersen.

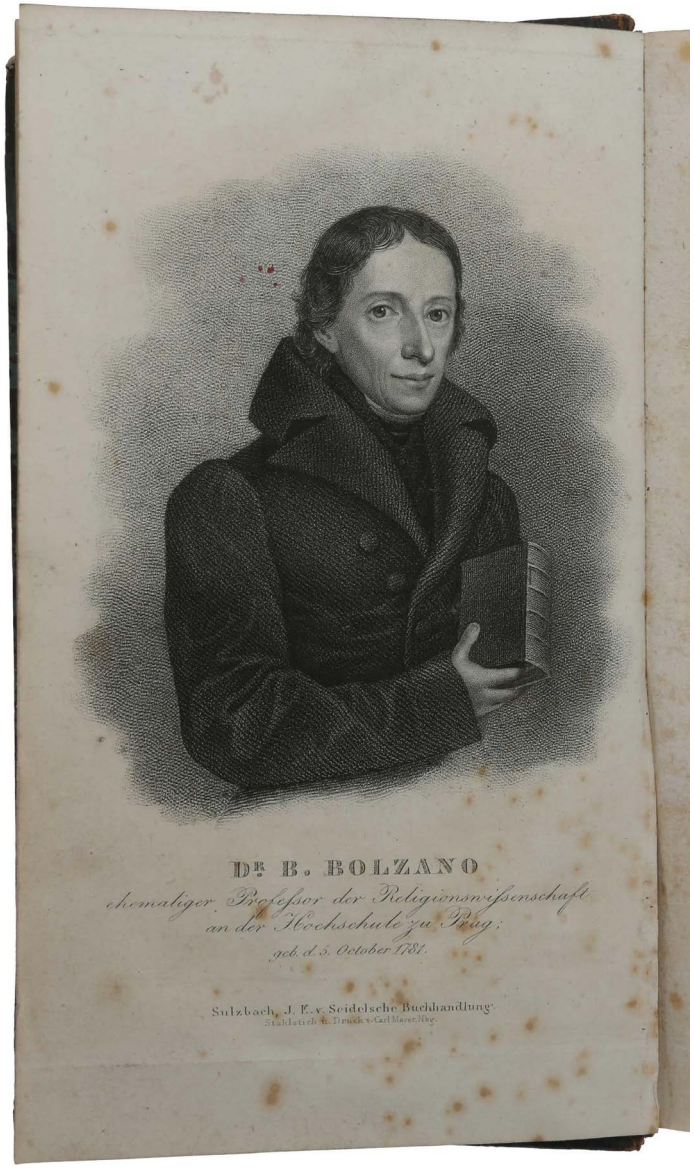
A LANDMARK WORK IN THE HISTORY OF CALCULUS

BOLZANO, Bernard. *Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege.* [Bound with two other works by Bolzano, his doctoral thesis and his autobiography, see below]. Prague: Gottlieb Haase, 1817.

$35,000

Three works bound in one vol., 8vo (188 x 108 mm), [Rein analytischer Beweis:] pp. 60; [Betrachtungen:] pp. [xvi], 63, [1], with one folding engraved plate; [Lebensbeschreibung:] pp. lvi, 272 with engraved portrait frontispiece. Contemporary half-roan and marbled boards, paper label on spine with manuscript title, two paper labels on covers (light edge-wear). Old Viennese library stamps on titles and elsewhere with their release on front free endpaper (some light browning and scattered foxing).

First edition, extremely rare, of this epoch-making paper in the history of mathematics, the first to provide a rigorous foundation for the calculus. "The main mathematical achievements of the paper include: (a) the formal definition of the continuity of a function of one real variable, correctly understood and applied (Preface); (b) the criterion for the (pointwise) convergence of an infinite series, although the proof of its sufficiency, prior to any definition or construction of the real numbers, is inevitably inadequate (Sect. 7); (c) the original form of the Bolzano-Weierstrass theorem (Sect. 12); (d) an analytic proof of the intermediate value theorem, now sometimes called Bolzano's theorem (Sect. 15). The theorem in the title of the paper, where 'equation' is understood as 'polynomial equation



in one real variable,' is deduced in the final paragraph (Sect. 18) from result (d)" (Russ, p. 157). It is usual to attribute (a), (b) and (d) to Augustin-Louis Cauchy's *Cours d'analyse*, which Bolzano anticipated by four years (Cauchy's definition of continuity actually still involved infinitesimals), and (d) was rediscovered by Karl Weierstrass half a century later (in ignorance of Bolzano's work). Bound with this important work are his first published work, *Betrachtungen uber einige Gegenstande der Elementargeometrie* (Prague: Karl Barth, 1804), an attempt to axiomatise plane Euclidean geometry, and also extremely rare; and his autobiography, *Lebenschreibung des Dr. B. Bolzano* (Sulzbach: J. E. v. Seidels, 1836). "Around the turn of the nineteenth century, mathematicians in Europe were concerned with two major problems. The first was the status of Euclid's parallel postulate, and the second was the problem of providing a solid foundation for mathematical analysis, so as to remove the so-called scandal of the infinitesimals" – this remarkable volume contains Bolzano's responses to both of these great problems. We are aware of only one other copy of Rein analytischer Beweis having appeared on the market in the last 30 years; no copy is listed on ABPC/RBH. OCLC lists eight copies of each of *Rein analytischer Beweis* and *Betrachtungen*, but no copy of either in the US. No copies in auction records.

"Bolzano's writings mark a turning-point in research on the foundations of mathematics – a transition from the mathematical style of the eighteenth century to that of the nineteenth … Bolzano was the first mathematician explicitly to reject the traditional geometric and spatial approach to foundations, calling instead, on explicitly logical grounds, for a 'purely analytic' grounding of the calculus – that is, a grounding in arithmetic. He thus stands at the head of two intertwined movements in nineteenth-century mathematics: the arithmetization of mathematics, a project that was to be carried forward by Cauchy, Gauss, Abel, Riemann, Dirichlet, Weierstrass, Heine, Cantor, Dedekind, and others; and the search for logical foundations that was pursued by Frege, Peirce, Peano, Russell, Brouwer, Hilbert, and Weyl" (Ewald, p. 168). "Although Bolzano's proofs are incomplete, and although they are somewhat clumsily presented, this paper is a milestone in the history of real analysis. It was the first successful attempt to free the calculus from infinitesimals, and it is the starting point for the modern theory of the continuum; the precision of Bolzano's definitions and the rigour of his deductions mark a break with the mathematics of the past. The project of putting the theory of the real line on a solid, arithmetical foundation was to be carried forward, largely in ignorance of Bolzano's work, throughout the nineteenth century – most notably by Cauchy, Abel, Dirichlet, Weierstrass, Cantor, and Dedekind" (*ibid*., p. 226).

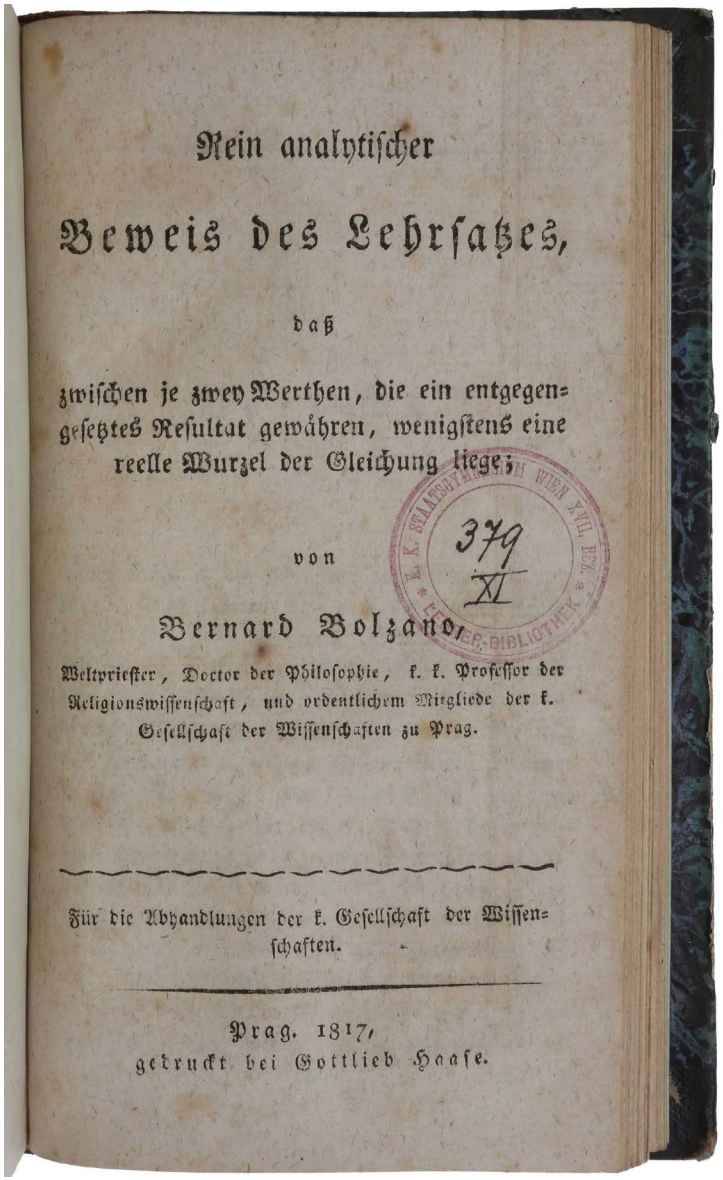
"Bolzano's most significant contribution to mathematics is his epoch-making paper on the foundations of real analysis, the *Rein analytischer Beweis* of 1817. In the years following the appearance of Berkeley's Analyst (1734), mathematicians had made various attempts to put the calculus on a firmer foundation. The most common approaches were to base the calculus on one of the following ideas: on motion (Newton, MacLaurin); on limits (D'Alembert, L'Huilier); on ratios of zeros (Euler); on infinitesimals (Leibniz and – with reservations – Carnot). In perhaps the most radical proposal, James (= Jacob/Jacques) Bernoulli proposed amending the laws of logic by abandoning, for infinitesimals, the Euclidean 'common notion' that, if equals are subtracted from equals, the remainders are equal. Perhaps the most influential approach was that of Lagrange, who, in his Théorie des fonctions analytiques (1797), assumed the existence of a Taylor-series expansion for every function; the full title of his work – Théorie des fonctions analytiques contenant les principes du calcul différentiel, dégagés de toute considération d'infiniment petits, d'évanouissans, de limites et de fluxions, et réduits a l'analyse algébrique des quantités finies – shows the scepticism with which he, and many other mathematicians, regarded the notion of limits, and his desire to reduce the calculus to 'l'analyse algébrique'. Bolzano's paper was not the first to attempt to find an analytic foundation for the calculus, nor was

it the first to employ the notion of limits. But, in contrast to his predecessors, Bolzano employed a limit-concept that was not based upon motion, and that was analytically defined and, more importantly, he was the first actually to use this definition to prove significant mathematical theorems.

"Bolzano's announced aim is to prove the intermediate value theorem – in his formulation, that if f and g are continuous functions such that $f(a) < g(a)$ while $f(b) > g(b)$, then for some x, $a < x < b$, $f(x) = g(x)$. This theorem he eventually proves in §15. But he begins with an important critique of previous proofs, and in Part II of the Preface he gives the first precise definition of a continuous function. His definition is essentially the same as that given by Cauchy in his *Cours d'analyse* in 1821; whether Cauchy knew of Bolzano's work is uncertain. (Bolzano improved on his definition in his unpublished *Functionenlehre*, written in 1834; there he gives a definition of pointwise continuity and distinguishes between left and right continuity.)

"In §7, Bolzano states and attempts to prove the sufficiency of the 'Cauchy condition' for the convergence of an infinite series. A rigorous proof requires a precise definition of the real numbers, which Bolzano did not possess; he himself (in the *Functionenlehre*) later admitted that the §7 proof was incomplete.

"Similarly, although Bolzano had a precise definition of continuity, he did not have the modern notion and definition of function. Lagrange, in the Théorie, had indeed defined a function of one or several quantities to be 'any mathematical expression in which those quantities appear in any manner, linked or not with some other quantities that are regarded as having given and constant values, whereas the quantities of the function may take all possible values'; but in practice he and his successors treated functions as equations. The modern conception did not enter mathematics until Dirichlet's paper, *Über die Darstellung ganz*



Rein analytischer

Beweis des Lehrsatzes,

daß

zwischen je zwey Werthen, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege;

von

Bernard Bolzano,

Weltpriester, Doctor der Philosophie, k. k. Professor der Religionswissenschaft, und ordentlichem Mitgliede der k. Gesellschaft der Wissenschaften zu Prag.

Für die Abhandlungen der k. Gesellschaft der Wissenschaften.

Prag. 1817,
gedruckt bei Gottlieb Haase.

willkurlicher Functionen, in 1837.

"Having defined continuity and stated the Cauchy condition, Bolzano proceeds (§12) to prove a lemma that was eventually to become the cornerstone of the theory of real numbers. This lemma (the greatest lower bound principle) is the first published version of the Bolzano-Weierstrass theorem, which, in modern terminology, says that every bounded infinite point-set has an accumulation point …

"It is important to appreciate the role Bolzano's objective conception of axioms, described above in the Beiträge zu einer begründeteren *Darstellung der Mathematik erste Lieferung*, 1810), played in the *Rein analytischer Beweis*. Bolzano was not driven by scepticism or by fear of paradox. He makes it clear that he did not doubt the truth of the intermediate value theorem; and he was not attempting to place it on firmer or more obvious foundations – for the greatest lower bound principle is, if anything, less evident than the theorem he is trying to prove. Similarly, his criticism of the proofs based upon motion is not that intuitions of motion are unreliable, but the logical objection that the proofs beg the question. Bolzano's ambition was not so much to attain some superior brand of mathematical certainty as to reveal the objective reasons for the truth of the intermediate value theorem – to uncover its true logical foundations. And a fortiori Bolzano's quest for rigour in his [*Rein analytischer Beweis*] was not prompted by the 'challenge to geometric intuition' presented by the discovery of continuous nowhere-differentiable functions; on the contrary, it was Bolzano's rigour that made his subsequent discovery [in the *Functionenlehre*] of such counter-intuitive phenomena possible" (*ibid*., pp. 225-7).

"The seeds of Bolzano's distinctive approach to mathematics seem to have been planted early. As a student at the University of Prague he studied Abraham Gotthelf Kästner's *Anfangsgründe der Arithmetik* (1758); and in a revealing passage in his autobiography Bolzano praised Kästner because 'he proved what is generally passed over because everyone already knows it, i.e. he sought to make the reader clearly aware of the basis (Grund) on which his judgements rest. That was what I liked most of all. My special pleasure in mathematics rested therefore particularly on its purely speculative parts, in other words, I prized only that part of mathematics which was at the same time philosophy' (*Lebensbeschreibung*, p. 64). 'Proving what everyone already knows' and 'prizing that part of mathematics which was at the same time philosophy' are precisely the traits that were to be characteristic of Bolzano's own mathematical work; but he was to pursue both far more deeply than anything in Kästner.

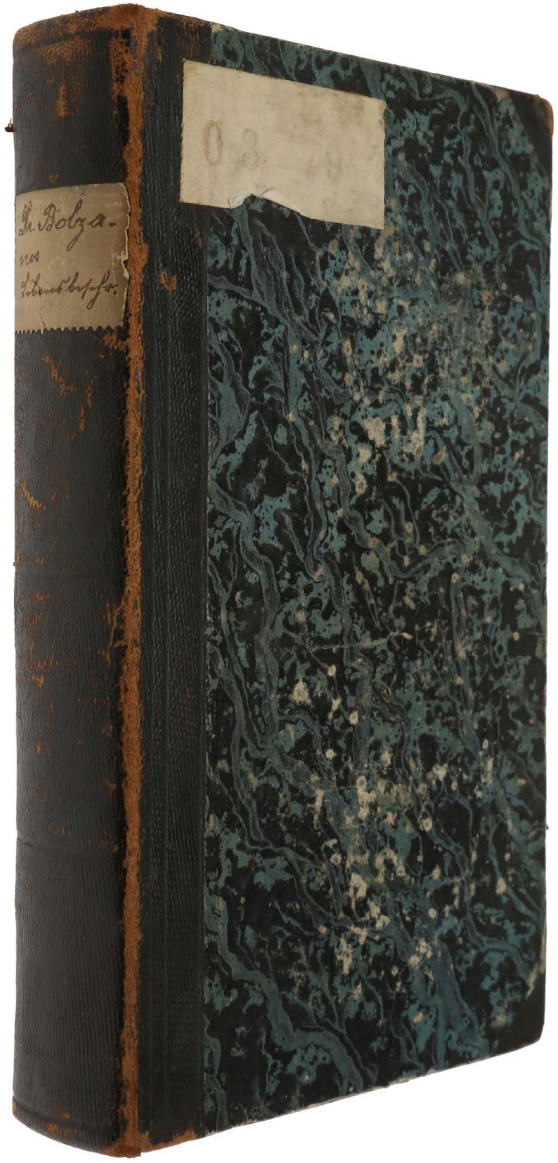
"Bolzano's new and more rigorous approach to axiomatics can already be detected in his earliest mathematical writings … Bolzano's predecessors, in contrast, had taken a more relaxed approach. In their view, the essential requirement for an axiom was that it be certain – an immediate and obvious truth on which the calculus of fluxions or Euclidean geometry could be founded. [The *Betrachtungen*] gives three reasons for rejecting this conception of axioms and for pursuing even obvious truths 'down to their ultimate grounds': such a procedure will be conducive to thoroughness, to the ease of learning the subject, and to the discovery of new theorems...

"Bolzano's conception of logical methodology led him to deepen his studies in the foundations of mathematics, and yielded him a rich harvest of theorems. The process begins in §§4-6 of the Preface to his [*Betrachtungen*], where Bolzano applies his general methodological principles to the particular case of the foundations of geometry. He criticizes earlier mathematicians for importing conceptions from the theory of motion into their discussions of geometry, pointing out that the theory of space is logically antecedent to the theory of the movement of objects

in space, and must therefore be developed without recourse to the latter theory. Bolzano explicitly criticizes Kant, Mercator, and Kästner on this point; but his remarks can equally well be read as a response to MacLaurin and Newton" (*ibid*., pp. 168-171).

"Bolzano's approach to mathematical problems was characterized by his ability to find new, non-traditional methods, and to use them to deal with problems that until then had withstood all attempts at solution. This approach manifests itself in geometry as well. Bolzano's first mathematical treatise 'Betrachtungen …' was aimed at the solution of the then popular problem of parallels. It is not essential that Bolzano solved the problem via a very general concept of similarity, but rather that already in this work he subjected to criticism the contemporary (mostly traditionally Euclidean) interpretation of elementary geometry. In Part 2 of his treatise he tried to define the straight line and the plane, starting from and studying the properties of the simplest geometric object, a pair of points. Thus he defined the notion of the direction of a pair of points, its distance, and in essence constructed geometrically the vector space, indicating also its three-dimensional analogue. In this way he arrived at a result analogous to that obtained in 1799 by C. Wessel in his geometrical interpretation of complex numbers ['Om Directionens analytiske Betegning'], or later (1844) H. Grassmann in a much more general setting ['*Die Ausdehnungslehre*'] (Folta, p. 25).

"Bolzano's philosophical methodology thus led him to introduce powerful new concepts and techniques and conjectures into mathematics. This is an important aspect of his work, and sets him apart from a thinker like [Johann Heinrich] Lambert, whose methodological observations on the Axiom of Parallels (Theorie der Parallellinien, 1786) were as shrewd as anything in Bolzano, but who was unable to put them to any actual use in the proving of new theorems. Indeed, the history of mathematics is strewn with similar examples of unexploited



anticipations of great advances- recall, for example, Kant's observations on the possibility of alternative geometries, or D'Alembert's discussion of the concept of a limit, or Leibniz's dream of a mathematical logic, or Lambert's remarks on formal axiom systems. Such insights, unless they can be shown to perform some actual mathematical work, tend to be sterile, and are only noticed years later when somebody else has demonstrated their significance. Bolzano managed both to have a crucial insight, and to show how to develop it into new branches of mathematics; unfortunately this accomplishment was no guarantee against being ignored, and the circumstances of his life kept his work from becoming widely known" (Ewald, p. 171).

"Bolzano was born in Prague, the youngest son of an Italian father (an art dealer) and a German mother. He entered the University of Prague in 1796, where he was educated in philosophy, mathematics, and physics. In philosophy, he read the *Metaphysica* (1739) of the Wolffian philosopher, Alexander Gottlieb Baumgarten; in mathematics, he was particularly influenced by his close study of Eudoxus, Euler, and Lagrange, as well as of Kästner's *Anfangsgründe der Arithmetik* (1758). In 1800 Bolzano took up the study of theology; he was called to the new chair of religion at the University of Prague in 1805. The chair had been established by the Emperor Franz I of Austria to shore up the position of the conservative Catholic hierarchy against the tide of freethinking and republicanism that had been rising in Central Europe since the French Revolution. From the point of view of the political and religious authorities, the appointment of Bolzano was not a happy choice. Although his appointment was confirmed in 1807, his own social, ethical, and religious sympathies inclined to the cause of Enlightenment, and he found himself in perpetual trouble with the authorities. (Among the doctrines that caused him difficulty was his publicly-expressed conviction that one day men would live without kings.) Bolzano was a popular lecturer, and in 1818 was elected head of the philosophy faculty; nevertheless, in 1819 he was dismissed from his professorship, forbidden to publish, and placed under police supervision. For the remaining decades of his life he lived in the countryside, writing on ethics, religion, politics, logic, and the foundations of science.

"Despite the clarity of his arguments, the power of his theorems, and the fruitfulness of his techniques, and although his *Paradoxien des Unendlichen* (1851) was known and admired by Peirce, Cantor, and Dedekind, Bolzano's work in real analysis – the work of an obscure theologian, most of it published by equally obscure Bohemian publishers – seems to have remained entirely unnoticed until Otto Stolz called attention to it in 1881. But by this time Bolzano's most important results had been independently discovered by Weierstrass and his school" (ibid., pp. 171-2).

Parkinson, *Breakthroughs*, p. 265 (*Rein analytischer Beweis*). Ewald, *From Kant to Hilbert* (1996); Folta, 'Life and scientific endeavor of Bernard Bolzano,' pp. 11-31 in *Bolzano and the Foundations of Mathematical Analysis*, Jarnik et al (eds.) (1981); Russ, 'A translation of Bolzano's paper on the intermediate value theorem,' *Historia Mathematica* 7 (1980), 156-185).

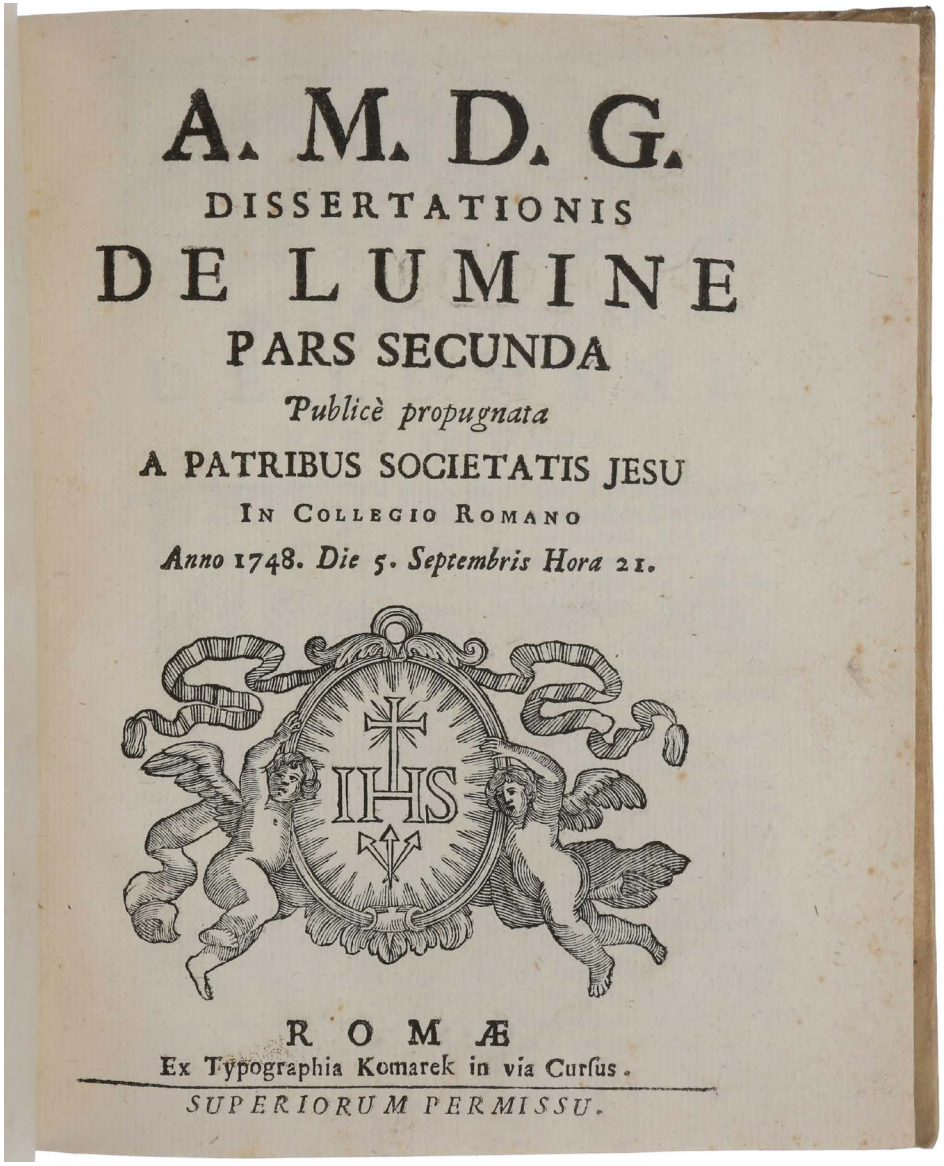
PRECURSOR TO HIS *THEORIA PHILOSOPHIAE NATURALIS* (1758)

BOSCOVICH, Rudjer Josip. *Dissertationis de lumine pars prima [– pars secunda].* Rome: A. de Rubeis (part 1) and Typographia Komarek (part 2), 1748.

$12,500

Two parts in one vol., 4to (208 x 172 mm), pp. 44; [ii], 58, with two folding engraved plates. Contemporary vellum, red speckled edges, later end-papers.

First edition, extremely rare, of Boscovich's major treatise on optics which also contains (in its second part) the first account of the essential elements of his theory of the structure of matter, elaborated a decade later in his epoch-making *Philosophiae naturalis theoria redacta ad unicam legem virium in natura existentium* (1758). "The 'Theory of Natural Philosophy' is now recognized as having exerted a fundamental influence on modern mathematical physics... As the title of his book implies, he considered that a single law was the basis of all natural phenomena and of the properties of matter; that the multiplicity of physical forces was only apparent and due to inadequate mathematical knowledge" (PMM). In 1958, Werner Heisenberg wrote: "Boscovich's work contains numerous ideas that have found their deserved place only in the modern physics of the past 50 years." "The fundamental features of his theory on indivisible points and the unique law of forces were published by Boscovich in 1745 in a dissertation entitled *De viribus vivis*, then in 1748 in a more comprehensive and elaborate form in the second part of the dissertation *De lumine*. On September 24, 1748, he wrote: 'The second dissertation expounds from its beginning the whole of my theory



A. M. D. G.

DISSERTATIONIS

DE LUMINE

PARS SECUNDA

Publicè propugnata

A PATRIBUS SOCIETATIS JESU

IN COLLEGIO ROMANO

Anno 1748. Die 5. Septembris Hora 21.

ROMÆ

Ex Typographia Komarek in via Curſus.

SUPERIORUM PERMISSU.

along its main lines. The big treatise will contain only a little more material related purely to physics, but there will be much more metaphysics, geometry and calculus'" (Marković, p. 129). "Finally, in the treatise *De Lumine*, Bošković formed a systematic explanation of his theory for the first time: (1) points of matter endowed with certain forces; (2) law of forces expressed by a continuous curve, whose graph is shown in Fig. 1; (3) application to the constant and permanent order of natural phenomena and to the structure of matter" (Martinović, p. 251). These principles allowed Boscovich to give in *De lumine* an explanation of the solidity of bodies, of changes of state, and even of the nature and properties of light. "Having reflected on the problems of light, Boscovich published in 1748 a treatise (in two parts) of a broadly critical nature. The central Newtonian positions in optics did not at all appear to him to be securely established. It is perhaps the most interesting feature of his critical attitude that he regarded rectilinear propagation as an unproved hypothesis, a question with which he dealt in detail. Some other aspects of optical phenomena he thought hidden or unclear even after Newton's discoveries. Discussing phenomena of parallax, he drew attention to the distance of fixed stars in dimensions of light years. He formulated, and was the first to do so, a general photometric law of illumination and enounced the law of emission of light known under Lambert's name. He was critical of Newton's account of colors arising from the passage of light through thin plates involving the ether and periodicity, and he provided an alternative interpretation in the spirit of his own theories of natural forces" (DSB). ABPC/RBH list only the Beltrame copy (in a modern binding) (Christie's, 30 November 2016, lot 296, £6000). OCLC lists five copies in US (Columbia, Linda Hall, Smithsonian, Madison (Wisconsin), and American Philosophical Society, the last being part 2 only).

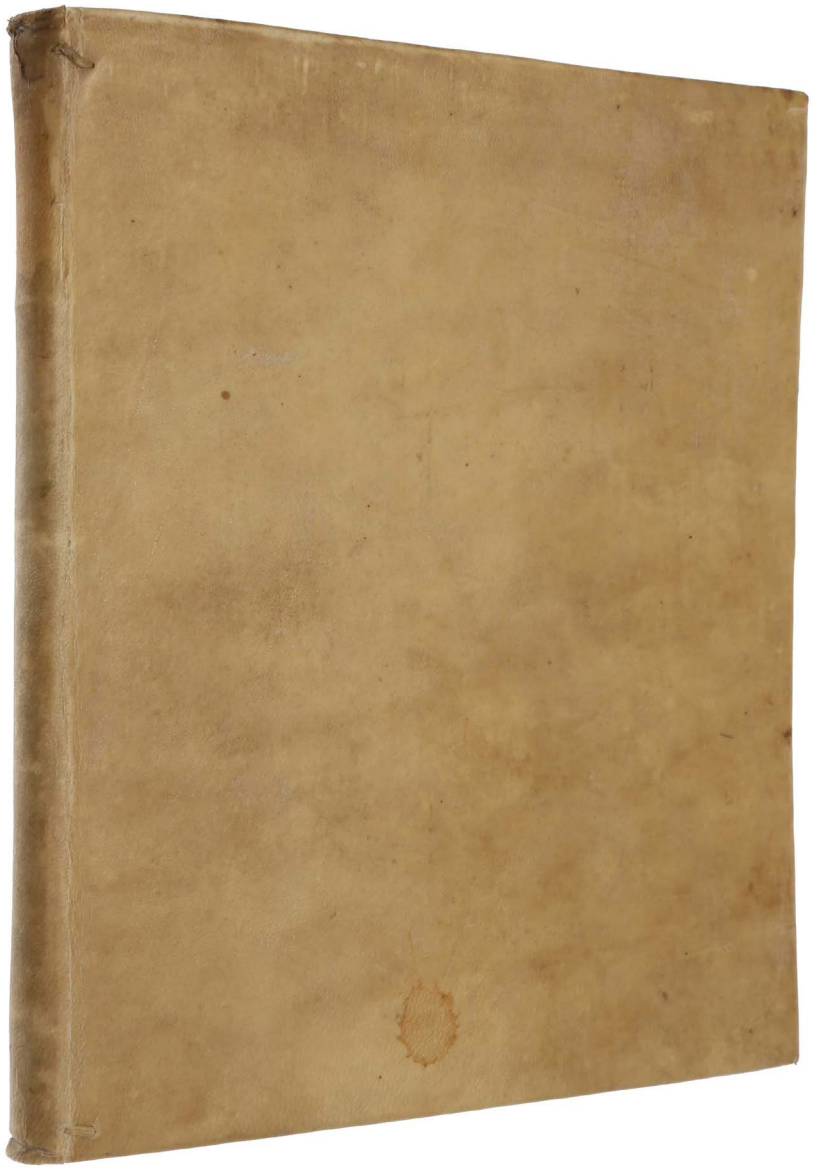
"Boscovich had been led to his atomic theory by the consideration of what appeared, at first sight, to be a very simple problem – the exact mechanism of atomic impact. Like most Newtonians, Boscovich had accepted the mechanical philosophy for the explanation of natural events. In this system, observable reality was the result of the motion, impact, and form of atoms. Atoms were like little billiard balls, only perfectly hard, elastic, and impenetrable. To most natural philosophers of the eighteenth century, the collision of two atoms was exactly analogous to the collision of two billiard balls. Boscovich realized that the situation was more complicated. When two billiard balls collide, the result is elastic deformation and recovery; the deformation is made possible by the fact that the molecules of the billiard balls can move relative to one another. It is the displacement of these molecules and their subsequent return to normal position that leads to elastic rebound. With atoms, such a process is clearly impossible. Since atoms have no parts and are perfectly hard, deformation is impossible. What happens, then, when two moving atoms meet? If they are little billiard balls, Boscovich argued, and if deformation is impossible, no time can elapse between contact and rebound. Thus, at the moment of impact, the atoms will have two velocities at the same time. They will have their original velocity and the velocity of rebound. To Boscovich this was logically and physically absurd yet it was a necessary consequence of the theory of material atoms. Boscovich's solution was a radical one; he eliminated matter completely as a separate entity and replaced it with attractive and repulsive forces. An atom was to be considered as a dimensionless mathematical point. Surrounding this point were forces which were alternately attractive and repulsive …

"Point atoms not only could explain phenomena as well as their Newtonian rivals, but in some instances, were notably more convenient. One of these was the problem of change of state. Here, again, was a seeming discontinuity in nature that the concept of billiard balls did little to explain. Why, when heat was added to a substance, did it suddenly change its physical form at a specific temperature? If, as Joseph Black, the discoverer of latent heat, suggested, the change from ice to water, or water to steam was a simple chemical reaction between a ponderable base and caloric, then this reaction was unique for only the physical properties

of the base were altered. If change of state was to be explained upon the purely mechanical principles of the repulsion of caloric, it was difficult to see why the change was so sudden and dramatic. With point atoms, however, no difficulties appeared. There were certain stable points on the curve; when heat (in the form of atomic or molecular motion) was added to a body composed of these atoms, their oscillations would eventually push them over the 'hump' of repulsion to a new stable position at a great distance from one another. Thus, the specificity of melting- and boiling-points of substances followed directly from a consideration of the curve of forces" (Pearce Williams, pp. 73-5).

According to Boscovich's 'curve of forces', at large distances from the atomic point the force is attractive and given by the inverse-square law of gravitation. Close to the atomic point the force is repulsive, increasing to infinity as the point is approached, reflecting the impenetrability of matter and represented by a vertical asymptote in the curve. Between these extremes the curve alternates between attractive and repulsive forces. Heisenberg said of this model, emphasizing its anticipation of quantum field theory: "He understands matter as a space filled up with fields (forces) in which elementary particles would only represent singular points of the fields."

"In the second part of his treatise De lumine (1748), Boscovich concluded the shaping of his curve of forces with an essential development in relation to his original exposition in De viribus vivis (1745). That is to say that Boscovich made concrete the meaning of null points in his curve of forces when he interpreted the cohesion of matter" (Martinovic, p. 203). "Of a special importance are those special points in which the curve has a vertical asymptote or where it intersects the X-axis, that is, the points of infinity and the zero points [null points] of the law of forces" (Marković, p. 139).



The zero points are of two kinds. In the first kind the force changes from repulsive to attractive as the distance increases; this is a point of stable equilibrium because a small increase in separation moves the element into a region of attractive force which returns the element to the zero point, while a decrease in separation moves the element into a region of repulsive force which again returns the element to the zero point. Boscovich calls zero points of this kind limits of cohesion. In the second kind the force changes from attractive to repulsive as the distance increases; this is a point of unstable equilibrium. Boscovich calls null points of this kind limits of non-cohesion. These transition points between the repulsive and attractive forces are thus decisive for the explanation of the phenomenon of cohesion. "The existence and the application of these two kinds of limits represent the fundamental discovery made by Boscovich. The existence of these 'limits of cohesion' gave Boscovich, firstly, the key for the understanding of cohesion and solidity of bodies …

"The behavior of arcs belonging to the repulsive and attractive forces, respectively, have a bearing on the explanation of 'fermentation', evaporation, sudden deflagrations and explosions, and also of light emission. 'Fermentation' arises when the shape and the distribution of the repulsive and the attractive arcs of the curve are such that the particle is forced to oscillate rapidly within definite limits. In fact, Boscovich introduces, as well as the points in which the curve intersects the axis, X, also limits of another kind in which the transition from the repulsive force to the attractive, or vice versa, does not occur by cancellation of the force but by transition through infinity. Thus new asymptotic branches are introduced into the curve of forces in addition to that near the origin …

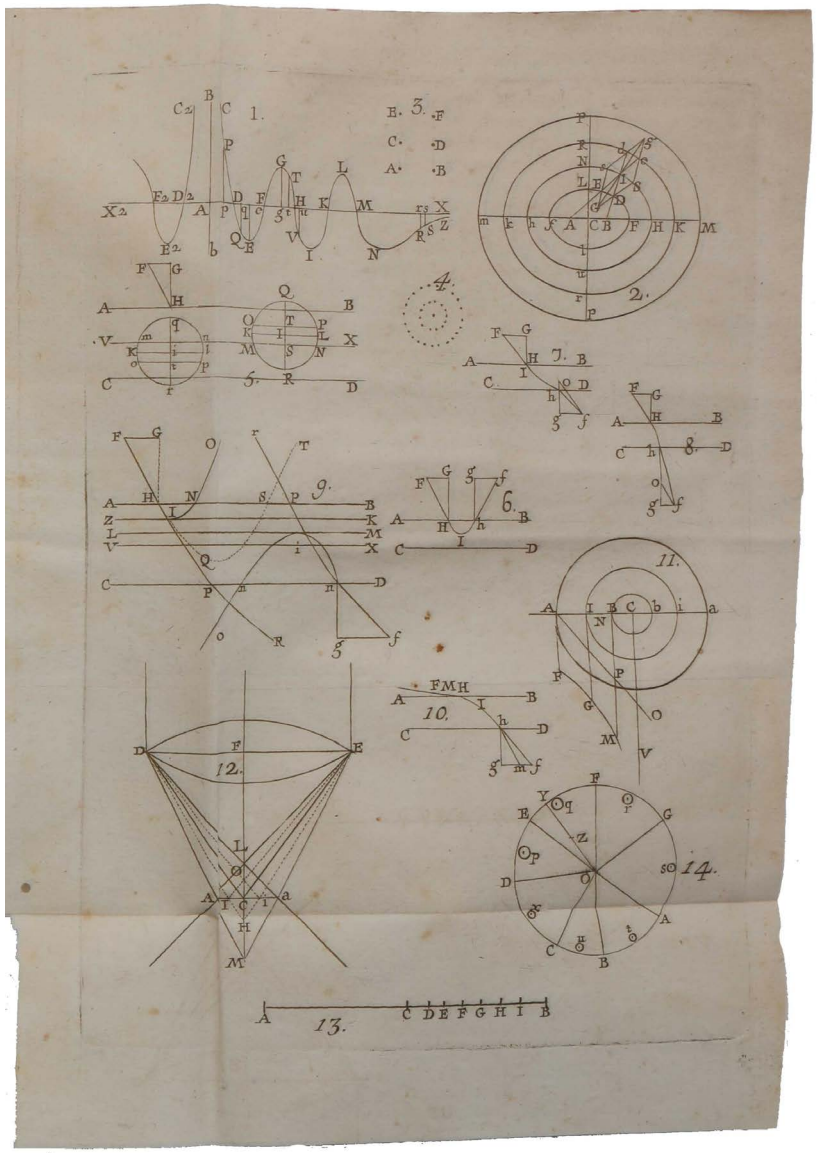
"So far we have dealt with interactions of two elements. The elements form first those tiny composite particles which are called by Boscovich 'of the first order'. An assembly of these forms the particles of the second and higher orders, which finally form a body … The variety of the law of forces is far greater when we consider a particle consisting of several elements and its action on an element outside it … A discussion of the case of three points induced Boscovich to ask himself: 'If the very simple case of three points yields such a harvest of theorems and problems, such a diversity of cases, what are we to think of the cases which appear when the free distribution of that enormous number of points is in question – the points of which this miraculous structure of the universe consists' [§32]" (*ibid*., pp. 139-40).

In this work "Boscovich was concerned with the question of the nature of light. Ever since mankind had begun to consider this problem there had been difficulties for by its very nature 'light' cannot be studied directly. It cannot be taken into a laboratory and weighed; in the eighteenth century it could not be subjected to the kind of experimental techniques which can now be applied, and for the formulation of a scientific theory on the character of light there was only the evidence of refraction, reflection, and the little understood phenomenon of dispersion which could be called upon. Moreover, the theory of the nature of light which Newton had proposed was, in the seventeenth century, considered to be the one satisfactory approach. Thus the scientific climate of Boscovich's day was favourable to developments along the lines of a corpuscular hypothesis. It must be remembered, however, that Newton's views on the nature of light were not exclusively 'corpuscular'. He had grappled with the problem of the partial reflection and refraction of light which occur at the surface of separation of transparent media and, as is well known, had provided an explanation in terms of 'fits' of 'easy' reflection and 'easy' refraction, the corpuscles being supposed to possess alternately 'fits' of each kind. However, Newton had gone further. He had invoked the aid of waves in the aether to give rise to the 'fits' of the corpuscles, but this side of his investigation had been forgotten.

"Boscovich tackled the difficult question anew, still, of course, making use of a corpuscular theory. It is clear that, whether or not he himself knew of or appreciated Newton's idea of aetheric waves to account for the 'fits', he was moved to find some other explanation. Boscovich therefore suggested that each corpuscle had 'magnetic' properties and, further, that these were different on different sides. Thus, he claimed, if the different polarities of each corpuscle were such that one side was attracted and one side repelled then, with the corpuscles being considered as rotating, they would either be attracted and so refracted through a medium, or replied and thus reflected.

"It is to be noted that Newton had made a similar proposal in connection with the phenomenon of double-refraction presented by Iceland Spar, and Boscovich may well have been aware of this. However, Boscovich's awareness or not of Newton's proposal does not lessen the credit due to him for his own theory. The idea of rotating corpuscles with sides of different polarity was an explanation based on a realistic approach to physical problems and of less esoteric a type than the theory of 'fits' put forward by Newton to account for the same phenomenon. Moreover if Boscovich knew of Newton's ideas about double-refraction – and it would seem likely that he did – he can be credited with using a similar explanation for yet another of the phenomena which light presented and so simplifying matters by bringing more observed effects under one physical explanation.

"In his own day Boscovich's theory was welcomed and developed. His proposal that solid matter was of a discontinuous nature, being made up of physical point atoms, with attractive and repulsive actions, was invoked by other physicists who used this theory, together with the idea of rotating corpuscles with different polarities on their sides, to suggest an explanation for how it was that light could penetrate solid bodies. Again, it was found that the puzzling phenomenon of phosphorescence could be explained by using these two theories because they



presented a means whereby a physical explanation on grounds of attractions of corpuscles could be used to account for the emission of light in darkness by a cold body. Finally Boscovich's theory was used to give a physical explanation of astronomical refraction. Ever since the days of Tycho Brahe (1546-1601) the existence of this phenomenon had been taken into account in astronomical computations. To allow for this effect tables of astronomical refraction had been drawn up, for example, by Brahe and by Kepler, but a theoretical explanation had not been available although it was recognized that as the density of the air increased nearer the Earth so the refraction of starlight for celestial objects closer to the horizon became more marked. Clearly Boscovich's corpuscular hypothesis and his atomic doctrine could, by using the concept of the attraction of light, provide an explanation" (Ronan, pp. 193-5).

Boscovich was born on 18 May 1711 in Dubrovnik, now in Croatia, then an independent city-state surrounded by the Ottoman Empire and the Venetian Republic. There is some ambiguity about how to write Boscovich's name. In English he is usually referred to as Roger Joseph Boscovich, in Italian as Ruggiero Giuseppe Boscovich, while in his native Croatian he is called Ruder Josip Bošković. His surname is derived from the word Boško, which in Croatian means "little God". "This Croatian physicist, mathematician, and astronomer is considered the last great polymath. Educated first at the Jesuit College in his native Dubrovnik, he entered the novitiate in Rome in 1725 and later attended the Collegium Romanum, beginning to teach mathematics there in 1740. A corresponding member of the Academy of Sciences at Paris, he travelled there in 1759. The following year he travelled to London, where he met Benjamin Franklin and was elected F.R.S.; he then travelled widely throughput Europe and Eastern Europe. On his return to Italy in 1763, he was elected to the chair of mathematics at Pavia, where he reorganized the department with an emphasis on applied mechanics, and also played a leading role in organizing the observatory at Brera. In 1772 his connection with the observatory was terminated, and, in response, he resigned his professorship; the Jesuit order was suppressed in the following year. At the urging of friends, he removed to Paris, but returned to Italy in 1782 to prepare the manuscripts for his Opera. His scientific interests were divided between astronomy, optics, geodesy, and mathematics, mechanics, and natural philosophy. As well as his religious and teaching duties, Boscovich undertook several practical and diplomatic commissions for both secular and ecclesiastical authorities" (Roberts & Trent, p. 43). Boscovich died of pneumonia on 13 February 1787 in Milan, and was quietly buried at Santa Maria Padone.

PMM 203 (for *Theoria*); Riccardi I, 136 (31); Sommervogel II 1834 36. Iltis, 'D'Alembert and the vis viva controversy,' pp. 135-144 in *Studies in History and Philosophy of Science*, Part A, 1 (1970); Kragh, *Higher Speculations: Grand Theories and Failed Revolutions in Physics and Cosmology*, 2011; Marković, 'Boscovich's Theoria,' pp. 127-152 in Whyte (see below); Martinović, 'Theories and inter-theory relations in Bošković,' pp. 247-262 in *International studies in the philosophy of science* 4 (1990); Pearce-Williams, Michael Faraday, 1965; Ronan, 'Boscovich's Optics and Design of Instruments,' pp. 193-199 in Whyte; Whyte (ed.), Roger Joseph Boscovich, S.J., F.R.S., 1711-1787: *Studies of His Life and Work on the 250th Anniversary of His Birth*, 1961.

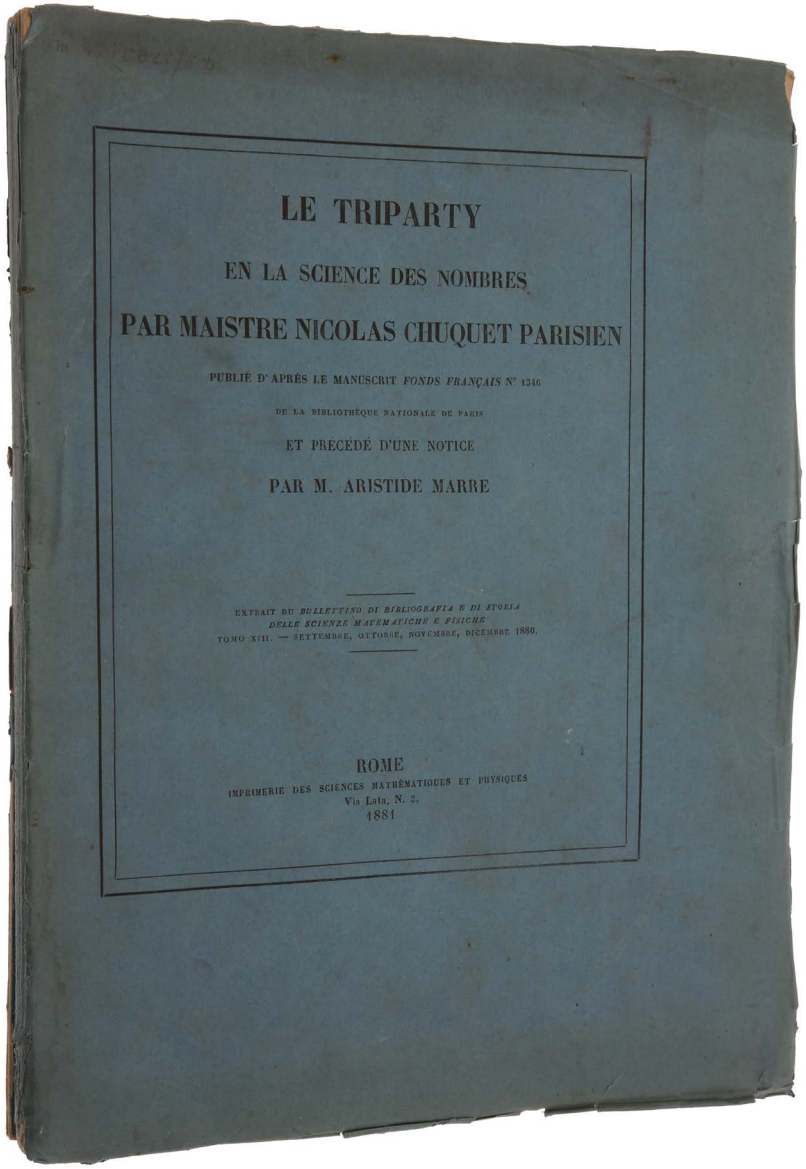
THE MOST ORIGINAL MATHEMATICAL WORK OF THE FIFTEENTH CENTURY

CHUQUET, Nicolas. *Le Triparty en la Science des Nombres. Par Maistre Nicolas Chuquet Parisien, publié d'après le manuscript fonds français n°1346 de la Bibliothèque nationale de Paris et précédé d'une Notice par M. Aristide Marre.* [Offered with:] *Problèmes numériques faisant suite et servant d'application au Triparty en la Science des nombres de Nicolas Chuquet, Parisien. Extrait de la seconde partie du ms. n°1346 du fonds français de la Bibliothèque nationale, annoté et publié par Aristide Marre..* Rome: Imprimérie des Sciences Mathématiques, 1881; 1882.

$2,800

Large 4to. [Le Triparty:] (310 x 228 mm), pp. 229; [Problèmes numériques:] (310 x 228 mm) [3], 4-50. Original printed wrappers, edges with some chipping and wear, spine strips worn.

First edition, the rare offprint issues, of the most original mathematical work of the fifteenth century, indeed the most important since Fibonacci's Liber Abaci almost three centuries earlier (a work which was also not published until the nineteenth century). Composed in 1484, but published here for the first time, this first French work on algebra introduced several groundbreaking innovations into European mathematics, notably the use of exponents to denote powers of a number and the use of negative numbers in the solution of equations. The first part of Le Triparty concerns the arithmetic operations on numbers, including an explanation of the Hindu-Arabic numerals. Chuquet gave a 'règle des nombres moyens' according to which a fraction could be found between any two given fractions by taking the sum of their numerators and dividing by the sum of their denominators. This rule


LE TRIPARTY

EN LA SCIENCE DES NOMBRES

PAR MAISTRE NICOLAS CHUQUET PARISIEN

PUBLIÉ D'APRÈS LE MANUSCRIT *FONDS FRANÇAIS* N° 1346

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ROME
IMPRIMERIE DES SCIENCES MATHÉMATIQUES ET PHYSIQUES
Via Lata, N. 2.
1881

could be used to find the solution of any problem soluble in rational numbers, once an upper and a lower bound for the solution had been found. The last, and most important part, concerns the 'règle des premiers', which is nothing less than what we would call 'algebra', 'premier' being Chuquet's name for the unknown. He also had specific names for the square, cube and fourth power of the unknown, but for higher powers he invented an exponential notation of great significance. In particular, it laid bare the laws of exponents, which played a crucial role in the subsequent invention of logarithms. Also in this part, Chuquet studied the solution of equations, making use for the first time of isolated negative numbers (and on one occasion of the number zero). Problèmes numériques contains Chuquet's statement and answers to 156 problems solved using the methods of Le Triparty. "Many of these problems have a long history going back at least as far as the Greek Anthology reputedly collected by Metrodorus some one thousand years earlier. In Chuquet they are solved by various methods, including the 'rule of three' and the 'rule of one and two positions', but the significant part of the section on problems is where Chuquet applies his 'rule of first terms,' that is, his algebra" (Flegg et al, p. 25). These works are offprints from the Bullettino di bibliografa e di storia delle scienze matematiche e fisiche, tomo XIII & XIV. OCLC lists Harvard only in US for Le Triparty, no copies of the Problèmes numériques. ABPC/RBH lists one copy of Le Triparty (Bloomsbury Auctions, 18 July 2014, lot 445, £1240) and none of Problèmes numériques.

Provenance: Problèmes numériques signed and inscribed by Marre on upper wrapper to 'Monsieur Ferdinand Denis.' Jean-Ferdinand Denis (1798-1890), a historian specializing in Brazil, was director of the Bibliothèque Sainte-Geneviève (Paris) from 1865 to 1885.

"The 'Triparty' is a treatise on algebra, although the word appears nowhere in the manuscript. This algebra deals only with numbers, but in a very broad sense of the term. The first part concerns rational numbers. Chuquet's originality in his rules for decimal numeration, both spoken and written, is immediately obvious. He introduced the practice of division into groups of six figures and used, besides the already familiar million, the words billion (1012), trillion (1018), quadrillion (1024), etc. …

"Chuquet's study of the rules of three and of simple and double false position, clear but commonplace, served as pretext for a collection of remarkable linear problems, expounded in a chapter entitled 'Seconde partie d'une position.' Here he did not reveal his methods but reserved their exposition for a later part of the work, where he then said that after having solved a problem by the usual methods — double position or algebra (his 'regle des premiers') — one must vary the known numerical quantities and carefully analyze the sequence of computations in order to extract a canon (formula). This analysis generally led him to a correct formula, although at times he was mistaken and gave methods applicable only for particular values.

"Another original concept occurred in this group of problems. In a problem with five unknowns, Chuquet concluded: 'I find 30, 20, 10, 0, and minus 10, which are the five numbers I wished to have.' He then pointed out that zero added to or subtracted from a number does not change the number and reviewed the rules for addition and subtraction of negative numbers. In the thirteenth century Leonardo Fibonacci had made a similar statement but had not carried it as far as Chuquet in the remainder of his work.

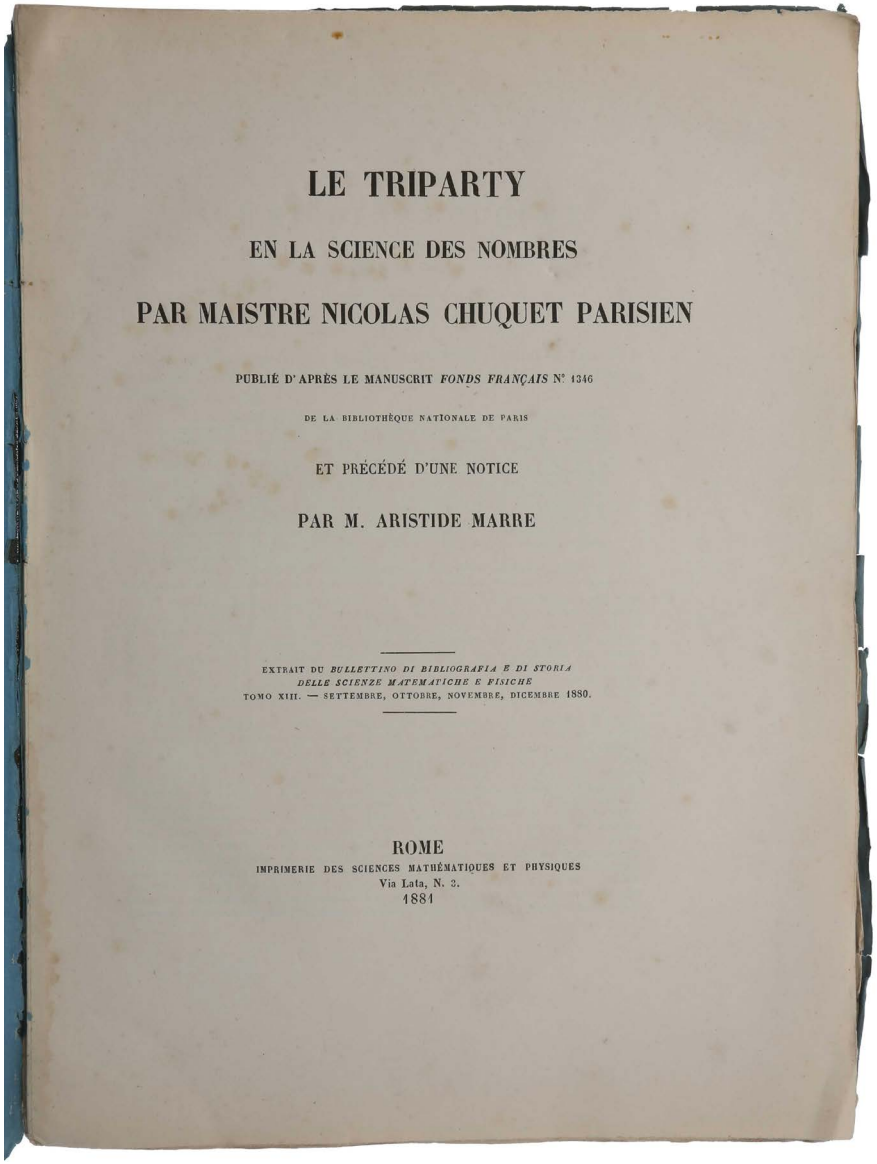
"The first part of the 'Triparty' ends with the 'regle des nombres moyens,' the only discovery to which Chuquet laid claim. According to him — and he was right — this rule allows the solution of many problems that are unapproachable by the classic rule of three or the rules of simple or double false position. It consisted of establishing that between any two given fractions a third can always

be interpolated that has for numerator the sum of the numerators of the other two fractions, and for denominator the sum of their denominators. It has been demonstrated in modern times that by repeating this procedure it is possible to arrive at all the rational numbers included between the two given fractions. It is obvious, therefore, that this rule, together with a lot of patience, makes it possible to solve any problem allowing of a rational solution. Further on, Chuquet utilizes it in order to approach indeterminately the square roots, cube roots, and so on, of numbers that do not have exact roots …

"The second part of the 'Triparty' deals with roots and 'compound numbers.' There is no trace of Euclidean nomenclature … The language has become simpler: there is no question of square roots or cube roots, but of second, third, fourth roots, "and so on, continuing endlessly." The number itself is its own first root. Moreover, everything is called a 'number' — whole numbers, rational numbers, roots, sums, and differences of roots — which, in the fifteenth century, was audacious indeed …

"The third part is by far the most original. It deals with the 'regle des premiers,' a "truly excellent" rule that "does everything that other rules do and, in addition, solves a great many more difficult problems." It is "the gateway and the threshold to the mysteries that are in the science of numbers." Such were the enthusiastic terms in which Chuquet announced the algebraic method. First, he explained his notation and his computational rules. The unknown, called the 'first number' (nombre premier), is written as 11. Therefore, where Chuquet wrote 40, we should read 4; if he wrote 51, we should read 5x; and if he wrote 73, we should read 7x3 …

"In order to justify his rules of algebraic computation, and particularly those touching the product of the powers of a variable, he called upon analogy. He considered the sequence of the powers of 2 and showed, for example, that 22



LE TRIPARTY

EN LA SCIENCE DES NOMBRES

PAR MAISTRE NICOLAS CHUQUET PARISIEN

PUBLIÉ D'APRÈS LE MANUSCRIT *FONDS FRANÇAIS* N° 1346

DE LA BIBLIOTHÈQUE NATIONALE DE PARIS

ET PRÉCÉDÉ D'UNE NOTICE

PAR M. ARISTIDE MARRE

EXTRAIT DU *BULLETTINO DI BIBLIOGRAFIA E DI STORIA DELLE SCIENZE MATEMATICHE E FISICHE*
TOMO XIII. — SETTEMBRE, OTTOBRE, NOVEMBRE, DICEMBRE 1880.

ROME
IMPRIMERIE DES SCIENCES MATHÉMATIQUES ET PHYSIQUES
Via Lata, N. 3.
1881

x 23 = 25. He wished only to make clear, by an example that he considered commonplace and that goes back almost to Archimedes, the algebraic rule that if squares are multiplied by cubes the result is the fifth power.

"In accordance with the custom of Chuquet's time, all these rules of computation were simply set forth, illustrated by a few examples, and at times justified by analogy with more elementary arithmetic, but never 'demonstrated' in the modern sense of the term. Having set down these preliminaries, Chuquet dealt with the theory of equations, which he called the 'method of equaling'" (DSB).

Chuquet's original manuscript is in four parts, the first two being Le Triparty and the collection of Problèmes numériques published by Marre in the offered works. The two remaining parts of the manuscript, on geometry and on commercial arithmetic, remained unstudied until the late 20th century, and the last is still unpublished. Itard in DSB states that the manuscript is the work of a firm of copyists, but it has been shown more recently that it is in Chuquet's hand.

The history of Chuquet's manuscript, which survives in only a single copy, is described by Marre in the introduction to Le Triparty. After Chuquet's death, probably in 1487, it fell into the possession of Étienne de la Roche (1470-1530), who may have been Chuquet's pupil. According to a note written in Latin on a protecting sheet at the beginning, after the manuscript had been in de la Roche's possession it was bought by an Italian, Leonardo de Villa. It then passed into the famous library of Jean Baptiste Colbert (1619-83), the finance minister of Louis XIV and founder of the French Academy of Sciences. In 1732, some 8000 volumes from Colbert's library passed into the Royal Library of Louis XV (1710-74), among which was Chuquet's manuscript. It then seems to have been forgotten for more than a century. In papers presented to the French Academy of Sciences in 1841 and 1842, Michel Chasles noted de la Roche's reference to Chuquet's 'Treatise on algebra' and expressed the hope that this treatise 'has not been completely lost.' "The publication of the first of the four sections of Chuquet's work by Aristide Marre in 1880 therefore created something of a sensation for historians of mathematics" (Flegg et al, p. 18). Marre had located the manuscript in the Bibliothèque Nationale, where it is now no. 1346 of the Fonds français.

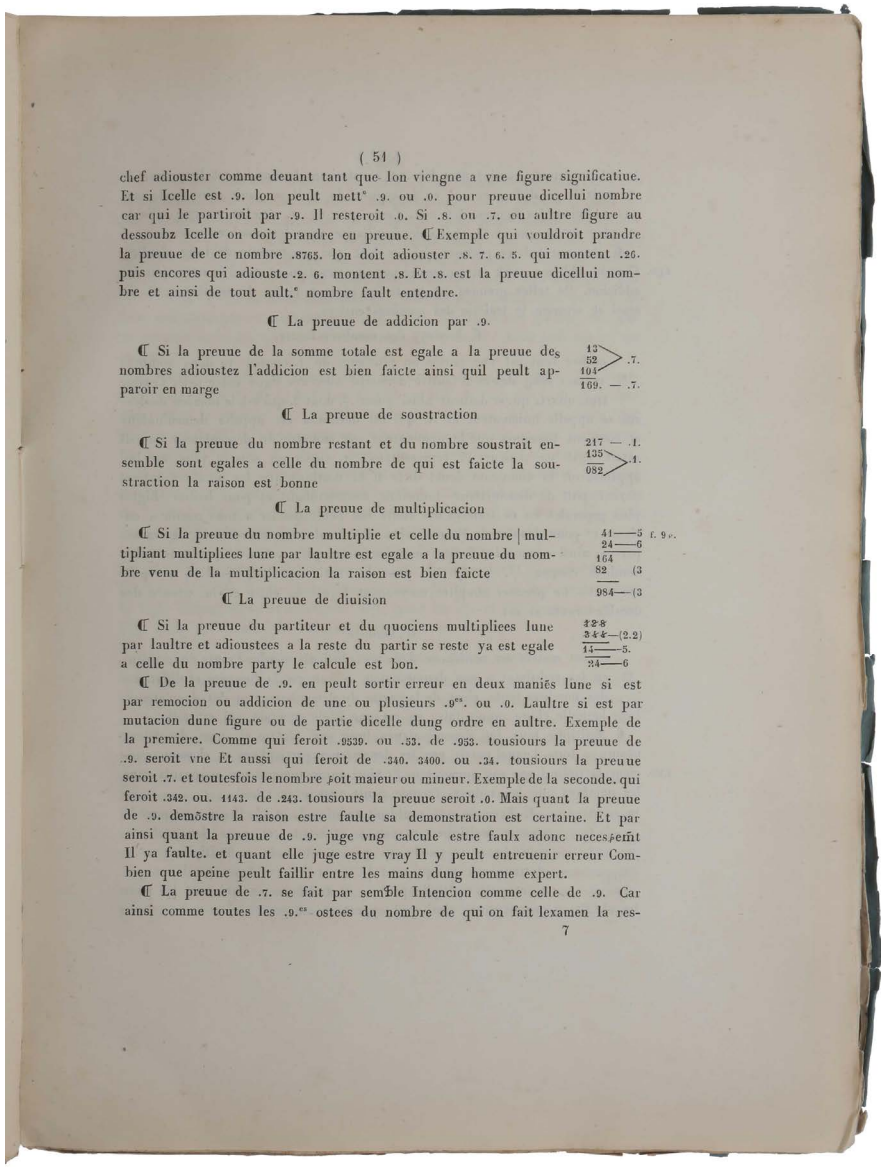
In his L'aristhmethique (1520), the first printed French work on algebra, de la Roche mentioned Le Triparty but then went on to plagiarise substantial portions of it without acknowledgement. Marre says of de la Roche, "without being accused of injustice or exaggeration one could say that he appropriated the work of Nicolas Chuquet, that he purely and simply copied the Triparty in a host of places, that he suppressed some of the most important passages, especially in the algebra, that he shortened or lengthened others, in order to compose his Arismethique, vastly inferior to the Triparty, and finally that, if for four centuries Nicolas Chuquet and his work have remained in the shadows, it is above all to him [de la Roche] that we must attribute the prime cause" (Flegg et al, p. 19).

Little is known of Chuquet's life. He "called himself a Parisian. He spent his youth in that city, where he was probably born and where the name is yet known. There he pursued his extensive studies, up to the baccalaureate in medicine (which implies a master of arts as well). It is difficult to say more about his life. He was living in Lyons in 1484, perhaps practicing medicine but more probably teaching arithmetic there as 'master of algorithms.' The significant place given to questions of simple and compound interest, the repayment of debts, and such in his work leads one to suppose this. However, he used these questions only as pretexts for exercises in algebra.

"Chuquet's mathematical learning was solid. He cites by name Boethius — whom everyone knew at that time — Euclid, and Campanus of Novara. He knew the

propositions of Archimedes, Ptolemy, and Eutocius, which he stated without indicating his sources (referring to Archimedes only as 'a certain wise man'). In geometry his language seems to be that of a translator, transposing terms taken from Greek or Latin into French. By contrast, in the parts devoted solely to arithmetic or algebra there is no borrowing of learned terminology. Everything is written in simple, direct language, with certain French neologisms that have not been preserved elsewhere" (DSB).

Flegg, Hay & Moss (eds.), *Nicolas Chuquet, Renaissance Mathematician*, 1985.



(51)

chef adiouster comme deuant tant que lon viengne a vne figure significatiue. Et si Icelle est .9. lon peult mett.e .9. ou .0. pour preuue dicellui nombre car qui le partiroit par .9. Il resteroit .0. Si .8. ou .7. ou aultre figure au dessoubz Icelle on doit prandre en preuue. ¶ Exemple qui vouldroit prandre la preuue de ce nombre .8765. lon doit adiouster .8. 7. 6. 5. qui montent .26. puis encores qui adiouste .2. 6. montent .8. Et .8. est la preuue dicellui nombre et ainsi de tout ault.e nombre fault entendre.

¶ La preuue de addicion par .9.

¶ Si la preuue de la somme totale est egale a la preuue des nombres adioustez l'addicion est bien faicte ainsi quil peult apparoir en marge

13
52
104
169. — .7.

¶ La preuue de soustraction

¶ Si la preuue du nombre restant et du nombre soustrait ensemble sont egales a celle du nombre de qui est faicte la soustraction la raison est bonne

217 — .1.
135
082 .1.

¶ La preuue de multiplicacion

¶ Si la preuue du nombre multiplie et celle du nombre | multipliant multipliees lune par laultre est egale a la preuue du nombre venu de la multiplicacion la raison est bien faicte

41——5 f. 9v.
24——6
164
82 (3
984——(3

¶ La preuue de diuision

¶ Si la preuue du partiteur et du quociens multipliees lune par laultre et adioustees a la reste du partir se reste ya est egale a celle du nombre party le calcule est bon.

428
344—(2.2)
14——5.
24——6

¶ De la preuue de .9. en peult sortir erreur en deux maniẽs lune si est par remocion ou addicion de une ou plusieurs .9es. ou .0. Laultre si est par mutacion dune figure ou de partie dicelle dung ordre en aultre. Exemple de la premiere. Comme qui feroit .9539. ou .53. de .953. tousiours la preuue de .9. seroit vne Et aussi qui feroit de .340. 3400. ou .34. tousiours la preuue seroit .7. et toutesfois le nombre soit maieur ou mineur. Exemple de la seconde. qui feroit .342. ou. 1143. de .243. tousiours la preuue seroit .0. Mais quant la preuue de .9. demõstre la raison estre faulte sa demonstration est certaine. Et par ainsi quant la preuue de .9. juge vng calcule estre faulx adonc necessairẽt Il ya faulte. et quant elle juge estre vray Il y peult entreuenir erreur Combien que apeine peult faillir entre les mains dung homme expert.

¶ La preuue de .7. se fait par semble Intencion comme celle de .9. Car ainsi comme toutes les .9.es ostees du nombre de qui on fait lexamen la res-

7

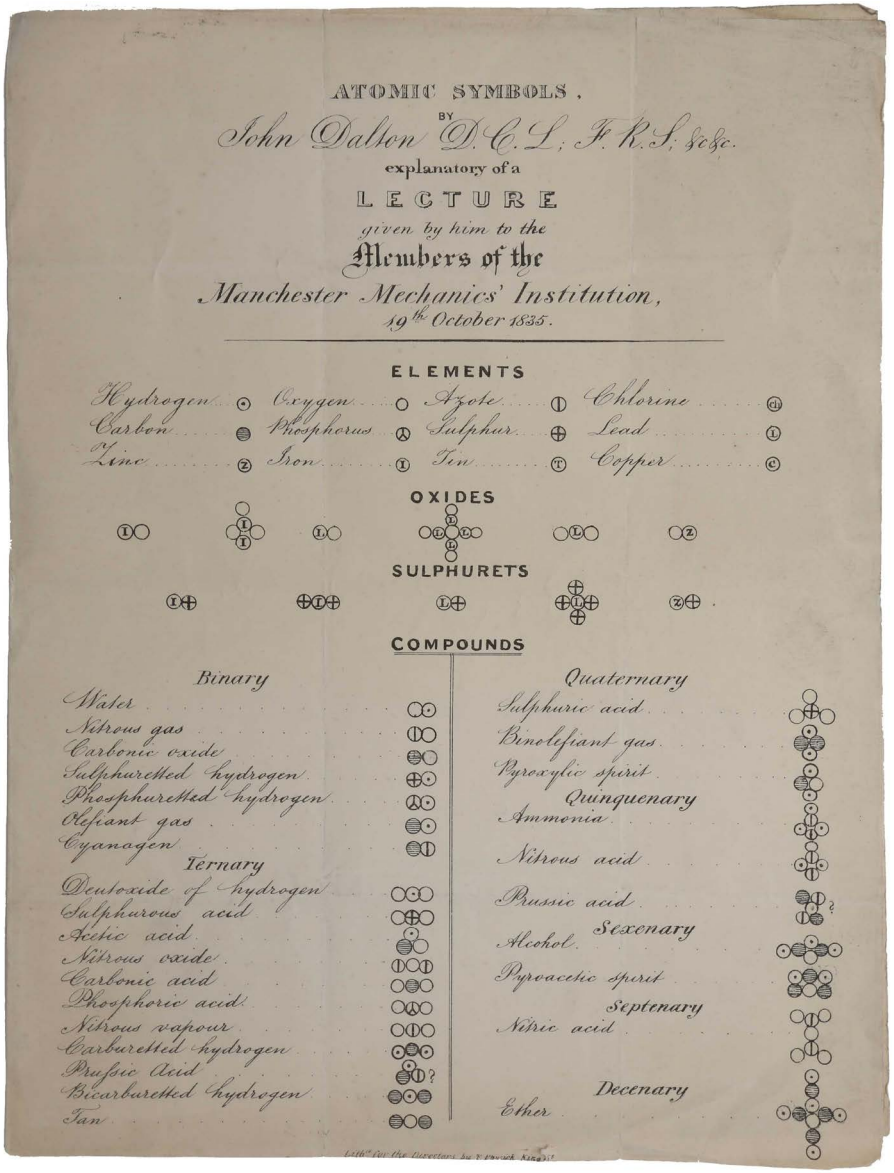
TABLE OF ATOMIC SYMBOLS

DALTON, John. *Atomic Symbols by John Dalton, explanatory of a Lecture given by him to the Members of the Manchester Mechanics' Institution, October 19th, 1835.* [Manchester]: Lith[ographed] for the Directors by F. Physick, King St., [1835]. [Offered with:] *An unidentified lithographed sheet of atomic symbols reproduced from a manuscript in Dalton's hand.*

$6,000

[Printed table:] Folio (291 x 223mm), two leaves, first leaf printed on recto only, second leaf blank, three horizontal and two vertical creases where folded. [Lithographed table:] One leaf (229 x 187mm), printed on recto only, horizontal and vertical crease where folded, another crease in top left corner, soiled.

First edition, extremely rare, of this table of atomic symbols, for both elements and compounds, which was distributed to those attending Dalton's lecture on atomic theory delivered to the Manchester Mechanics' Institution on 19 October, 1835. This is a famous image, which was reprinted in W. C. Henry's Memoirs of the Life and Scientific Researches of John Dalton (1854), and is much reproduced even today, but original copies are of the greatest rarity. We know of no other separately printed tables of his atomic symbols. The other lithographed sheet of atomic symbols has the appearance of an early draft of the printed table, and differs from it in some respects (e.g., chlorine appears in the printed table but not in the draft. "Dalton made greater use of his symbols in lectures on the atomic theory than in his published works or even in his laboratory note-books … A famous lecture given by Dalton was in October 1835, when he lectured at the Manchester Mechanics' Institution. The subject was the atomic theory and the



audience was issued with a lithographed sheet of atomic symbols. The sheet contains examples of compounds containing from two to ten atoms" (Crosland, pp. 261-2). "Dalton's chemical atomic theory was the first to give significance to the relative weights of the ultimate particles of all known compounds, and to provide a quantitative explanation of the phenomena of chemical reaction. Dalton believed that all matter was composed of indestructible and indivisible atoms of various weights, each weight corresponding to one of the chemical elements, and that these atoms remained unchanged during chemical processes. Dalton's work with relative atomic weights prompted him to construct the first periodic table of the elements to formulate laws concerning their combination and to provide schematic representations of various possible combinations of atoms. His equation of the concepts 'atom' and 'chemical element' was of fundamental importance, as it provided the chemist with a new and enormously fruitful model of reality" (Norman 575). The number of copies of this table printed was presumably limited to the expected size of Dalton's audience for this single lecture, and its ephemeral nature must mean that only a small fraction of those printed have survived. OCLC lists two copies (University of Delaware and Chemical Heritage Foundation). We know of only one example having appeared in commerce, tipped in to the Norman copy of Dalton's New System of Chemical Philosophy. We are not aware of any other manuscript material by Dalton relating to his atomic theory having appeared on the market.

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

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

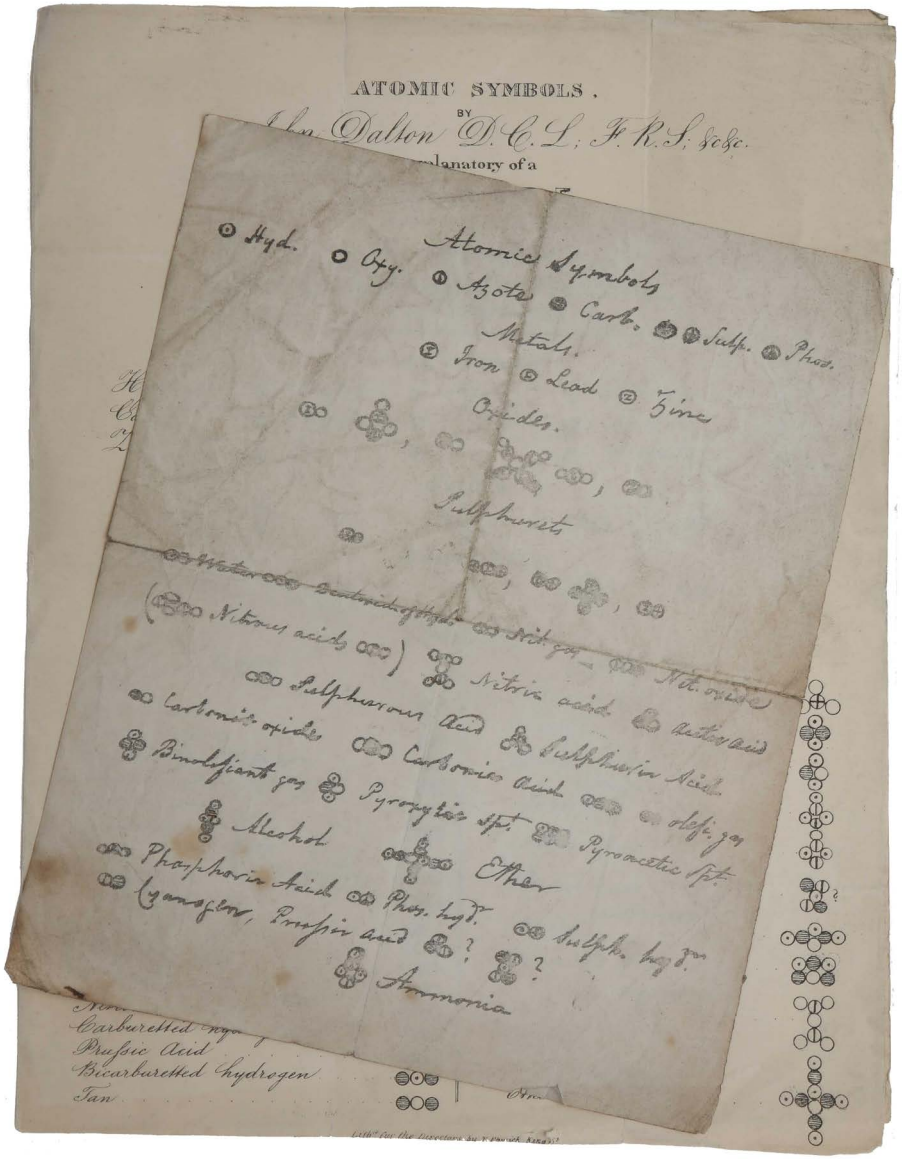
Born in a small village in the English Lake District, Dalton (1766-1844) moved to Manchester in 1793. After he arrived, he at first taught mathematics and natural philosophy at New College, a dissenting academy, and began observing the behavior of gases, but after six years he resigned. Thereafter he devoted his

life to research, which he financed by giving private tuition. By 1800, Dalton had become the secretary of the Manchester Literary and Philosophical Society, and in 1801 he presented the first of a series of papers to the society describing the properties of 'mixed gases'. These papers laid the foundations of his atomic theory; a paper of 1803 included the first table of atomic weights. In 1808 Dalton began the publication of his great work, A New System of Chemical Philosophy, which set out his atomic theory in detail; it was completed only in 1827.

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

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

"The establishment of societies throughout England, Wales and Scotland, and



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

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

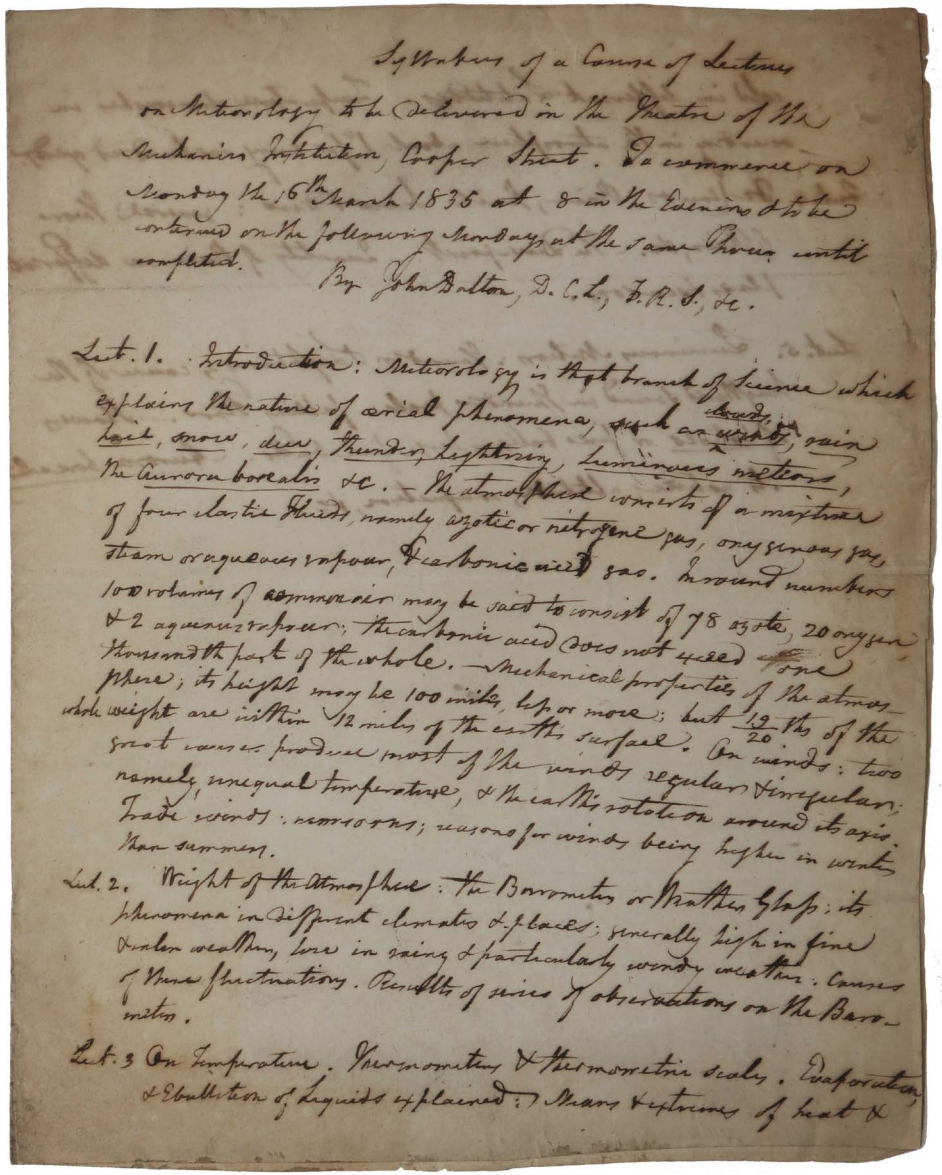
AUTOGRAPH METEOROLOGICAL MANUSCRIPT IN DALTON'S HAND

DALTON, John. *Autograph syllabus of a course of five lectures on meteorology delivered by Dalton at the Manchester Mechanics' Institution in March and April 1835. With Dalton's signature at the end of the title.* [Manchester, 1835].

$5,000

Folio (267 x 211 mm), two leaves, the first written on recto and verso, the second blank.

Autograph manuscript in the hand of the great English chemist John Dalton (1766-1844). This is a remarkable survival, as "the great bulk of Dalton's manuscript material was destroyed in an air-raid on Manchester in 1940" (Thackray, p. 145). Dalton is famous today for his atomic theory of matter, but this grew out of his early work on meteorology, which resulted in his Meteorological Observations and Essays (1793). "It created little stir at first but contained original ideas that, together with Dalton's more developed articles, marked the transition of meteorology from a topic of general folklore to a serious scientific pursuit" (*Britannica*). In this book Dalton stated that 'water evaporated is not chymically combined with the aerial fluids but exists as a peculiar fluid diffused amongst the rest,' and that 'when a particle of vapour exists between two particles of air let their equal and opposite pressures upon it be what they may, they cannot bring it nearer to another particle of vapour.' "The ideas that in a mixture of gases every gas acts as an independent entity (Dalton's law of partial pressures) and that the air is not a vast chemical solvent were thus first stated in the Meteorological Observations" (DSB). "Dalton's



Syllabus of a Course of Lectures on Meteorology to be delivered in the Theatre of the Mechanics Institution, Cooper Street. To commence on Monday the 16th March 1835 at 8 in the Evening & to be continued on the following Monday at the same hour until completed.

By John Dalton, D. C. L., F. R. S., &c.

Lect. 1. Introduction: Meteorology is that branch of science which explains the nature of aerial phenomena, such as clouds, rain, hail, snow, dew, thunder, lightning, luminous meteors, the Aurora borealis &c. – The atmosphere consists of a mixture of four elastic fluids, namely azotic or nitrogene gas, oxygenous gas, steam or aqueous vapour, & carbonic acid gas. In round numbers 100 volumes of common air may be said to consist of 78 azote, 20 oxygen & 2 aqueous vapour; the carbonic acid does not exceed one thousandth part of the whole. – Mechanical properties of the atmosphere; its height may be 100 miles, 150 or more; but 19/20ths of the whole weight are within 12 miles of the earth's surface. On winds: two great causes produce most of the winds, regular & irregular; namely, unequal temperature, & the earth's rotation around its axis. Trade winds: monsoons; reasons for winds being higher in winter than summer.

Lect. 2. Weight of the Atmosphere: the Barometer or Mercurial Gauge: its phenomena in different climates & places; generally high in fine dry weather, low in rainy & particularly windy weather: Causes of these fluctuations. Results of series of observations on the Barometer.

Lect. 3 On Temperature. Thermometers & thermometric scales. Evaporation & Ebullition of Liquids explained: Means & systems of heat &

first scientific experiments stemmed from his life-long interest in meteorology … One of his most important scientific conclusions was that water is a component of air at all temperatures, and he produced a table of the vapour pressure of water at different temperatures from his own experiments. This work led on to what is now called the Law of Partial Pressures … In lectures to the Royal Institution in 1810 Dalton himself attributed the origins of his atomic theory to his studies on the properties of mixed gases" (Lappert & Murrell, p. 3817). Manuscript material by Dalton with scientific content is of great rarity on the market.

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Lect. 3. On temperature. Thermometers and thermometric scales. Evaporation and Ebullition of Liquids explained; Means & extremes of heat and cold in different Latitudes. Temperature diminishes in ascending in the atmosphere about 1° for every one hundred yards.

Lect. 4. On Clouds, Rain, Hail, Snow & Dew: general theory of Rain, etc. The Dew-point. Quantity of rain in different places and seasons.

Lect. 5. Luminous meteors: Thunder & Lightning, cause of the lengthened sound in thunder peals; falling Stars. Showers of bolides or fire balls. Larger fire balls. Aurora borealis, its nature, altitude, magnetism, etc.

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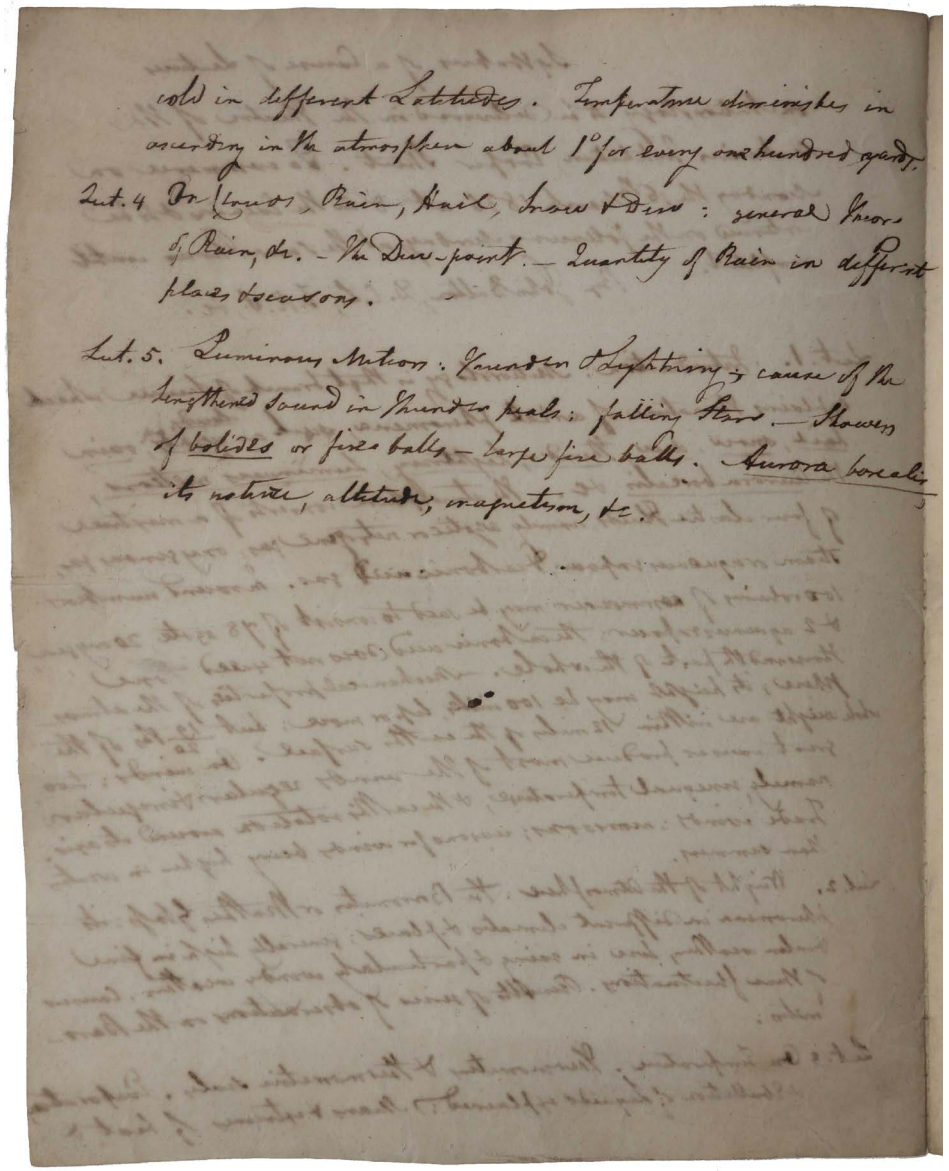
The Manchester Mechanics' Institution was established on 7 April 1824. The original prospectus of the institution stated: 'The Manchester Mechanics' Institution is formed for the purpose of enabling Mechanics and Artisans, of whatever trade they may be, to become acquainted with such branches of science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

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

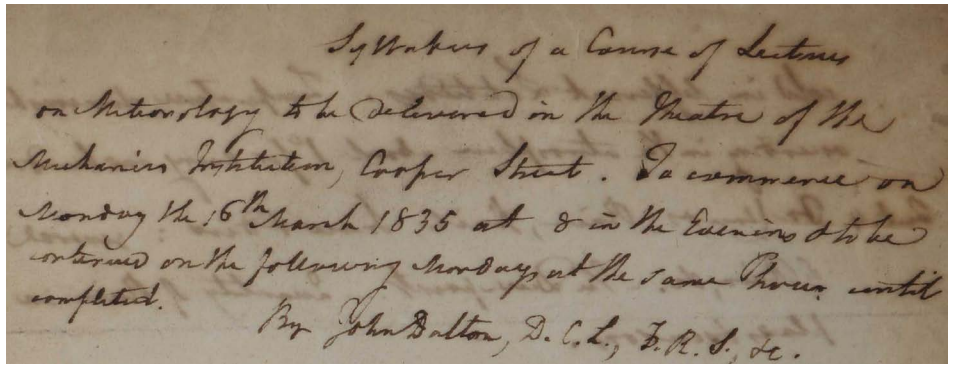

cold in different Latitudes. Temperature diminishes in ascending in the atmosphere about 1° for every one hundred yards.

Lect. 4 On Clouds, Rain, Hail, Snow & Dew: general theory of Rain, &c. – The Dew-point. – Quantity of Rain in different places & seasons.

Lect. 5. Luminous Meteors: Thunder & Lightning – cause of the lengthened sound in Thunder peals: falling Stars. – Showers of bolides or fire balls – large fire balls. Aurora borealis its nature, altitude, magnetism, &c.

men had become aware of the success of recent experiments in the education of working men and had been personally associated with at least one of these enterprises" (Tylecote, p. 1).

Lappert & Murrell, 'John Dalton, the man and his legacy: the bicentenary of his Atomic Theory,' *Dalton Transactions* (2003), 3811-20. Smyth, *John Dalton 1766-1844. A Bibliography of works by him and about him* (1966). Thackray, 'Fragmentary remains of John Dalton,' *Annals of Science* 22 (1966), 145-174. Tylecote, The Mechanics Institutes of Lancashire and Yorkshire before 1851 (1957).


Syllabus of a Course of Lectures
on Meteorology to be delivered in the Theatre of the
Mechanics Institution, Cooper Street. To commence on
Monday the 16th March 1835 at 8 in the Evening & to be
continued on the following Mondays at the same Hour, until
completed.
By John Dalton, D. C. L., F. R. S. &c.

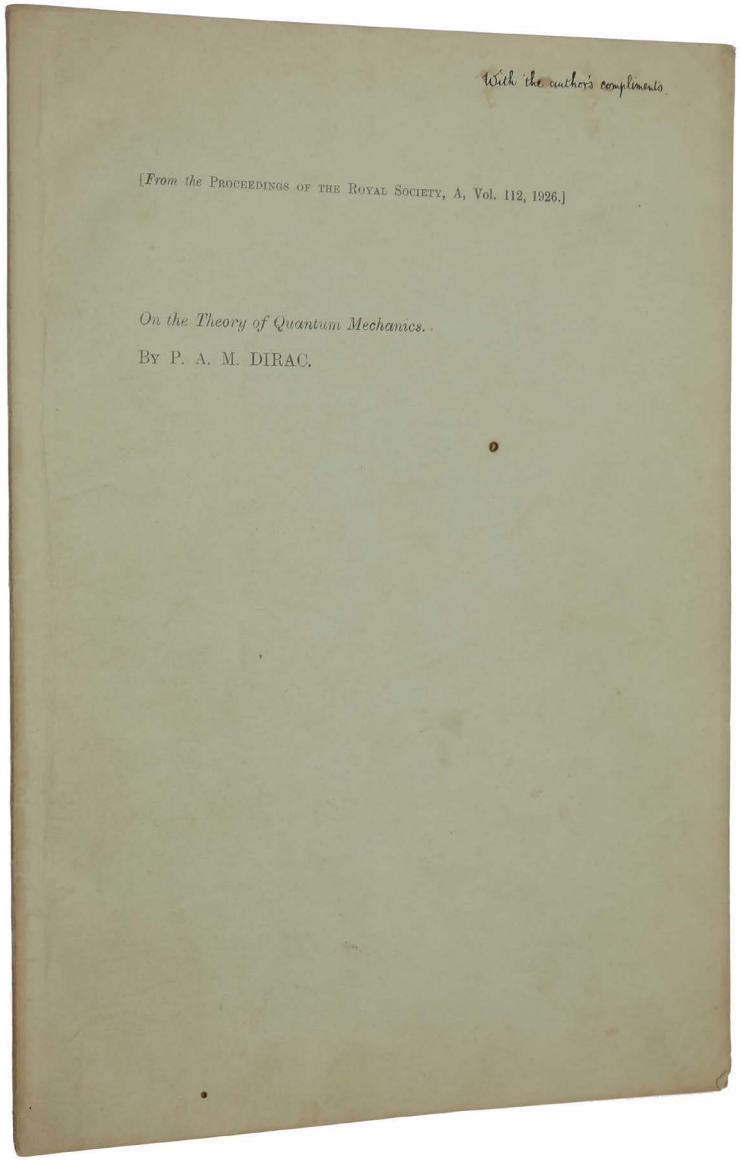
FERMI-DIRAC STATISTICS INSCRIBED PRESENTATION COPY

DIRAC, Paul Adrien Maurice. *On the theory of quantum mechanics.* [London: Harrison & Sons for the Royal Society, 1926].

$27,500

Offprint from Proceedings of the Royal Society A, 112 (1926). 8vo (253 x 178 mm), pp. 661- 677. Original printed wrappers, light soiling, two small burn-holes in front wrapper (not affecting text). Presentation copy, inscribed by Dirac on the front wrapper: "With the author's compliments."

First edition, inscribed presentation offprint, of Dirac's paper, which "is justly seen as a major contribution to quantum theory" (Kragh, p. 36). It introduced his quantum mechanical derivation of what is now called Fermi-Dirac statistics, which describes a distribution of particles (now known as fermions, a name coined by Dirac in 1945) in certain systems containing many identical particles that obey the Pauli exclusion principle—meaning that no two of the particles can occupy the same quantum state simultaneously. The paper "will be remembered as the first in which quantum mechanics is brought to bear on statistical mechanics. Recall that the earliest work on quantum statistics, by Bose and by Einstein, predates quantum mechanics. Also, Fermi's introduction of the exclusion principle in statistical problems, though published after the arrival of quantum mechanics, is still executed in the context of the "old" quantum theory. All these contributions were given their quantum mechanical underpinnings by Dirac, who was, in fact, the first to give the correct justification of Planck's law, which started it all" (Pais, p. 6).


With the author's compliments.

[*From the* Proceedings of the Royal Society, A, Vol. 112, 1926.]

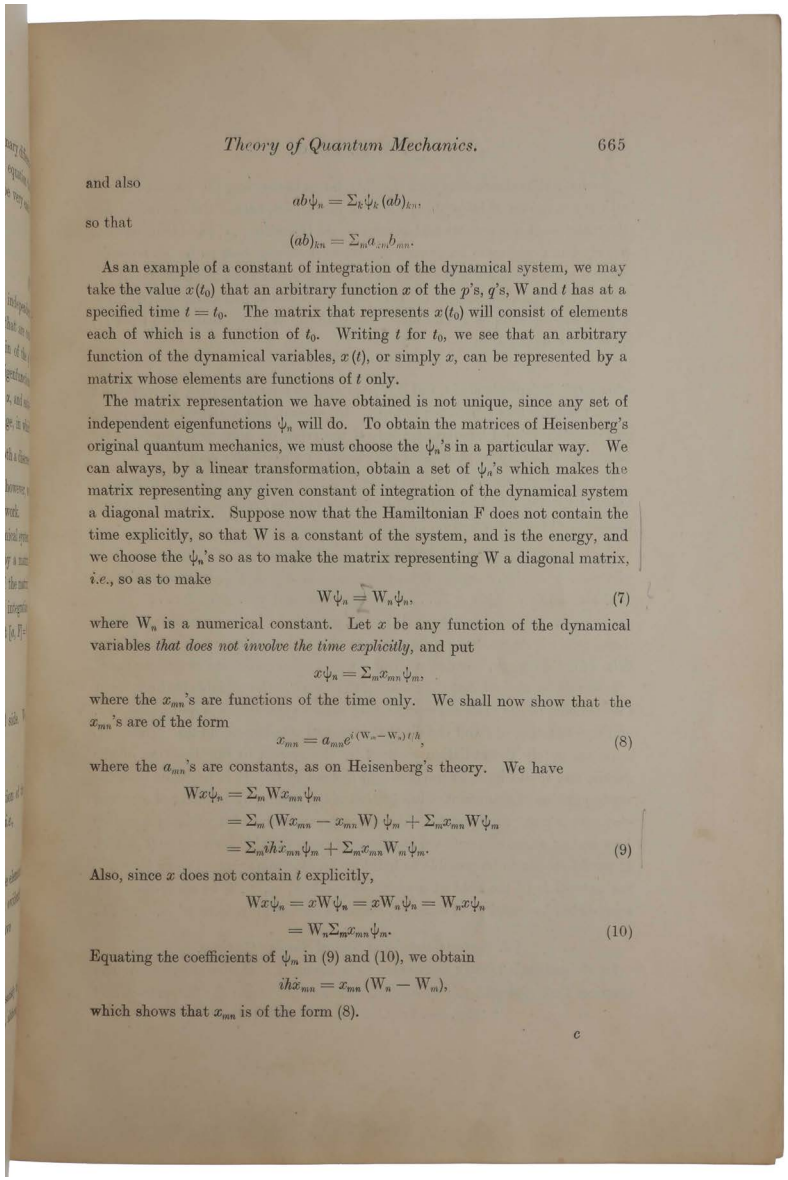
On the Theory of Quantum Mechanics.

By P. A. M. DIRAC.

Dirac's paper may also be considered the birth of quantum electrodynamics. He applied Schrödinger's wave mechanics (his earlier papers had used exclusively Heisenberg's matrix mechanics) to develop a theory of time-dependent perturbations and applied it to the emission and absorption of radiation. "Radiation theory was the subject of the last section of the important paper "On the theory of quantum mechanics." There Dirac considered a system of atoms subjected to an external perturbation that could vary arbitrarily with the time. Of course, the particular perturbation he had in mind was an incident electromagnetic field, but, characteristically, he stated the problem in the most general way possible" (Kragh, pp. 120-1).

As indicated in the above citation, Dirac and Enrico Fermi discovered Fermi-Dirac statistics independently of one another. Several months before the appearance of Dirac's paper, Fermi had published his own in which he applied Pauli's exclusion principle to his theory of an ideal monatomic gas, rather than for general systems of identical fermions, and in the context of the "old" quantum theory, not quantum mechanics. "When he was asked about it several decades later, [Dirac] remarked: 'I had read Fermi's paper on Fermi statistics and had forgotten it completely. When I wrote my work on the anti-symmetric wave functions, I did not refer to it at all. Then Fermi wrote and told me and I remembered that I had previously read about it' . . . Fermi's letter had the effect that Dirac later on never forgot to mention the priority of his Italian colleague when referring to the statistics obeyed by electrons and the like. In spite of this admitted priority of Fermi it was essentially Dirac's paper that helped the physicists tremendously in understanding the meaning of the new statistical methods" (Mehra & Rechenberg, p. 767).

"Following the publication of Dirac's paper, the new statistics was eagerly taken up and applied to a variety of problems. The first application was made by Dirac's former teacher, Fowler; as an expert in statistical physics, he was greatly



Theory of Quantum Mechanics. 665

and also

$$ab\psi_n = \Sigma_k \psi_k (ab)_{kn},$$

so that

$$(ab)_{kn} = \Sigma_m a_{km} b_{mn}.$$

As an example of a constant of integration of the dynamical system, we may take the value $x(t_0)$ that an arbitrary function x of the p's, q's, W and t has at a specified time $t = t_0$. The matrix that represents $x(t_0)$ will consist of elements each of which is a function of t_0. Writing t for t_0, we see that an arbitrary function of the dynamical variables, $x(t)$, or simply x, can be represented by a matrix whose elements are functions of t only.

The matrix representation we have obtained is not unique, since any set of independent eigenfunctions ψ_n will do. To obtain the matrices of Heisenberg's original quantum mechanics, we must choose the ψ_n's in a particular way. We can always, by a linear transformation, obtain a set of ψ_n's which makes the matrix representing any given constant of integration of the dynamical system a diagonal matrix. Suppose now that the Hamiltonian F does not contain the time explicitly, so that W is a constant of the system, and is the energy, and we choose the ψ_n's so as to make the matrix representing W a diagonal matrix, *i.e.*, so as to make

$$W\psi_n = W_n\psi_n, \tag{7}$$

where W_n is a numerical constant. Let x be any function of the dynamical variables *that does not involve the time explicitly*, and put

$$x\psi_n = \Sigma_m x_{mn}\psi_m,$$

where the x_{mn}'s are functions of the time only. We shall now show that the x_{mn}'s are of the form

$$x_{mn} = a_{mn} e^{i(W_m - W_n)t/\hbar}, \tag{8}$$

where the a_{mn}'s are constants, as on Heisenberg's theory. We have

$$\begin{aligned} Wx\psi_n &= \Sigma_m W x_{mn}\psi_m \\ &= \Sigma_m (Wx_{mn} - x_{mn}W)\psi_m + \Sigma_m x_{mn} W\psi_m \\ &= \Sigma_m i\hbar\dot{x}_{mn}\psi_m + \Sigma_m x_{mn} W_m \psi_m. \end{aligned} \tag{9}$$

Also, since x does not contain t explicitly,

$$\begin{aligned} Wx\psi_n = xW\psi_n = xW_n\psi_n &= W_n x\psi_n \\ &= W_n \Sigma_m x_{mn}\psi_m. \end{aligned} \tag{10}$$

Equating the coefficients of ψ_m in (9) and (10), we obtain

$$i\hbar\dot{x}_{mn} = x_{mn}(W_n - W_m),$$

which shows that x_{mn} is of the form (8).

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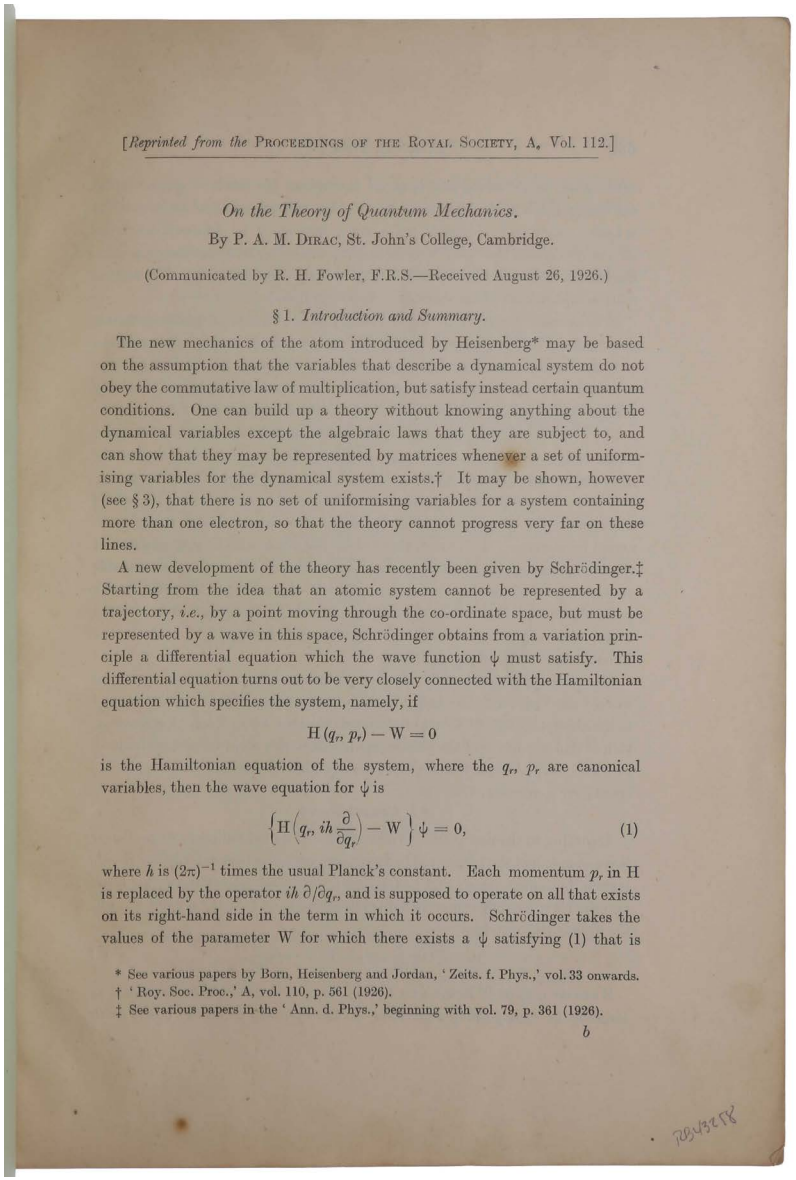
interested in the Fermi-Dirac result. Fowler studied a Fermi-Dirac gas under very high pressure, thus beginning a chapter in astrophysics that, a few years later, would be developed into the celebrated theory of white dwarfs by his student Chandrasekhar. In Germany, Pauli and Sommerfeld made other important applications of the new quantum statistics, with which they laid the foundation for the quantum theory of metals in 1927" (Kragh, p. 36).

H. Kragh, *Dirac: A Scientific Biography*, 1990; J. Mehra & H. Rechenberg, *The Historical Development of Quantum Theory*, Vol. 5, 2000; A. Pais, "Paul Dirac: Aspects of his life and work," in *Paul Dirac: The Man and his Work*, ed. P. Goddard, 1998, pp. 1-45.



[*Reprinted from the* Proceedings of the Royal Society, A, Vol. 112.]

On the Theory of Quantum Mechanics.

By P. A. M. Dirac, St. John's College, Cambridge.

(Communicated by R. H. Fowler, F.R.S.—Received August 26, 1926.)

§ 1. *Introduction and Summary.*

The new mechanics of the atom introduced by Heisenberg* may be based on the assumption that the variables that describe a dynamical system do not obey the commutative law of multiplication, but satisfy instead certain quantum conditions. One can build up a theory without knowing anything about the dynamical variables except the algebraic laws that they are subject to, and can show that they may be represented by matrices whenever a set of uniformising variables for the dynamical system exists.† It may be shown, however (see § 3), that there is no set of uniformising variables for a system containing more than one electron, so that the theory cannot progress very far on these lines.

A new development of the theory has recently been given by Schrödinger.‡ Starting from the idea that an atomic system cannot be represented by a trajectory, *i.e.*, by a point moving through the co-ordinate space, but must be represented by a wave in this space, Schrödinger obtains from a variation principle a differential equation which the wave function ψ must satisfy. This differential equation turns out to be very closely connected with the Hamiltonian equation which specifies the system, namely, if

$$H(q_r, p_r) - W = 0$$

is the Hamiltonian equation of the system, where the q_r, p_r are canonical variables, then the wave equation for ψ is

$$\left\{H\left(q_r, ih\frac{\partial}{\partial q_r}\right) - W\right\}\psi = 0, \qquad (1)$$

where h is $(2\pi)^{-1}$ times the usual Planck's constant. Each momentum p_r in H is replaced by the operator $ih\,\partial/\partial q_r$, and is supposed to operate on all that exists on its right-hand side in the term in which it occurs. Schrödinger takes the values of the parameter W for which there exists a ψ satisfying (1) that is

* See various papers by Born, Heisenberg and Jordan, 'Zeits. f. Phys.,' vol. 33 onwards.
† 'Roy. Soc. Proc.,' A, vol. 110, p. 561 (1926).
‡ See various papers in the 'Ann. d. Phys.,' beginning with vol. 79, p. 361 (1926).

b

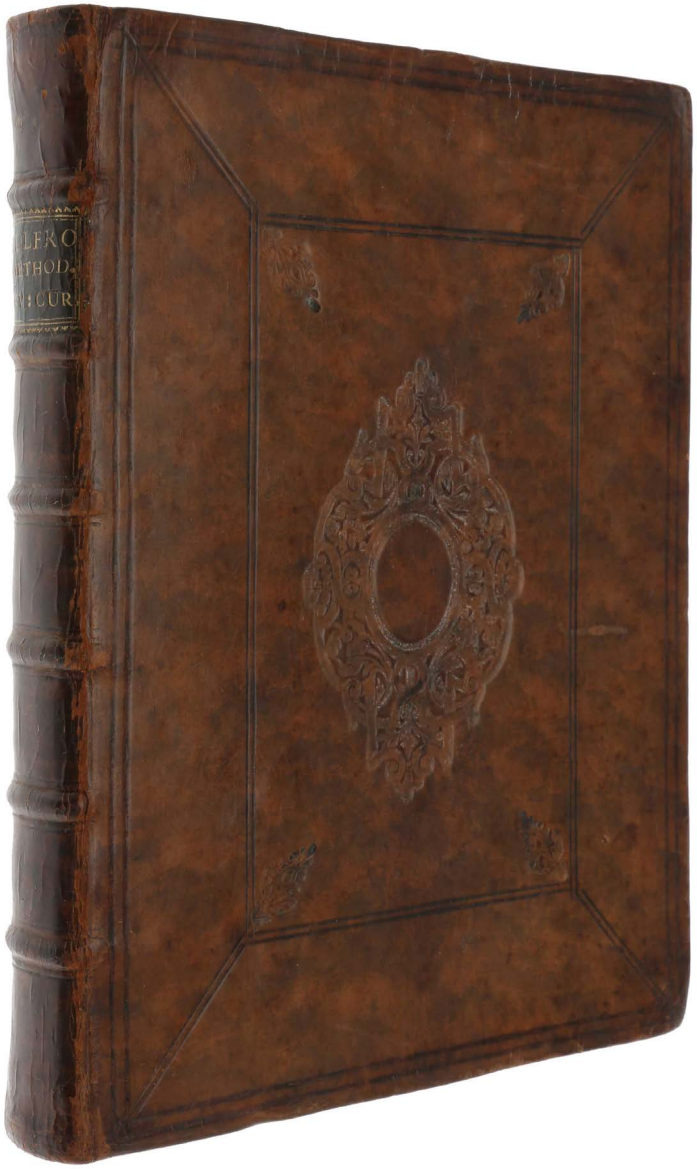
THE FINEST COPY WE HAVE SEEN

EULER, Leonhard. *Methodus inveniendi lineas curvas maximi minimive proprietate gaudentes.* Lausanne & Geneve: Marcum-Michaelem Bosquet & Socios, 1744.

$12,500

4to (240 x 193 mm), pp. [2], [1], 2-322, [1], with five engraved folding plates. Title in red and black with large engraved device and large woodcut headpiece and initial on first page of text. Contemporary panelled calf with large medallions in blind at centre of each cover, black lettering-piece on spine. Exceptionally rare in usch fine condition.

First edition, and an exceptionally fine copy, of "Euler's most valuable contribution to mathematics in which he developed the concept of the calculus of variations" (Norman). "This work displays an amount of mathematical genius seldom rivaled" (Cajori, p. 234). "The book brought him immediate fame and recognition as the greatest living mathematician" (Kline, p. 579). This is the finest copy we have ever seen, in a beautiful untouched contemporary binding and without any of the browning that almost always affects this book. Du Pasquier (pp. 50-51) called the Methodus inveniendi "one of the finest monuments of the genius of Euler" who, he continued, "founded the calculus of variations which has become, in the twentieth century, one of the most efficient of the means of investigation employed by mathematicians and physicists. The recent theories of Einstein [i.e., general relativity] and the applications of the principle of relativity have greatly increased the importance of the calculus of variations which Euler created". "The Methodus inveniendi is of two-fold interest for historians of mathematics. First, it was a highly successful synthesis of what was then known about problems of



optimization in the calculus, and presented general equational forms that became standard in the calculus of variations. Euler's method was taken up by Joseph Louis Lagrange (1736–1813) 20 years later and brilliantly adapted to produce a novel technique for solving variational problems. The two appendices to Euler's book applied variational ideas to problems in statics and dynamics, and these too became the basis for Lagrange's later researches. Second, in Euler's book some of his distinctive contributions to analysis appear for the first time or very nearly the first time: the function concept, the definition of higher-order derivatives as differential coefficients; and the recognition that the calculus is fundamentally about abstract relations between variable quantities, and only secondarily about geometrical curves. The Methodus inveniendi is an important statement of Euler's mathematical philosophy as it had matured in the formative years of the 1730s" (Fraser, p. 169). The Methodus inveniendi consist of six chapters, delivered to the publisher in July 1743, and two appendices, delivered in December 1743, the first on elastic curves, the second on the principle of least action.

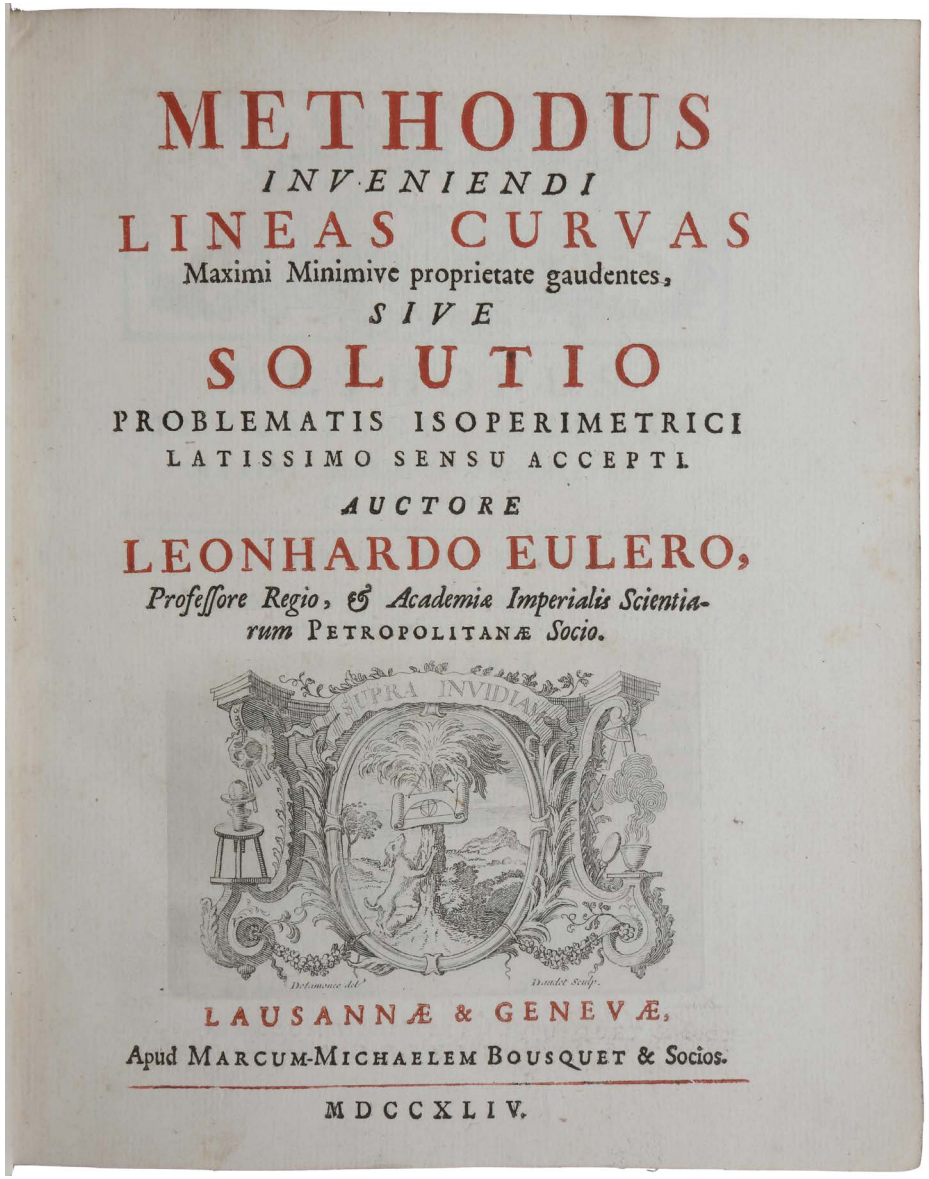
"By the spring of 1741 at the latest, Euler had finished in Saint Petersburg a draft of Methodus inveniendi lineas curvas maximi minimive proprietate gaudentes (A method of finding curves that show some property of maximum or minimum), though without the two appendixes that would come later. In May 1743 the publisher Marcus-Michael Bousquet visited Berlin to present the king a copy of Johann I Bernoulli's Opera omnia; Bousquet was impressed with the work of Euler, who handed him the main body of the Methodus inveniendi manuscript. That month Euler wrote to the Genevan mathematician Gabriel Cramer, asking him to proofread and correct for Bousquet that small book written on isoperimetric problems. Cramer was known to Euler in part for his commentaries and annotations to Bernoulli's Opera. Finding the manuscript admirable, Cramer agreed. Daniel Bernoulli, attempting at the time to determine maxima and minima of elastic curves, recommended to Euler the addition to the Methodus inveniendi text of two appendixes on elasticity. Among all curves of a stated length that had tangents at the ends, Euler would minimize the integral of an element of the arc length divided by the radius of curvature squared. By the end of that summer Euler had completed the appendixes, but they were not sent to Bousquet in Lausanne until December" (Calinger, pp. 202-3).

"The 320-page Methodus inveniendi in quarto format was the first book Euler published in the 1740s. A landmark treatise, it appeared in print in September 1744. This was fast, for Euler had submitted its appendixes only the previous December. The study made him the principal creator of the first stage of a new branch of mathematics, the classical calculus of variations, which in the paths of motion sought to determine maximal or minimal lengths of plane curves, if any existed, and pursued extreme values for integrals (often named functionals). Its first section asserts that 'since the fabric of the universe is most perfect, and is the work of a most Wise Creator, nothing whatsoever takes place in the universe in which some relation of maximum and minimum does not appear.' A letter of December 1745 to Maupertuis repeats Euler's conclusion from the Methodus inveniendi that 'in the natural course of movements there is a constant maximum or minimum, and I have determined … that all trajectory curves, and all bodies drawn toward a fixed center or mutually drawn together have been so described.' The baroque title of the work derives from Euler's perception of the new field as a Leibnizian ars inveniendi, or method of discovery. Its twentieth-century editor Constantin Caratheodory called it 'one of the most beautiful mathematical works ever written.'

"Through skillful organization in arranging more than a hundred increasingly complex problems in eleven categories and providing new direction and ideas in the Methodus inveniendi, Euler replaced the previous ad hoc procedures for special case problems in the formative stage of the calculus of variations, instead offering standard differential equations for general solutions and giving techniques for reaching these equations. His work impressively extended and

refined that of Brook Taylor, Jakob Hermann and Jakob and Johann I Bernoulli, and was the culmination of their efforts; Euler's success where they had failed in creating the new field magnified his reputation. His methods were closest to Jakob Bernoulli's in that they require two degrees of freedom to extremalize or optimize a curve. Euler's attention to curves, including the tautochrone, isochrone, and brachistochrone, and his use of isoperimetry, a subject popular in the late seventeenth century, kept the new field largely geometric. This was one of the several occasions on which Euler searched for a different solution for the brachistochrone problem [that of determining the curve of quickest descent joining two given points]. Chapter 3 of the Methodus invenendi generalizes this early optimization problem, essentially finding sets of extremals, and section 45 provides the most elegant solution up to 1744, correcting Hermann's solution of the brachistochrone problem in a resisting medium. Chapter 2 contains the vital innovation, the Euler differential equation or the first necessary condition for extremals (now known as the Euler-Lagrange Equation, it is the basic equation in the modern calculus of variations), and chapter 4 takes up the problem of its invariance, but Euler did not recognize that the equation was insufficient to guarantee an extremum. As was typical for Euler, he gave some hundred examples to illustrate its results.

"In 'De curvis elasticis,' the first of two significant appendixes to the Methodus inveniendi that Daniel Bernoulli had proposed in a series of letters, Euler – over sixty-six pages with ninety-seven problems – presented the earliest study in print to employ the calculus of variations to solve problems in the theory of elastic curves and surfaces. It is thus the initial general tract on the mathematical theory of elasticity. To obtain his equations, Euler employed the methods of final causes and efficient causes; these came initially from Aristotle, and among others Leibniz had recently studied them. Final causes are teleological, giving the purpose or design of something and contrast with mechanical explanations, which Euler



METHODUS
INVENIENDI
LINEAS CURVAS
Maximi Minimive proprietate gaudentes,
SIVE
SOLUTIO
PROBLEMATIS ISOPERIMETRICI
LATISSIMO SENSU ACCEPTI.
AUCTORE
LEONHARDO EULERO,
Professore Regio, & Academiæ Imperialis Scientiarum PETROPOLITANÆ Socio.

LAUSANNÆ & GENEVÆ,
Apud MARCUM-MICHAELEM BOUSQUET & Socios.
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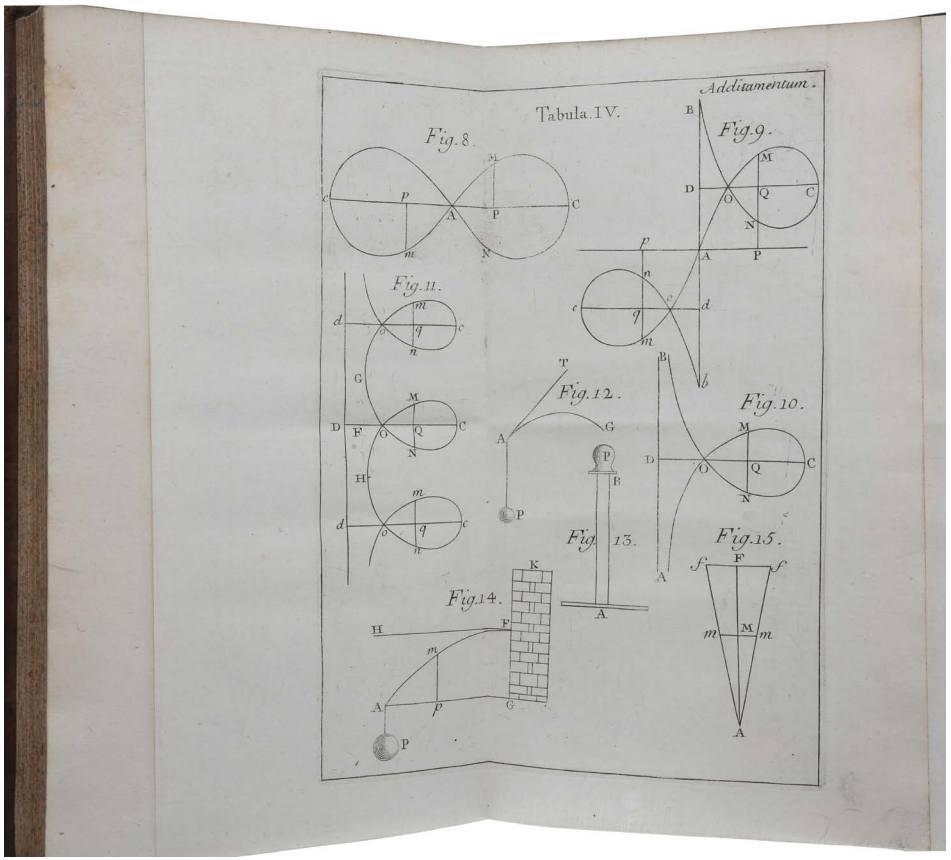
believed to draw upon existing variational principles. Closest to the modern definition of the term cause, efficient causes probe the properties of matter and mechanics explaining phenomena. Euler computed the shape of elastica from the forces of efficient causes, and checked to confirm that both approaches led to the same answer; without an appeal to both, the best explanations might not be reached. In his inventory of problems, Euler enumerated nine species of elastic curves and explained how elastic bands bend and oscillate.

"The appendix's topics include the problem of the vibrating membrane, at the same time that Daniel Bernoulli was investigating the simpler vibrating string. Euler's buckling formula first appears here; it determines the maximum critical load, now called the first elastostatic eigenvalue, which an ideal, slender, long rod pinned at its ends can carry before it buckles. The critical axial load applied at its center of gravity needed to bend the rod depends upon the stiffness of its material and how the rod is supported at its ends, and it is proportional to the inverse square of the length of the rod. Euler also computed elastokinetic eigenvalues, eigenfrequencies of oscillations of the rod's transversal, and associated eigenfunctions, giving the shapes of a deformed rod.

"The other appendix, the ten-page 'De motu projectorum in medio non resistente, per methodum maximorum ac minimorum determinando' (On the motion of bodies in a non-resisting medium determined by the method of maxima and minima), introduces a general form of the principle of least action and experimentally determines absolute elasticity. His letters to Daniel Bernoulli show that by late 1738 Euler had mastered that principle" (ibid., pp. 223-7)

Dibner 111; Evans 9; Horblit 28; Norman 731; Sparrow 60. Craig G. Fraser in: *Landmarks Writings in Western Mathematics*, chapter 12; Cajori, *History of Mathematics*; Calinger, Leonhard Euler. *Mathematical Genius of the Enlightenment; Du Pasquier, Léonard Euler et ses amis*; Kline, *Mathematical Thought from Ancient to Modern Times*; Roberts & Trent, *Bibliotheca Mechanica*, p. 104: "For the purposes of mechanics, the significance of this work lies in the appendix, which deals with geometrical forms of elastic curve … The present work illustrates the first solution to the problem of the buckling of a column."



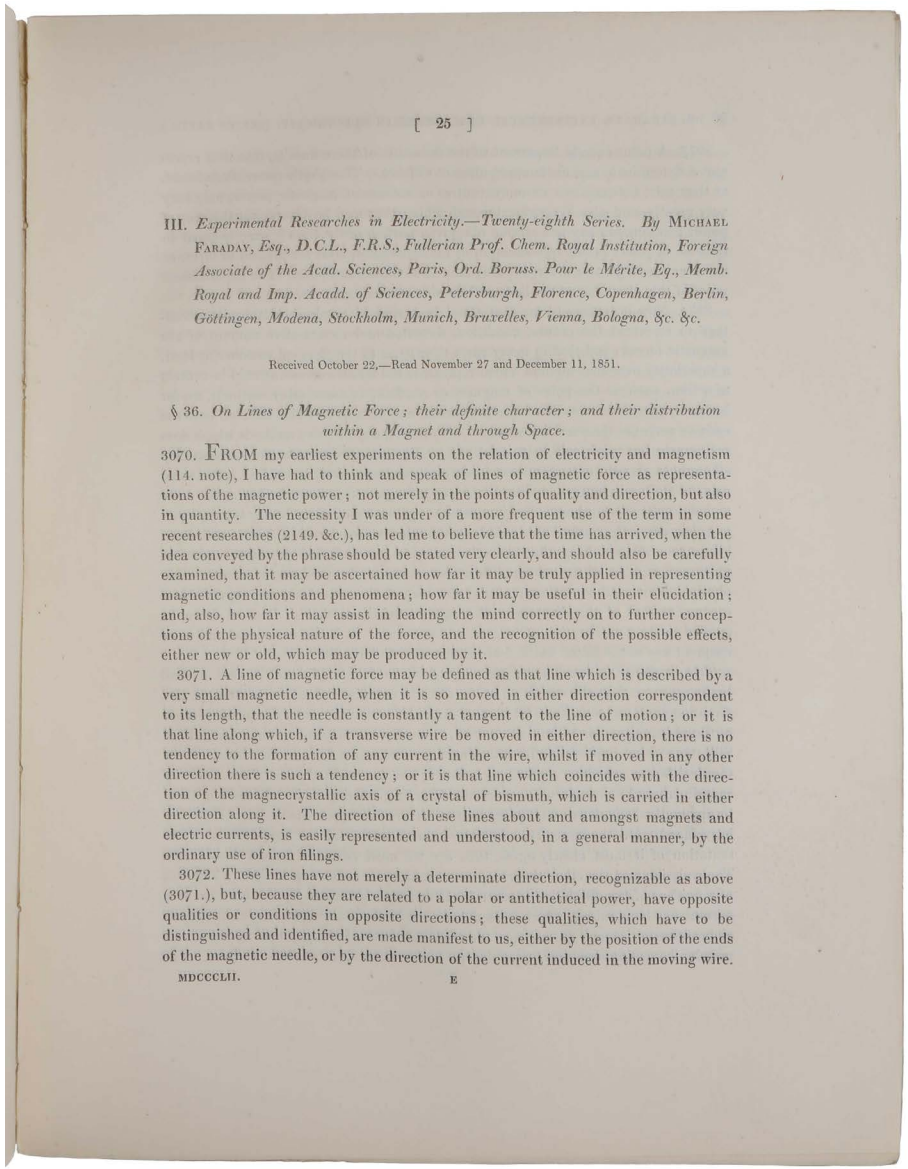
LINES OF FORCE

FARADAY, Michael. *Experimental researches in electricity – twenty-eight series. On the Lines of Magnetic Force; their definitive character; and their distribution within a Magnet and through Space. [With:] Ibid. – twenty-ninth series. On the employment of the Induced Magneto-electric Current as a test and measure of Magnetic Forces.* London: Richard Taylor and William Francis, 1852.

$2,800

In: Philosophical Transactions, Vol. 142, Part I, pp. xii, 206 [2:blank] and 10 plates. 4to (301 x 231 mm) spine strip with a little wear, 7cm closed tear to lower part of front hinge. Uncut and unopned. Custom cloth box.

First edition, journal issue, of these two papers containing Faraday's detailed investigations of the nature of the 'lines of force' he had proposed in his first paper on electromagnetism, 'On some new electro-magnetical motions, and on the theory of magnetism', originally published in the 21 October 1821 issue of the Quarterly Journal of Science. These investigations laid the foundations of field theory. "Faraday's work on electromagnetic rotations led him to take a view of electromagnetism different from that of most of his contemporaries. Where they focused on the electrical fluids and the peculiar forces engendered by their motion (Ampère's position), he was forced to consider the line of force. He did not know what it was in 1821, but he suspected that it was a state of strain in the molecules of the current carrying wire and the surrounding medium produced by the passage of an electrical "current" (whatever that was) through the wire … It was the line of force which tied all his researches on electricity and magnetism together" (DSB). This volume contains the 28th and 29th series of the 30 series of



[25]

III. *Experimental Researches in Electricity.—Twenty-eighth Series. By* Michael Faraday, *Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, Foreign Associate of the Acad. Sciences, Paris, Ord. Boruss. Pour le Mérite, Eq., Memb. Royal and Imp. Acadd. of Sciences, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, Munich, Bruxelles, Vienna, Bologna, &c. &c.*

Received October 22,—Read November 27 and December 11, 1851.

§ 36. *On Lines of Magnetic Force; their definite character; and their distribution within a Magnet and through Space.*

3070. From my earliest experiments on the relation of electricity and magnetism (114. note), I have had to think and speak of lines of magnetic force as representations of the magnetic power; not merely in the points of quality and direction, but also in quantity. The necessity I was under of a more frequent use of the term in some recent researches (2149. &c.), has led me to believe that the time has arrived, when the idea conveyed by the phrase should be stated very clearly, and should also be carefully examined, that it may be ascertained how far it may be truly applied in representing magnetic conditions and phenomena; how far it may be useful in their elucidation; and, also, how far it may assist in leading the mind correctly on to further conceptions of the physical nature of the force, and the recognition of the possible effects, either new or old, which may be produced by it.

3071. A line of magnetic force may be defined as that line which is described by a very small magnetic needle, when it is so moved in either direction correspondent to its length, that the needle is constantly a tangent to the line of motion; or it is that line along which, if a transverse wire be moved in either direction, there is no tendency to the formation of any current in the wire, whilst if moved in any other direction there is such a tendency; or it is that line which coincides with the direction of the magnecrystallic axis of a crystal of bismuth, which is carried in either direction along it. The direction of these lines about and amongst magnets and electric currents, is easily represented and understood, in a general manner, by the ordinary use of iron filings.

3072. These lines have not merely a determinate direction, recognizable as above (3071.), but, because they are related to a polar or antithetical power, have opposite qualities or conditions in opposite directions; these qualities, which have to be distinguished and identified, are made manifest to us, either by the position of the ends of the magnetic needle, or by the direction of the current induced in the moving wire.

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Faraday's Experimental Researches in Electricity, comprising sections 3070-3176 and 3177-3242, respectively.

"It was not until July of 1851 that Faraday was able to turn his attention fully to the investigation of the intimate nature of lines of force. In part this was due simply to the press of business and the fact that as he grew older he could no longer work as he had done in the 1850s. His mind, although still able to rise to great peaks of originality was, nevertheless, failing. His memory was increasingly bad and he found it ever more difficult to keep the object of his researches before him ... The character of his thought also changed at about this time. Before 1850 he had rather carefully hidden his theoretical ideas from the scientific world, using them to guide him from discovery to discovery. By 1850 the long string of discoveries that were to guarantee him immortality in the history of science had come to an end. Never again was he to startle the learned world with some new effect which few, if any, of his colleagues had suspected but which he had deduced from his own hypotheses. The decade of the 1850s, rather, was to be spent in the exposition and defence of his theories. He was not, of course, prepared to abandon experiment but his experiments were now overtly the ammunition with which he supported theoretical positions taken up publicly and in print. His purpose was nothing less than to supply a general view of the modes of action of force. Central to this view was the physical reality of the lines of force.

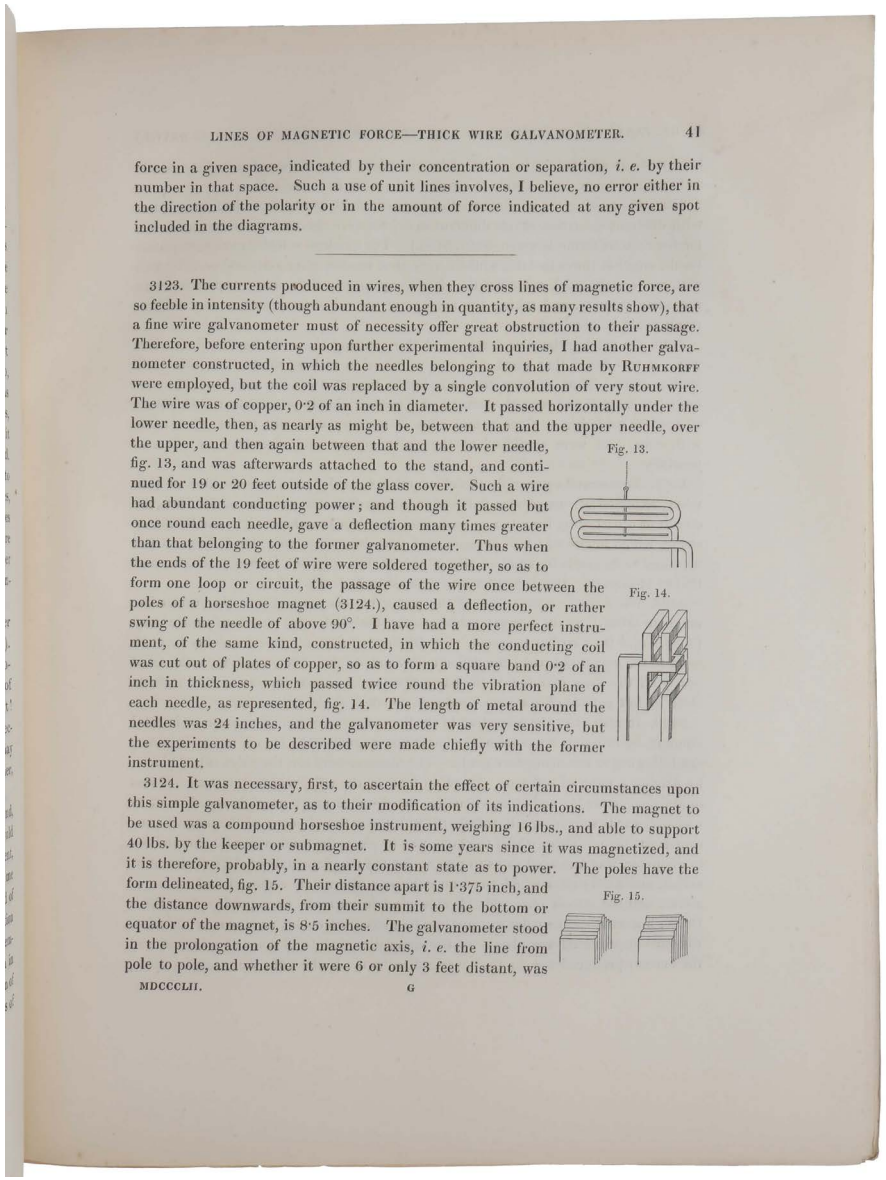
"The basic question to which Faraday turned in the summer of 1851 concerned the interpretation of the pattern made by iron filings sprinkled on a card over a magnet. The filings arranged themselves in lines; were these lines 'real' or were they merely the result of the interaction of the magnet and the iron filings? Faraday had long viewed them as strains of some sort but it was now time to discover their true nature. If strains, to what were they connected so that the strain could be imposed along the line of force? The electrostatic line of force was firmly anchored in electrically excited matter and the strain, transmitted along the curves of the intervening polarized particles, ended in positively and negatively charged surfaces. An electrostatic line of force could start in a charged sphere and leap across a room to the wall. If the sphere were positively charged, the part of the wall where the line of force ended would be negative. The line, and the particles in between were all polar having 'positive' and 'negative' ends. Magnetic lines were peculiar in that they always returned to the body from which they emanated. It was impossible to hold up a sphere 'charged' with north magnetism and trace a line of magnetic force across a room to a south pole on the wall. Wherever a north pole existed, a south was also to be found, nearby, in the same body. The ends of the line of force, then, had to be the poles of the magnet. This was where the strain originated; here must be where the original tension was applied.

"When examined critically this explanation made little sense. An iron magnet was, after all, relatively homogeneous. Why, then, should two particular spots, indistinguishable from other places, become poles? Why, to put it another way, should the lines of force terminate at all? From 1845 to 1850 Faraday had gradually convinced himself that the actual particles of magnetic or diamagnetic substances counted for very little in magnetic phenomena. Why, then, call in particles merely to have an anchor for the lines of force? Could not poles be dispensed with altogether?

"The first thing that had to be done was to make certain that the lines of force really existed independently of the iron filings that illustrated their forms so beautifully. Since iron itself was magnetic, it was possible that the magnetic curves might be the result of placing iron filings over a magnet and that when the filings were not present, the curves vanished. The use of a compass needle was open to the same objections. If the lines of force were created by the interaction of the needle and the magnet, the needle would still trace them out as if the lines existed independently of the needle. One method alone appeared free from fault. A conducting wire in the presence of a magnet showed no effect; when the wire was moved across the

lines of force, a current was generated. The moving wire involved no attraction, repulsion, or other polar effects. The lines of force detected by this method would, therefore, not appear to be created by the presence of the wire. 'So,' Faraday concluded, 'a moving wire may be accepted as a correct philosophical indication of the presence of magnetic force' (3083).

"The existence of the lines of force gave no hints about their essential properties. Were they continuous curves, or were they actually attached to points in the magnet called poles? If they were continuous curves, then the lines of force ought to pass through the magnet as well as around it in the external medium. Could these lines be detected inside the magnet? Faraday devised a very simple apparatus for this purpose. Two bar magnets were placed side by side with similar poles next to one another. The two magnets were separated by a thin piece of wood, reaching from the middle of the magnets to one end. The two magnets were then placed in a wooden axle so that they could be rotated about their mutual axis. A copper collar was then placed around the magnets at their middle. A loop of wire could now be arranged so as to make contact with the collar at one end and with a galvanometer at the other. Another wire ran from the galvanometer, down the groove left between the two magnets, and then up to the collar. Each element in the apparatus could be rotated separately; the two magnets around their mutual axis, the wire running down the centre on its axis, and the loop of copper wire around an axis more or less coincident with the extension of the magnetic axis. With this apparatus, Faraday could hope to detect lines of force if they ran through the magnet as well as through the medium in which the magnet was immersed. He first repeated the experiments he had done in 1832 with the rotating magnet to be certain that the lines of force did not rotate with the magnet. 'No mere rotation of a bar magnet on its axis, produces any induction effect on circuits exterior to it', he reported. 'The system of power about the magnet must not be considered as necessarily revolving with the magnet, any more than the rays of light which emanate from



LINES OF MAGNETIC FORCE—THICK WIRE GALVANOMETER. 41

force in a given space, indicated by their concentration or separation, *i. e.* by their number in that space. Such a use of unit lines involves, I believe, no error either in the direction of the polarity or in the amount of force indicated at any given spot included in the diagrams.

3123. The currents produced in wires, when they cross lines of magnetic force, are so feeble in intensity (though abundant enough in quantity, as many results show), that a fine wire galvanometer must of necessity offer great obstruction to their passage. Therefore, before entering upon further experimental inquiries, I had another galvanometer constructed, in which the needles belonging to that made by RUHMKORFF were employed, but the coil was replaced by a single convolution of very stout wire. The wire was of copper, 0·2 of an inch in diameter. It passed horizontally under the lower needle, then, as nearly as might be, between that and the upper needle, over the upper, and then again between that and the lower needle, fig. 13, and was afterwards attached to the stand, and continued for 19 or 20 feet outside of the glass cover. Such a wire had abundant conducting power; and though it passed but once round each needle, gave a deflection many times greater than that belonging to the former galvanometer. Thus when the ends of the 19 feet of wire were soldered together, so as to form one loop or circuit, the passage of the wire once between the poles of a horseshoe magnet (3124.), caused a deflection, or rather swing of the needle of above 90°. I have had a more perfect instrument, of the same kind, constructed, in which the conducting coil was cut out of plates of copper, so as to form a square band 0·2 of an inch in thickness, which passed twice round the vibration plane of each needle, as represented, fig. 14. The length of metal around the needles was 24 inches, and the galvanometer was very sensitive, but the experiments to be described were made chiefly with the former instrument.

Fig. 13.

Fig. 14.

3124. It was necessary, first, to ascertain the effect of certain circumstances upon this simple galvanometer, as to their modification of its indications. The magnet to be used was a compound horseshoe instrument, weighing 16 lbs., and able to support 40 lbs. by the keeper or submagnet. It is some years since it was magnetized, and it is therefore, probably, in a nearly constant state as to power. The poles have the form delineated, fig. 15. Their distance apart is 1·375 inch, and the distance downwards, from their summit to the bottom or equator of the magnet, is 8·5 inches. The galvanometer stood in the prolongation of the magnetic axis, *i. e.* the line from pole to pole, and whether it were 6 or only 3 feet distant, was

Fig. 15.

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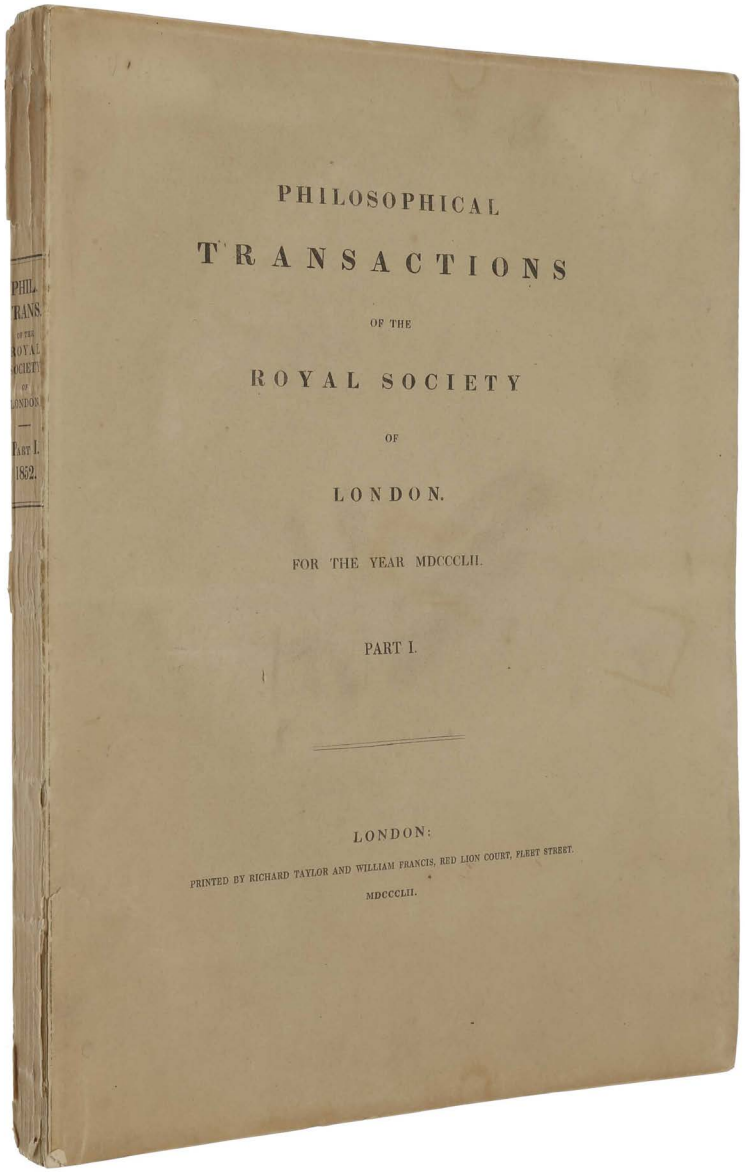
the sun are supposed to revolve with the sun' (3090). The conclusion that the lines of force did not move with the magnet reinforced the idea that they were, in a sense, independent of the magnet. This independence must also exist within the magnet. Such independence now could easily be shown. The power of a magnet could be measured precisely in terms of the current generated in a wire cutting the lines of force. Faraday clearly showed that the current (or, better, in modern terms, the electromotive force) directly proportional to the number of lines cut. When all the lines of force were cut, no matter whether the cut was perpendicular or oblique to the lines, the current in the detecting wire was the same (3109-3114). 'The quantity of electricity thrown into a current is directly as the amount of curves intersected' (3113). Knowing this, the existence of the lines of force within the magnet could be determined with great precision. 'there exists lines of force within the magnet, of the same nature as those without. What is more, they are exactly equal in amount to those without. They have a relation in direction to those without; and in fact are continuations of them, absolutely unchanged in their nature, so far as the experimental test can be applied to them. Every line of force therefore, at whatever distance it may be taken from the magnet, must be considered as a closed circuit, passing in some part of its course through the magnet, and having an equal amount of force in every part of its course' (3116-7).

The implications … were literally revolutionary. If Faraday were correct and the lines of force did actually exist with the properties he attributed to them, then the whole structure of orthodox electric and magnetic science must come tumbling down. The orthodox theories were founded upon central forces acting inversely as the square of the distance; Faraday's new theory rejected central forces. The polarity that was the necessary complement of central forces had been banished. There was no polarity exclusive of the line of force and even this polarity was an odd one … polarity was the direction of the line of force, and as such, it was a polarity without poles. Since attraction and repulsion must be attraction to or a repulsion from some point (which then could be considered a pole) Faraday explicitly rejected attraction and repulsion as real magnetic phenomena. Not only did his work on magnetic conduction contradict the older forms of attraction and repulsion, but these older ideas were now capable of preventing further progress by blinding men to new approaches. 'To assume that pointing is always the direct effect of attractive and repulsive forces acting in couples (as in the cases in question, or as in bismuth crystals), is to shut out ideas, in relation to magnetism, which are already applied in the theories of the nature of light and electricity; and the shutting out of such ideas may be an obstruction to the advancement of truth and a defence of wrong assumptions and error' (3156).

"There is no doubt that Faraday knew exactly how unorthodox he was and that his ideas were bound to meet with opposition. He knew, too, from which quarter the opposition would come. Hence his insistence upon the experimental aspect of his theory. 'I keep working away at Magnetism,' he wrote to Schoenbein, 'whether well or not I will not say. It is at all events to my own satisfaction. Experiments are beautiful things and I quite revel in the making of them. Besides they give one such confidence and, as I suspect that a good many think me somewhat heretical in magnetics or perhaps rather fantastical, I am very glad to have them to fall back upon.' The mathematical physicist was unlikely to reject the simplicity of the inverse square law for anything so distinctly unmathematical as the lines of force. It was to this point that Faraday addressed himself in what may well be called the credo of the experimentalist. 'As an experimentalist', he wrote, 'I feel bound to let experiment guide me into any train of thought which it may justify; being satisfied that experiment, like analysis, must lead to strict truth if rightly interpreted; and believing also, that it is in its nature far more suggestive of new trains of thought and new conditions of natural power (3159). Experiment and his own theories had led him to the physical reality of the lines of force. It was with considerable hesitancy, however, that he presented his new conclusions on the nature of the lines of force at the end of the Twenty-eighth Series.

'Whilst writing this paper I perceive, that, in the late Series of these Researches, Nos. XXV, XXVI, XXVII, I have sometimes used the term lines of force so vaguely, as to leave the reader doubtful whether I intended it as a merely representative idea of the forces, or as the description of the path along which the power was continuously exerted. What I have said in the beginning of this paper … will render that matter clear. I have as yet found no reason to wish any part of those papers altered, except these doubtful expressions; but that will be rectified if it be understood, that, wherever the expression line of force is taken simply to represent the disposition of the forces, it shall have the fullness of that meaning; but that wherever it may seem to represent the idea of the physical mode of transmission of the force, it expresses in that respect the opinion to which I incline at present. The opinion may be erroneous, and yet all that relates or refers to the disposition of the force will remain the same' (3175).

"It was not until 1852 that Faraday insisted upon the reality of the lines of force. In his paper 'On the Physical Character of the Lines of Force', he informed the reader that 'I am now about to leave the strict line of reasoning for a time, and enter upon a few speculations respecting the physical character of the lines of force, and the manner in which they may be supposed to be continued through space' (3243). There can be no doubt that Faraday was firmly convinced that the lines of force were real. The fact that the magnetic force was transmitted along curves, and that these curves were continuous was evidence enough for him. 'I cannot conceive curved lines of force without the conditions of a physical existence in that immediate space' (3258). The reality of the physical lines of force was thus established. But this reality immediately raised a new question. How was the magnetic force transmitted through the lines of force? The search for an answer to this question led Faraday to the foundations of field theory" (Pierce Williams, *Michael Faraday*, pp. 444-450).



PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCLII.

PART I.

LONDON:

PRINTED BY RICHARD TAYLOR AND WILLIAM FRANCIS, RED LION COURT, FLEET STREET.

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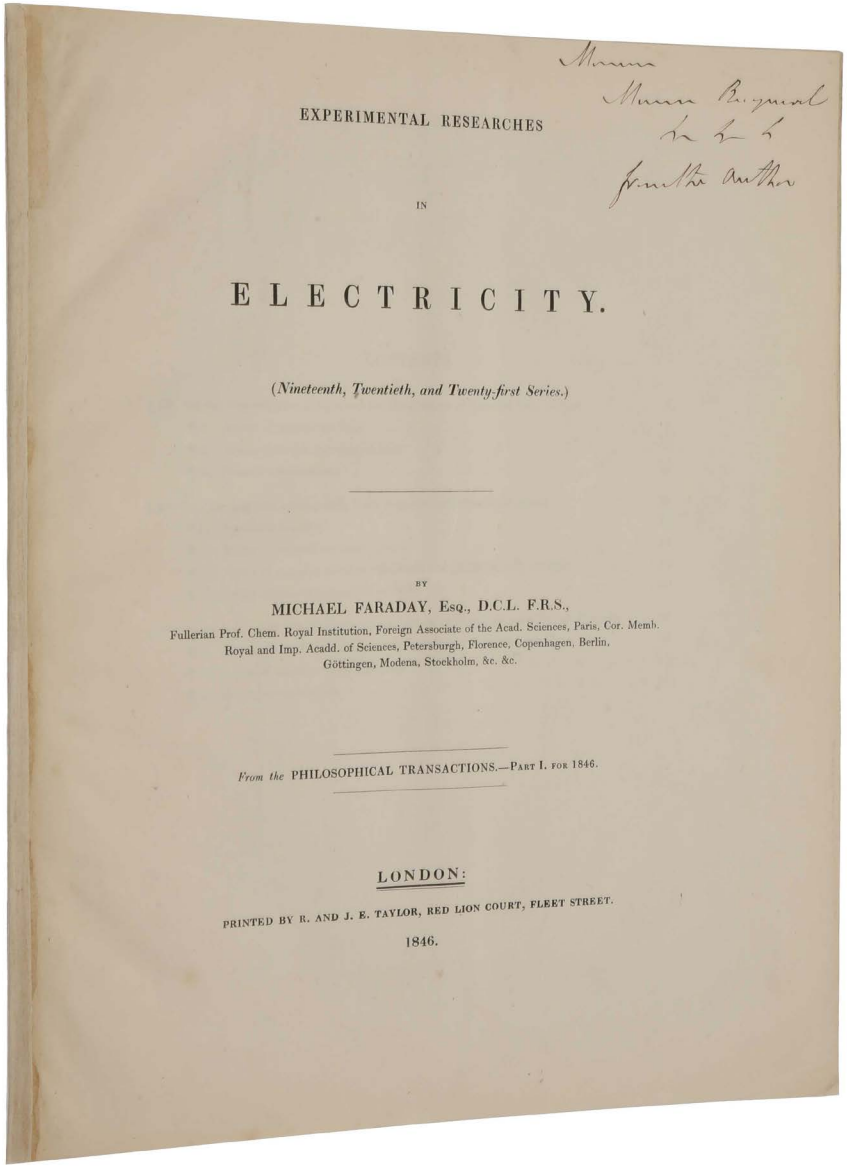
FARADAY, Michael. *Experimental researches in electricity (Nineteenth, Twentieth and Twenty-first Series.) On the magnetization of light and the illumination of magnetic lines of force. On new magnetic actions, and on the magnetic condition of all matter. On new magnetic actions, and on the magnetic condition of all matter – continued.* London: R. & J. E. Taylor, 1846.

$14,500

Offprint from: Philosophical Transactions, Vol. 136, Part I. 4to, pp. [4], 62. Original printed wrappers, spine reinforced with paper, contained in a cloth folding case.

First edition, very rare inscribed presentation offprint, of these three papers containing two of Faraday's major discoveries: the 'Faraday effect,' i.e., the effect of magnetism on the plane of polarisation of light (19th series); and 'diamagnetism' (20th and 21st series). These were "the last, and in many ways the most brilliant, of Faraday's series of researches" (DSB). In August 1845 William Thomson (later Lord Kelvin) suggested to Faraday that electricity might affect polarized light. In fact, Faraday had been searching for this effect since the 1820s, but without success. Faraday realized, however, that the force of an electromagnet was far stronger and might therefore be able to produce a measurable effect. "On 13 September 1845 his efforts finally bore fruit. The plane of polarization of a ray of plane-polarized light was rotated when the ray was passed through a glass rhomboid of high refractive index in a strong magnetic field. The angle of rotation was directly proportional to the strength of the magnetic force and, for Faraday, this indicated the direct effect of magnetism upon light. "That which is magnetic in the forces of matter" he wrote "has been affected, and in turn has affected that



EXPERIMENTAL RESEARCHES

IN

ELECTRICITY.

(*Nineteenth, Twentieth, and Twenty-first Series.*)

BY

MICHAEL FARADAY, Esq., D.C.L. F.R.S.,

Fullerian Prof. Chem. Royal Institution, Foreign Associate of the Acad. Sciences, Paris, Cor. Memb. Royal and Imp. Acadd. of Sciences, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, &c. &c.

From the PHILOSOPHICAL TRANSACTIONS.—Part I. for 1846.

LONDON:

PRINTED BY R. AND J. E. TAYLOR, RED LION COURT, FLEET STREET.

1846.

which is truly magnetic in the force of light" The fact that the magnetic force acted through the mediation of the glass suggested to Faraday that magnetic force could not be confined to iron, nickel, and cobalt but must be present in all matter. No body should be indifferent to a magnet, and this was confirmed by experiment. Not all bodies reacted in the same way to the magnetic force. Some, like iron, aligned themselves along the lines of magnetic force and were drawn into the more intense parts of the magnetic field. Others, like bismuth, set themselves across the lines of force and moved toward the less intense areas of magnetic force. The first group Faraday christened 'paramagnetic,' the second, 'diamagnetic'" (DSB). OCLC lists copies of this offprint at Burndy, Huntington and North Carolina State. No copies in auction records.

Provenance: Alexandre-Edmond Becquerel (1820-91) (presentation inscription in Faraday's hand on upper wrapper). "Becquerel's most important achievements in science were in electricity, magnetism, and optics. In electricity he measured the properties of currents and investigated the conditions under which they arose. In 1843 he showed that Joule's law governing the production of heat in the passage of an electrical current applied to liquids as well as to solids. In 1844 he rectified Faraday's law of electrochemical decomposition to include several phenomena that had not been taken into account, and in 1855 he discovered that the mere displacement of a metallic conductor in a liquid was sufficient to produce a current of electricity" (DSB). In 1839, at age 19, Becquerel created the first photovoltaic cell, the technology behind modern solar panels.

"Throughout the spring and early summer of the same year [1845], with what little energy he had, Faraday continued his researches although there were many weeks when the Diary record is blank. Then, suddenly, on 30 August, there was the first entry on another attempt to discover the electrotonic state. This was to begin a period of feverish activity culminating in the discovery of the action of a magnetic field upon light and of diamagnetism …

"The stimulus for the renewal of old lines of research was the young William Thomson, future Baron Kelvin of Largs. Thomson, only 21 years old in 1845, was one of the few scientists who took Faraday's concept of the line of force seriously. Faraday had met him only a little time before and was greatly impressed with him. In 1845, Amadeo Avogadro sent Faraday a copy of an article on electrical theory which was far too mathematical for him. He turned it over to his young friend asking Thomson his opinion. On 6 August 1845, Thomson wrote Faraday a long letter in which he briefly summarized Avogadro's paper and then went on to tell Faraday of his researches … At the end of his letter, Thomson asked Faraday about experiments that Thomson thought ought to be performed, for his theory seemed to predict effects that had not yet been observed.

'I have long wished to know [Thomson wrote] whether any experiments have been made relative to the action of electrified bodies on the dielectrics themselves, in attracting them or repelling them, but I have never seen any described. Any attraction which may have been perceived to be exercised upon a nonconductor, such as sulphur, has always been ascribed to a slight degree of conducting power. A mathematical theory based on the analogy of dielectrics to soft iron would indicate attraction, quite independently of any induced charge (such, for instance, as would be found by breaking a dielectric and examining the parts separately). Another important question is whether the air in the neighbourhood of an electrified body, if acted upon by a force of attraction or repulsion, shows any signs of such forces by a change of density, which, however, appears to me highly improbable. A third question which, I think, has never been investigated, is relative to the action of a transparent dielectric on polarized light. Thus it is known that a very well defined action, analogous to that of a transparent crystal, is produced upon polarized light when transmitted through glass in any ordinary state of violent constraint. If the constraint, which may be elevated to be on the

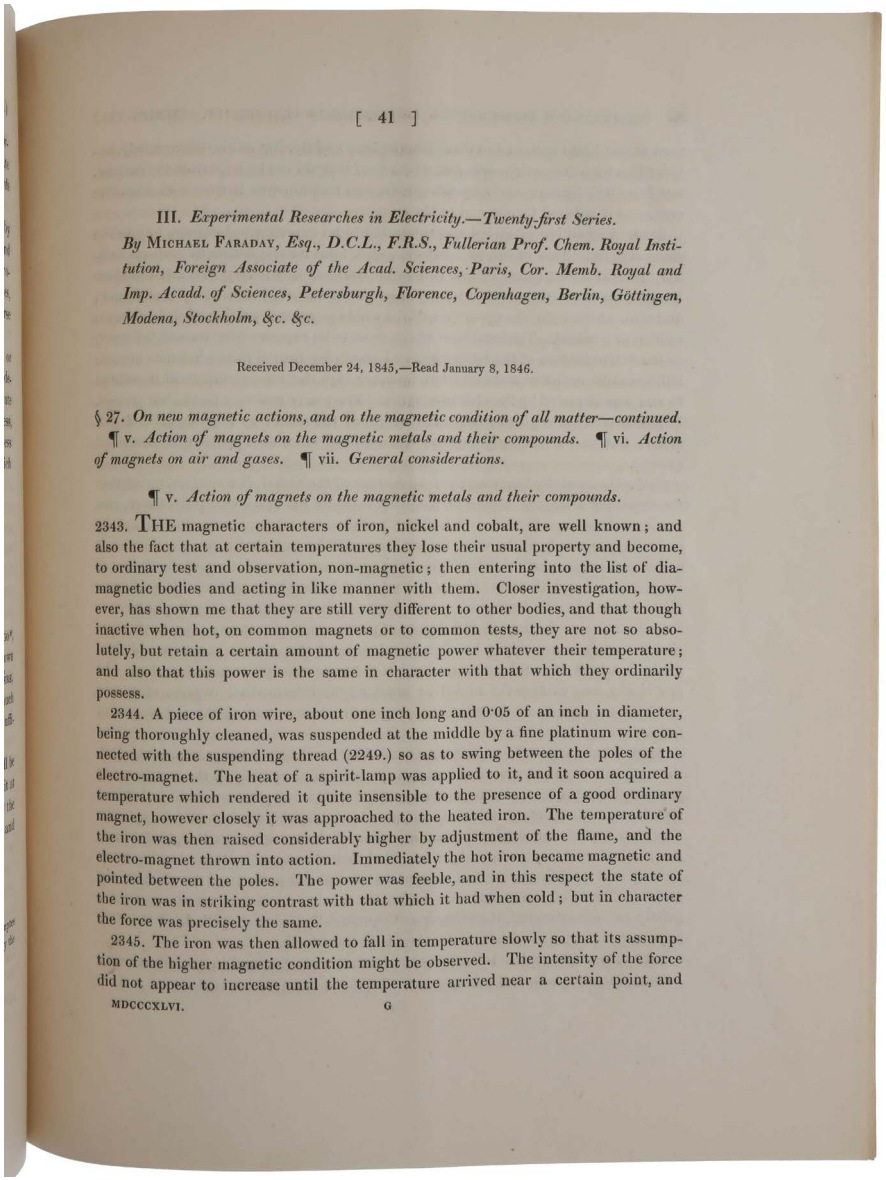
point of breaking the glass, be produced by electricity, it seems probable that a similar action might be observed.'

"All three of Thomson's queries are worthy of attention. The first clearly implied that dielectrics (and later, by analogy, diamagnetics) should mutually affect one another when transmitting the electric force — an effect in diamagnetics under magnetic influence that Faraday was to seek for in vain. The second, with magnetic substituted for electric force, was to lead Faraday to broad and general views of terrestrial magnetism and its variations. The third was, of course, the most important. It described an effect that Faraday had long sought for and was never to find. It was discovered by Dr. John Kerr in 1875. But, in August 1845, this passage seems to have stimulated Faraday to try once more to detect the strain that, for a quarter of a century, he was convinced must exist in bodies through which an electric current was passing. As he wrote to Thomson:

'I have made many experiments on the probable attraction of dielectrics. I did not expect any, nor did I find any, and yet I think that some particular effect (perhaps not attraction or repulsion) ought to come out when the dielectric is not all of the same inductive capacity, but consists of parts having different inductive capacity.

'I have also worked much on the state of the dielectric as regards polarized light, and you will find my negative results at paragraphs 951-955 of my Experimental Researches. I purpose resuming this subject hereafter. I also worked hard upon crystalline dielectrics to discover some molecular conditions in them (see par. 1688 etc. etc.) but could get no results except negative. Still I Firmly believe that the dielectric is in a peculiar state whilst induction is taking place across it' …

"There had to be an effect! Faraday's whole theory of electrolysis and induction was based upon the creation of an intermolecular strain in substances through which



[41]

III. *Experimental Researches in Electricity.—Twenty-first Series.*
By Michael Faraday, *Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, Foreign Associate of the Acad. Sciences, Paris, Cor. Memb. Royal and Imp. Acadd. of Sciences, Petersburgh, Florence, Copenhagen, Berlin, Göttingen, Modena, Stockholm, &c. &c.*

Received December 24, 1845,—Read January 8, 1846.

§ 27. *On new magnetic actions, and on the magnetic condition of all matter—continued.* ¶ v. *Action of magnets on the magnetic metals and their compounds.* ¶ vi. *Action of magnets on air and gases.* ¶ vii. *General considerations.*

¶ v. *Action of magnets on the magnetic metals and their compounds.*

2343. The magnetic characters of iron, nickel and cobalt, are well known; and also the fact that at certain temperatures they lose their usual property and become, to ordinary test and observation, non-magnetic; then entering into the list of diamagnetic bodies and acting in like manner with them. Closer investigation, however, has shown me that they are still very different to other bodies, and that though inactive when hot, on common magnets or to common tests, they are not so absolutely, but retain a certain amount of magnetic power whatever their temperature; and also that this power is the same in character with that which they ordinarily possess.

2344. A piece of iron wire, about one inch long and 0·05 of an inch in diameter, being thoroughly cleaned, was suspended at the middle by a fine platinum wire connected with the suspending thread (2249.) so as to swing between the poles of the electro-magnet. The heat of a spirit-lamp was applied to it, and it soon acquired a temperature which rendered it quite insensible to the presence of a good ordinary magnet, however closely it was approached to the heated iron. The temperature of the iron was then raised considerably higher by adjustment of the flame, and the electro-magnet thrown into action. Immediately the hot iron became magnetic and pointed between the poles. The power was feeble, and in this respect the state of the iron was in striking contrast with that which it had when cold; but in character the force was precisely the same.

2345. The iron was then allowed to fall in temperature slowly so that its assumption of the higher magnetic condition might be observed. The intensity of the force did not appear to increase until the temperature arrived near a certain point, and

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an electric current passed. Perhaps the fault lay with his approach. The 'tension' (i.e. voltage) created by a galvanic apparatus was small; would not the much higher 'tensions' produced by static electricity be more effective in throwing the particles of a dielectric into the electrotonic state? A piece of glass was placed between the terminals of an electrostatic machine and polarized light was passed through it in various directions. Once again no effect was detected … The temptation to quit and to disavow the electrotonic state once again must have been strong. Yet, the hypothesis had served him so well, and Thomson's independent reasonings supported his own so closely that it almost seemed impossible that this state did not exist. Writing to his old friend, Sir John Herschel, he later declared, 'It was only the very strongest conviction that Light, Mag[netism] and Electricity must be connected that could have led me to resume the subject and persevere through much labour before I found the key.'

"There was one final experimental path still to be explored. Perhaps even with electrostatic tension, the forces involved were too small to be easily detected. Even a highly charged electrical body could hold only a small weight suspended from it. Compare this to electromagnets which could hold masses of hundreds of pounds in their power. From the patterns shown by iron filings, it was obvious that the magnetic power was exerted in curved lines. In the case of electrostatic induction Faraday had argued that the fact that induction took place along curved lines implied that the transmission of force was from particle to particle. The intermolecular strain thus created was the electrotonic state. Surely the curves of the magnetic lines of force implied equally a 'magneto-tonic' state and, since the magnetic power could be multiplied almost at will by the use of electromagnets, this state might be detectable where the electrotonic state was not.

"On 13 September 1845, Faraday began to work with electromagnets. Again his efforts were unavailing. The magnet had no effect on polarized light when passed through flint glass, rock crystal, or calcareous spar. There were, in the laboratory, pieces of the heavy glass that Faraday had made back in 1830 for the Royal Society. This glass had an extraordinarily high refractive index, indicating that it acted powerfully upon light. Given the correlation of forces in which Faraday believed so strongly, should this substance not, perhaps, also be acted upon magnetically in such a way as to affect the plane of polarized light of a ray passing through it. The experiment was easily performed and, finally, the expected result was observed …

"Faraday threw himself with all his energy into an intensive examination of the new effect. Once again he followed the same course as he had with other new effects. All possible combinations of factors were tried so that a simple law of action could be established. The magnetic poles were placed in every conceivable position relative to one another; simple current-carrying helices were substituted for iron-core electromagnets; all the common transparent laboratory substances were substituted for the heavy glass and the effect observed. The thickness of the heavy glass was increased by putting a number of polished pieces together. From these experiments, he was able to draw a number of general laws which he stated in the published paper which made up the Nineteenth Series of the Experimental Researches in Electricity …

"There were two puzzling things about the new mode of action of matter. Why, if a state of tension were created by a magnet, did not the state of tension interact with the magnet to create attractions or repulsions of the diamagnetic? And why, if the state were analogous to the electrotonic state, were gases unaffected? These were the questions which Faraday now set out to answer, firmly believing that there must be interaction between diamagnetics and magnets, and that gases could not be exempt from what must be a universal force of nature …

"It was not until 4 November that success was achieved. Again Faraday's persistence should be noted in the face of repeated failures. There had to be an

interaction, for such an interaction was a necessary consequence of his theory. Failure, therefore, only meant that the experimental set-up was not appropriate to detect the effect, and not that the effect did not exist.

'The bar of heavy glass … was suspended by cocoon silk in a glass jar on principle as before … and placed between the poles of the last magnet … When it was arranged and had come to rest, I found I could affect it by the Magnetic forces and give it position; thus touching dimagnetics [sic] by magnetic curves and observing a property quite independent of light, by which also we may probably trace these forces into opaque and other bodies, as the metals, etc. The nature of the affection was this. Let N and S represent the poles and G the bar of heavy glass … Then on making N and S active by the Electric current, G traversed not so as to point between N and S but across them, and when the current was stopped the glass returned to its first position. Next arranged the glass when stationary, then put on power, and now it moved in the contrary direction to take up cross position as before; so that the end which before went to the left-hand now went to the right, that being the neutral or natural condition' …

"Faraday did not know it at the time but he was by no means the first person to observe this effect. Brugmans first observed the repulsion of bismuth by a magnet in 1778. Coulomb appears to have seen a needle of wood set itself across a magnetic field; Edmond Becquerel [the recipient of the present offprint] reported the effect on wood in 1827, the same year that le Baillif published a paper on the magnetic repulsion of bismuth and antimony. In 1828 Seebeck reported the same effect with other substances. Faraday's success was not, therefore, the result of exceptional experimental skill. That the discovery of the class of diamagnetics is always associated with Faraday's name is due to the fact that he knew what to do with the discovery whereas the others did not. Commenting on le Baillif's paper on the magnetic repulsion of bismuth and antimony, Faraday remarked, 'It is astonishing that such an experiment has remained so long without further results.' There was, however, nothing really astonishing about it. None of those who observed the effect before Faraday had any room for it in their theories of magnetic action. Magnets either attracted or repelled — they did not set bodies on edge. If they did, it was an anomaly that had to be explained away, not explained. Faraday, however, had already recognized that the new diamagnetic force was a rather odd one. Therefore, although the setting of his glass across the lines of magnetic force was peculiar, it was completely consonant with the peculiarity of diamagnetic action in general.

"Faraday, once more, followed his usual experimental procedure. Having found a new effect, he set out to see how general it was. Everything from glass to foolscap paper, from litharge to raw meat was suspended between the poles of his powerful electromagnet. In the Twentieth Series, he listed over fifty substances exhibiting diamagnetic properties …

"Diamagnetism, Faraday seemed to be saying, was not a rare and exotic thing but connected intimately with the very marrow of our being. It was magnetism which was the exception and diamagnetism that was the rule. Surely such a power, Faraday was convinced, could not help but play a major role in the overall economy of nature …

"The one area in which neither magnetism nor diamagnetism appeared to intrude was that of the gases. To Faraday, this was impossible. These were basic forces of matter and, even given the exceptional properties of gases which seemed to set them apart from other species of matter, they still must share in its fundamental properties. His work on the condensation of gases, and especially his study of the critical point, had convinced him that there was a basic continuity between the liquid and the gaseous state. If liquids exhibited magnetic and diamagnetic properties, then gases could not be indifferent to the power of the magnet.

"In 1845 the action of gases in a magnetic field eluded Faraday. No matter how he tried, he could detect no reaction whatsoever. The gases always occupied the zero position between magnetic and diamagnetic bodies. When they were compressed or when they were rarefied, they still registered 0° when polarized light was passed through them … There was a possible explanation of the failure of gases to respond to magnetic forces that Faraday suggested. Supposing all bodies really were magnetic as Coulomb had suggested. Since these bodies were immersed in an ocean of air, the difference in their reaction to a magnetic field might be caused by the magnetic properties of the air itself … This explanation had the attraction of both accounting for the peculiar action of gases and emphasizing the basic unity of magnetic action. It was an explanation to which Faraday would return some seven years later only to reject it. In 1845 there were seeming insuperable obstacles to its adoption. The gases still preserved their uniqueness by not acting upon a polarized ray of light when in a magnetic field. Thus, the continuity assumed by the hypothesis was really illusory since the gases acted here neither like magnetics or diamagnetics, each of which class of substances did act upon a ray of polarized light. A more serious objection was that the rarefaction of gases had no effect whatsoever upon their magnetic action. Thus, by extrapolation, empty space would have magnetic properties and this strained credulity a bit too much. How, after all, could Nothing (which was what space was, by definition) have any properties? In 1845 Faraday recognized the difficulty and rejected the hypothesis he, himself, had introduced.

'Such a view [he admitted] also would make mere space magnetic, and precisely to the same degree as air and gases. Now though it may very well be, that space, air and gases, have the same general relation to magnetic force, it seems to me a great additional assumption to suppose that they are all absolutely magnetic, and in the midst of a series of bodies, rather than to suppose that they are in a normal or zero state. For the present, therefore, I incline to the former view, and consequently to the opinion that diamagnetics have a specific action antithetically distinct from ordinary magnetic action, and have thus presented us with a magnetic property new to our knowledge' (2440).

"Perhaps the most revolutionary of Faraday's ideas was to be the assignment of magnetic properties to empty space. In the 1850s he would quietly and without fuss or bother introduce the idea that empty space could transmit magnetic forces and must, therefore, itself be in a state of strain. Upon this idea, modern field theory was to be built" (Pierce Williams, *Michael Faraday*, pp. 382-394).

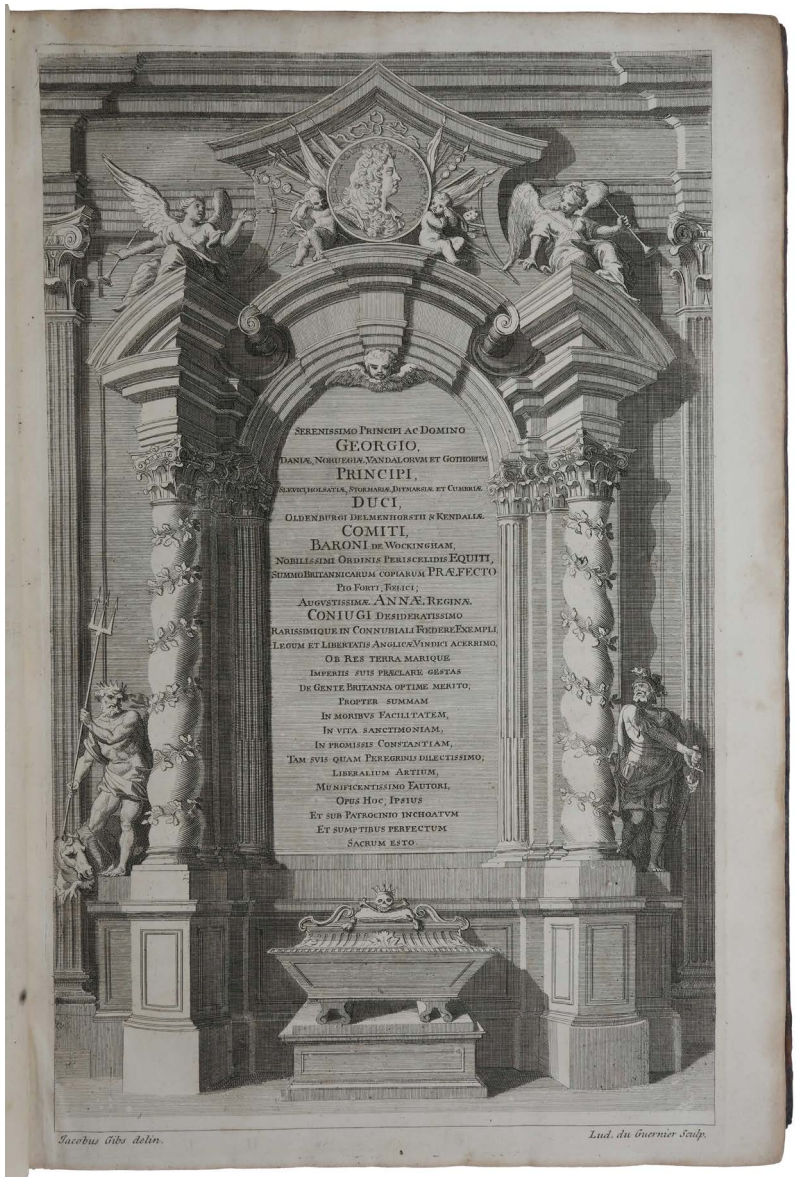
THE FOUNDATION WORK OF MODERN OBSERVATIONAL ASTRONOMY

FLAMSTEED, John. *Historiae coelestis libri duo: quorum prior exhibet catalogum stellarum fixarum Britannicum novum & locupletissimum, una cum earundem planetarumque omnium observationibus sextante, micrometro, &c. habitis; posterior transitus syderum per planum arcus meridionalis et distantias eorum a vertice complectitur. Observante Johanne Flamsteedio in Observatorio Regio Grenovicensi continua serie ab anno 1676 ad annum 1705 completum.* London: J. Matthews, 1712.

$185,000

Large folio (390 x 267 mm), pp. [vi], vi, 60; [1], 2-360, [2], 362, [1], 363-388, [1]; [2], 120, [2, errata], with four folding plates engraved by John Senex. Contemporary calf with gilt arms of Queen Anne in centre of each cover, some surface restoration ot the leather. Engraved and illustrated half title with author's portrait, signed by Juan Bautista Catenaro and George Vertue, following full-page dedication letter illustrated and engraved by Jacobus Gibs and Louis du Guernier.

The true first edition, extremely rare, of Flamsteed's catalogue of fixed stars and sextant observations, the foundation of modern observational astronomy. Flamsteed's catalogue was far more extensive and accurate than anything that had gone before. It was the first constructed with instruments using telescopic sights and micrometer eyepieces; Flamsteed was the first to study systematic errors in his instruments; he was the first to urge the fundamental importance of using clocks and taking meridian altitudes; and he insisted on having assistants to repeat the observations and the calculations. The catalogue contains about 3000 naked eye stars (Ptolemy and Tycho listed 1000, Hevelius 2000) with an accuracy



of about 10 seconds of arc. However, Flamsteed, although appointed Astronomer Royal in 1675, by the turn of the eighteenth century had still not published any of his observations. Isaac Newton and Edmond Halley pressed him to do so; Flamsteed's refusal led to one of the most famous, and bitterest, disputes in the history of astronomy, and to the present work being published against Flamsteed's will. Flamsteed's response, in 1716, was to destroy 300 of the 400 copies printed, so just a few years after publication no more than 100 copies survived. Flamsteed published his own, 'authorised', version of his star catalogue in 1725. ABPC/RBH list three copies: 1. Sotheby's, April 3, 1985, lot 287, £11,000; Bonham's, November 26, 1975, lot 171, £5,400; previously sold: Sotheby's, May 7, 1935, lot 98, £29 (Halley's annotated copy, lacking the star catalogue). 3. Sotheby's, May 7, 1935, lot 99, £10.10s (the present copy). OCLC lists 11 copies in the US.

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

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

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

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

"None of Newton's proposals found favour with Flamsteed. The offer in November 1694 'to gratify you to your satisfaction' brought the answer that he was not

tempted with 'covetousness' and the lament that Newton could have ever thought so meanly of him. An offer in 1695 to pay Flamsteed's scribe two guineas for his transcriptions brought an equally forthright rejection. It was enough, Newton was told, to offer 'verball acknowledgements'; a 'superfluity of monys', he found, 'is always pernicious to my Servants it makes them run into company and wast their time Idly or worse'. If Newton asked for 'your Observations only', Flamsteed complained of being treated like a drudge; if, however, calculations were asked for as well, Flamsteed would respond that such work required all kinds of tedious analysis for which he had little time …

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

"The two continued to see each other and to discuss Flamsteed's lunar observations until January 1699. This part of the correspondence ends with Flamsteed lecturing Newton on pride and humility. His own humility, he proudly told Newton, allowed him to 'excuse small faults in all mankind', and to 'bear great injurys without resentment'.

"The second stage of the dispute began on 11 April 1704 with a visit by Newton to Greenwich. Newton had yet to complete his lunar theory and could scarcely have looked forward to the prospect of another prolonged quarrel with Flamsteed. He seems to have decided to attempt to resolve the problem in a more direct manner.

Using his position as President of the Royal Society, and his connections at Court, he sought to pressurise Flamsteed into publishing his long-awaited catalogue, thus putting all his observations into the public domain. The approach was rejected. Newton, Flamsteed noted, was too obviously someone who 'would be my friend no further than to serve his own ends … spiteful, and swayed by those that were worse than himself' (Baily, p. 66).

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

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

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

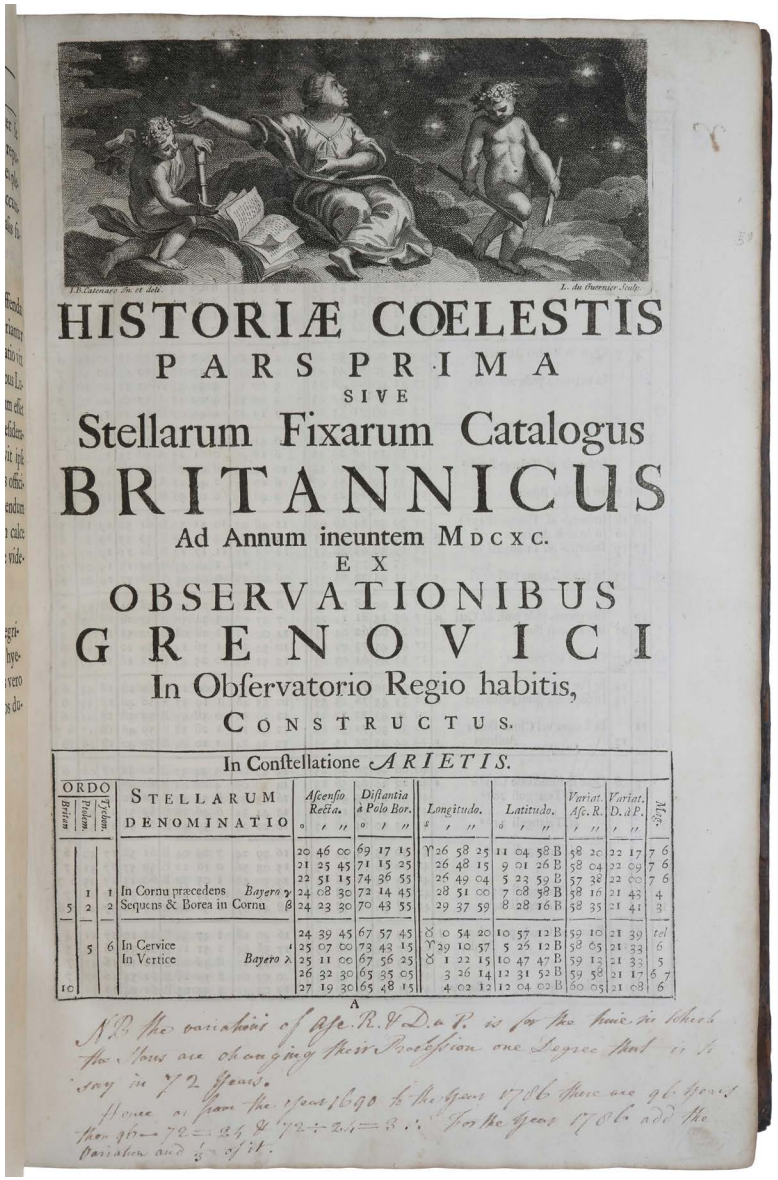
In the period 1699–1701, Flamsteed had begun to draw up pages listing stars in the zodiacal constellations, working his way through Orion and Monoceros to Lyra and Cygnus. After a hiatus of several years, Flamsteed began in August 1708 till January 1709 to work up some of the remaining constellations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

"Ironically, one of the features of the 'corrupted' 1712 edition that Flamsteed

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

With the accession of George I in 1714 Flamsteed found that at last he had friends at Court. "On his petition, Flamsteed was accordingly awarded a warrant ordering that of the 340 copies of the 1712 Historia coelestis still in the hands of [the printer] Awnsham Churchill, 300 should be handed over to him 'as a present from his Majty'. After a long delay, the copies were delivered. Flamsteed immediately took them to Greenwich. There he separated out the section printed when he had still been able to correct the press, setting aside 'Halley's corrupted edition of my catalogue, and [his] abridgement of my observations, no less spoiled by him.' He kept the former to be inserted into the edition he himself still hoped to complete. A few copies of the latter he annotated and sent to friends, as cautionary 'Evidence of ye malice of Godlesse persons'. But in spring 1716 Flamsteed built a pyre on Greenwich Hill, and burned the sheets containing the catalogue and abridged observations. As he himself put it, they made a good 'sacrifice to TRUTH.' He would do the same to 'all the rest of my editor's pains of the like nature,' he declared, 'if the Author of Truth should hereafter put them into my power.'

"If 340 copies remained, some 60 had been distributed. Along with the remaining books, the government also demanded a full account of the dispersal of these copies. The account made clear that, as had always been intended, the first copies of the Historia coelestis were not published in a commercial sense. Instead they had been envisaged as royal 'presents', to be given to a selective list of recipients



J.B. Latenay Sc. et delt. — L. du Guernier Sculp.

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

In Conſtellatione ARIETIS.

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

A

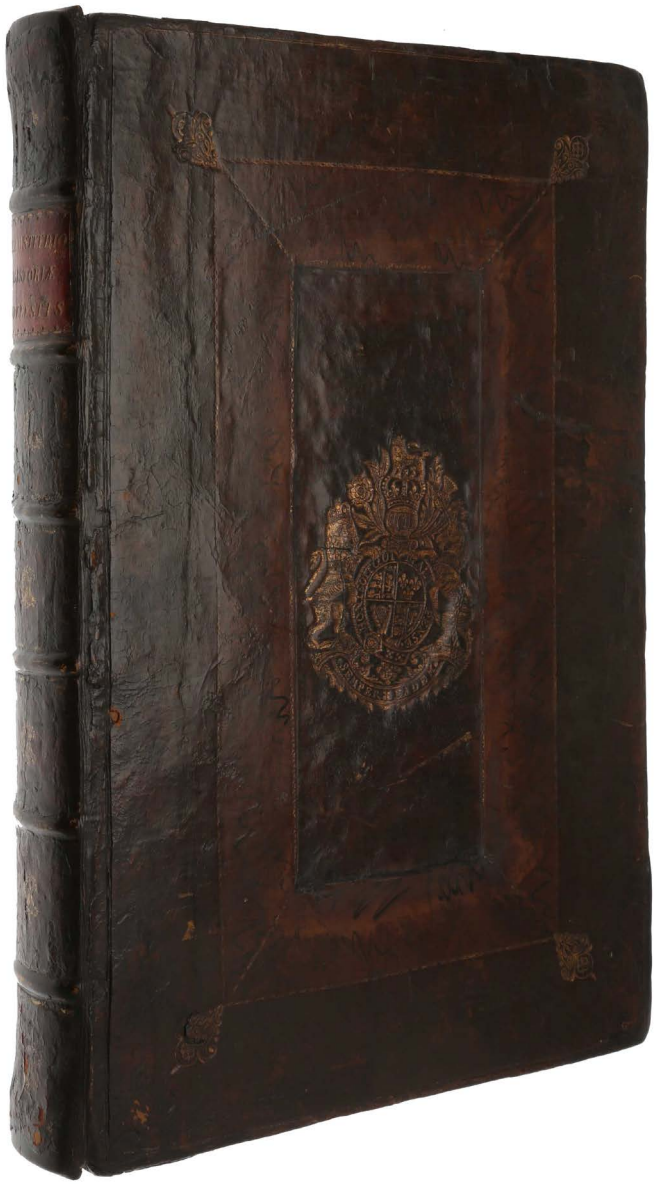
N.B. the variation of Aſc. R. & D. à P. is for the time in which the Stars are changing their Proteſtion one Degree that is to say in 72 Years.

Hence as from the year 1690 to the year 1786 there are 96 years then 96 ÷ 72 = 24 & 72 ÷ 24 = 3 ∴ for the year 1786 add the Variation and ⅓ of it.

… More than fifty of the copies no longer in Churchill's warehouse had been dispersed. Ten had gone to courtiers, thirty to the Treasury (richly bound for use as diplomatic gifts), and ten more to the observatory and Royal Academy in Paris. Newton and Halley had got one each, Flamsteed two" (Johns, pp. 607-9).

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

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



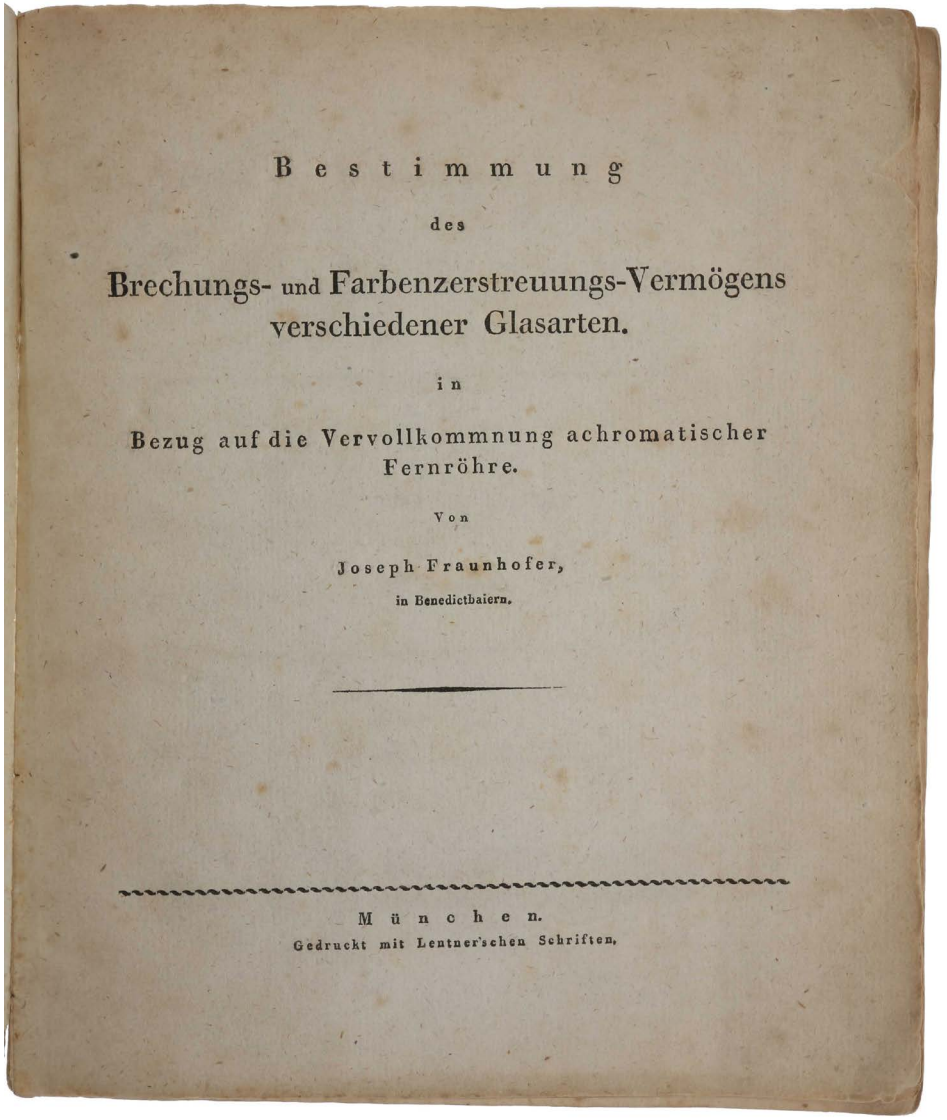
THE FOUNDING WORK OF ASTROPHYSICS

FRAUNHOFER, Joseph. *Bestimmung des Brechungs- und Farbenzerstreuungs-Vermögens verschiedener Glasarten.* Münich: Lentner, [1817].

$55,000

Offprint from Denkschriften der königlichen Academie der Wissenschaften zu München für die Jahre 1814 und 1815. 4to (248 x 210 mm), pp. [ii], 193-226, with three engraved plates (two folding). Original drab wrappers, as issued (front wrapper soiled and lightly chipped, rear wrapper more heavily chipped, spine worn).

First edition, the extremely rare offprint, of the founding work of astrophysics, the discovery of the absorption lines in the solar spectrum; the second plate, which reproduces Fraunhofer's drawing of these lines (etched by Fraunhofer himself), is the first illustration of the solar spectrum. "In 1802, when describing his new process for measuring the refraction of light, W. H. Wollaston reported the occurrence of dark lines in the solar spectrum but regarded them as simply natural dividing lines between the colours. Fraunhofer, originally not a scientist, but a practising optician, concentrated on these dark lines, and the title of his paper describes the method and purpose of his investigations: 'Definition of the Capacity of Refraction and Colour-diffusion of various kinds of Glass' … His achievements justify describing him as the founder of astrophysics. He charted several hundred lines, which have been known as 'Fraunhofer lines' ever since" (PMM). This paper was read before the Bavarian Academy of Sciences in 1815 and published in Denkschriften der königlichen Academie der Wissenschaften



Bestimmung

des

Brechungs- und Farbenzerstreuungs-Vermögens verschiedener Glasarten.

in

Bezug auf die Vervollkommnung achromatischer Fernröhre.

Von

Joseph Fraunhofer,

in Benedictbaiern.

München.

Gedruckt mit Lentner'schen Schriften.

zu München für die Jahre 1814 und 1815, Band V, pp. 193-226 (the volume dated 1817). This offprint, with separate title-page but retaining the journal pagination, is not listed on ABPC/RBH or on OCLC. There is also a separately-paginated issue, with identical title page, a copy of was offered in the sale of Haskell F. Norman (Christie's New York, 29 October 1998, lot 1084, $19550). It is surely logical to assume that the separately-paginated issue is later than that with the original journal pagination.

"Fraunhofer (1787-1826) came from humble parentage in Straubing near Munich and had very little formal education, having lost both parents when he was eleven. In 1807, at the age of 20, he was hired by the Mathematical Mechanical Institute Reichenbach, Utzschneider and Liebherr, a firm founded in 1804 for the production of military and surveying instruments, for which high-quality optical glass for lenses was essential. The optical works of the firm were outside Munich, at a disused monastery in Benediktbeuern, where Fraunhofer received his training from a Swiss named Pierre Guinand (1748-1824). Guinand's considerable reputation rested on his skill in the production of relatively large and optically pure pieces of crown and flint glass. However, owing to a clash of personalities, Guinand resigned his contract in 1814 and returned to Switzerland, and at this time the whole firm passed into the hands of Joseph von Utzschneider and Fraunhofer.

"The success of this famous early glass factory lay in the production of optical crown and flint glass free from bubbles and veins. The technique of stirring the molten glass was discovered by Guinand and developed by Fraunhofer. The use of these glasses enabled Fraunhofer to construct achromatic optical instruments of hitherto unsurpassed quality, and this was undoubtedly a key factor in his successful pioneering work in solar spectroscopy. Fraunhofer embarked on a careful examination of the optical properties of his glass, so as to measure the refractive index and dispersion. His work on the solar spectrum can therefore be seen as the means to Fraunhofer's end goal of perfecting optical instruments, for he realized that accurate refractive indices must be measured in monochromatic light. For, having rediscovered the solar absorption lines, he saw that the lines defined the precise wavelength of the light far better than the mere sensation of colour to the human eye.

"Fraunhofer observed the solar spectrum using a telescope of 25 mm aperture taken from one of his theodolites. A prism was mounted in front of the objective, and this enabled him to focus a relatively pure spectrum for direct visual inspection through the eyepiece. His introductory words are almost reminiscent of those used by Newton: 'In a shuttered room I allowed sunlight to pass through a narrow opening in the shutters, which was about 15 seconds broad and 36 minutes high, and thence onto a prism of flint glass, which stood on the theodolite … The theodolite was 24 feet from the window, and the angle of the prism measured about 60 degrees … I wanted to find out whether in the colour-image [i.e., spectrum] of sunlight, a similar bright stripe was to be seen, as in the colour-image of lamplight. But instead of this I found with the telescope almost countless strong and weak vertical lines, which however are darker than the remaining part of the colour-image; some seem to be nearly completely black' [p. 10].

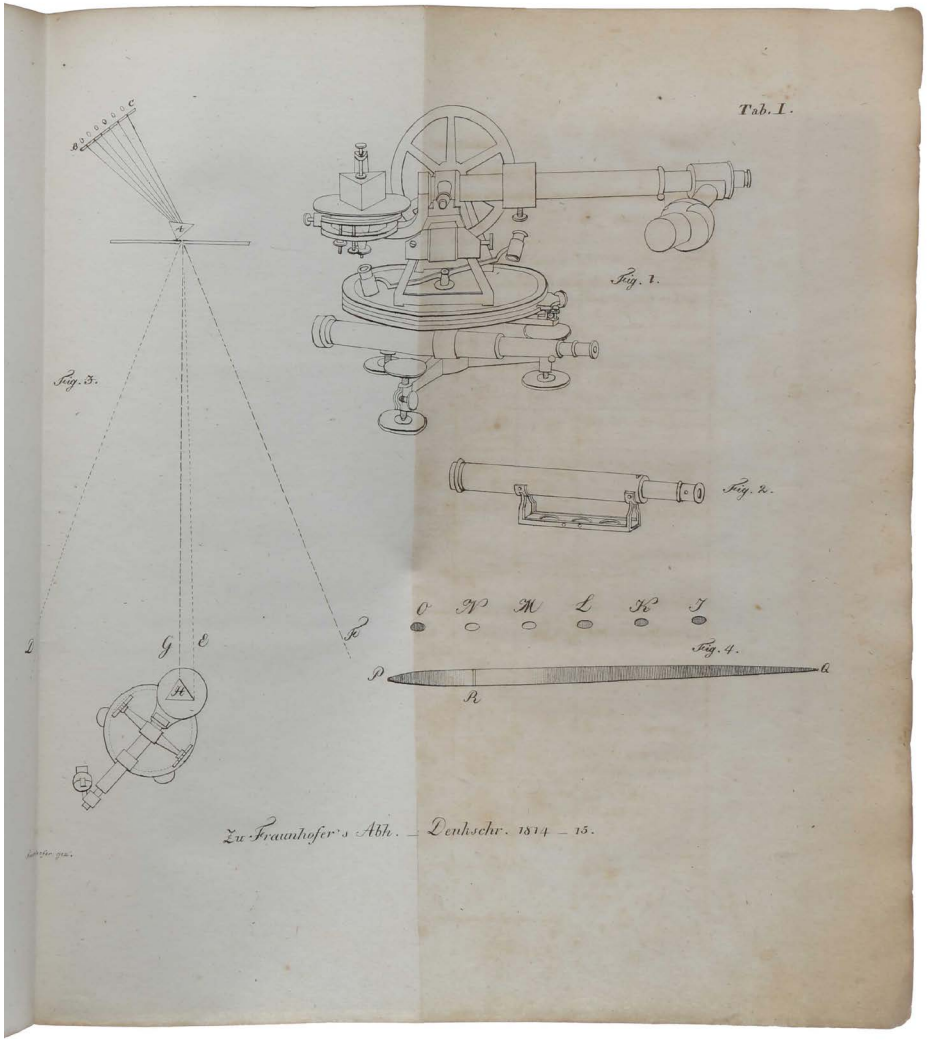
"Fraunhofer convinced himself that the lines in no way represent colour boundaries, as the same colour is found on both sides of a line with only a gradual and continuous colour change throughout the spectrum. Ten of the strongest lines were labeled with the letters A, a, B, C, D, E, b, F, G and H from the far red to the limit of the eye's vision in the violet. The last letter was used for the pair of strong violet lines that we now know are due to absorption by calcium. He noted that A was very near the red limit of the spectrum, but he was still able to see some red light beyond this feature. He showed the D feature to be composed of two close dark lines which exactly coincide with the bright lines emitted by lamplight, while

b consists of three very strong lines, amongst the strongest in the solar spectrum. The G feature was also found to be composite, consisting of 'many lines clustered together, among which several stand out through their strength. The two stripes at H are the most extraordinary; they are both almost completely the same and consist of many lines; in their middle is a strong line which is very black' [p. 12].

"Between the lines B and H, Fraunhofer observed 574 fainter lines and was able to give precise positions for some 350 of these in his drawing of the solar spectrum. In this figure he also indicated by the curve the approximate intensity distribution of the light in the spectrum as judged by the eye … Fraunhofer did not attempt to explain the origin of the dark solar lines. He knew they were intrinsic to the nature of sunlight, and not any instrumental effect. He restricted himself to careful and accurate observation rather than the speculation that characterized the work of some other spectroscopists over the next four decades.

"Fraunhofer's spectroscopic work did not stop at the Sun. Half a century ahead of his time, he initiated the science of planetary and stellar spectroscopy. With his theodolite telescope he observed the spectra of Venus, Sirius and other first-magnitude stars. For Venus, he wrote: 'I have seen the lines D, E, b, F perfectly defined … I have convinced myself that the light from Venus is in this respect of the same nature as sunlight.' For stars Fraunhofer found something surprisingly different: 'I have seen with certainty in the spectrum of Sirius three broad bands which appear to have no connection with those of sunlight; one of these bands is in the green, two are in the blue. In the spectra of other fixed stars of the first magnitude one can recognize bands, yet these stars, with respect to these bands, seem to differ among themselves' [p. 28] …

"In 1819 the optical section (lens production) of the instrument firm was shifted to Munich. Fraunhofer therefore spent most of his time there, going only on occasions



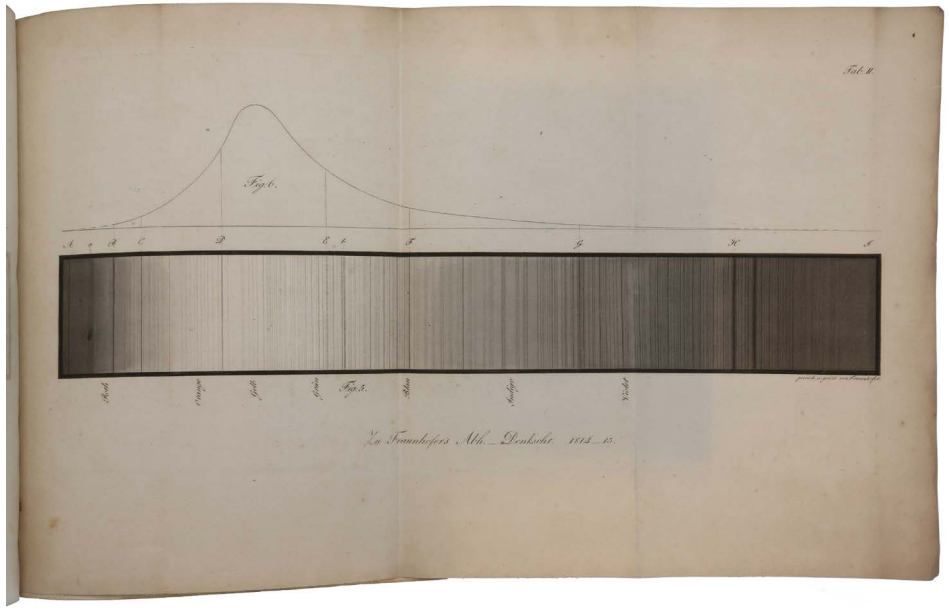
to Bedediktbeuern, where the glass works were kept. He died of tuberculosis when only 39, and it is likely he would have made further outstanding contributions to spectroscopy and telescope design had he survived a more normal lifespan. From 1826 the optical section of the firm was directed by Georg Mertz (1793-1867), who had been a pupil of Fraunhofer's since 1808" (Hearnshaw, ⌈ The Analysis of Starlight: Two Centuries of Astronomical Spectroscopy, pp. 17-20).

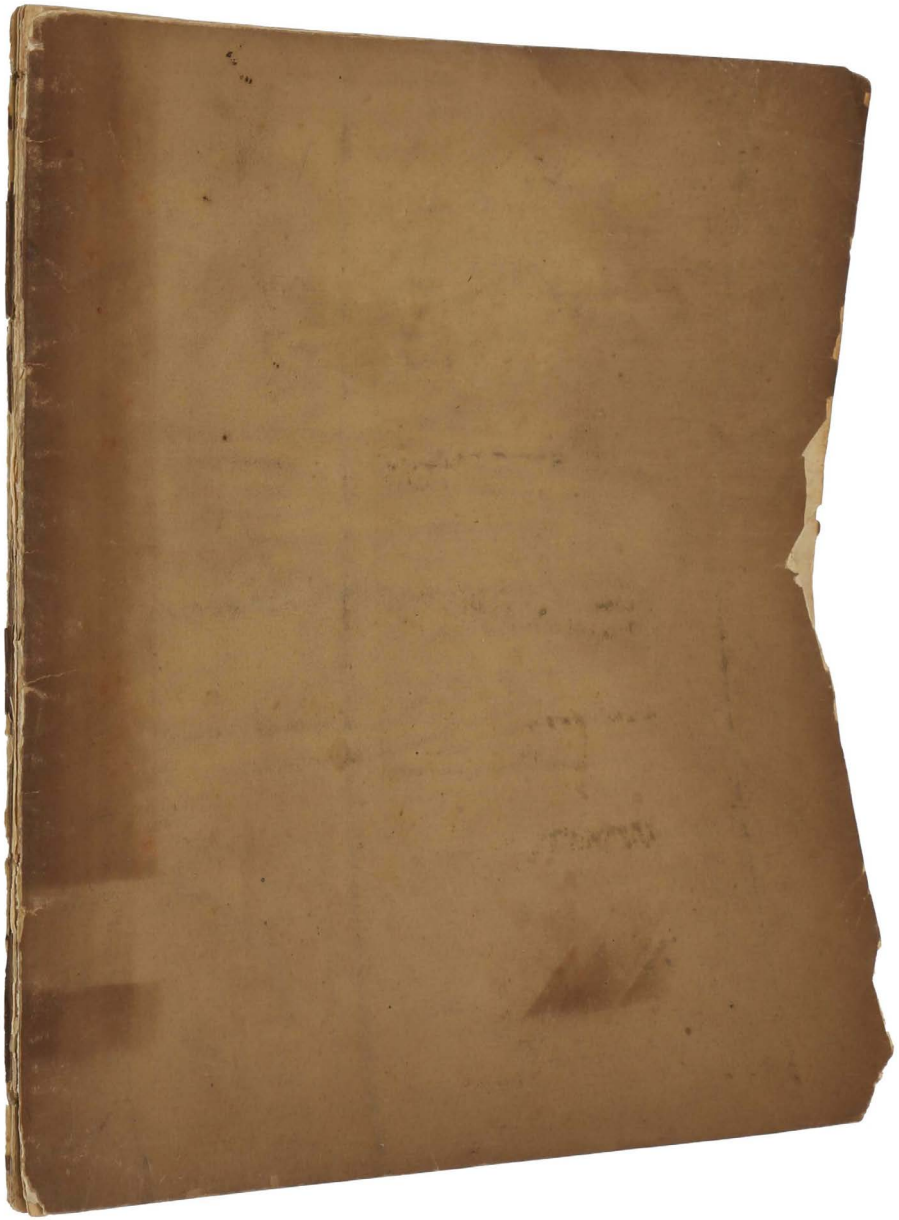
"Fraunhofer's discovery represented the beginning of what later came to be called chemical spectral analysis, the development of which was associated with the names of David Brewster, John Herschel, William Henry Fox Talbot, Charles Wheatstone, Antoine-Philibert Masson, Anders Jonas Ångström and William Swan. These investigators examined the origin of the dark lines in the solar spectrum – the so-called Fraunhofer lines – and suggested that they might be created by the selective absorption of light emitted by the sun in its atmosphere. The question then arose as to which chemical substances emitted which particular discrete lines. The final and conclusive steps towards chemical spectral analysis, however, were taken by the chemist Robert Bunsen and the physicist Gustav Kirchhoff" (Mehra & Rechenberg I, p. 157). They concluded that the cool, outer regions of the solar atmosphere contained iron, calcium, magnesium, sodium, nickel and chromium and probably cobalt, barium, copper and zinc as well.

The plate of the solar spectrum is regarded as one of the finest etchings produced hitherto. "Looking back at the beginning of that century, we appreciate even more the incredible skill with which the 'amateur' Fraunhofer had [produced] his famous map of the solar spectrum in 1814. Not even its later revisions, made between 1823 and 1831, could reproduce the intricate way in which Fraunhofer managed to convey the brilliance of the yellow-green region of the spectrum against its violet and red ends. Fraunhofer had carefully etched the map, timing the corrosion of each trace in proportion to the intensity of the respective spectrum line … for the black-and-white version of his map, to be published in the Memoirs of the Bavarian Academy of Sciences, he asked an unnamed printer to superimpose an Indian-ink wash on this etching to intensify the impression of darkness towards both ends of the visible spectrum; I have not seen this done in such sophistication on any other nineteenth-century spectrum plate. The resulting intensity gradient in Fraunhofer's map is so even that more than one print expert consulted mistook it for an aquatint … a few hand-colored versions of Fraunhofer's plate have been preserved. But unlike the published plate of 1814, these color versions lack the Indian-ink wash of the shaded areas into the red and violet extremes of the spectrum, displaying instead the full range of colors … These color plates indirectly also let us infer that the original illustration for the Munich memoirs was produced in a two-stage process: first a copper etching of all the line matter such as labels, caption, and the curve, as well as the spectrum lines inside the box. Microscopic inspection of the spectrum lines with their different widths and intensities confirms that they were etched rather than engraved to guarantee evenness of line width as well as consistent variation in line intensity. After this first printing stage was complete, the Indian-ink wash was added by hand. This involved two-stage process also explains the strange black frame enclosing Fraunhofer's solar spectrum – a feature absent from other spectrum representations. The fairly thick frame gave the printer a few millimeters leeway in positioning a screen protecting the areas outside it, so that he could then apply the Indian ink liberally to guarantee a smoother gradient in the spectrum strip itself at the center. The ink-wash stage, which made the final result more expensive, was omitted for the few sheets destined to be hand-colored, as well as in the later reprinting for the Astronomische Nachrichten, where a French translation of Fraunhofer's paper appeared in 1823" (Hentschel, ⌈ Mapping the Spectrum: Techniques of Visual Representation in Research and Teaching, pp. 116-7). Hentschel found an invoice from the printer showing that that Fraunhofer ordered 421 copies of each plate.

The imprint of our copy is 'Munich: Lentner,' but a copy at Augsburg has a different imprint, 'Benedictbaiern: Franz'; neither of these are dated. Both of these imprints are different from that of the journal volume, which simply states 'Auf Kosten der Akademie' and is dated 1817. It is normally assumed that this offprint was published in the same year as the journal volume, but at this period the Denkschriften were published only every three years and it is possible that parts of the journal, and hence some of the offprints, were printed earlier than the complete journal volume. The Huntington assigns a date of 1816 to their copy (a separately-paginated offprint without separate title-page).

Dibner 153 (erroneous collation); Norman 836; Parkinson, *Breakthroughs*, 260; PMM 278a (journal issue); Richard Green 125 (journal issue); Sparrow 70 (journal issue); *The Dawn of Science and Technology* 91 (journal issue).





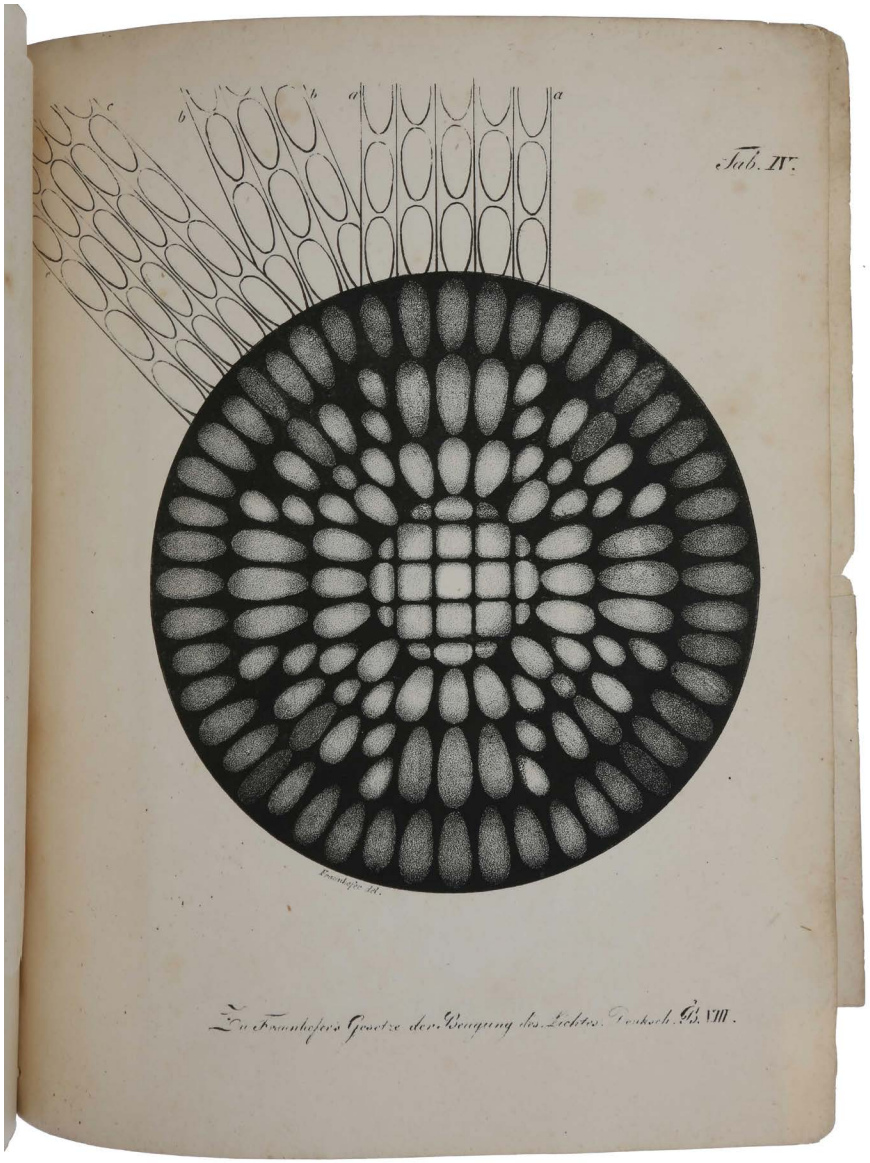
ON THE DIFFRACTION OF LIGHT

FRAUNHOFER, Joseph. *Neue Modifikation des Lichtes durch gegenseitige Einwirkung und Beugung der Strahlen, und Gesetze derselben.* Münich: Lentner, [1822].

$4,500

Offprint from: Denkschriften der kniglichen Academie der Wissenschaften zu München für die Jahre 1821 und 1822, Band VIII, 1821-22. 4to (245 x 200 mm), pp. [ii], [3]-76 with 6 folding plates, the first 2 engraved and the last 4 chalk lithographs. Disbound (paper spine strip, with some wear).

First edition, the rare offprint issue, of the first quantitative discussion of the diffraction of light. Four years earlier, Fraunhofer had published his epochal discovery of dark lines in the solar spectrum, now called 'Fraunhofer lines,' which inaugurated the fields of spectroscopy and astrophysics. "In 1821 and 1823, shortly after Fresnel's studies of interference phenomena had received general attention, Fraunhofer published two papers in which he observed and analyzed certain diffraction phenomena and interpreted them in terms of a wave theory of light. In the 1821 paper [offered here] he discussed his examination of the spectra resulting from light diffracted through a single narrow slit and quantitatively related the width of the slit to the angles of dispersion of the different orders of spectra. Extending his observations to diffraction resulting from a large number of slits, he constructed a grating with 260 parallel wires. Although David Rittenhouse and Thomas Young had previously noted some effects of crude diffraction gratings, Fraunhofer made the first quantitative study of the phenomena. The presence of the solar dark lines enabled him to note that the dispersion of the spectra was greater with his grating than with his prism.



Hence, he examined the relationship between dispersion and the separation of wires in the grating. Utilizing the dark lines as bench marks in the spectrum for his dispersion determinations, he concluded that the dispersion was inversely related to the distance between successive slits in the grating. From the same study Fraunhofer was able to determine the wavelengths of specific colors of light. Somewhat later he also constructed a grating by ruling lines on glass covered by gold foil and, even later, constructed a reflecting grating. The latter prompted him to consider the effects of light obliquely incident to the grating" (DSB). ABPC/RBH list two copies of this offprint (Christie's 23 April 2008, lot 127, $1988; Christie's 10 December 1999, $4370).

"Fraunhofer (1787-1826) came from humble parentage in Straubing near Munich and had very little formal education, having lost both parents when he was eleven. In 1807, at the age of 20, he was hired by the Mathematical Mechanical Institute Reichenbach, Utzschneider and Liebherr, a firm founded in 1804 for the production of military and surveying instruments, for which high-quality optical glass for lenses was essential. The optical works of the firm were outside Munich, at a disused monastery in Benediktbeuern, where Fraunhofer received his training from a Swiss named Pierre Guinand (1748-1824). Guinand's considerable reputation rested on his skill in the production of relatively large and optically pure pieces of crown and flint glass. However, owing to a clash of personalities, Guinand resigned his contract in 1814 and returned to Switzerland, and at this time the whole firm passed into the hands of Joseph von Utzschneider and Fraunhofer" (Hearnshaw, ⌈The Analysis of Starlight: Two Centuries of Astronomical Spectroscopy, p. 17).

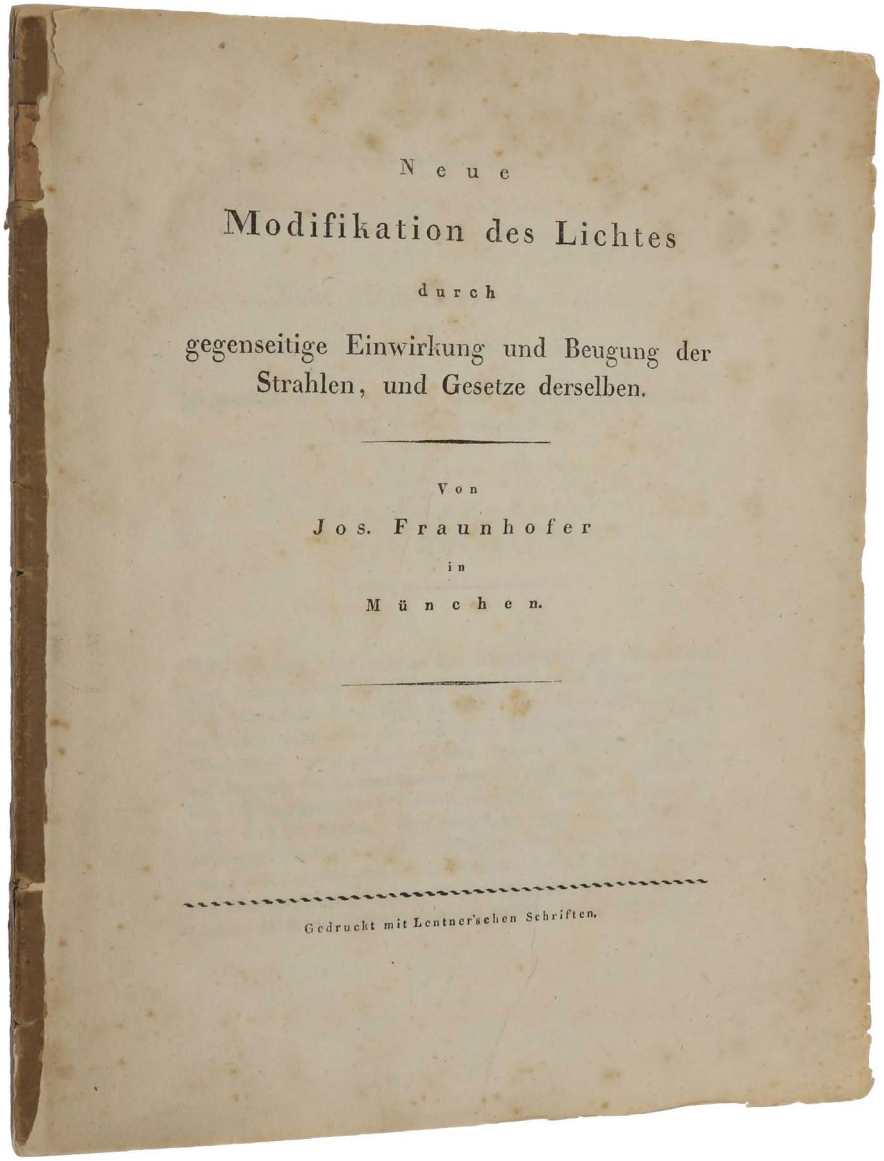
"The glass experiments of Fraunhofer and a co-worker, financed by Utzschneider, soon gave the Benediktbeuern workshop considerable advantage over its competitors. To make better achromatic lens systems, for example, Fraunhofer needed precise determinations of the dispersion and refraction indices of flint and crown glass. He first made the necessary measurements using as a source of monochromatic light the bright yellow lines in the flame of a lamp. Then he tried sunlight with the same purpose in mind, which led him to a major discovery: 'In a darkened room, through a small opening in the shutter approximately 15 seconds wide and 36 minutes high, I let light fall on a prism of flint glass, which stood on the . . . theodolite. The theodolite was 24 feet from the shutter, and the angle of the prism measured approximately 60° … I wanted to determine if in the spectrum of sunlight a similar bright line was to be seen as in the spectrum of lamplight, but instead of that I found, with the telescope, almost countless strong and weak vertical lines, which are darker than the remaining part of the spectrum; some appeared to be almost completely black.'

"With these 'Fraunhofer lines,' as they came to be called, he had discovered the ideal means for determining the optical constants of the various kinds of glass used in the manufacture of optical instruments. He also presented to physicists, astronomers, and chemists a new phenomenon and a stimulus to develop the technique of spectrum analysis" (Jungnickel & McCormmach, pp. 269-270). Fraunhofer's paper, 'Bestimmung des Brechungs- und Farbenzerstreuungs-Vermögens verschiedener Glasarten,' describing this discovery, was published in Denkschriften der kniglichen Academie der Wissenschaften zu München 5 (1817).

"This was the first of a number of scientific papers by Fraunhofer. In 1821, [Fraunhofer] published an influential paper on the diffraction of light, which he introduced with a characteristic reference to the dependence of the advance of science on precision instruments: it is well known, he said, that all researches carried out by scientists whose eyes are equipped with 'optical tools' are distinguished by a 'high degree of accuracy.' In the case of researches on diffraction, he said, adequate tools did not exist, which he thought was a likely

reason why this part of physical optics was retarded and 'so few of the laws of this modification of light' were known … For readers who were unfamiliar with it, he described the elementary phenomenon of diffraction: if the ray of light admitted into the darkened room is intercepted by a dark screen, itself containing a small opening, and if the light passing through it is allowed to fall on a white surface, the illuminated part of the surface is larger than the opening and exhibits colored fringes at the edges. He went on to give an account of his new experiments, which were of a measuring nature: 'To receive in the eye all the light diffracted through a small opening and to see the phenomena greatly enlarged and, still more, to be able to measure directly the angle of inflection of the light, I put a screen containing a narrow vertical opening, which could be made wider or narrower by means of a screw, in front of the lens of a theodolite telescope. In a dark room, by means of a heliostat, I let sunlight fall through a narrow opening onto the screen, at an opening of which it was consequently diffracted. Through the telescope I could then observe the phenomena produced by the diffraction of light, enlarged and yet with sufficient brightness, and at the same time measure the angles of inflection with the theodolite.' By this means, Fraunhofer determined that the angle of bending of light in diffraction is inversely proportional to the width of the opening. Likewise, using gratings of parallel fibers, such as silver or gold wires, he determined that the diffraction of light is inversely proportional to the separation of the wires. His gratings required fine workmanship to make the separation extremely small; with his grating of 260 wires, the separation was only about 0.003862 Paris inch and the thickness of the wires about 0.00202I inch. Later he ruled very fine gratings of glass covered with gold foil. By examining the dark solar lines with his gratings, he was able to calculate the wavelengths of specific colors of the spectrum. The precision he achieved with this 'optical tool in physical optics was impressive.

"By this time Fraunhofer was back in Münich, since for financial reasons



Neue

Modifikation des Lichtes

durch

gegenseitige Einwirkung und Beugung der Strahlen, und Gesetze derselben.

Von

Jos. Fraunhofer

in

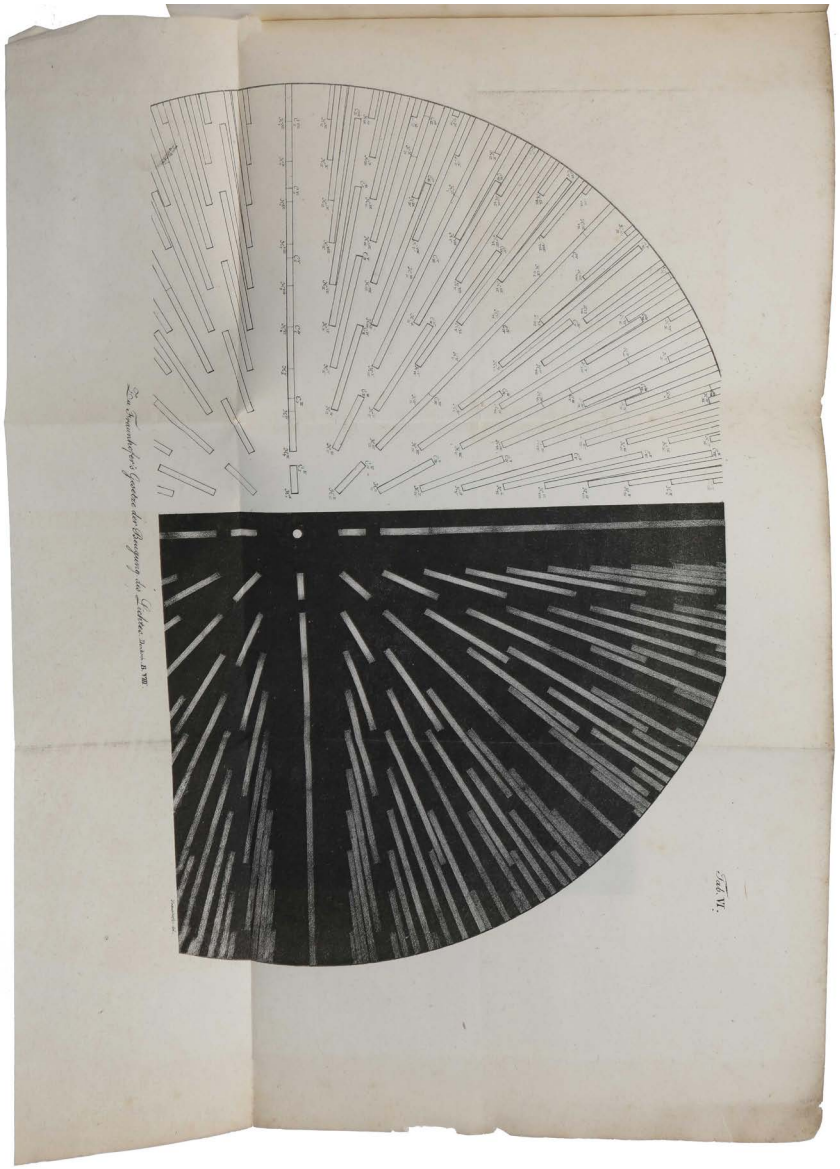
München.

Gedruckt mit Lentner'schen Schriften.

Utzschneider had had to sell the monastery in Benediktbeuem. By then he was also 'Royal Professor,' a title bestowed on him in recognition of his work, and the government had taken to calling on him for technical services. Soon after completing his paper on diffraction, in 1823 he asked the king for, and was granted, a salary as a member of the Bavarian Academy, which was to help him continue his theoretical and practical work. Later in 1823, he was named 'second curator' of the mathematical-physical collection of the academy. His duties included lectures for the educated public, a program the academy started in 1824. At Utzschneider's place, to an audience of his choice, Fraunhofer lectured twice weekly on 'mathematical-physical optics, accompanied by experiments.' Around this time there was some discussion of the state acquiring the optical institute, but the idea was dropped when Fraunhofer died in 1826. After Fraunhofer, the institute continued as a commercial firm under Utzschneider and later Siegmund Merz" (ibid., pp. 270-272).

This offprint retains the journal pagination – in the journal each section is separately-paginated, this being the first article in the section 'Classe der Mathematik und Naturwissejchaften'. The journal volume in which this article appears is dated 1824, but at this period the Denkschriften were published only every three years and it is possible that parts of the journal, and hence some of the offprints, were printed earlier than the complete journal volume, possibly as early as 1821.

Norman 837; Poggendorff I, 796; Wellcome III, p. 65.



GALILEO'S DEFENCE OF COPERNICUS

GALILEI, Galileo. *Dialogo. Dove ne i congressi di quattro giornate si discorre sopra i due massimi sistemi del mondo Tolemaico, e Copernicano.* Florence: Giovanni Batista Landini, 1632.

$125,000

4to, pp. [8], 458, [16], including errata leaf and frontispiece signed Stefano della Bella, showing Aristotle, Ptolemy and Copernicus in disputation. Printed correction slip in margin of F6v (= p. 92). 18th century black leather lettering-piece on spine.

First edition of this epoch-making work, Galileo's celebrated defence of the Copernican view of the solar system, the most notorious banned book of the 17th century. Written in dialogue form, it "was designed both as an appeal to the great public and as an escape from silence … it is a masterly polemic for the new science. It displays all the great discoveries in the heavens which the ancients had ignored; it inveighs against the sterility, wilfulness, and ignorance of those who defend their systems; it revels in the simplicity of Copernican thought and, above all, it teaches that the movement of the earth makes sense in philosophy, that is, in physics … The Dialogo, more than any other work, made the heliocentric system a commonplace" (PMM). "The Dialogo, far more than any work, convinced men of the truth of the Copernican system" (Gingerich). Pope Urban VIII was not persuaded, however, and immediately convened a special commission to examine the book and make recommendations. In casting the Pope as the simple-minded Aristotelian Simplicius, Galileo brought upon himself arrest, trial by the Inquisition and life imprisonment. The sentence was commuted to permanent house arrest, while the printing of any of his works was forbidden. The Dialogo

remained on the index until 1832.

In August 1597, Galileo wrote to Kepler expressing his sympathies for Copernicanism, having received a copy of the Mysterium cosmographicum (1596) from him. At this time Galileo's support for Copernicus was Earth-based: Galileo had devised a theory of the tides involving the combined rotational motions of the Earth around its axis and, after Copernicus, around the Sun. Everything changed early in 1610 when Galileo first turned a telescope to the skies. Not only was the moon revealed to be mountainous and the Milky Way to consist of separate stars, contrary to Aristotelian principles, but a host of new fixed stars and four satellites of Jupiter were promptly discovered. Galileo's account of these discoveries was published in the Sidereus nuncius (Venice, 1610). Galileo saw in the satellites of Jupiter a miniature planetary system in which, as in Copernican astronomy, it could no longer be held that all moving heavenly bodies revolved exclusively about the earth. Galileo first spoke out decisively in print for the Copernican hypothesis in his 1613 work on sunspots, Istoria e dimostrazioni intorno alle macchie solari. During its composition he had taken pains to determine the theological status of the idea of incorruptibility of the heavens, finding that this was regarded by churchmen as an Aristotelian rather than a Catholic dogma. But attacks against Galileo and his followers soon appeared in ecclesiastical quarters. These came to a head with a denunciation from the pulpit in Florence late in 1614. A year later Galileo went to Rome (against the advice of his friends and the Tuscan ambassador) to clear his own name and to prevent, if possible, the official suppression of the teaching of Copernicanism. In the first, he succeeded, but on the second he failed: Galileo was instructed on 26 February 1616 to abandon the holding or defending of that view. No action was taken against him, nor were any of his books suspended. Returning to Florence, Galileo took up less theologically controversial topics, culminating in the publication of Il Saggiatore in 1623. Just before it emerged from the press, Maffeo Barberini became pope as Urban VIII.

320 Dialogo terzo

SIMP. Sia questo segnato A. il luogo del globo terrestre.

SALV. Benestà. Sò secondariamente, che voi sapete benissimo, che essa terra non è dentro al corpo solare, nè meno a quello contigua, ma per certo spazio distante, e però assegnate al Sole qual'altro luogo più vi piace remoto dalla terra a vostro beneplacito, e questo ancora contrassegnate.

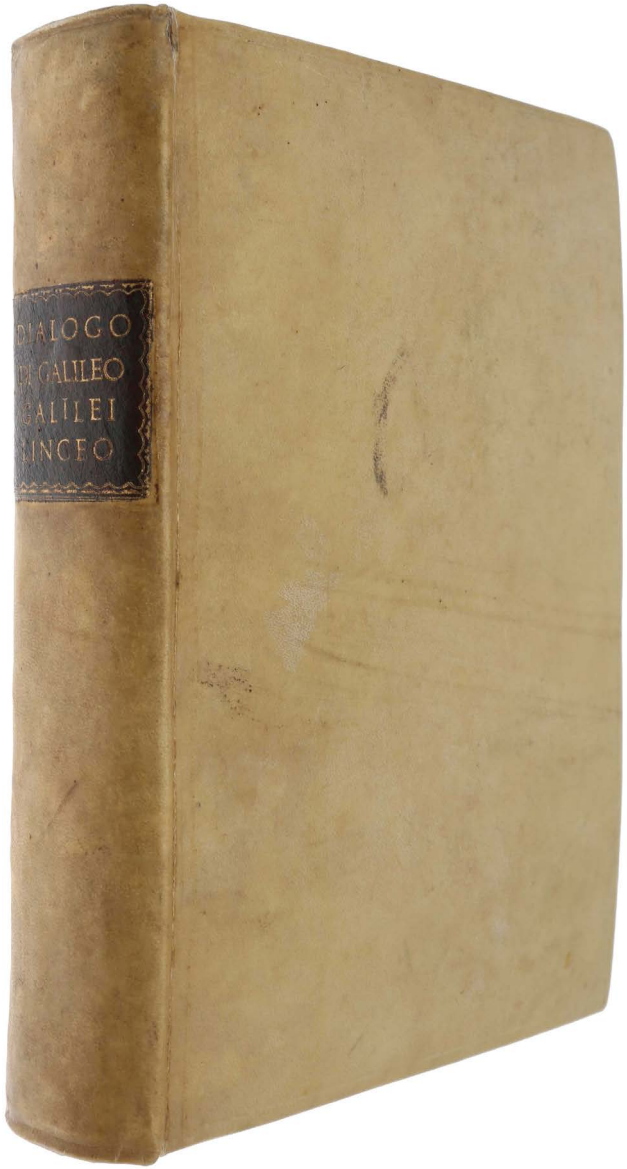
SIMP. Ecco fatto: Sia il luogo del corpo solare questo segnato O.

SALV. Stabiliti questi due, voglio, che pensiamo di accomodar'il corpo di Venere in tal maniera, che lo stato, e mouimento suo possa sodisfar'a ciò, che di essi ci mostrano le sensate apparenze,

Galileo journeyed to Rome in 1624 to pay his respects to Urban, and secured from him permission to discuss the Copernican system in a book, provided that the arguments for the Ptolemaic view were given an equal and impartial discussion. Urban refused to rescind the edict of 1616, although he remarked that had it been up to him, the edict would not have been adopted.

"The Dialogue Concerning the Two Chief World Systems occupied Galileo for the next six years. It has the literary form of a discussion between a spokesman for Copernicus, one for Ptolemy and Aristotle, and an educated layman for whose support the other two strive. Galileo thus remains technically uncommitted except in a preface which ostensibly supports the anti-Copernican edict of 1616. The book will prove, he says, that the edict did not reflect any ignorance in Italy of the strength of pro-Copernican arguments. The contrary is the case; Galileo will add Copernican arguments of his own invention, and thus he will show that not ignorance of or antagonism to science, but concern for spiritual welfare alone, guided the Church in its decision.

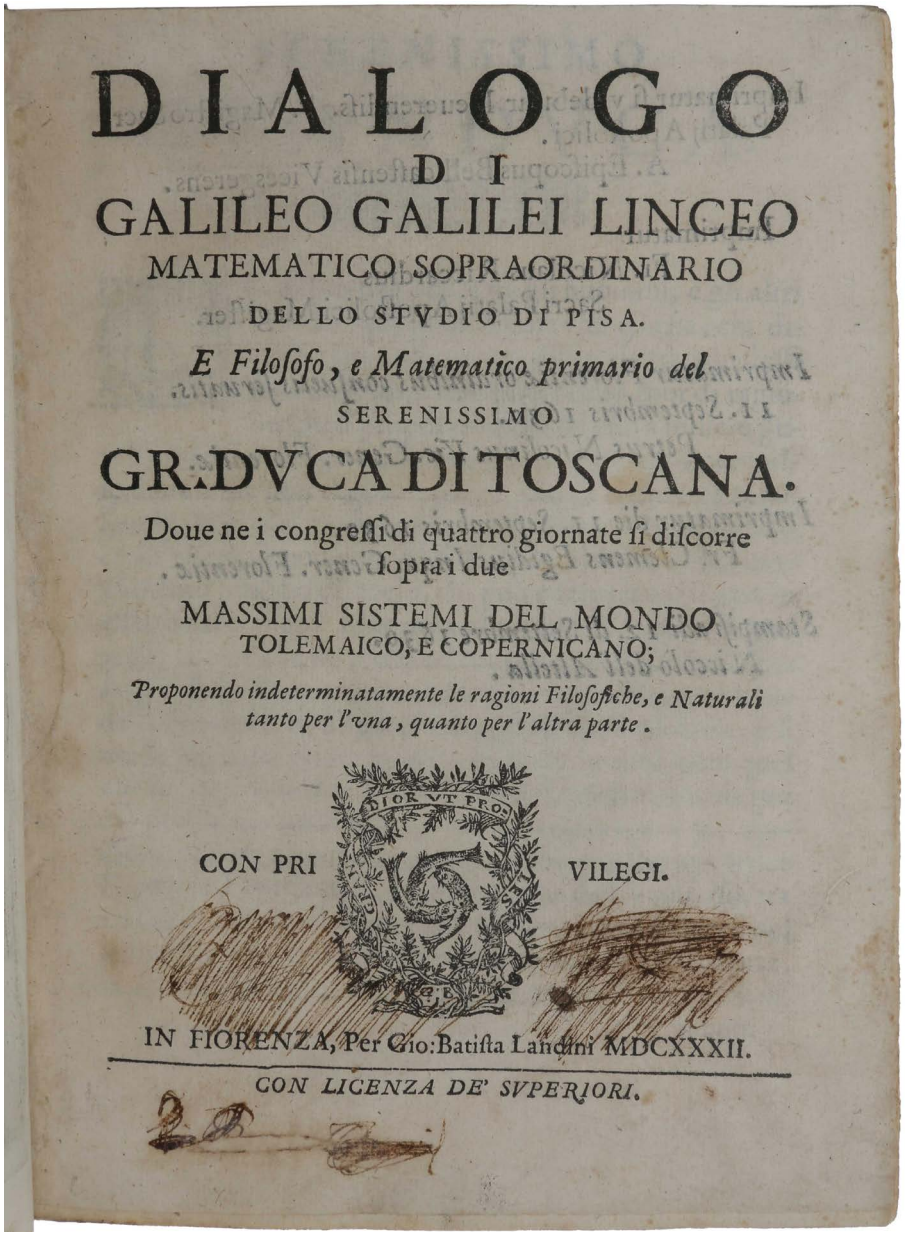
"The opening section of the Dialogue critically examines the Aristotelian cosmology. Only those things in it are rejected that would conflict with the motion of the earth and stability of the sun or that would sharply distinguish celestial from terrestrial material and motions. Thus the idea that the universe has a center, or that the earth is located in such a center, is rejected, as is the idea that the motion of heavy bodies is directed to the center of the universe rather than to that of the earth. On the other hand, the Aristotelian concept of celestial motions as naturally circular is not rejected; instead, Galileo argues that natural circular motions apply equally to terrestrial and celestial objects. This position appears to conflict with statements in later sections of the book concerning terrestrial physics. But uniform motion in precise circular orbits also conflicts with actual observations of planetary motions, whatever center is chosen for all



orbits. Actual planetary motions had not been made literally homocentric by any influential astronomer since the time of Aristotle. Galileo is no exception; in a later section he remarked on the irregularities that still remained to be explained. Opinion today is divided; some hold that the opening arguments of the Dialogue should be taken as representative of Galileo's deepest physical and philosophical convictions, while others view them as mere stratagems to reduce orthodox Aristotelian opposition to the earth's motion.

"Important in the Dialogue are the concepts of relativity of motion and conservation of motion, both angular and inertial, introduced to reconcile terrestrial physics with large motions of the earth, in answer to the standard arguments of Ptolemy and those added by Tycho Brahe. The law of falling bodies and the composition of motions are likewise utilized. Corrections concerning the visual sizes and the probable distances and positions of fixed stars are discussed. A program for the detection of parallactic displacements among fixed stars is outlined, and the phases of Venus are adduced to account for the failure of that planet to exhibit great differences in size to the naked eye at perigee and apogee. Kepler's modification of the circular Copernican orbits is not mentioned; indeed, the Copernican system is presented as more regular and simpler than Copernicus himself had made it. Technical astronomy is discussed with respect only to observational problems, not to planetary theory.

"To the refutation of conventional physical objections against terrestrial motion, Galileo added two arguments in its favor. One concerned the annual variations in the paths of sunspots, which could not be dynamically reconciled with an absolutely stationary earth. Geometrically, all rotations and revolutions could be assigned to the sun, but their conservation would require very complicated forces. The Copernican distribution of one rotation to the sun and one rotation and one revolution to the earth fitted a very simple dynamics. The second new argument



DIALOGO
DI
GALILEO GALILEI LINCEO
MATEMATICO SOPRAORDINARIO
DELLO STVDIO DI PISA.
E Filosofo, e Matematico primario del
SERENISSIMO
GR. DVCA DI TOSCANA.
Doue ne i congressi di quattro giornate si discorre
sopra i due
MASSIMI SISTEMI DEL MONDO
TOLEMAICO, E COPERNICANO;
Proponendo indeterminatamente le ragioni Filosofiche, e Naturali tanto per l'vna, quanto per l'altra parte.

CON PRI VILEGI.

IN FIORENZA, Per Gio: Batista Landini MDCXXXII.

CON LICENZA DE' SVPERIORI.

concerned the existence of ocean tides, which Galileo declared, quite correctly, to be incapable of any physical explanation without a motion of the earth. His own explanation happened to be incorrect; he argued that the earth's double motion of rotation and revolution caused a daily maximum and minimum velocity, and a continual change of speed, at every point on the earth. The continual variation of speed of sea basins imparted different speeds to their contained waters. The water, free to move within the basins, underwent periodic disturbances of level, greatest at their coasts; the period depended on sizes of basins, their east-west orientations, depths, and extraneous factors such as prevailing winds. In order to account for monthly and annual variations in the tides, Galileo invoked an uneven speed of the earth-moon system through the ecliptic during each month, caused by the moon's motion with respect to the earth-sun vector; for annual seasonal effects, he noted changes of the composition of rotational and revolutional components in the basic disturbing cause.

"The Dialogue was completed early in 1630. Galileo took it to Rome, where it was intended to be published by the Lincean Academy. There he sought to secure a license for its printing. This was not immediately granted, and he returned to Florence without it. While the matter was still pending, Federico Cesi died, depriving the Academy of both effective leadership and funds. Castelli wrote to Galileo, intimating that for other reasons he would never get the Roman imprimatur and advising him to print the book at Florence without delay. Negotiations ensued for permission to print the book at Florence. Ultimately these were successful, and the Dialogue appeared at Florence in March 1632. A few copies were sent to Rome, and for a time no disturbance ensued. Then, quite suddenly, the printer was ordered to halt further sales, and Galileo was instructed to come to Rome and present himself to the Inquisition during the month of October …

"Confined to bed by serious illness, he at first refused to go to Rome. The grand duke and his Roman ambassador intervened stoutly in his behalf, but the pope was adamant. Despite medical certificates that travel in the winter might be fatal, Galileo was threatened with forcible removal in chains unless he capitulated. The grand duke, feeling that no more could be done, provided a litter for the journey, and Galileo was taken to Rome in February 1633.

"The outcome of the trial, which began in April, was inevitable. Although Galileo was able to produce an affidavit of Cardinal Bellarmine to the effect that he had been instructed only according to the general edict that governed all Catholics, he was persuaded in an extrajudicial procedure to acknowledge that in the Dialogue he had gone too far in his arguments for Copernicus. On the basis of that admission, his Dialogue was put on the Index, and Galileo was sentenced to life imprisonment after abjuring the Copernican "heresy." The terms of imprisonment were immediately commuted to permanent house arrest under surveillance. He was at first sent to Siena, under the charge of its archbishop, Ascanio Piccolomini. Piccolomini, who is said to have been Galileo's former pupil, was very friendly to him. Within a few weeks he had revived Galileo's spirits—so crushed by the sentence that his life had been feared for—and induced him to take up once more his old work in mechanics and bring it to a conclusion. While at Siena, Galileo began the task of putting his lifelong achievements in physics into dialogue form, using the same interlocutors as in the Dialogue" (DSB).

Copies of this book in contemporary bindings are relatively uncommon, most copies appearing on the market being in 18th century, or later, bindings. After the prohibition of the Dialogo in 1633, the book could not be legally sold in Italy. Since there is no documentary record of copies being systematically destroyed, unsold copies may have been withheld from the market. These copies would probably have been released early in the 18th century, when secular Florentine political authorities took a decisive and quite aggressive stance against the Church

relating to Galileo (a second edition of the *Dialogo* was published at Florence in 1710, and the second edition of the *Opera* appeared there in 1718). This may explain why so many copies of the *Dialogo* are found in 18th century bindings.

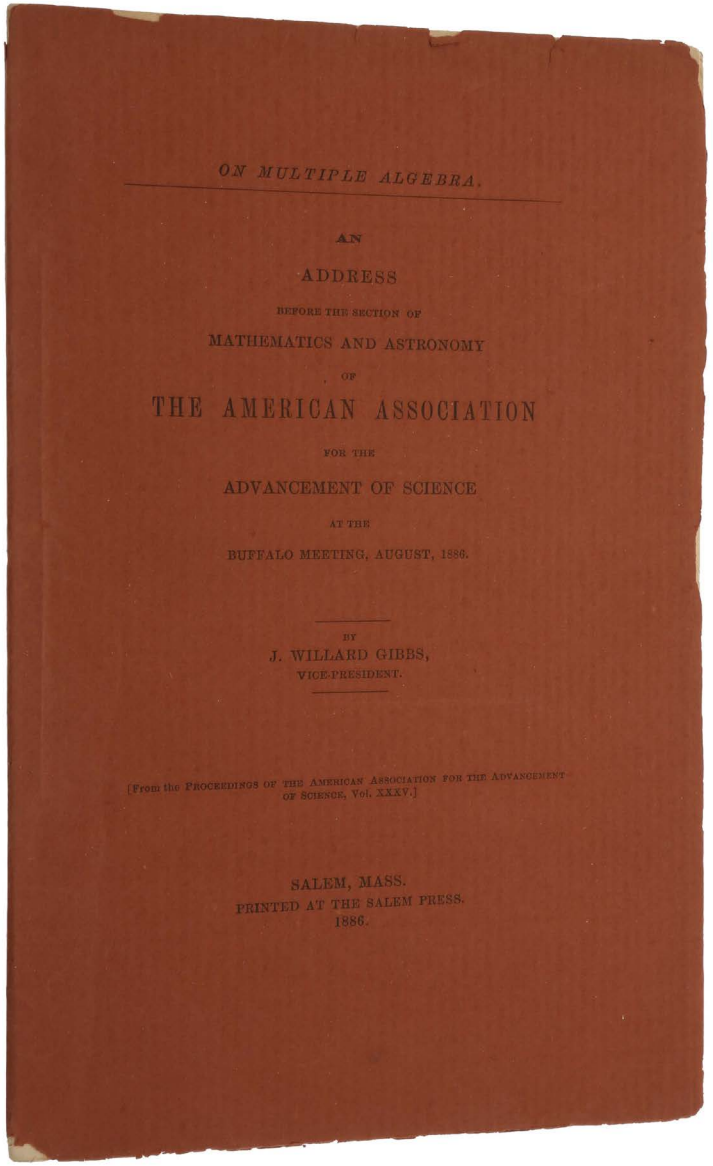
GIBBS ON ABSTRACT ALGEBRA

GIBBS, Josiah Willard. *On multiple algebra: an address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science at the Buffalo meeting, August, 1886.* Salem, Mass.: Salem Press, 1886.

$3,000

Offprint from: Proceedings of the American Association for the Advancement of Science, Vol. XXXV. 8vo (246 x 155 mm), pp. 32. Original printed wrappers (a few small marginal chips), unopened.

First edition, very rare offprint issue. "One of Gibbs' most famous papers was entitled 'On Multiple Algebra.' It was given as an 'Address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science, by the Vice-President' and was published in 1886. Of all Gibbs' writings this essay gives the best picture of his conception of the place of vector analysis within the wider fields of algebra and mathematics in general" (Crowe, p. 158). "In the year 1844 two remarkable events occurred, the publication by [William Rowan] Hamilton of his discovery of quaternions, and the publication by [Hermann Günther] Grassmann of his 'Ausdehnungslehre.' With the advantage of hindsight we can see that Grassmann's was the greater contribution to mathematics, containing the germ of many of the concepts of modern algebra, and including vector analysis as a special case … When Grassmann's work finally became known, mathematicians were divided into quaternionists and antiquaternionists, and were spending more energy in polemical arguments for and against quaternions than in trying to understand how Grassmann and Hamilton might be fitted together into a larger scheme of things. So it was left to



ON MULTIPLE ALGEBRA.

AN

ADDRESS

BEFORE THE SECTION OF

MATHEMATICS AND ASTRONOMY

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE

AT THE

BUFFALO MEETING, AUGUST, 1886.

BY

J. WILLARD GIBBS,

VICE-PRESIDENT.

[From the PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, Vol. XXXV.]

SALEM, MASS.

PRINTED AT THE SALEM PRESS.

1886.

the physicist Gibbs to present for the first time in his 1886 lecture the essential ideas of Grassmann and Hamilton side by side. The last words of his lecture are, 'We begin by studying multiple algebras; we end, I think, by studying MULTIPLE ALGEBRA'" (Dyson, p. 644). In modern terms, a 'multiple algebra' is an 'algebra', i.e., a 'vector space' equipped with a product that is distributive with respect to addition (a(b + c) = ab + ac). The algebras considered by Grassmann and Gibbs were associative ((ab)c = a(bc)), but not necessarily commutative (ab = ba).

"Gibbs began with a brief treatment of the history of multiple algebra and considered such men as Möbius, Grassmann, Hamilton, Saint-Venant, Cauchy, Cayley, Hankel, Benjamin Peirce, and Sylvester. By this analysis Gibbs hoped among other things 'to illustrate the fact, which I think is a general one, that the modern geometry is not only tending to results which are appropriately expressed in multiple algebra, but that it is actually striving to clothe itself in forms which are remarkably similar to the notations of multiple algebra, only less simple and general and far less amenable to analytical treatment, and therefore, that a certain logical necessity calls for throwing off the yoke under which analytical geometry has so long labored.'

The strongest praise and fullest treatment was bestowed upon Grassmann. Considering in detail a number of the products defined by Grassmann, Gibbs discussed their significance and argued that Grassmann's system provides a rich and encompassing point of view. Gibbs concluded the paper with a discussion of applications of multiple algebra to physical science. Thus he stated: 'First of all, geometry, and the geometrical sciences which treat of things having position in space, kinematics, mechanics, astronomy, physics, crystallography, seem to demand a method of this kind, for position in space is essentially a multiple quantity and can only be represented by simple quantities in an arbitrary and cumbersome manner. For this reason, and because our spatial intuitions are more developed than those of any other class of mathematical relations, these subjects are especially adapted to introduce the student to the methods of multiple algebra.'

"Proceeding then to specifics, Gibbs noted that through Maxwell electricity and magnetism had become associated with the methods of multiple algebra but that astronomy had so far remained aloof. Having noted that for geometrical applications multiple algebra will generally take the form of a point or a vector analysis, Gibbs stated that 'in mechanics, kinematics, astronomy, physics, or crystallography, Grassmann's point analysis will rarely be wanted.' However arguments were given on behalf of the usefulness of point analysis for investigations in pure mathematics. The concluding paragraph and the concluding sentence in particular have now become classic. 'But I do not so much desire to call your attention to the diversity of the applications of multiple algebra, as to the simplicity and unity of its principles. The student of multiple algebra suddenly finds himself freed from various restrictions to which he has been accustomed. To many, doubtless, this liberty seems like an invitation to license. Here is a boundless field in which caprice may riot. It is not strange if some look with distrust for the result of such an experiment. But the farther we advance, the more evident it becomes that this too is a realm subject to law. The more we study the subject, the more we find all that is most useful and beautiful attaching itself to a few central principles. We begin by studying multiple algebras; we end, I think, by studying MULTIPLE ALGEBRA.'

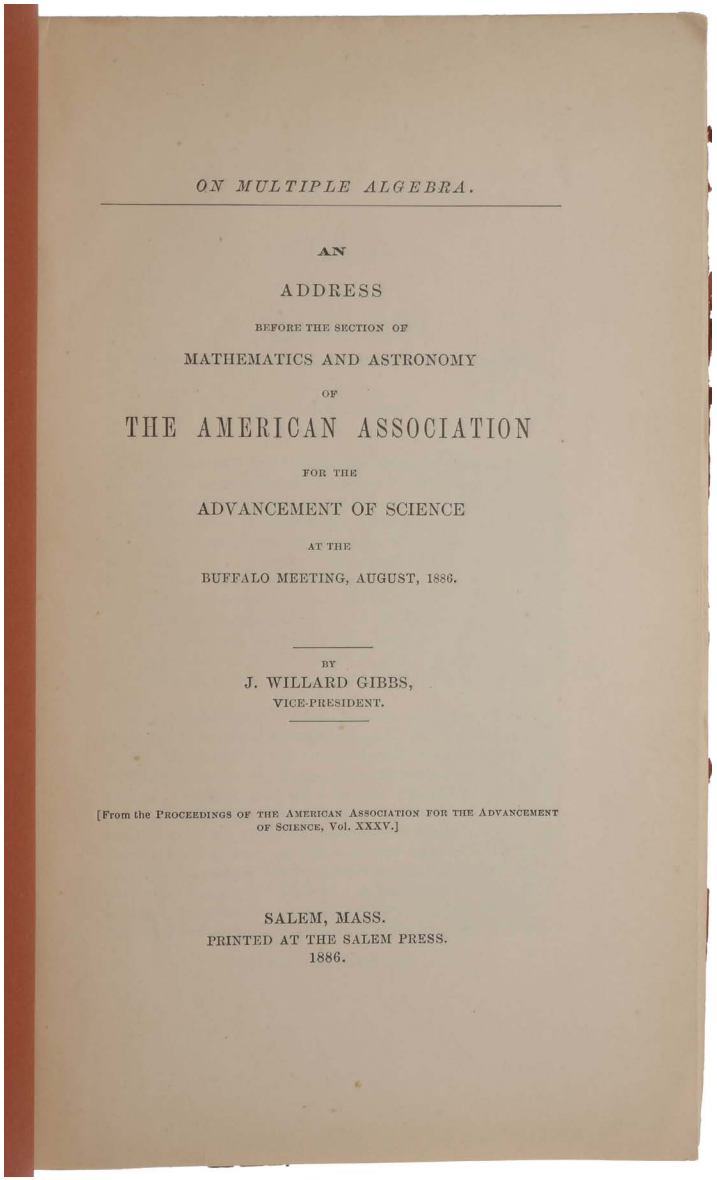
"Gibbs' paper may be taken as symbolic of the ever-increasing interest expressed at that time by pure and applied mathematicians in multiple algebra. The increasing interest in vector analysis was a part, in one way the most influential part, of that trend. Gibbs was able, as some later writers were not, to view vector analysis in the broad perspective offered by multiple algebra. That Gibbs was deeply interested in multiple algebra is shown by the facts that every two or three years he gave a course in multiple algebra and that he planned to publish additional writings on

multiple algebra and had actually done research in this regard" (ibid., pp. 158-160).

"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.

"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" (ibid., p. 151).

J. Crowe, *A History of Vector Analysis*, 1967. F. Dyson, 'Missed opportunities,' *Bulletin of the American Mathematical Society* 78 (1972), 635-652.



ON MULTIPLE ALGEBRA.

AN

ADDRESS

BEFORE THE SECTION OF

MATHEMATICS AND ASTRONOMY

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE

AT THE

BUFFALO MEETING, AUGUST, 1886.

BY

J. WILLARD GIBBS,

VICE-PRESIDENT.

[From the PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, Vol. XXXV.]

SALEM, MASS.

PRINTED AT THE SALEM PRESS.

1886.

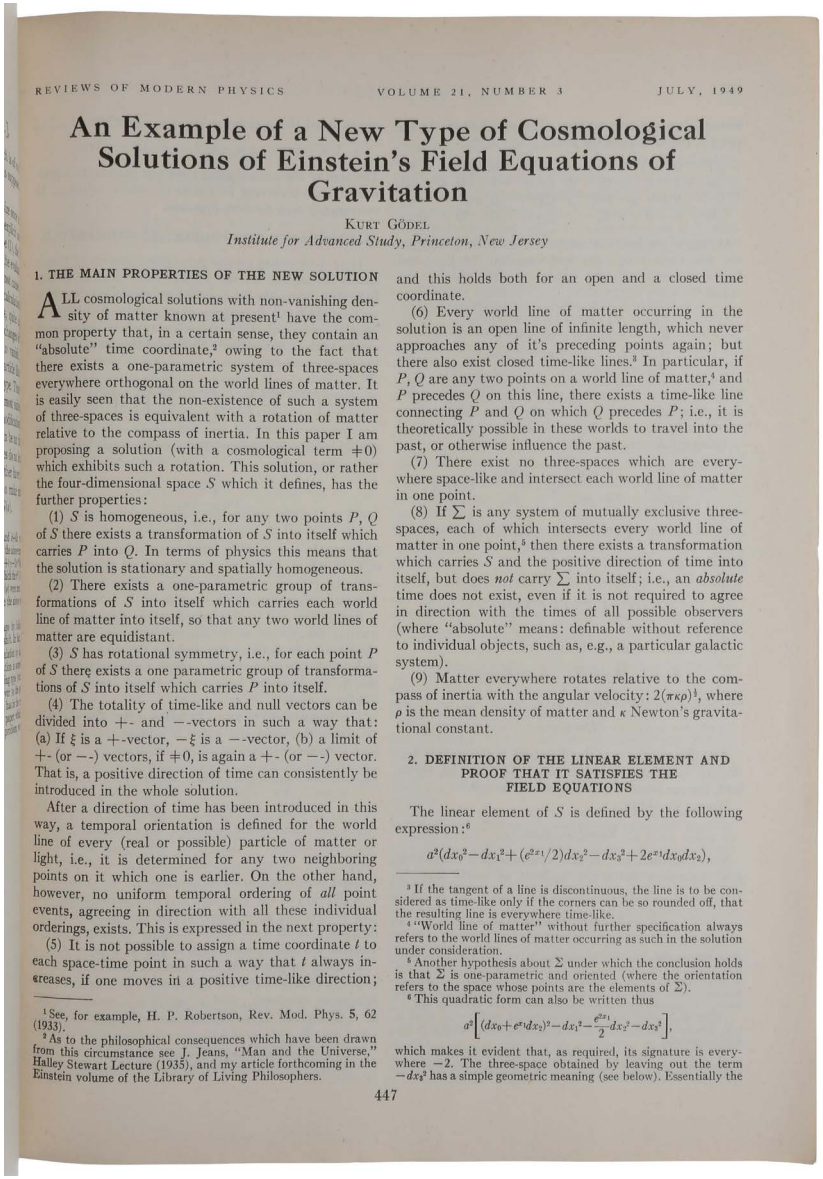
‘ONE OF THE MOST IMPORTANT PAPERS ON RELATIVITY SINCE MY OWN’ - ALBERT EINSTEIN

GÖDEL, Kurt. *An Example of a New Type of Cosmological Solutions of Einstein’s Field Equations of Gravitation.* Lancaster: American Physical Society, 1949.

$3,000

In: Reviews of Modern Physics, vol. 21, no. 3, 1949, 4to (261 x 200 mm), pp. 447-50. The complete issue offered here in original printed wrappers.

First edition, journal issue, in original printed wrappers, of Gödel’s ‘time-travel paper,’ ‘one of the most important [papers] on relativity since my own original paper appeared’ (Einstein to Morgenstern, 1952). “In the 1920s and 1930s, the Friedmann-Robertson-Walker cosmological models had been introduced as the simplest solutions of the equations of Einstein’s general theory of relativity that were consistent with the observed red-shift of distant galaxies. These models were spatially homogenous and isotropic, and were expanding but were non-rotating. Gödel was the first to consider models that were rotating. The possible rotation of the universe has a special significance in general relativity because one of the influences that led Einstein to the theory in 1915 was Mach’s principle. The exact formulation of the principle is rather obscure, but it is generally interpreted as denying the existence of absolute space. In other words, matter has inertia only relative to other matter in the universe. The principle is generally taken to imply that the local inertial frame defined by gyroscopes should be non-rotating with respect to the frame defined by distant galaxies. Gödel showed that it was possible



REVIEWS OF MODERN PHYSICS VOLUME 21, NUMBER 3 JULY, 1949

An Example of a New Type of Cosmological Solutions of Einstein’s Field Equations of Gravitation

KURT GÖDEL
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1. THE MAIN PROPERTIES OF THE NEW SOLUTION

ALL cosmological solutions with non-vanishing density of matter known at present[1] have the common property that, in a certain sense, they contain an “absolute” time coordinate,[2] owing to the fact that there exists a one-parametric system of three-spaces everywhere orthogonal on the world lines of matter. It is easily seen that the non-existence of such a system of three-spaces is equivalent with a rotation of matter relative to the compass of inertia. In this paper I am proposing a solution (with a cosmological term $\neq 0$) which exhibits such a rotation. This solution, or rather the four-dimensional space S which it defines, has the further properties:

(1) S is homogeneous, i.e., for any two points P, Q of S there exists a transformation of S into itself which carries P into Q. In terms of physics this means that the solution is stationary and spatially homogeneous.

(2) There exists a one-parametric group of transformations of S into itself which carries each world line of matter into itself, so that any two world lines of matter are equidistant.

(3) S has rotational symmetry, i.e., for each point P of S there exists a one parametric group of transformations of S into itself which carries P into itself.

(4) The totality of time-like and null vectors can be divided into +- and −-vectors in such a way that: (a) If ξ is a +-vector, $-\xi$ is a −-vector, (b) a limit of +- (or −-) vectors, if $\neq 0$, is again a +- (or −-) vector. That is, a positive direction of time can consistently be introduced in the whole solution.

After a direction of time has been introduced in this way, a temporal orientation is defined for the world line of every (real or possible) particle of matter or light, i.e., it is determined for any two neighboring points on it which one is earlier. On the other hand, however, no uniform temporal ordering of *all* point events, agreeing in direction with all these individual orderings, exists. This is expressed in the next property:

(5) It is not possible to assign a time coordinate t to each space-time point in such a way that t always increases, if one moves in a positive time-like direction; and this holds both for an open and a closed time coordinate.

(6) Every world line of matter occurring in the solution is an open line of infinite length, which never approaches any of it’s preceding points again; but there also exist closed time-like lines.[3] In particular, if P, Q are any two points on a world line of matter,[4] and P precedes Q on this line, there exists a time-like line connecting P and Q on which Q precedes P; i.e., it is theoretically possible in these worlds to travel into the past, or otherwise influence the past.

(7) There exist no three-spaces which are everywhere space-like and intersect each world line of matter in one point.

(8) If Σ is any system of mutually exclusive three-spaces, each of which intersects every world line of matter in one point,[5] then there exists a transformation which carries S and the positive direction of time into itself, but does *not* carry Σ into itself; i.e., an *absolute* time does not exist, even if it is not required to agree in direction with the times of all possible observers (where “absolute” means: definable without reference to individual objects, such as, e.g., a particular galactic system).

(9) Matter everywhere rotates relative to the compass of inertia with the angular velocity: $2(\pi\kappa\rho)^{\frac{1}{2}}$, where ρ is the mean density of matter and κ Newton’s gravitational constant.

2. DEFINITION OF THE LINEAR ELEMENT AND PROOF THAT IT SATISFIES THE FIELD EQUATIONS

The linear element of S is defined by the following expression:[6]

$$a^2(dx_0^2-dx_1^2+(e^{2x_1}/2)dx_2^2-dx_3^2+2e^{x_1}dx_0dx_2),$$

[1] See, for example, H. P. Robertson, Rev. Mod. Phys. 5, 62 (1933).

[2] As to the philosophical consequences which have been drawn from this circumstance see J. Jeans, “Man and the Universe,” Halley Stewart Lecture (1935), and my article forthcoming in the Einstein volume of the Library of Living Philosophers.

[3] If the tangent of a line is discontinuous, the line is to be considered as time-like only if the corners can be so rounded off, that the resulting line is everywhere time-like.

[4] “World line of matter” without further specification always refers to the world lines of matter occurring as such in the solution under consideration.

[5] Another hypothesis about Σ under which the conclusion holds is that Σ is one-parametric and oriented (where the orientation refers to the space whose points are the elements of Σ).

[6] This quadratic form can also be written thus

$$a^2\left[(dx_0+e^{x_1}dx_2)^2-dx_1^2-\frac{e^{2x_1}}{2}dx_2^2-dx_3^2\right],$$

which makes it evident that, as required, its signature is everywhere −2. The three-space obtained by leaving out the term $-dx_3^2$ has a simple geometric meaning (see below). Essentially the

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to have solutions of the Einstein field equations in which the galaxies were rotating with respect to the local inertial frame. He therefore demonstrated that general relativity does not incorporate Mach's principle … In [the offered paper] Gödel presented a rotating solution that was not expanding but was the same at all points of space and time. This solution was the first to be discovered that had the curious property that in it was possible to travel into the past. This leads to the paradoxes such as 'What happens if you go back and kill your father when he was a baby?' It is generally agreed that this cannot happen in a solution that represents our universe, but Gödel was the first to show that it was not forbidden by the Einstein equations. His solution generated a lot of discussion of the relation between general relativity and the concept of causality" (Stephen Hawking, p. 189 in Kurt Gödel: Collected Works: Volume II: Publications 1938-1974).

"Gödel's brilliant burst into the world of physics in 1949 came as a surprise to those who knew him "only" as one of the greatest logicians of all time and thus as a very pure mathematician. However, to his colleagues at the Institute for Advanced Study (IAS) in Princeton, it was less surprising. At IAS, he had famously befriended Einstein, and much earlier, before switching over to mathematics, he had even entered the University of Vienna (in 1924) as a physics student and attended lectures by Hans Thirring, one of the earliest protagonists of Einstein's theories. Moreover, although this was not apparent from his published work, Gödel had maintained a lifelong interest in physics, attending the physics seminars at IAS and keeping abreast of ongoing developments. Then came the crucial trigger: the year 1949 brought Einstein's seventieth birthday, and Gödel was expected to contribute to the planned Festschrift for his friend. Not for the first time did pressure prove conducive to invention" (Rindler, p. 185). Gödel's Festschrift contribution, 'A Remark about the Relationship between Relativity Theory and Idealistic Philosophy' (pp. 557-562 in Albert Einstein: Philosopher-Scientist, P.A. Schilpp (ed.), 1949), appeared almost simultaneously with the offered paper. It treated the philosophical implications of Gödel's model, while the offered work provides the technical derivation from the Einstein field equations.

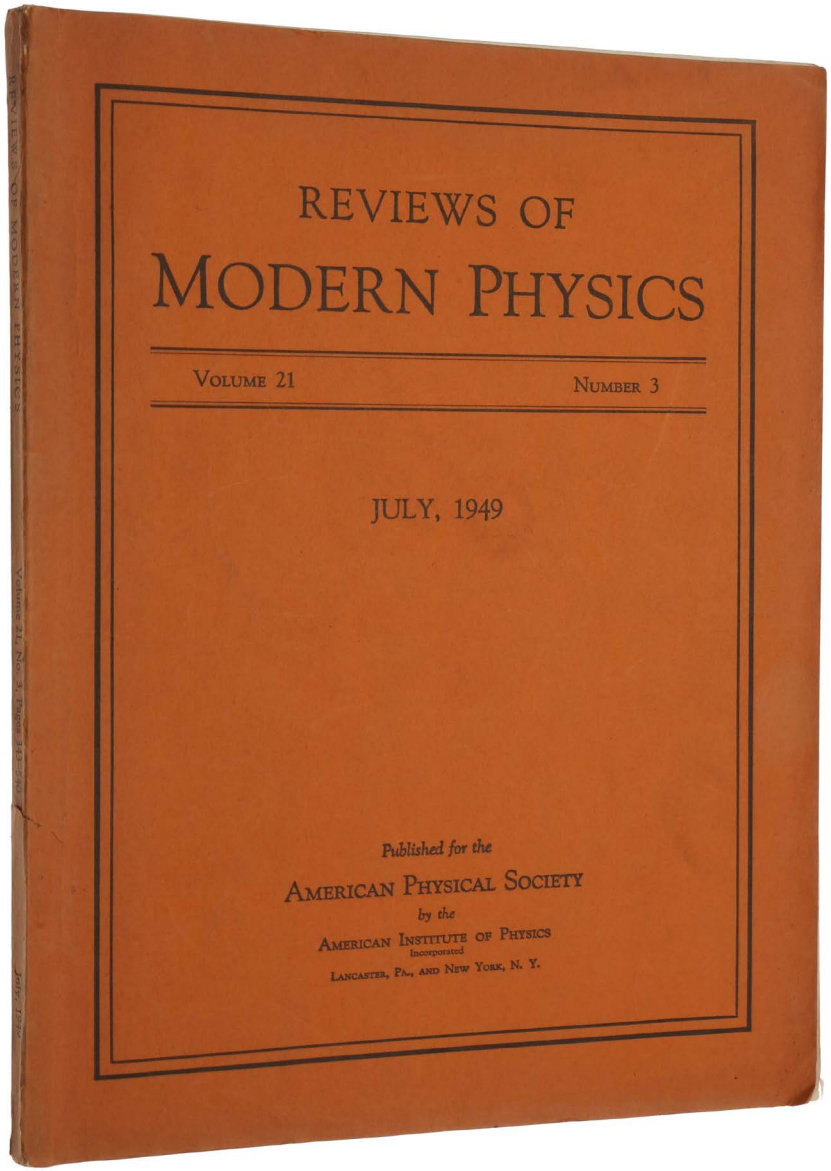
"Gödel stated that he was motivated to invent his model universe from sympathy for Kant's philosophy of time. It was to serve as the first counterexample on the cosmic scale to the objective view of time, which treats time as an infinity of layers "now" coming into existence successively. By 1905, Einstein had already shown this view to be problematic with his special theory of relativity. Indeed, one of the greatest shocks delivered by that theory was the discovery that simultaneity is relative … The situation becomes even worse with the space-times of general relativity that correspond to real-life irregular matter distributions. Only in the idealized homogeneous-isotropic universes introduced by [Alexander] Friedman ['Über die Krümmung des Raumes' & 'Über die Möglichkeit einer Welt mit konstanter negativer Krümmung des Raumes,' Zeitschrift für Physik, 1922 & 1924] of which the 1917 static Einstein universe was a special case ['Kosmologische Betrachtungen zur allgemeinen Relativitatstheorie,' Sitzungsberichte der Königlich preussischen Akademie der Wissenschaften], do we find an absolutely (geometrically) determined worldwide time. These universes (except the Einstein universe) expand with a single expansion function, and their intrinsically determined time slices correspond to constant values of their steadily diminishing density. Thus the objective (or absolute) view of time got a reprieve from Friedmanian cosmology – which Gödel dismissed as accidental. His purported aim was to show that in more general cosmologies, no such objective time need exist" (ibid., p. 188).

The idea for the particular model Gödel constructed probably arose from his reading of a paper by George Gamow ('Rotating universe?', Nature vol. 158, 19 October 1946, p. 549) which suggested that the whole universe might be in a state of uniform rotation and that this rotation might explain the observed rotation of galactic systems: "One of the most mysterious results of the astronomical studies

of the universe lies in the fact that all successive degrees of accumulation of matter, such as planets, stars and galaxies, are found in the state of more or less rapid axial rotation. In various cosmogonical theories the rotation of planets has been explained as resulting from the rotation of stars from which they were formed. The rotation of stars themselves can be presumably reduced to their origin from the rotating gas-masses which form the spiral arms of various galaxies. But what is the origin of galactic rotation?" (abstract of Gamow's paper). "Suddenly he had a problem worthy of his genius and the perfect gift for Einstein! However, that is not all. Gödel seems to have recognised that in a rotating universe, there would be no absolute time so that his Kantian ambition of superseding Friedman would also come true. Thus fortune placed in his hands not only a significant relativistic problem but one that even fell within his original Kantian program and that, when it was all done, would turn out to be far more beautiful than he could possibly have foreseen" (Rindler, p. 189).

At the time Gödel wrote his papers on cosmology, "General relativity was Einstein's new (now a century old) theory of gravity, in which Newton's force of gravity is replaced by the curvature of four-dimensional space-time and where free matter moves along the natural rails of this curved space-time, namely, its geodesics … Our actual universe is, of course, lumpy … Instead one studies the smoothed-out version of actual universes and makes the assumption that the dynamics are effectively the same. The smoothed-out counterpart of any universe is its substratum, and not only must the lumpiness be smoothed out but so must the locally irregular motions. The actual galaxies then sit on this (generally expanding) substratum more or less uniformly distributed and with only relatively small irregular proper motions.

"For the standard models of general relativity, as well as for Gödel's model, these substrata satisfy the so-called cosmological principle. This hypothesis, which is



well supported by observation, asserts that the universe is regular and that our place in it, and in fact that of any other galaxy, is not special. Thus, for the sake of constructing the model, the substratum is assumed to be perfectly homogeneous at all times.

"Additionally, our universe is known to expand, and it is commonly believed to have originated in the big bang some fourteen billion years ago. A realistic substratum must therefore expand. Gödel's model, although homogeneous, ignores this expansion: it is stationary, the same at all times. There is yet another difference from the usual models of general relativity: Gödel's model is not isotropic. Its substratum is somewhat like a homogeneous crystal, having preferred directions at each point. We can picture it as a stack of identical layers, infinite in all directions. Each layer is actually a Lobachevski plane, a two-dimensional space of constant negative curvature. On this layered spatial framework exists an overall time …

"So far, this all looks fairly harmless, but now come the surprises. Consider an inertial compass, also called a 'gyrocompass'. This instrument contains a number of gyroscopes and has the property of always pointing in the same direction in space. Install such a gyrocompass suitably in a stunt airplane and point it, for example, at the sun, and then fly any number of loops, twists and turns. The gyrocompass ignores them all and keeps steadily pointing at the sun. Now fix such a gyrocompass to every galaxy in Gödel's universe, and behold: they all rotate in unison about the normal of the layers. This seems to indicate that the entire universe rotates rigidly in the opposite direction – but relative to what? … Gödel laconically commented: 'Evidently this state of affairs shows that the inertial field is to a large extent independent of the state of the matter. This contradicts Mach's principle, but it does not contradict relativity theory' …
"Mach's principle, as formulated by Einstein in his early quest for general relativity, was supposed to explain the mysterious existence of the preferred set of inertial frames against which rotation and acceleration are measured in both Newton's theory and Einstein's special theory of relativity. Mach's principle says that the local inertial frame, or inertial field, is actively determined by some average of the motions of all the masses in the universe. Einstein had hoped that general relativity would show in detail how this determination works, but for a number of reasons, he later (in the 1930s) discarded Mach's principle. Therefore the inertial properties of Gödel's model, although paradoxical, were not totally unacceptable to Einstein …

"Now for a second surprise. Consider a large circle in one of the layers of Gödel's substratum. Now travel along this circle at a very large velocity. Behold: you return to the galaxy from which you started at an earlier time than when you left, yet by your own reckoning, you have aged normally during the trip. You could now encounter your own father when he was a child, and, if you were wicked, you could kill him, thereby preventing your own birth. That is an awful paradox, and one would hope that nature has ways to prevent space-times such as Gödel's from actually materializing. (In special relativity, where a similar danger lurks, nature prevents it by imposing a universal speed limit – the speed of light.) That hope, indeed, was Einstein's reaction to Gödel's result. Gödel himself – surprisingly perhaps – defended his model on the grounds that it would cost impossible amounts of energy for a space traveller to accomplish such a journey. Later, he granted that one could simply send a light signal, guided by suitably placed mirrors, along a sufficiently large polygonal path to do the same damage, but the radius would have to be so immense as to render even this procedure impracticable" (ibid., pp. 186-8).

Gödel's original cosmological model is static and so cannot represent the expanding universe of actual observation. To deal with this problem, Gödel later produced an expanding rotating model ('Rotating universes in general relativity

theory,' Proceedings of the International Congress of Mathematicians (1950), p. 175). However, this later model does not actually possess any closed timelike curves, so the conundrums raised in the preceding paragraphs do not arise. But conflict with actual observation remains, as the rotation that is characteristic of all the Gödel models is not actually observed.

"Despite what has sometimes been suggested, Gödel's cosmological models were well appreciated by the relativity community. In particular, Hawking explicitly cited Gödel's example, when he addressed the possibility that the presence of closed timelike curves might supply an escape route from the singularity theorems, the conclusion being that closed timelike curves do not appear to supply such an escape route. The general view appears to be that it is not unreasonable to dismiss such closed timelike curves as unphysical, but there are some dissenting opinions" (Penrose, p. 4).

Roger Penrose, Gödel, Relativity, and Mind,' Journal of Physics: Conference Series 82 (2007), pp. 1-5. Wolfgang Rindler, 'Gödel, Einstein, Mach, Gamow, and Lanczos: Godel's remarkable excursion into cosmology', pp. 185-212 in Kurt Gödel and the Foundations of Mathematics: Horizons of Truth, Baaz et al (eds.), Cambridge University Press, 2011.

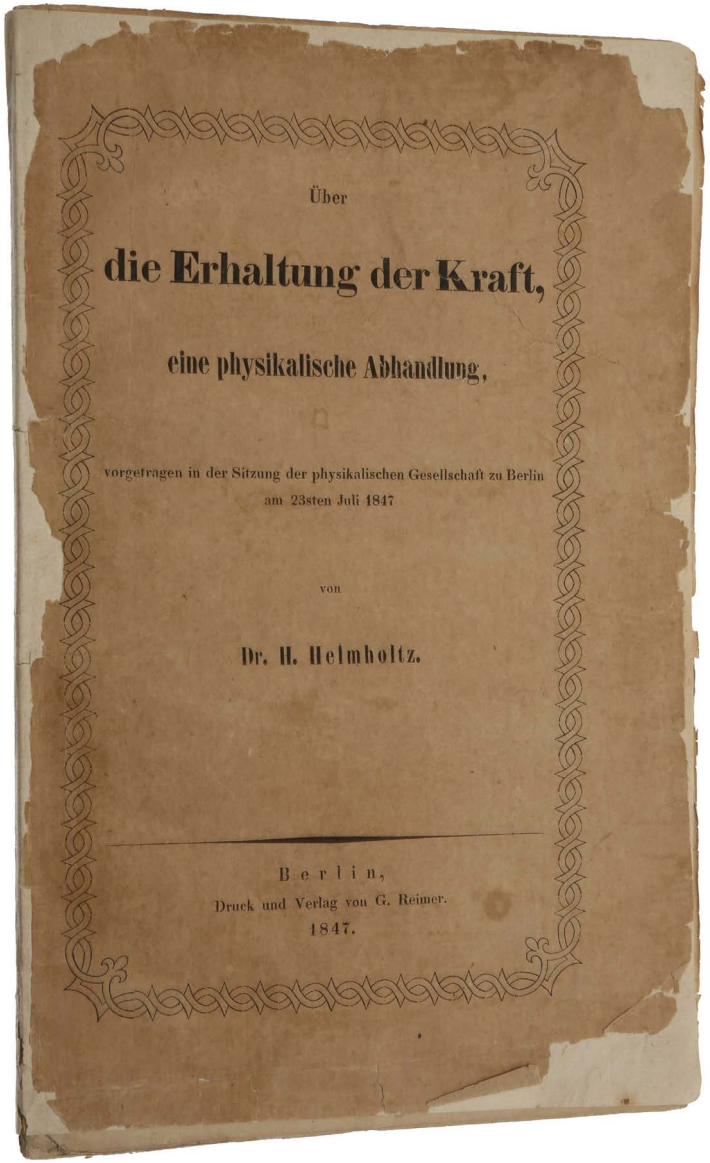
PMM 323 -
THE CONSERVATION OF ENERGY

HELMHOLTZ, Hermann. *Über die Erhaltung der Kraft, eine physikalische Abhandlung.* Berlin: Georg Reimer, 1847.

$15,000

8vo (232 x 147 mm), pp. [iv], 72. Disbound, uncut, original front printed wrapper mounted on old paper, at early date, and used as a spine strip to hold the leaves together (wrapper chipped at edges).

First edition, rare, of "the first comprehensive statement of the first law of thermodynamics: that all modes of energy, heat, light, electricity, and all chemical phenomena, are capable of transformation from one to the other but are indestructible and cannot be created" (PMM). "On the basis of this short paper, written when he was only twenty-six, Helmholtz is ranked as one of the founders, along with Joule and Mayer, of the principle of conservation of energy. The paper sets forth the philosophical and physical basis of the energy conservation principle: Helmholtz maintained that the scientific world view was based on two abstractions, matter and force, and since the only possible relationship that can exist among the ultimate particles of matter is a spatial one, then ultimate forces must be moving forces radically directed. This can be inferred from the impossibility of producing work continually from nothing. Helmholtz analyzed different forms of energy and different types of force and motion, grouping them into two categories, active (kinetic) and tension (potential). He also gave mathematical expression to the energy of motion, providing an experimental measure for research on all forces, including those of muscle physiology and


Über

die Erhaltung der Kraft,

eine physikalische Abhandlung,

vorgetragen in der Sitzung der physikalischen Gesellschaft zu Berlin
am 23sten Juli 1847

von

Dr. H. Helmholtz.

Berlin,
Druck und Verlag von G. Reimer.
1847.

chemistry" (Norman). "Intended expressly for 'physicists,' Helmholtz's 1847 paper must be counted as one of the most impressive first publications in the history of physics. Helmholtz had the highest regard for the principle he developed there, speaking of it fifteen years later, for example, as the most important scientific advance of the century because it encompassed all laws of physics and chemistry. On the occasion of Helmholtz's hundredth birthday, in 1921, his former student Wilhelm Wien could write that the significance of the principle was still growing" (Jungnickel & McCormach, p. 161). ABPC/RBH record 10 copies in the last fifty years, the most recent being a copy in a modern binding, without wrappers, sold at PBA Galleries in 2015, which made $27,000.

"Benjamin Thompson, Count Rumford, the American-born scientist largely responsible for the foundation of the Royal Institution and the founder of the Royal Society's Rumford Medal, was the first to challenge successfully the accepted theory that heat was the manifestation of an imponderable fluid called 'caloric'. He declared, and gave experimental proof before the Royal Society in I798, that heat was a mode of motion. Rumford was, in fact, conspicuous in his day for what was considered his old-fashioned theory of heat. He harked back to the seventeenth-century views of Bacon, Locke and Newton in opposition to the fashionable modern theory of caloric, which, indeed, worked very well, especially in chemistry.

"Sadi Carnot, in 1824, approached very close to the principle of the conservation of energy and his brother found among his papers an almost explicit statement of it, although Carnot had actually used the caloric theory in his researches. J. R. Mayer, in Liebig's Annalen, 1842, demonstrated its application in physiological processes, but his paper made little impression until it was reprinted as a polemic in 1867. J. P. Joule made a manuscript translation of Mayer's thesis for his own use, and, in a series of papers in the Philosophical Magazine, 1840-3, provided experimental proof of the mechanical equivalent of heat for physical phenomena" (PMM). Helmholtz worked out the principle of the conservation of force soon after completing his education in Berlin. While a student at the gymnasium in nearby Potsdam where his father taught classical languages, he had decided that he wanted to study physics. Since his father could afford this plan only if he studied physics within a medical education, in 1838 he entered the Friedrich-Wilhelms-Institut in Berlin. This state medical-surgical institution trained army physicians by providing them with a free medical education at Berlin University. Helmholtz wrote his dissertation at the university on the physiology of nerves under Johannes Muller and received his M.D. in 1842. In 1843 he published his first independent investigation in Muller's Archiv and that year took up his duties as army surgeon at Potsdam. There at the army post, he set up a small physical-physiological laboratory. He also kept up the scientific associations he had formed at Berlin University.

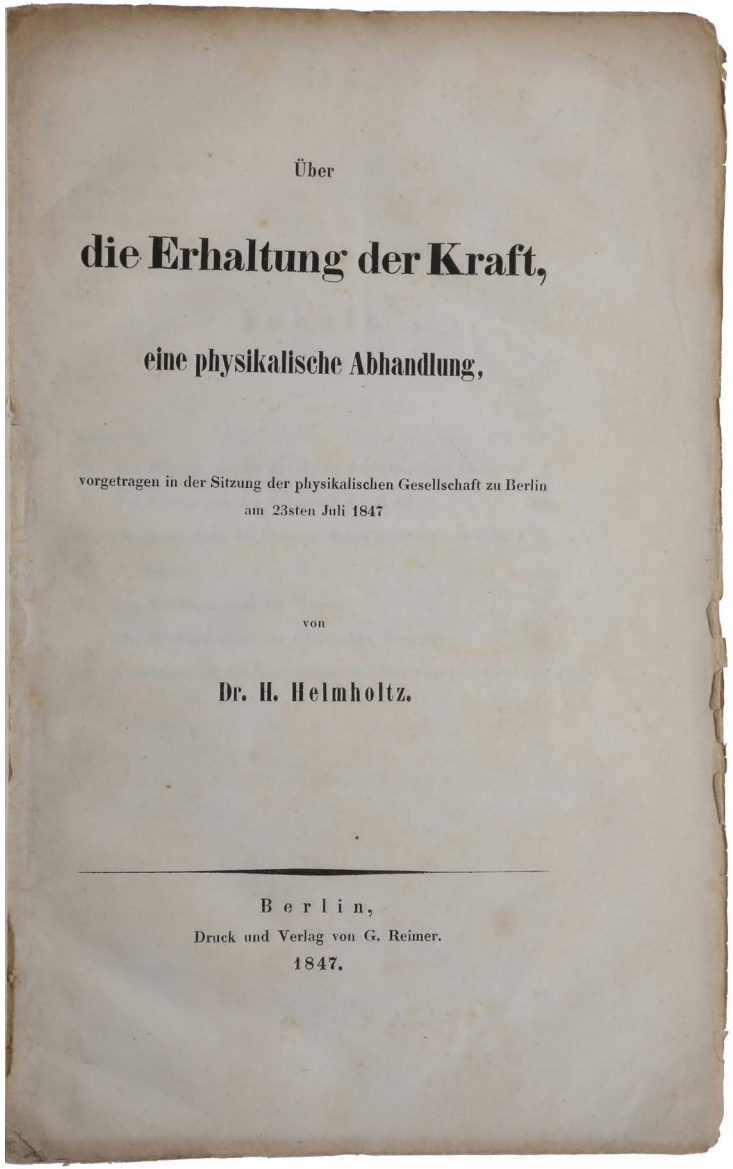
In Berlin, Helmholtz was drawn into the circle of Muller's students, befriending especially du Bois-Reymond and Brucke, who were united in their desire to eliminate from physiology the concept of life force, in their eyes an unscientific concept left over from nature philosophy. They wanted to see how far physics and chemistry could go in explaining life processes, which brought them into contact with physicists in Berlin, above all with Magnus. Because a state examination for physicians required Helmholtz to spend a half year in Berlin, in the winter of 1845-46 he worked regularly in Magnus's private laboratory. Du Bois-Reymond, who had participated in Magnus's physical colloquium, introduced Helmholtz to the newly formed Berlin Physical Society, which was soon to provide the first audience for his work on the conservation of force. He regularly attended the meetings of the society, and for the first volume, as for later volumes, of the Fortschritte der Physik, he reported on researches in physiological heat.

"Helmholtz's work on the conservation of force required a sound knowledge of mathematical physics, which he had acquired in his early years in Berlin. He

had read extensively in the literature; in 1841, for example, after his first medical examinations were over, he was left with some free time, which he devoted to the study of mathematics and the advanced parts of mechanics. On his own, or with a friend, he studied the writings of Laplace, Biot, Poisson, Jacobi, and others. He attended no lectures in mathematical physics or in mathematics at Berlin; in these subjects he was largely self-taught.

"By 1847, the year of his publication on the conservation of force, Helmholtz had already been 'convinced for years,' according to his friend and biographer Leo Koenigsberger, of the validity of a principle of this sort. He recognized that the question of whether living beings are to be understood by the action of a life force or by the action of the same forces that occur in lifeless nature is closely connected with a conservation principle for forces. He also recognized that to establish to the satisfaction of the scientific world a mathematically formulated conservation principle would require a series of investigations in various parts of physiology and physics. In 1845, for example, he published a paper in which he tested his physical understanding of a difficult physiological problem, namely, the chemical changes occurring in muscles owing to their mechanical action. Studying frogs with the help of a self-constructed electrical machine and a Leyden jar, he succeeded in demonstrating these changes and even obtained quantitative results. For even more exact results, he had to determine the relations between the action of muscles and the heat developed, which required new investigations. In the Fortschritte der Physik for 1845, which appeared in 1847, Helmholtz published a report on theories of physiological heat which he later acknowledged as belonging to his work on the conservation of force.

"In February 1847, while still an army surgeon at Potsdam, Helmholtz wrote to du Bois-Reymond that in his latest reworking of an essay on the conservation of force, he had 'thrown overboard everything that smells of philosophy,' and



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Druck und Verlag von G. Reimer.
1847.

he was anxious for du Bois-Reymond's opinion of how it would go down with the physicists. That summer, Helmholtz read the completed work to the Berlin Physical Society, where it aroused enthusiasm, at least among some members. Helmholtz immediately sent it to Magnus asking him to forward it to Poggendorff for publication in the Annalen der Physik. Poggendorff appreciated the importance of the problem Helmholtz addressed and his handling of it, but he rejected the paper. It was too long to be fitted into the Annalen that year, Poggendorff said; but his main reason for rejecting it had to do with its nature: 'The Annalen is necessarily dependent above all on experimental investigations,' and Poggendorff would have to sacrifice some of these if he wished to 'open the door to theoretical' investigations like Helmholtz's.

"Through Magnus, Poggendorff recommended that Helmholtz have the work published privately. Helmholtz accordingly approached the Berlin publisher G. A. Reimer, to whom he explained that the work would not be expensive to produce: it was not long, required no copper plates, and had 'relatively little mathematical type.' The subject of the work was the generalization of a 'fundamental law of mechanics,' he explained further; he had reached his result by 'extensive and exact' work on 'all branches of physics.' Privately, he had learned of considerable interest in this work; he submitted to Reimer letters about it from Magnus, du Bois-Reymond, and Brucke, and he added that Muller could testify to his scientific ability. He realized that he could expect no money from its publication and only wanted fifteen free reprints. Reimer agreed to publish it and, to Helmholtz's surprise, paid him an honorarium.

"From his study of the older mechanical treatises, Helmholtz learned the strong proof of the impossibility of perpetual motion. In his physiological studies, he questioned the possibility of perpetual motion outside mechanics, in heat, electricity, magnetism, light, and chemistry. His solution to the problem of determining precisely which relations must obtain between natural forces so that perpetual motion is impossible in general was the principle of the conservation of force.

"Helmholtz based the principle on either of two maxims, which he proved equivalent. One is that from any combination of bodies, it is impossible continuously to produce moving force from nothing. The other is that all actions can be reduced to attractive and repulsive forces that depend solely on the distance between material points. The problem of science is, he said, to reduce all phenomena to unchanging causes, which are the unchanging forces between material points. As the 'solvability of this problem is also the condition of the complete comprehensibility of nature,' the problem of 'theoretical natural science' will be solved once this 'reduction of natural phenomena to simple forces is completed and at the same time is proven to be the only possible reduction the phenomena allow.'

"The impossibility of unlimited moving force had been adopted as a maxim by Carnot and Clapeyron in their theoretical studies of heat, and Helmholtz made it his 'purpose' to extend it throughout 'all branches of physics.' The maxim is equivalent in mechanics to the principle of the conservation of 'living force' (or 'vis viva' or 'kinetic energy'). Helmholtz proved that this principle requires that the forces be 'central,' that is, that they depend on the distance between material points and act along their joining line. He showed that the increase in the living force of a material point due to the action of a central force equals the sum of the 'tension forces' due to the change in the position of the point … Helmholtz concluded that the sum of the living forces and the tension forces is constant. This he called the 'principle of the conservation of force.'

"Helmholtz applied this conservation principle to some mechanical theorems and then to the other parts of physics, which provided the truly interesting cases and the testing ground. A supporter of the mechanical theory of heat, he

accounted for the apparent loss of living force of two bodies after undergoing an inelastic collision by the conversion of their living force into tension forces and heat. He was especially interested in applying the conservation principle to electricity, magnetism, and electrodynamics, subjects which offered manifold instances of force conversions. For example, he deduced the electromotive force of two metals in a cell by equating through the conservation principle the heat developed chemically in the cell to that developed electrically in the wire. In this example, in which heat serves as a measure of the forces, he brought together nearly all of the known quantitative laws of electric current: Ohm's law, Lenz's law for the heat developed in a length of wire, James Prescott Joule's more general law for the heat developed in any circuit, the laws of complex circuits that Kirchhoff was then working out, and Faraday's law of electrolysis. In another example, Helmholtz applied the conservation principle to connect the chemical, thermal, and mechanical processes entering the electrodynamic interaction of a fixed, closed current produced by a cell and a nearby magnet free to move in space. Here he made use of Neumann's potential for a closed current; with it and by simple mathematical steps, he derived a number of Neumann's cases of induced currents. In addition to recovering these known results, he derived a new result, showing the power of the conservation principle to link the parts of physics; by equating his and Neumann's formulas for the current in the wire, he showed that Neumann's empirical, undetermined constant from electrical theory e is the reciprocal of the mechanical equivalent of heat. In these examples, to apply the conservation principle, Helmholtz did not need a detailed knowledge of the mathematical form of the acting central forces, the existence of which the principle presumably guaranteed. The forces were often still unknown or problematic; for example, with regard to Weber's fundamental law of electric action, which relates the force between electric masses to their relative motion, Helmholtz observed that no hypothesis had yet been established that could reduce inductive phenomena to 'constant central forces.'

"Throughout his paper on the conservation of force, Helmholtz referred not only to theoretically founded laws but also to a good deal of experimental work on the establishment of laws by Riess, Poggendorff, Weber, and others. He pointed to his predictions as waiting to be tested. His purpose was not just theoretical; it was also to show the experimental significance of the new results he obtained by joining established laws by means of the conservation principle. He concluded his study with the observation that the 'complete confirmation' of the conservation principle was a main task of physics in the immediate future.

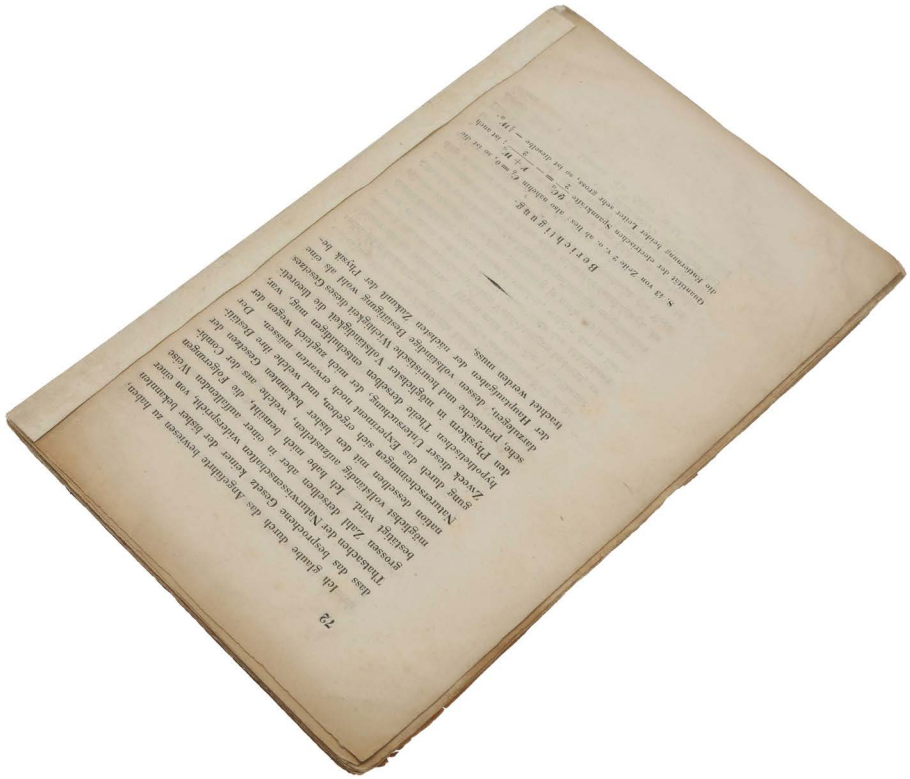
"But as Poggendorff noted when he rejected it, Helmholtz's paper did not report original experiments. For that reason it could seem overly speculative to experimental physicists, who were not at first persuaded of the conservation principle. When physicists did admit it into their literature, they did so with caution. 'I have received the first part of the physics annual report,' Helmholtz wrote to du Bois-Reymond about the Fortschritte der Physik for 1847, 'and was not a little surprised to see my Erhaltung der Kraft placed by Karsten with physiological heat phenomena, although I had submitted it written up separately.' Later Helmholtz reported for the Fortschritte on related papers by Robert Mayer and others, and he took the occasion to place his own paper in the physical context he had originally intended for it.

"The task of persuading physicists of the conservation principle was not entirely Helmholtz's in any case. With marked differences of approach and purpose, Mayer and several other natural scientists in the 1840s worked on problems arising from a widely shared belief in the unity of nature and the indestructibility and transformability of forces. As one of several statements of the measure of the relations between the forces of nature, Helmholtz's came to be regarded as the mathematical foundation for the principle of the conservation of 'energy.' Within a few years, Helmholtz acknowledged that his terms 'living force' and 'tension force' were synonymous with W. J. M. Rankine's 'actual [kinetic] energy' and

'potential energy' and that Rankine's term 'conservation of energy' was preferable to his own 'conservation of force.'

"With Helmholtz's principle, the several, often qualitative, assertions of the conservation and convertibility of forces received precise expression. The many newly discovered relations between the forces of nature did not require any major change in the understanding of these forces, which derived from the example of Newton's gravitational force; this was one of the more remarkable implications of Helmholtz's paper. Physicists who accepted Helmholtz's reasoning were concerned on the most fundamental level, with just those things mechanics was concerned with: material points, constant central forces, relative positions and motions, the laws of motion and associated principles such as the principle of virtual velocities, and the principle of the conservation of force, or energy, which Helmholtz saw as an extension of the principle of the conservation of living force in mechanics. Helmholtz claimed that his paper of 1847 was independent of metaphysical considerations; later, in 1881, when he included it in his collected papers, he acknowledged that he had been indebted to Kant's philosophy in his view that the law of causality was essential for understanding nature and that central forces were ultimate causes. He derived his conservation principle within a certain picture of the physical world, one governed by mechanical concepts and laws. It was one of the great conceptions of nature underlying much nineteenth-century physical research, and in Germany Helmholtz gave it a complete definition; over the course of his long career, he developed its implications throughout physics" (Jungnickel & McCormach, pp. 156-61).

Dibner 159. Garrison-Morton (online) 611. Horblit 48. Norman 1039. *Printing and the Mind of Man* 323. Jungnickel & McCormach, Intellectual Mastery of Nature, Vol. 1, *The Torch of Mathematics* 1800-1870 (1986).



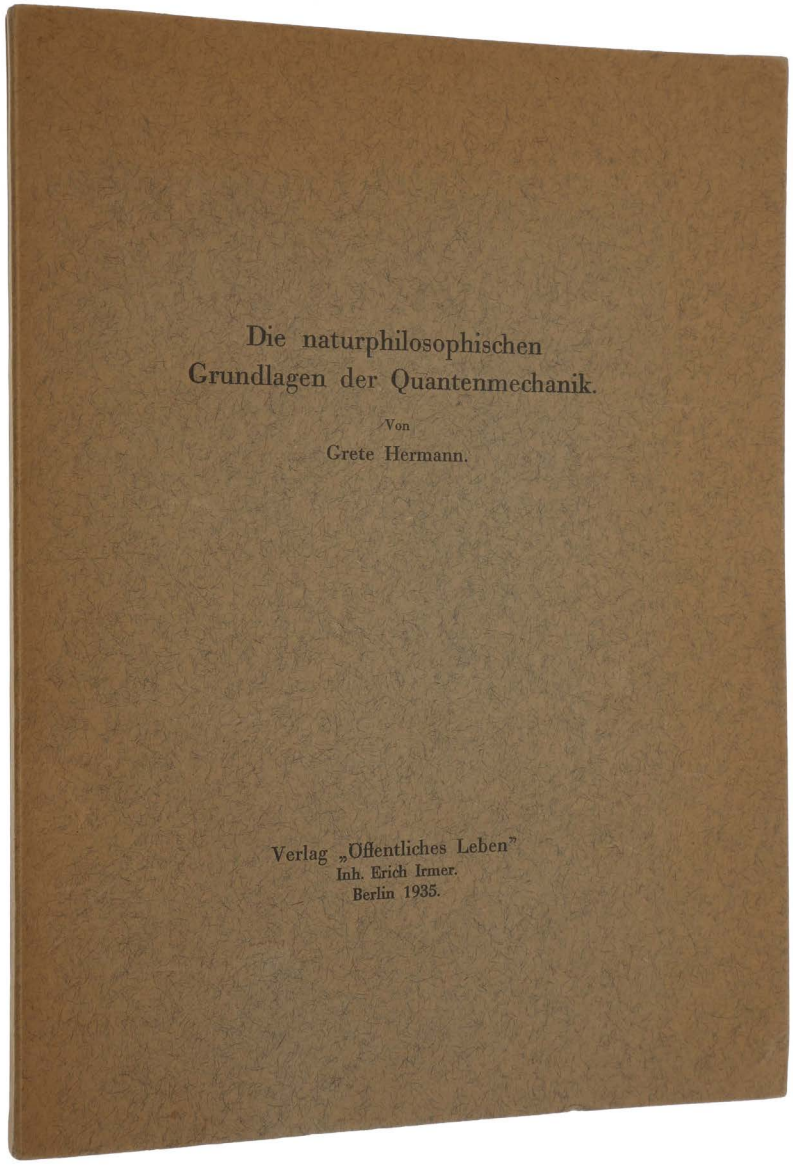
REFUTING VON NEUMANN ON HIDDEN VARIABLES 30 YEARS BEFORE JOHN BELL

HERMANN, Grete. *Die naturphilosophischen Grundlagen der Quantenmechanik.* Berlin: Verlag Öffentliches Leben, 1935.

$1,500

Offprint from Abhandlungen der Fries'schen Schule, Neue Folge, 6. Band, 2. Heft. 8vo (237 x 172 mm), pp. 84. Original printed wrappers. A fine copy.

First edition, very rare offprint issue, of Hermann's doctoral thesis. "In 1935 Hermann published an argument demonstrating an apparent flaw in John von Neumann's 1932 proof which was widely claimed to show that a hidden variable theory of quantum mechanics was impossible. Hermann's result went unnoticed by the physics community until it was independently discovered and published by John Stewart Bell in 1966, and her earlier discovery was pointed out by Max Jammer in 1974. Some have posited that had her critique not remained nearly unknown for decades, the historical development of quantum mechanics may have been greatly affected; in particular, it has been speculated that a wider awareness of her work would have put in question the unequivocal acceptance of the Copenhagen interpretation of quantum mechanics, by providing a credible basis for the further development of nonlocal hidden variable theories … This work has been referred to as "one of the earliest and best philosophical treatments of the new quantum mechanics." In this work, she concludes: "The theory of quantum mechanics forces us … to drop the assumption of the absolute character of knowledge about nature, and to deal with the principle of causality independently of this assumption. Quantum mechanics has therefore not contradicted the law of



causality at all, but has clarified it and has removed from it other principles which are not necessarily connected to it"" (Wikipedia, accessed 17 October 2017). Carl Friedrich von Weizsäcker referred to her research the "first positive and undeniable contribution to the elucidation of the epistemological implications of quantum mechanics." Werner Heisenberg devoted an entire chapter of his book Physics and Beyond: Encounters and Conversations (1971) to a reconstruction of discussions that he had on quantum mechanics and Kantian philosophy with Hermann and von Weizsäcker, and he indicated that both contributed important insights to this topic. In 1936 Hermann was awarded the Richard Avenarius Prize by the Academy of Sciences of Saxony in Leipzig for her work on the significance of quantum theory and field theory of modern physics for the theory of knowledge. COPAC: Cambridge only.

Provenance: Manfred Moritz (1909-90), German-Swedish philosopher (signature, 'Lund 4.4.42', on front free endpaper). According to the Routledge Encyclopaedia of Philosophy, Moritz was "Sweden's foremost expert on Kant's ethics after Hägerström".

"John von Neumann was primarily engaged in the mathematical aspects of quantum mechanics when, during the late 1920s, he was privatdozent at the University of Berlin. But he also took great interest in the discussions on the philosophical implications of the new theory which were held frequently at the end of the famous Physics Colloquia of that University. The main problem that attracted von Neumann's attention was the precise nature of the statistical character of quantum mechanics, the question why, in spite of the unambiguous definition of the state of a system by the state function, only statistical statements could be made about the value of the physical quantities (observables) involved.

"The fact that ensembles described by the same state function exhibit dispersion suggested to him two a priori conceivable interpretations: either (1) the individual systems, though described by the same [wave function], differ in additional hidden parameters whose values determine the precise outcome of the measurements, or (2) all individual systems of the ensemble are in the same state "but the laws of nature are not causal: dispersion results from nature's disregard of the principle of sufficient reason" (Jammer, pp. 265-6). Possibility (1) became known as the hypothesis of 'hidden variables'.

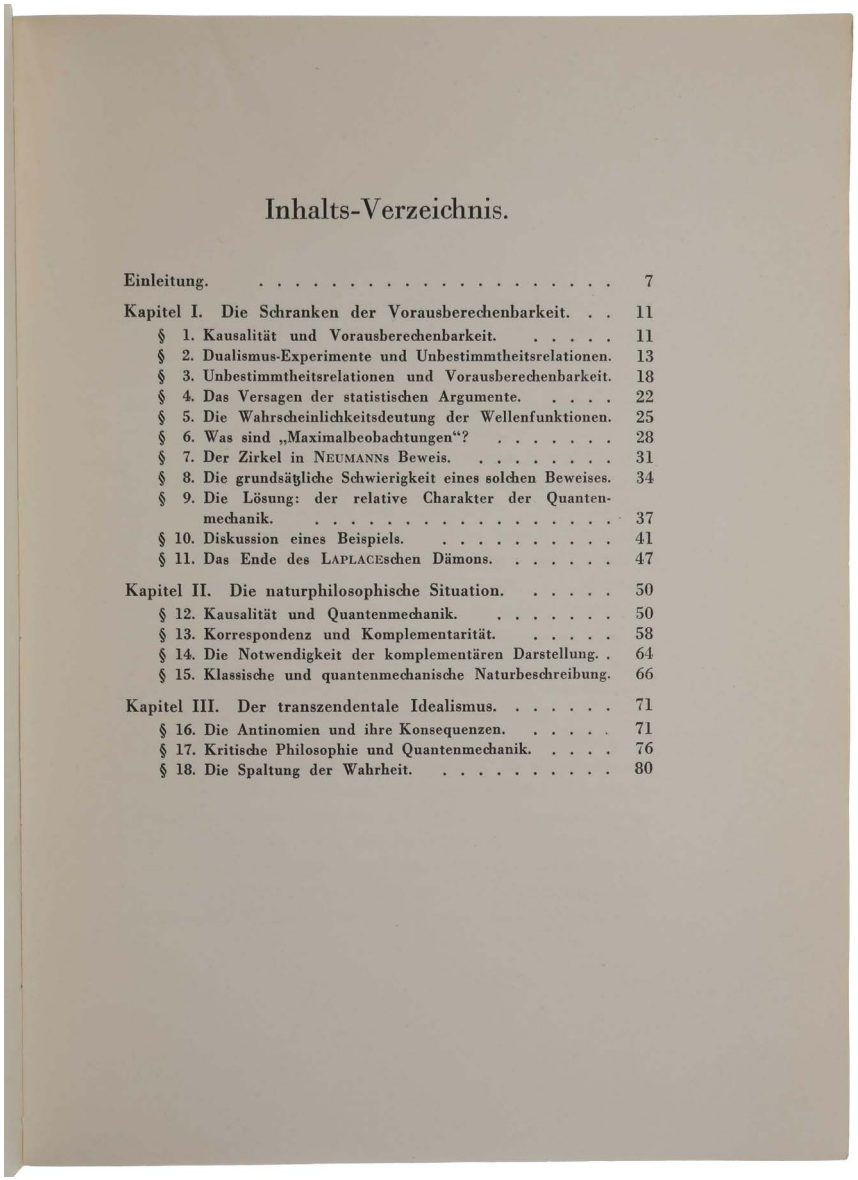
In his 1932 work, Mathematische Grundlagen der Quantenmechanik, von Neumann gave a mathematical 'proof' that hidden variable theories were not consistent with quantum mechanics. This seems to have been accepted until Bell in his 1966 paper 'On the problem of hidden-variables in quantum mechanics' (Reviews of Modern Physics 38, pp. 447-452) showed, by analyzing the de Broglie-Bohm 'pilot wave' theories, that von Neumann's proof must be incorrect. In 1974, Jammer pointed out (Philosophy of Quantum Mechanics, p. 272) that Bell had been anticipated by Hermann in her doctoral thesis, published almost four decades earlier. Hermann's criticism is part of a wider discussion of the idea of causality in quantum mechanics.

"Having started her academic career with the study of mathematics under Emmy Noether in Göttingen, Grete Hermann became greatly influenced by the philosopher Leonard Nelson, the founder of the Neo-Frisian school, and in the spring of 1934 joined Heisenberg's seminar in Leipzig. Leipzig in the early 1930s was not only, next to Göttingen and Copenhagen, one of the foremost centers for the study of quantum mechanics and its applications (due to the presence of Felix Bloch, Lev Landau, Rudolf Peierls, Friedrich Hund, and Edward Teller), it also became famous for its study of the philosophical foundations and epistemological implications of the quantum theory, particularly after Carl Friedrich von Weizsäcker, at the age of only 18 years, joined the Heisenberg group …

"Although not a specialist in physics by schooling, she was able, assisted by B. L. van der Waerden and C. F. von Weizsäcker, to participate most actively in the work of the seminar. As a result of these discussions Grete Hermann published in March 1935 a long essay on the philosophical foundations of quantum mechanics [offered here], which still deserves attention today. Hermann's point of departure – which led her to the relational conception of quantum mechanical description – was the empirical fact of the unpredictability of precise results in the measurements on microphysical objects. The usual way out of such a situation by searching for a refinement of the state description in terms of additional parameters is denied by the theory.

"Since Hermann rejected von Neumann's proof of the impossibility of hidden variables, … she raised the question of what justifies this denial. To reject the possibility of such a refinement of the state description merely on the basis of its present unavailability would violate the principle of the incompleteness of experience. The sufficient reason for renouncing as futile any search for the causes of an observed result, she declared, can be only this: One already knows the causes. The dilemma which quantum mechanics faces is therefore this: Either the theory provides the causes which determine uniquely the outcome of a measurement – but then why should not the physicist be able to predict the outcome? – or the theory does not provide such causes But then how could the possibility of discovering them in the future be categorically denied? Hermann saw the solution of this dilemma in the relational or, as she expressed it, the 'relative' character of the quantum mechanical description, which she regarded as the 'decisive achievement of this remarkable theory.'

"By renouncing the classical principle of objectivity and replacing it by that of the instrument-dependency in conjunction with the idea that from the factual result of a measurement the physical process, leading to the result, can be causally



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reconstructed, Hermann explained why the theory prevents predictability without excluding a post factum identification of the causes of the particular outcome. How this can be achieved in detail has been described by Hermann for the case of the Weizsäcker-Heisenberg experiment, which was a forerunner of the Einstein-Podolsky-Rosen thought-experiment, which, in its turn, led Bohr to the relational conception of quantum mechanical states. Grete Hermann's resolution of the dilemma was, as she stated, approved by Heisenberg with the words: 'That's it what we have tried for so long to make clear!'" (Jammer, pp. 207-9).

"The earliest criticism leveled against von Neumann's proof is contained in Grete Hermann's essay [offered here]. Hermann charged von Neumann with having committed a petitio principii by introducing into the formal presuppositions of his proof a statement which is logically equivalent to the assertion to be proved. The point in question concerns the additivity of the expectation value for arbitrary ensembles … Grete Hermann now pointed out that since an arbitrary ensemble is a mixture of pure cases it presumably suffices to claim the additivity only for ensembles all elements of which are described by the same (pure state) wave function. For such ensembles, however, von Neumann, according to Hermann, resorted to the mathematical formalism $(\varphi, (R + S)\varphi) = (\varphi, R\varphi) + (\varphi, S\varphi)$ as a valid relation irrespective of whether [quantum mechanical observables] R and S commute. Hermann now objected that, as long as the possibility of hidden variables has not yet been disproved, $(\varphi, R\varphi)$ denotes the expectation value of R for such ensembles E alone whose elements are described by [the wave function] φ. This does not imply that also sub-ensembles E1, of E, defined by perhaps not yet available criteria (hidden variables), have the same expectation value of R, nor that the latter satisfies the additivity condition. Thus an important step in von Neumann's proof is lacking. But to retain this assumption, as von Neumann did, is tantamount to assuming that the elements of an ensemble, described by φ, cannot be further differentiated by any criteria on which the result of an R measurement may depend. Since the denial of the existence of such criteria is precisely the thesis that has to be proved, Hermann concluded that von Neumann's proof is circular" (ibid., pp. 272-3).

Four decades later, Bell also pointed to the additivity postulate as the flaw in von Nuemann's argument. He first constructed a consistent hidden variable theory for particles of spin ½ in which the additivity postulate fails. "Having thus shown that von Neumann's argumentation depended decisively on the validity of the additivity postulate, Bell clarified the situation by pointing out that it was not the 'objective verified predictions of quantum mechanics,' as von Neumann asserted, but rather von Neumann's arbitrary additivity assumption, postulated to be valid also for dispersion- free states, that precluded the possibility of hidden variables. Its validity for quantum states, Bell realized, is a peculiarity of quantum mechanics and not at all a priori obvious; for in the case of incompatible observables R, and S the apparatus to measure R + S is in general entirely different from the devices required to measure the individual observables and a priori no statistical relations between the corresponding results can be expected. It is valid for quantum states because, in the words of Frederik J. Belinfante, "it so happens that the other axioms and postulates of quantum theory conspire to make [the expectation value of R] expressible at $\int \varphi^* R \varphi dx$. If this expectation value additivity is waived for dispersion-free states … the proof would break down" (ibid., p. 305).

A brief summary of this work was published in Die Naturwissenschaften, 23. Band, 42. Heft, pp. 718-721 (18 October 1935).

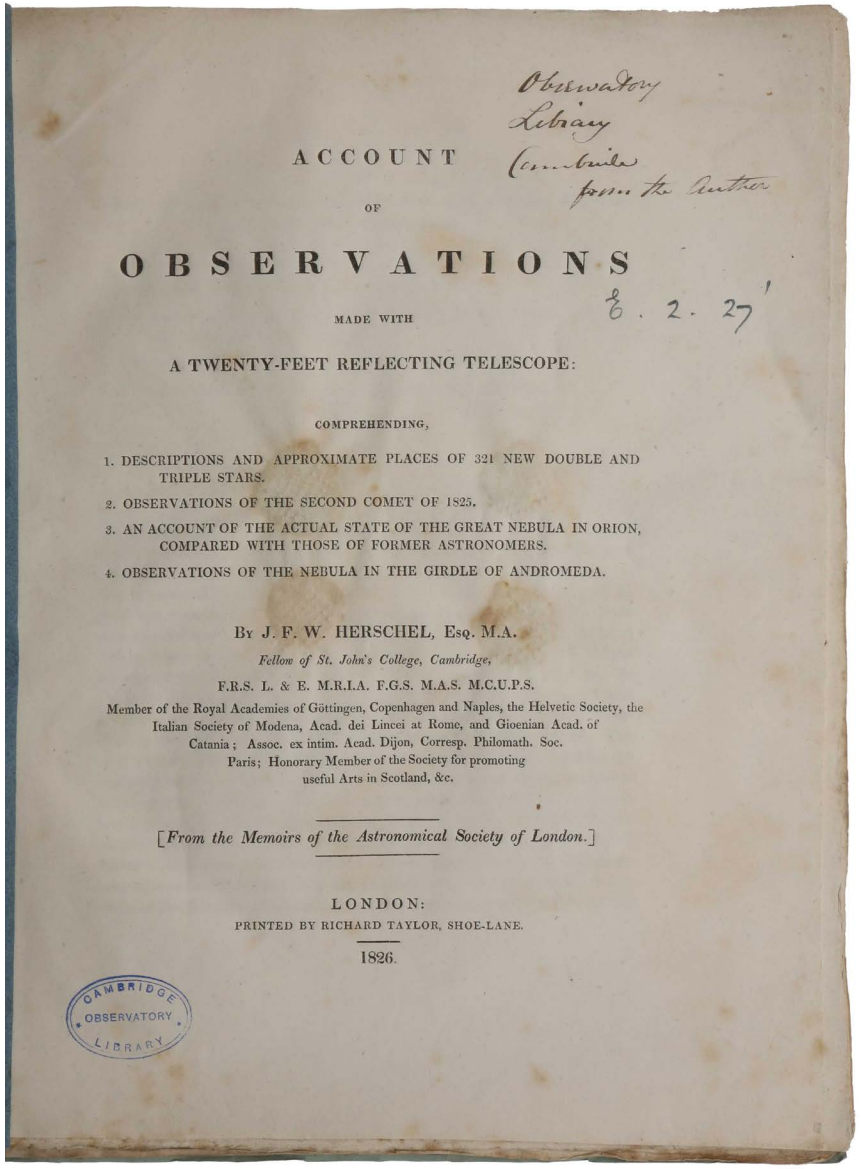
DOUBLE STARS AND THE ORION NEBULA

HERSCHEL, John Frederick William, Sir. *Account of observations made with a twenty-feet reflecting telescope: Comprehending, 1. Descriptions and approximate places of 321 new double and triple stars. 2. Observations of the second comet of 1825. 3. An account of the actual state of the great nebula in Orion, compared with those of former astronomers. 4. Observations of the nebula in the girdle of Andromeda.* London: Richard Taylor, 1826.

$1,800

Offprint from Memoirs of the Royal Astronomical Society. 4to (288 x 227 mm), pp. [ii], [1], 460-497, with three engraved plates numbered Vi-VIII (plates foxed, four small spots of glue used to attach bookplate to title verso with some show-through to recto and with offsetting onto first leaf of text). Later (early 20th century?) plain wrappers with original front wrapper bound in.

First edition, rare offprint issue, inscribed by Herschel. The work begins with a description of the reflecting telescope, with an 18-inch aperture and 20-feet focal length, the largest telescope in the world at the time, which was constructed in 1820 by John and his father William. Herschel used this telescope to make the observations which subsequently led to his fame as an astronomer. Part 2 contains the first accurate drawing of the Orion nebula. "One of the most impressive pieces of celestial cartography ever produced is Herschel's detailed drawing of the Great Nebula in Orion. By far the most striking of all nebulae …" (Buttmann, The Shadow of the Telescope (1974), p. 101). "In 1826 Herschel published a monograph on the Orion nebula and one on the nebula in Andromeda, together with other



Observatory Library Cambridge from the Author

ACCOUNT

OF

OBSERVATIONS

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A TWENTY-FEET REFLECTING TELESCOPE:

COMPREHENDING,

1. DESCRIPTIONS AND APPROXIMATE PLACES OF 321 NEW DOUBLE AND TRIPLE STARS.
2. OBSERVATIONS OF THE SECOND COMET OF 1825.
3. AN ACCOUNT OF THE ACTUAL STATE OF THE GREAT NEBULA IN ORION, COMPARED WITH THOSE OF FORMER ASTRONOMERS.
4. OBSERVATIONS OF THE NEBULA IN THE GIRDLE OF ANDROMEDA.

By J. F. W. HERSCHEL, Esq. M.A.

Fellow of St. John's College, Cambridge,

F.R.S. L. & E. M.R.I.A. F.G.S. M.A.S. M.C.U.P.S.

Member of the Royal Academies of Göttingen, Copenhagen and Naples, the Helvetic Society, the Italian Society of Modena, Acad. dei Lincei at Rome, and Gioenian Acad. of Catania; Assoc. ex intim. Acad. Dijon, Corresp. Philomath. Soc. Paris; Honorary Member of the Society for promoting useful Arts in Scotland, &c.

[*From the Memoirs of the Astronomical Society of London.*]

LONDON:

PRINTED BY RICHARD TAYLOR, SHOE-LANE.

1826.

observations made with the 20-foot telescope. The purpose of the paper on the Orion nebula was to determine from a comparison with earlier observations whether any noticeable changes in structure or brightness had occurred. The text is supplemented by a drawing of the nebula" (ibid., p. 50). The first part of this four-part paper comprises Herschel's second catalogue of double stars, building upon the catalogue of 380 double stars he had produced in collaboration with James South. OCLC lists four copies in US (Amherst, Smithsonian, US Naval Observatory, Yale). ABPC/RBH list just one copy (not a presentation copy).

Provenance: Cambridge Observatory Library (ink stamp on outer front wrapper and title page, 'Observatory' written on insider front wrapper, 'Observatory Library Cambridge from the Author' written in Herschel's hand on title page, bookplate of Cambridge Observatory inscribed 'presented by J. F. W. Herschel, March 1831' on title verso).

"Searching for a life occupation in his early years, [John] Herschel (1792-1871) turned briefly to chemistry, an interest terminated by a failed application for the chair of chemistry at Cambridge. He then tried the law as a profession in London, where he met astronomer James South, before returning to Cambridge, first as a subtutor in mathematics. In 1816, when Herschel took his master's degree, he was elected a fellow of St. John's College; in that same year, his ailing father appealed to John to carry on his work …

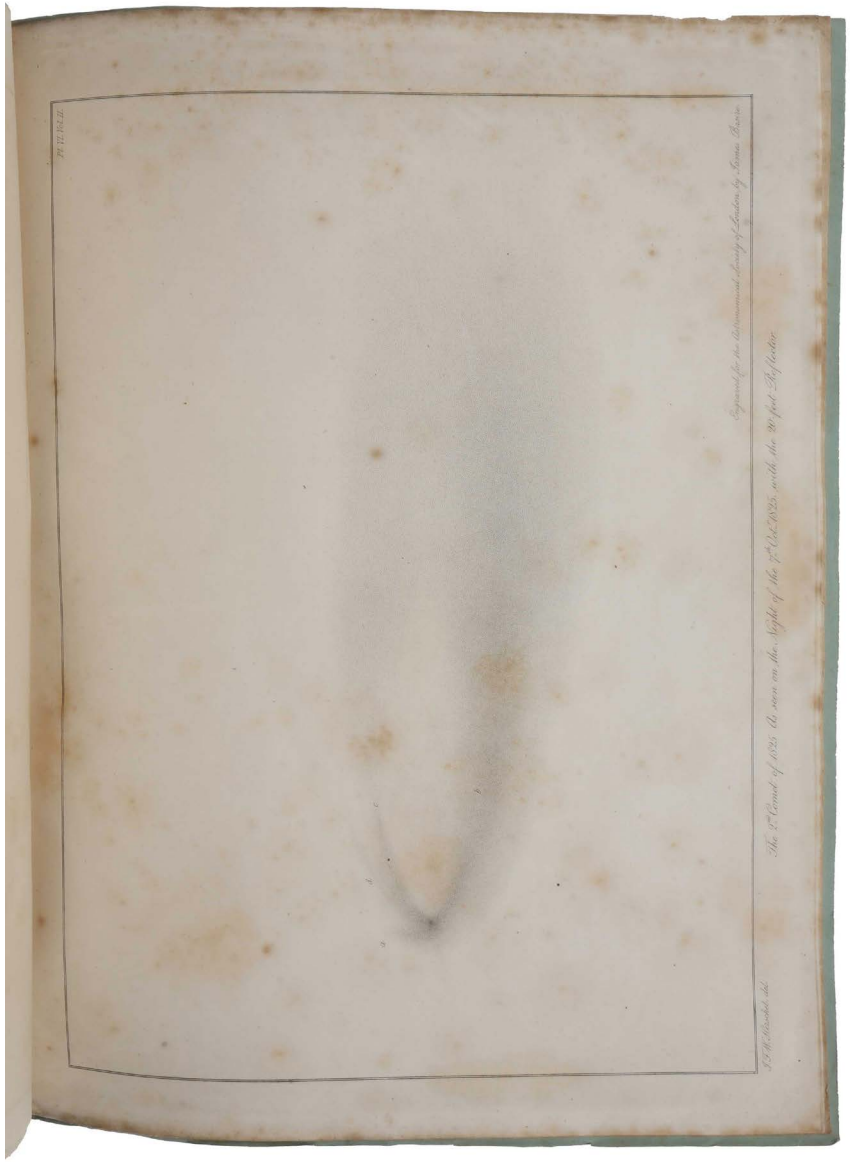
"John took up his father's last project, the discovery and observation of double stars. Originally, William had targeted them in the hope that if a stellar pair consists of one very remote component accidentally nearly aligned with a nearer one, this fortuitous coincidence could help determine the parallax of the nearer star. William's work demonstrated instead that double stars are mostly close pairs gravitationally bound; the goal of extending this project was the discovery of orbital motions. Herschel and South used refractors fitted with positional circles for making observations that led to their catalog of 380 double stars published in 1824, earning them the Gold Medal of the Astronomical Society and the Lalande Prize of the Paris Academy of Sciences …

"From 1825 to 1833, Herschel was deeply involved in astronomical observations, using one of the refractors obtained from South and a 20-ft. reflector with 18-in. aperture, still the world's largest telescope. The observations involved a prodigious amount of work, leading to the publication in a series of papers listing a total of 5,075 double stars, … together with the general catalogue of nebulae and clusters derived from observations covering the whole northern sky using the sweeping survey technique devised by his father" (Biographical Encyclopedia of Astronomers, p. 493). A table in the present paper "exhibits, in eight columns, the approximate places of 321 new double and triple stars, for Jan. 1, 1825, with their estimated angles of position, distances, magnitudes, and other particulars. A great many of the double stars tabulated in this paper, exhibit the highly interesting and curious phenomenon of contrasted colours; in combinations of white and blue or purple, yellow, orange, or red, large stars, with blue or purple small ones: red and white combinations also sometimes occur, though with less frequency. In all these cases, the excess of rays belonging to the less refrangible end of the spectrum falls to the share of the large star, and those of the more refrangible portion to the small. Another fact not less remarkable, and rendering highly probable some other relation than that of mere juxtaposition, is, that though red single stars are common enough, no example of an insulated blue, green, or purple one has yet been produced" (Annals of Philosophy 12 (1826), p. 233).

"Apart from William Herschel, his son John was the greatest discoverer of nebulae and star clusters. About his motivation he wrote in 1826 at Slough that 'The nature of nebulae, it is obvious, can never become more known to us than at present, except in two ways, - either by the direct observation of changes in the form or

physical condition of some one or more among them, or from the comparison of a great number, so as to establish a kind of scale or graduation from the most ambiguous, to objects of whose nature there can be no doubt' [p. 487]. The first way had already been realised through his detailed observations of the Orion and Andromeda Nebulae [in the present paper]. The second – the study of a large number of objects – was mastered in the years 1825-33, reproducing and extending the observations of his father … For his observations John Herschel used a reflector with an aperture of 18¼", which was completed in 1820. It used two mirrors, one made by his father alone and another one cast and ground under his father's supervision. The telescope resembles William Herschel's famous 'large 20ft'" (Steinicke, Observing and Cataloguing Nebulae and Star Clusters: From Herschel to Dreyer's New General Catalogue (2010), p. 52).

Plates VII and VIII depict the Orion nebula, Plate VI the comet that appeared early in 1825 in the constellation Taurus and which Herschel observed in October. He noted that it "has no sharp, star-like centre, but a much brighter yet quite milky, round kernel of about 15 seconds to 20 seconds diameter, shading insensibly but almost suddenly away. Its coma is irregular. On the preceding side it extends further, and is partly detached …Tail distinctly bifid, divided into two great branches which go off immediately from the nucleus at an angle of 45° with each other …" (p. 487).



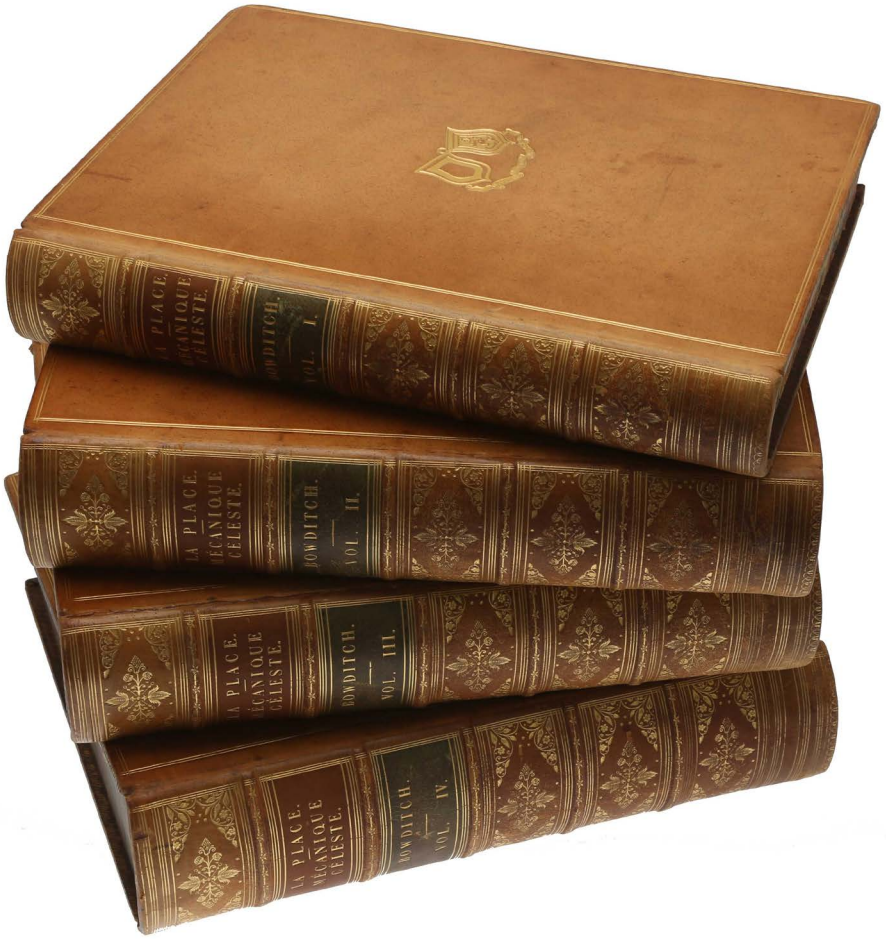
THE 'EIGHTEENTH CENTURY ALMAGEST' IN ENGLISH

LAPLACE, Pierre-Simon, Marquis de; BOWDITCH, Nathaniel. *Mécanique Céleste. By the Marquis de la Place... Translated, with a commentary, by Nathaniel Bowditch...* Boston: Butts, Hilliard, Gray, Little and Wilkins, 1829-1832-1834-1839.

$8,000

Four vols., large 4to, pp. xxiv, 746, [1]; xviii, 990, [1]; xxix, [1], 910, [107]; 168, xxxvi, 1018, including three engraved portraits (Laplace, Bowditch, and his wife), numerous mathematical diagrams in text. Nineteenth century calf with gilt-stamped armorial shields on the covers, spines decoratively tooled in gilt, raised bands, morocco lettering pieces, marbled endpapers and edges.

First edition of Bowditch's magnificent annotated English translation of Laplace's masterpiece, the Traité de Mécanique Céleste (1799-1825). Printed in an edition of only 250 copies at the author's expense, Bowditch's translation was the first significant work on physical science published in the United States. In his Traité de Mécanique Céleste, the foundation of modern theoretical astronomy, termed "the eighteenth-century Almagest" and "a sequel to Newton's Principia" by Horblit, Laplace, the 'Newton of France', codified and developed the theories and achievements of Newton, Euler, d'Alembert, and Lagrange. Bowditch undertook the translation of Laplace's great book in order to supply steps omitted from the original text, to incorporate later results into the translation, and to give credits omitted by Laplace, almost doubling the size of the original work in the process. Although he completed the manuscript for the work in 1818, it took another decade to arrange publication. "Bowditch's commentary in the footnotes is an



indispensable vade mecum for the study of Laplace, explaining and filling out the demonstrations, and containing a great body of historical as well as mathematical and astronomical elucidation" (DSB, under Laplace). "Outside of France, particularly in English-speaking countries, Bowditch's edition, rather than the original, was often the means of learning about the mechanics of the heavens" (DSB, under Bowditch). Bowditch was assisted in his translation by Benjamin Peirce (1809-80), who read the proof sheets while he was still an undergraduate at Harvard.

"Laplace maintained that while all planets revolve round the sun their eccentricities and the inclinations of their orbits to each other will always remain small. He also showed that all these irregularities in movements and positions in the heavens were self-correcting, so that the whole solar system appeared to be mechanically stable. He showed that the universe was really a great self-regulating machine and the whole solar system could continue on its existing plan for an immense period of time. This was a long step forward from the Newtonian uncertainties in this respect … Laplace also offered a brilliant explanation of the secular inequalities of the mean motion of the moon about the earth – a problem which Euler and Lagrange had failed to solve … He also investigated the theory of the tides and calculated from them the mass of the moon" (PMM).

Volumes 1 and 2 of Laplace's Traité constitute a general theoretical basis for mathematical astronomy. Volume 1 comprises Book I, 'On the general laws of equilibrium and motion' and Book II, 'On the law of universal gravitation, and the motions of the centres of gravity of the heavenly bodies'; Volume 2, in three books, treats the shapes of celestial bodies and their motion around their centres of gravity, and the tides. Volume 3, in two books, deals with the theory of the Moon, and Volume 4 treats the motion of the moons of Jupiter, Saturn and Uranus, and the theory of comets.

"By 1818 Bowditch had completed his translation of the first four volumes of the Mécanique Céleste. His purpose was threefold: to supply steps omitted from the original text; to incorporate later results into the translation; and to give credits omitted by Laplace. There is no evidence that Laplace ever responded to any communication from Bowditch, a fact sometimes ascribed to the third purpose. The four volumes appeared in 1829, 1832, 1834, and 1839, the last posthumously. The delay in publication was undoubtedly due in part to financial problems. Bowditch, who would not have people subsidize, out of regard for him or other irrelevant reasons, a book they could not read, printed the work at his own expense. It is also most likely that he continued to work over the volumes between 1818 and their appearance, particularly to bring the subject matter up to date. The fifth volume of the Mécanique Céleste appeared too late for translation by Bowditch. Probably the only person who aided Bowditch was Benjamin Peirce, who read over part of the text for errors. Printed in a small edition, the work was perhaps more widely admired than read, simply serving to confirm the translator's already high reputation. Nevertheless, outside of France, particularly in English-speaking countries, Bowditch's edition, rather than the original, was often the means of learning about the mechanics of the heavens" (DSB). "Bowditch sent complimentary copies of his volumes to prominent scientists around the world, and their reactions provide insight into the current perception of Laplace's original version. There was universal gratitude for his clarification in the notes on difficult passages (and also for his provision of cross-references and descriptions of much later work). He probably spoke for many when he said: 'Whenever I meet in La Place with the words "Thus it plainly appears", I am sure that hours, and perhaps days, of hard study will alone enable me to discover how it plainly appears'" (Grattan-Guinness, p. 316). Bowditch did not translate the fifth and final volume of Laplace's work, which appeared too late.

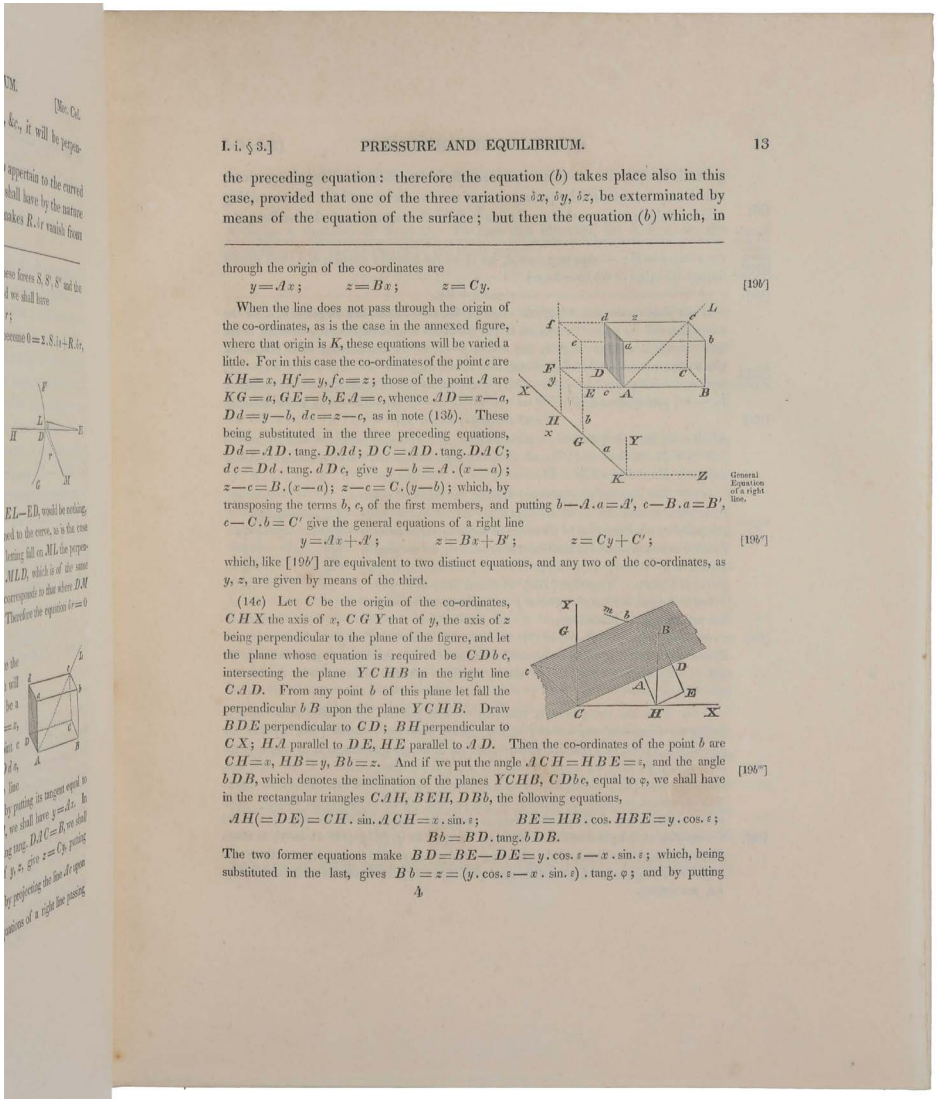
"Such mathematicians as Lacroix, Legendre, Bessel, and Puissant recognized the great value of [Bowditch's] additions. Mr. Babbage, in a letter to Dr. Bowditch,

under date of August 5, 1832, wrote: “It is a proud circumstance for America that she has preceded her parent country is such an undertaking; and we in England must be content that our language is made the vehicle of the sublimest portion of human knowledge, and be grateful to you for rendering it more accessible.” Letters of similar import were received by Dr. Bowditch from Airy, Francis Baily, Herschel, the Bishop of Cloyne (Dr. Brinkley), and Cacciatore” (Lovering, p. 193).

Nathaniel Bowditch (1775-1838), the fourth of seven children, was born in Salem, Massachussetts. He left school at the age of ten to work in his father’s cooperage, before becoming indentured aged 12 as a bookkeeping apprentice. Bowditch began to study algebra at age 14, and two years later he taught himself calculus. He also taught himself Latin and French, which enabled him to read the important European scientific works.

In the course of several sea voyages beginning in 1795, Bowditch became intenselyinterested in the mathematics involved in celestial navigation. He worked initially with John Hamilton Moore’s London-published Navigator, but finding this to be full of errors Bowditch recomputed all of Moore’s tables, and rearranged and expanded the work. The result was published in 1802 as The New American Practical Navigator, which became the western hemisphere shipping industry standard for the next century and a half.

After returning to Salem in 1803 he resumed his mathematical studies and entered the insurance business. Bowditch’s mathematical and astronomical work during this time led to offers of chairs at several prominent academic institutions, including Harvard, but he turned them all down. By 1819, Bowditch’s international reputation had grown to the extent that he was elected a fellow of the Royal Societies of Edinburgh and London and the Royal Irish Academy. In 1823 Bowditch moved from Salem to Boston, taking with him 2,500 books, more



I. i. § 3.] PRESSURE AND EQUILIBRIUM. 13

the preceding equation: therefore the equation (b) takes place also in this case, provided that one of the three variations δx, δy, δz, be exterminated by means of the equation of the surface; but then the equation (b) which, in

through the origin of the co-ordinates are

$$y = Ax; \qquad z = Bx; \qquad z = Cy. \qquad [19b']$$

When the line does not pass through the origin of the co-ordinates, as is the case in the annexed figure, where that origin is K, these equations will be varied a little. For in this case the co-ordinates of the point c are $KH = x$, $Hf = y$, $fc = z$; those of the point A are $KG = a$, $GE = b$, $EA = c$, whence $AD = x - a$, $Dd = y - b$, $dc = z - c$, as in note (13b). These being substituted in the three preceding equations, $Dd = AD$. tang. DAd; $DC = AD$. tang. DAC; $dc = Dd$. tang. dDc, give $y - b = A.(x - a)$; $z - c = B.(x - a)$; $z - c = C.(y - b)$; which, by transposing the terms b, c, of the first members, and putting $b - A.a = A'$, $c - B.a = B'$, $c - C.b = C'$ give the general equations of a right line

General Equation of a right line.

$$y = Ax + A'; \qquad z = Bx + B'; \qquad z = Cy + C'; \qquad [19b'']$$

which, like [19b'] are equivalent to two distinct equations, and any two of the co-ordinates, as y, z, are given by means of the third.

(14c) Let C be the origin of the co-ordinates, CHX the axis of x, CGY that of y, the axis of z being perpendicular to the plane of the figure, and let the plane whose equation is required be $CDbc$, intersecting the plane $YCHB$ in the right line CAD. From any point b of this plane let fall the perpendicular bB upon the plane $YCHB$. Draw BDE perpendicular to CD; BH perpendicular to CX; HA parallel to DE, HE parallel to AD. Then the co-ordinates of the point b are $CH = x$, $HB = y$, $Bb = z$. And if we put the angle $ACH = HBE = \varepsilon$, and the angle bDB, which denotes the inclination of the planes $YCHB$, $CDbc$, equal to φ, we shall have in the rectangular triangles CAH, BEH, DBb, the following equations, [19b'''']

$$AH (= DE) = CH. \sin. ACH = x. \sin. \varepsilon; \qquad BE = HB. \cos. HBE = y. \cos. \varepsilon;$$
$$Bb = BD. \text{tang.} bDB.$$

The two former equations make $BD = BE - DE = y. \cos. \varepsilon - x. \sin. \varepsilon$; which, being substituted in the last, gives $Bb = z = (y. \cos. \varepsilon - x. \sin. \varepsilon). \text{tang.} \varphi$; and by putting

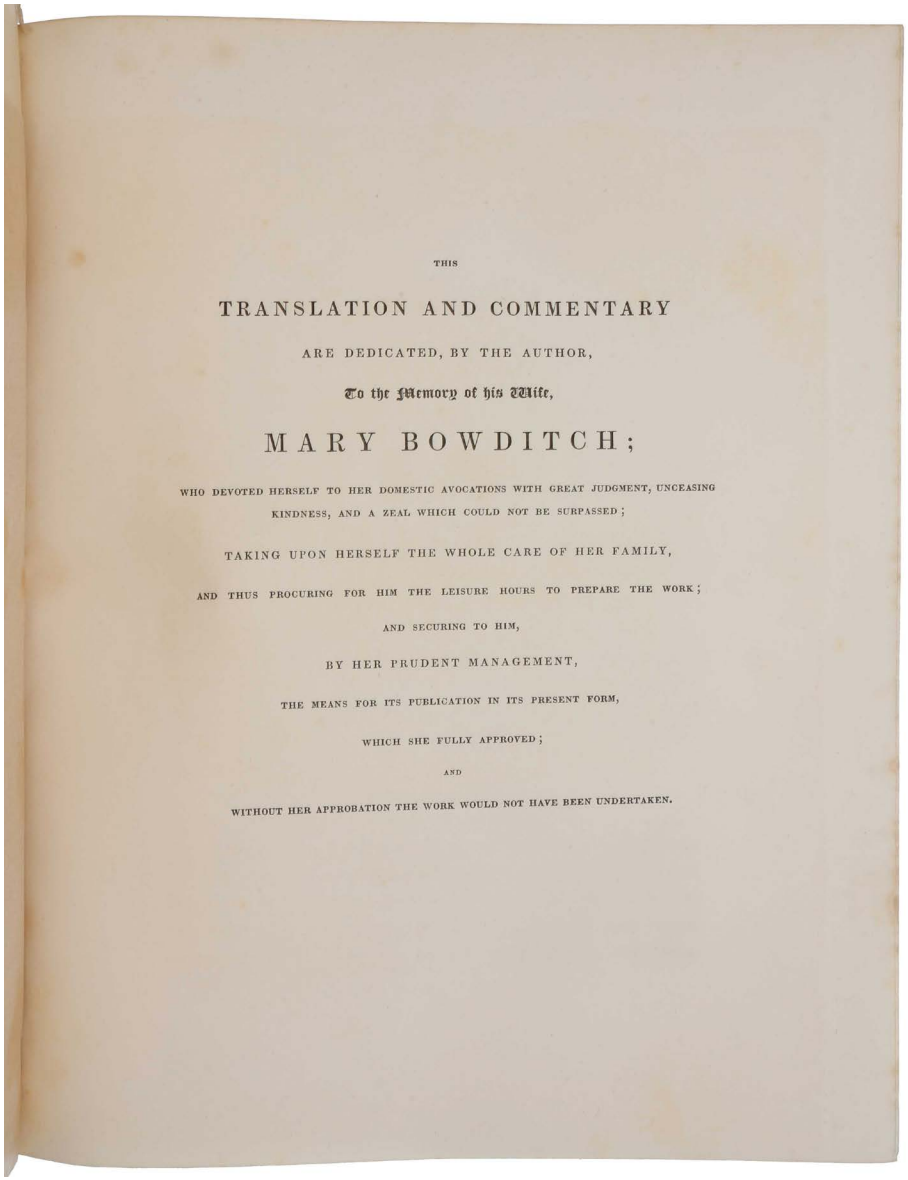
4

than 100 maps and charts, and 29 volumes of his own manuscripts. In Boston he became president of the Massachusetts Hospital Life Insurance Company, where he enjoyed enough material success that he could afford the $12,000 it cost to have his translation of Laplace published.

"There are two early English translations of Book I and Books I and II respectively: by John Toplis (London–Nottingham, 1814) and by Henry Harte, 2 vols. (Dublin, 1822-1827). Both were entirely superseded by the splendid work of Nathaniel Bowditch, Mécanique céleste by the Marquis de Laplace, Translated With a Commentary, 4 vols. (Boston, 1829-1839)" (DSB, under Laplace).

Babson 82; Dibner 14 (note); Horblit 63 (1st ed.); PMM 252 (note); Sotheran I 2444-2445. Grattan-Guinness, Convolutions in French Mathematics, 1800-1840, 1990. Lovering, 'The "Mécanique Céleste" of Laplace, and Its Translation, with a Commentary by Bowditch,' Proceedings of the American Academy of Arts and Sciences, Vol. 24 (1889), pp. 185-201.

Dibner 159. Garrison-Morton (online) 611. Horblit 48. Norman 1039. Printing and the Mind of Man 323. Jungnickel & McCormach, Intellectual Mastery of Nature, Vol. 1, *The Torch of Mathematics 1800-1870* (1986).



THIS

TRANSLATION AND COMMENTARY

ARE DEDICATED, BY THE AUTHOR,

To the Memory of his Wife,

MARY BOWDITCH;

WHO DEVOTED HERSELF TO HER DOMESTIC AVOCATIONS WITH GREAT JUDGMENT, UNCEASING KINDNESS, AND A ZEAL WHICH COULD NOT BE SURPASSED;

TAKING UPON HERSELF THE WHOLE CARE OF HER FAMILY,

AND THUS PROCURING FOR HIM THE LEISURE HOURS TO PREPARE THE WORK;

AND SECURING TO HIM,

BY HER PRUDENT MANAGEMENT,

THE MEANS FOR ITS PUBLICATION IN ITS PRESENT FORM,

WHICH SHE FULLY APPROVED;

AND

WITHOUT HER APPROBATION THE WORK WOULD NOT HAVE BEEN UNDERTAKEN.

LEIBNIZ'S CALCULATING MACHINE

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

$32,000

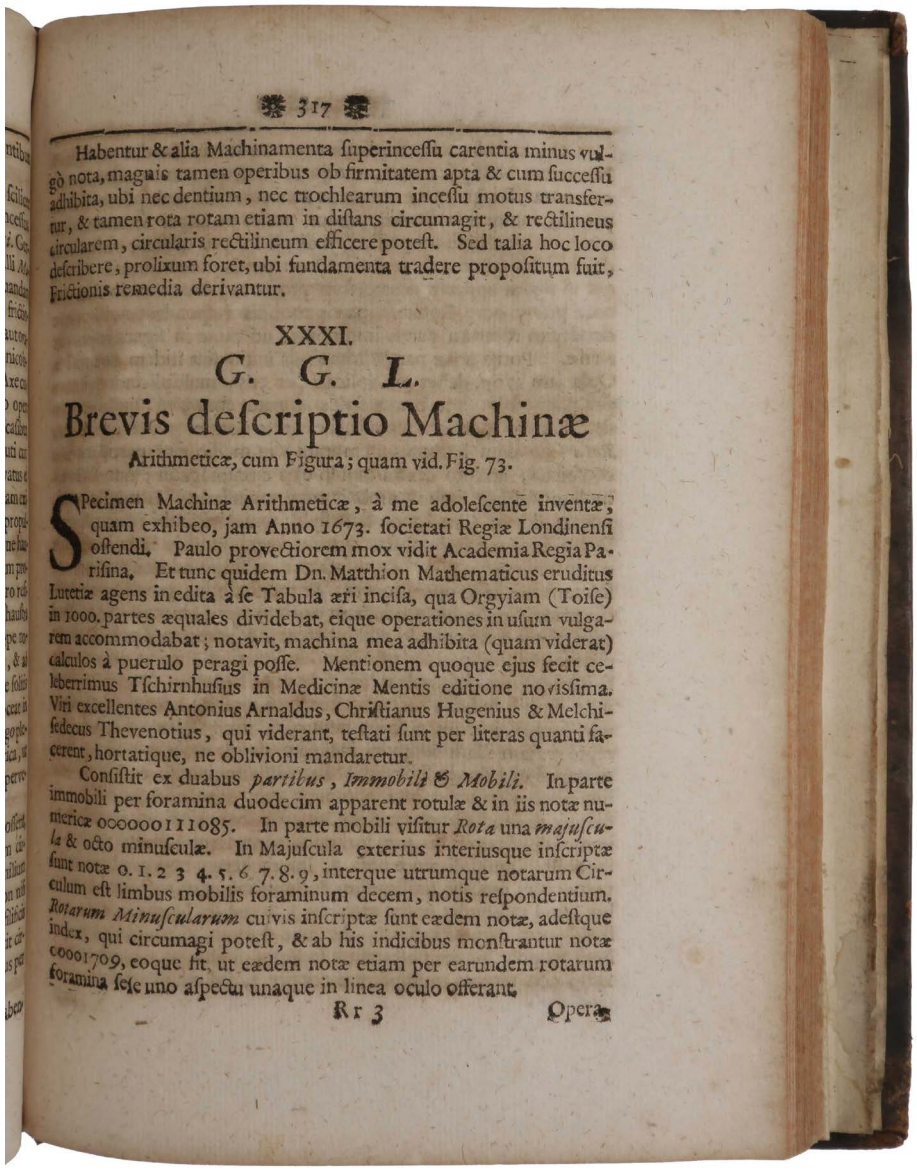
Pp. 317-319 and one folding plate in Miscellanea Berolinensia ad incrementum scientiarum... Complete journal volume, 4to (206 x 160 mm), pp. [xxiv, including frontispiece], 394, with engraved allegorical frontispiece, and 31 folding engraved plates. Contemporary half calf, capitals chipped, some wear to rear lower hinge, some browning (as usual), Leibniz paper with less browning and plate of the calculator very nice, in all a very good and unsophisticated copy.

First edition, rare, of this milestone in computer history, Leibniz's description of his famous calculating machine, the first stepped-drum calculator, and the first machine that could perform multiplication and division. "Leibniz studied Morland's and Pascal's various designs and set himself the task of constructing a more perfect and efficient machine. To begin with, he improved Pascal's device by adding a stepped-cylinder to represent the digits 1 through 9 … In 1694, Leibniz built his calculating machine, which was far superior to Pascal's and was the first general purpose calculating device able to meet the major needs of mathematicians and bookkeepers" (Rosenberg, The Computer Prophets, p 48). "[He] invented a device now known as the Leibniz wheel and still in use in some machines . The mechanism enabled him to build a machine which surpassed Pascal's in that it could do not only addition and subtraction fully automatically but also multiplication and division. Leibniz's device enabled his machine to perform the operation of multiplication automatically by repeated additions. His idea was apparently re-invented in 1820 by Charles Xavier de Colmar" (Goldstine,

The Computer from Pascal to Neumann, p. 7). Although Leibniz demonstrated his machine before the Royal Society and elsewhere, no description of it appeared in print until in the present form. It is contained in the first volume of the journal of the Berlin Academy of Science, which Leibniz founded. Although the volume is naturally present in some institutional holdings, it is absent from many, and is very rare on the market. It contains several other important papers by Leibniz on mathematics and physics (see Ravier for a full list).

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

"Leibniz conceived the idea of a calculating machine in the early 1670s with the aim of improving upon Blaise Pascal's calculator, the Pascaline. He concentrated on expanding Pascal's mechanism so it could multiply and divide. The first recorded indirect reference is in a letter from the French mathematician Pierre de Carcavy dated June 20, 1671 in which Pascal's machine is referred to as "la machine du temps passé." Leibniz demonstrated a wooden model of his calculator



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

XXXI.

G. G. L.

Brevis deſcriptio Machinæ

Arithmeticæ, cum Figura; quam vid. Fig. 73.

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

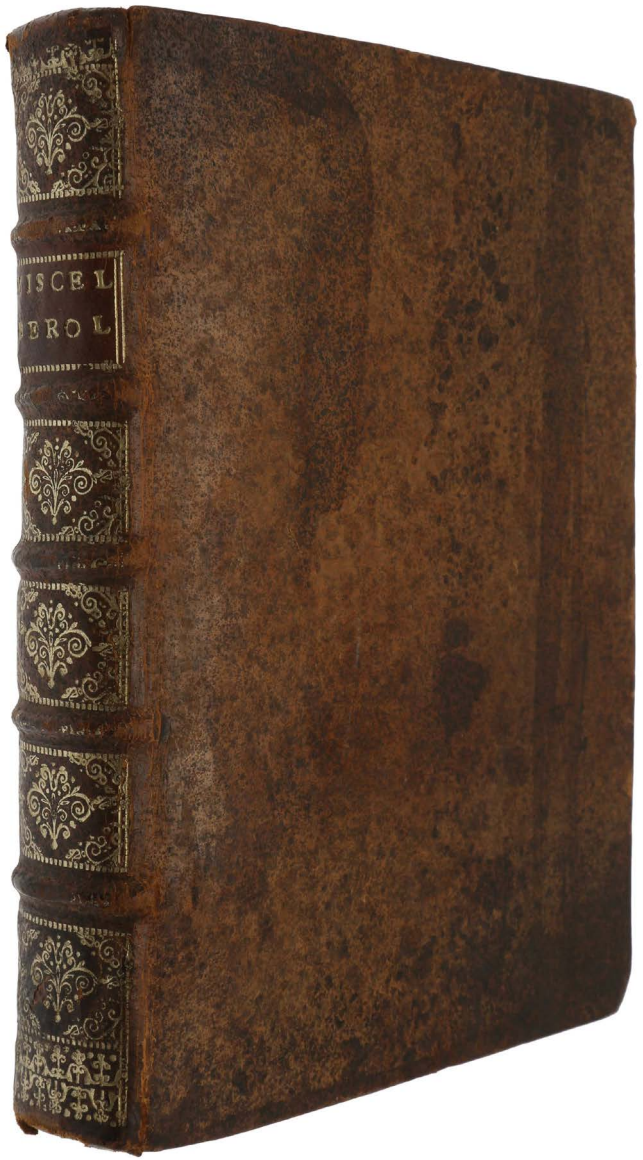
Conſiſtit ex duabus *partibus*, *Immobili & Mobili*. In parte immobili per foramina duodecim apparent rotulæ & in iis notæ numericæ 000000111085. In parte mobili viſitur *Rota* una *majuſcula* & octo minuſculæ. In Majuſcula exterius interiusque inſcriptæ ſunt notæ 0. 1. 2 3 4. 5. 6 7. 8. 9, interque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

Rr 3 Opera

at the Royal Society of London on February 1, 1673, though the machine could not yet perform multiplication and division automatically. In a letter of March 26, 1673 to Johann Friedrich, where he mentioned the presentation in London, Leibniz described the purpose of the 'arithmetic machine' as making calculations "leicht, geschwind, gewiß" [sic], i.e., easy, fast, and reliable. Leibniz also added that theoretically the numbers calculated might be as large as desired, if the size of the machine was adjusted: "eine zahl von einer ganzen Reihe Ziphern, sie sey so lang sie wolle (nach proportion der größe der Maschine)" ("a number consisting of a series of figures, as long as it may be in proportion to the size of the machine").

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

"After Leibniz's departure, work on the calculating machine continued under the supervision of his Danish friend Friedrich Adolf Hansen (1652-1711), and Leibniz continued to correspond with Olivier. The Leibniz archive includes three letters from Olivier, dated March 24 and July 29, 1677 and November 15, 1678; indeed Leibniz seems to have had some effort made to have Olivier called to Hanover to continue his work. After about 1678 work on the machine seems to have lapsed until Leibniz began to develop a new prototype in the early 1690s. At some point Leibniz's wooden model and his first metal machine were lost.



The second machine, which was built from 1690 to 1720, is preserved in the Niedersächsische Landesbibliothek, Hanover.

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

"The contract comprises 20 meticulously detailed clauses, describing in detail the machine and the financial and practical arrangements for its construction: it is to produce numbers up to three figures; it is to be capable of multiplication and division, as well as addition and subtraction, with the mechanism (consisting of a system of fixed and mobile pieces, and equal and unequal cogs) described in detail, first for multiplication and division, then for addition and subtraction, noting that the operations should be effected immediately 'et non pas comme dans la machine du temps passé après un delay ou intervalle'; the machine is to be perfectly finished, made of iron or steel, and enclosed in 'une petite boëtte propre, à fin qu'il ne paroisse que ce qu'il faut pour l'opération'; the operation of the machine is then specified. The contract goes on to note that Olivier had previously agreed to construct such a machine in one or two months for a payment of 'cent écus blancs ou trois cens francs', part of which has been advanced, but that he had failed (in part because of illness) to give satisfaction; he now engages to complete the work in three months, with his goods as surety; and he is to show the progress of his work to Hansen, and inform Leibniz by letter, each week" (Catalogue of Valuable Manuscripts and Printed Books, Sale 1550, Christie's, King Street, London, 21 May 2014).

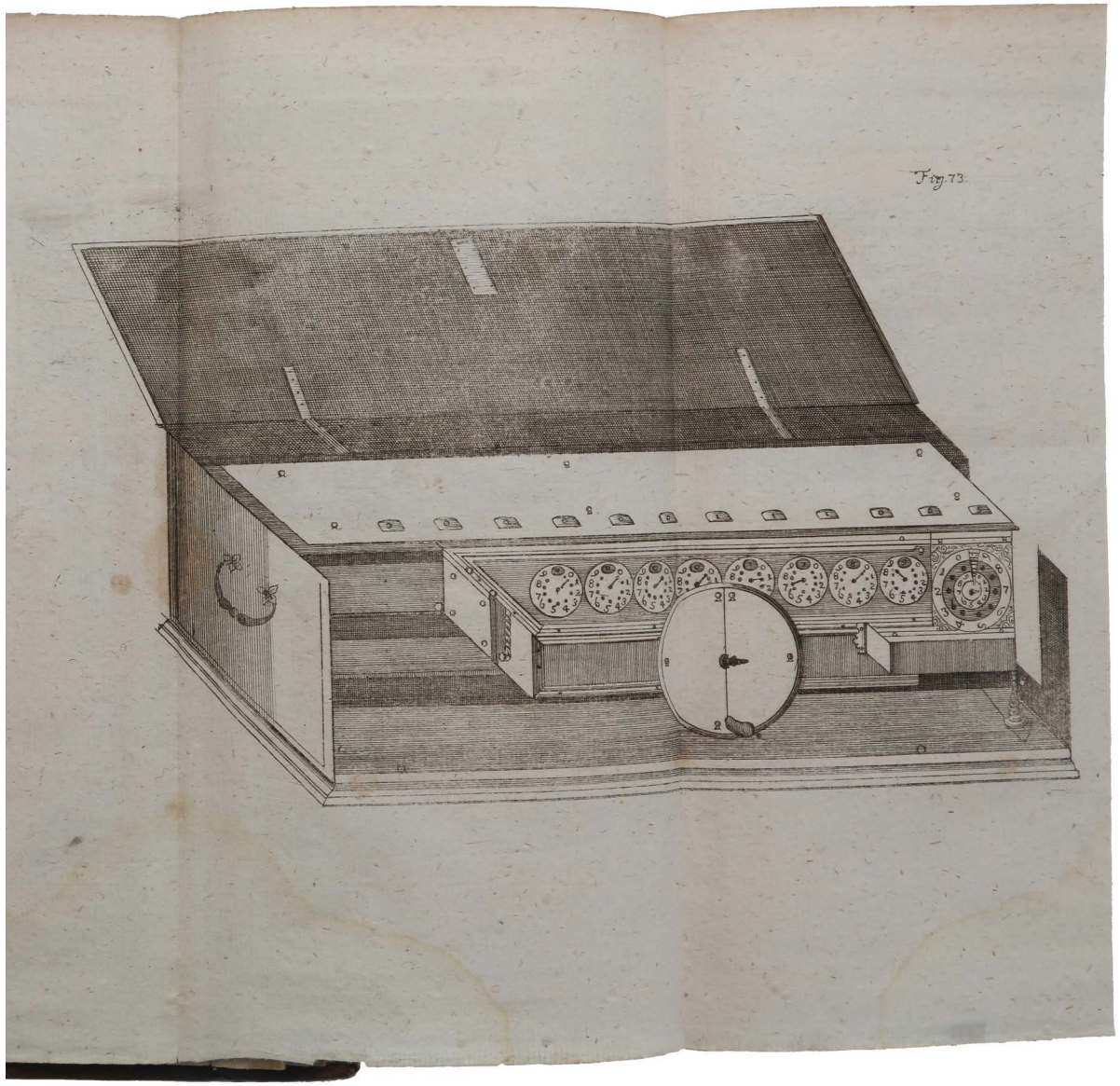


Fig. 73.

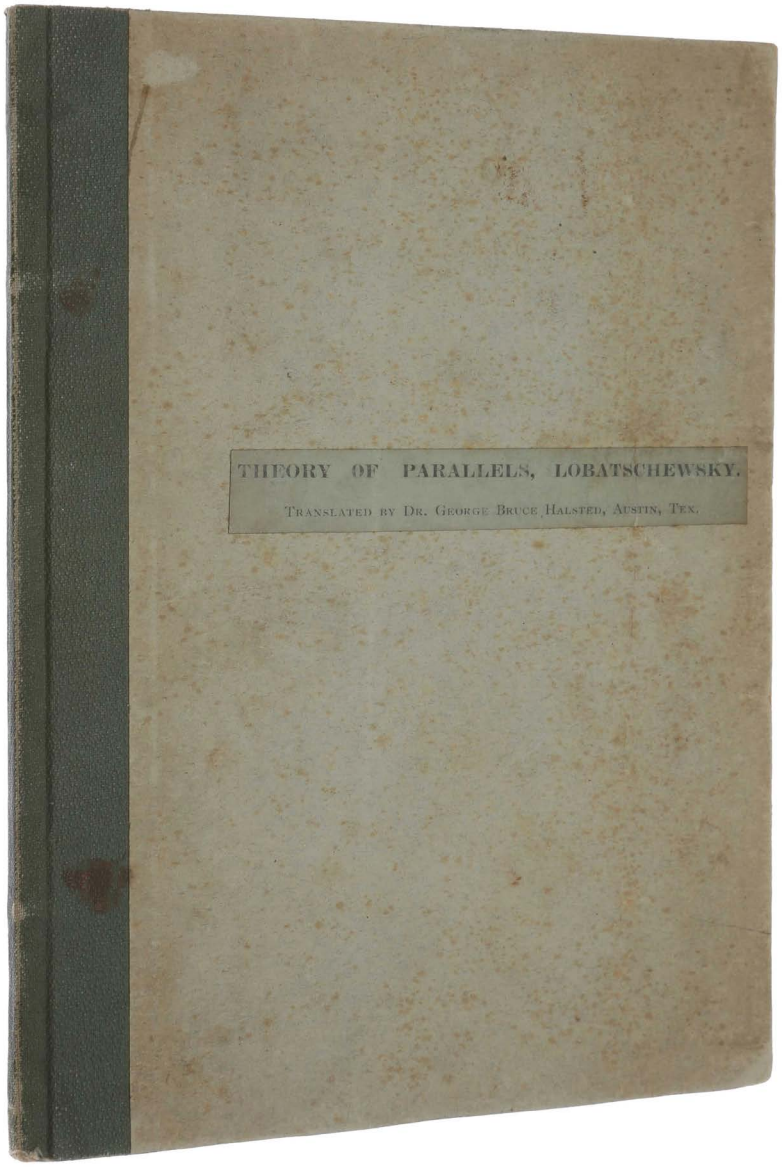
THE TRUE FIRST APPEARANCE OF NON-EUCLIDEAN GEOMETRY IN ENGLISH

LOBACHEVSKY, Nikolai Ivanovich. *Geometrical Researches on the Theory of Parallels … Translated from the Original by George Bruce Halstead.* Rolla, Missouri: School of Mines Press, 1891.

$75,000

Offprint from: Scientiae Baccalaureus, A Quarterly Journal of Scientific Research, Vol. 1, No. 3, February, 1891. 8vo (207 x 153 mm), pp. [3], 126-165 with numerous diagrams in text. Contemporary light-green boards with green cloth spine. The University of Virginia copy of the offprint is bound in the original light-green printed wrappers. The front wrapper bears the text 'Reprinted from // Scientiae Baccalaureus // Vol. 1 February, 1891 No. 3 // Theory of Parallels, Lobatschewsky, // Translated by Dr. George Bruce Halsted, Austin, Tex.' In our copy a light-green printed label with the text of the last two lines is pasted onto the front cover. Evidently this was cut from the original printed wrappers in which our copy was originally bound.

The true first publication in English, incredibly rare offprint issue (only one other copy known), of Lobachevsky's revolutionary discovery of non-Euclidean geometry. This work, published in the short-lived and little-known journal Scientiae Baccalaureus, is a translation of Geometrische Untersuchungen zur Theorie der Parallellinien (Berlin, 1840), which was the first complete account of Lobachevsky's work to be published in a Western European language; "following its publication, in 1842, Lobachevsky was, on the recommendation of Gauss, elected to the Göttingen Gesellschaft der Wissenschaften" (DSB). "Cet ouvrage est un vrai chef-d'oeuvre de la littérature mathématique" (Piccard, p. 19). Lobachevsky sent



Gauss a copy of the 1840 work, and Gauss's opinion of it is recorded in a letter, dated 28 November 1846, written to his colleague the astronomer H. C. Schumacher: "In developing the subject, the author followed a road different from the one I took myself; Lobachevsky carried out the task in a masterly fashion and in a truly geometric spirit. I consider it a duty to call your attention to this book, since I have no doubt that it will give you a tremendous pleasure." "The researches that culminated in the discovery of non-Euclidean geometry arose from unsuccessful attempts to 'prove' the axiom of parallels in Euclidean geometry. This postulate asserts that through any point there can be drawn one and only one straight line parallel to a given straight line. Although this statement was not regarded as self-evident and its derivation from the other axioms of geometry was repeatedly sought, no one openly challenged it as an accepted truth of the universe until Lobatchewsky published the first non-Euclidean geometry … In Lobatchewsky's geometry an infinity of parallels can be drawn through a given point that never intersect a given straight line … His fundamental paper was read to his colleagues in Kazan in 1826 but he did not publish the results until 1829-30 when a series of five papers appeared in the Kazan University Courier … He amplified his findings (still in Russian) in 1836-8 under the title 'New Elements of Geometry, with a Complete Theory of Parallels'. In 1840 he published a brief summary in Berlin under the title Geometrische Untersuchungen zur Theorie der Parallellinien" (PMM). We have located only one other offprint of this work, a presentation copy from the journal's editor, W. H. Echols, held by the University of Virginia. Even the journal itself is exceptionally rare, with fewer than 10 institutional holdings. No copies of the offprint or the journal in auction records.

"In his early lectures on geometry, Lobachevsky himself attempted to prove the fifth postulate [i.e., the axiom of parallels]; his own geometry is derived from his later insight that a geometry in which all of Euclid's axioms except the fifth postulate hold true is not in itself contradictory. He called such a system "imaginary geometry," proceeding from an analogy with imaginary numbers. If imaginary numbers are the most general numbers for which the laws of arithmetic of real numbers prove justifiable, then imaginary geometry is the most general geometrical system. It was Lobachevsky's merit to refute the uniqueness of Euclid's geometry, and to consider it as a special case of a more general system.

"In Lobachevskian geometry, given a line a and a point A not on it, one can draw through A more than one coplanar line not intersecting a. It follows that one can draw infinitely many such lines which, taken together, constitute an angle of which the vertex is A. The two lines, b and c, bordering that angle are called parallels to a and the lines contained between them are called ultraparallels, or diverging lines; all other lines through A intersect a. If one measures the distance between two parallel lines on a secant equally inclined to each, then, as Lobachevsky proved, that distance decreases indefinitely, tending to zero, as one moves farther out from A … A comparison of Euclidean and Lobachevskian geometry yields several immediate and interesting contrasts [notably that] for all triangles in the Lobachevskian plane the sum of the angles is less than two right angles" (DSB). In Euclidean geometry the sum of the angles of a triangle equals two right angles, and in spherical geometry it is always greater.

Of particular interest are the curves called 'horocycles.' These are the limiting curves of the circles that share a tangent at a given point, as the radius of the circles tends to infinity. In Euclidean geometry this limiting curve would be a straight line, but in Lobachevskian geometry it is a new kind of curve. By rotating the horocyle around the line perpendicular to the tangent, Lobachevsky obtained a 'horosphere' and he proved the remarkable fact that the geometry on a horosphere is Euclidean, so that Euclidean geometry is in a sense contained within non-Euclidean geometry.

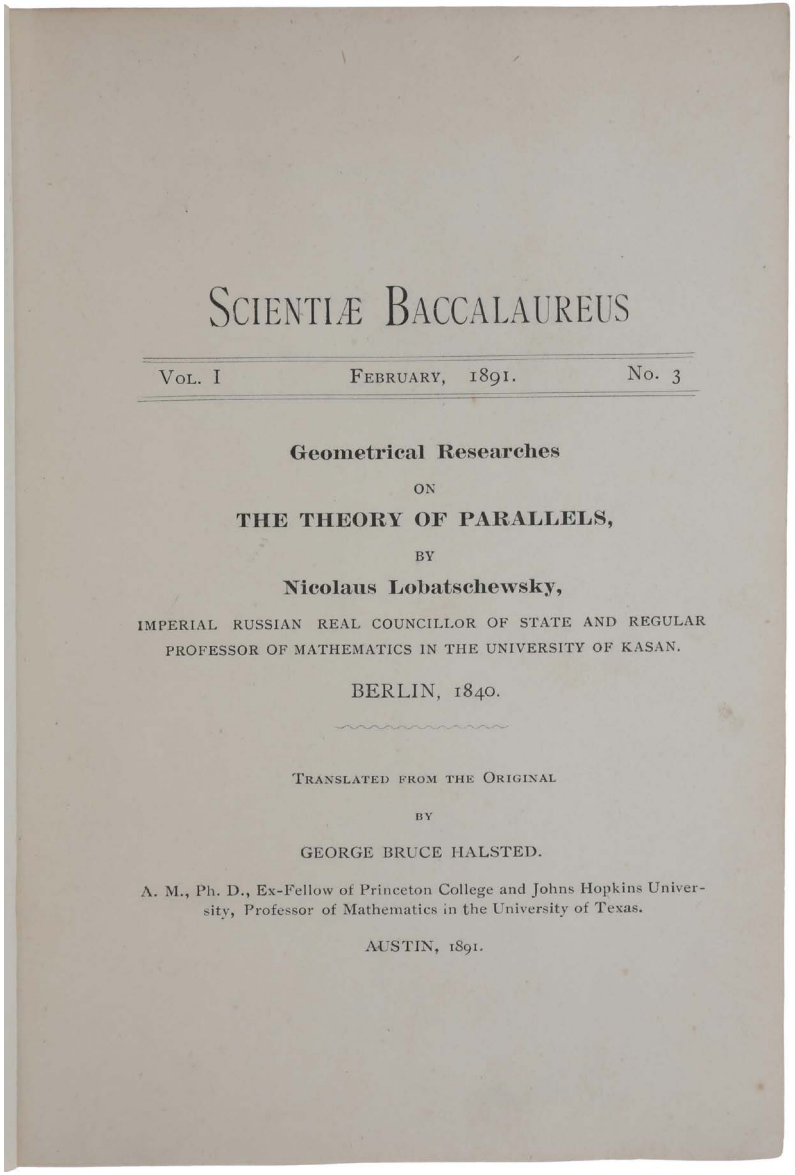
"Working from the geometry (and, hence, trigonometry) of the Euclidean plane

on horospheres, Lobachevsky derived trigonometric formulas for triangles in the Lobachevskian plane … Comparing these formulas with those of spherical trigonometry on a sphere of radius r, … Lobachevsky discovered that the formulas of trigonometry in the space he defined can be derived from formulas of spherical trigonometry if the sides of triangles are regarded as purely imaginary numbers or, put another way, if the radius r of the sphere is considered as purely imaginary … In this Lobachevsky saw evidence of the non-contradictory nature of the geometry he had discovered" (ibid.).

"[An] important development of Lobachevsky's geometry came from Riemann, whose address of 1854, Uber die Hypothesen, welche der Geometrie zu Grunde liegen, was published in 1866. Developing Gauss's idea of intrinsic geometry of a surface, Riemann presented a notion of multidimensional curved space (now called Riemannian space) … It subsequently, then, became clear that Lobachevskian space is a Riemannian space of constant negative curvature …

"The presence of specifically Lobachevskian geometry is felt in modern physics in the isomorphism of the group of motions of Lobachevskian space and the Lorentz group. This isomorphism opens the possibility of applying Lobachevskian geometry to the solution of a number of problems of relativistic quantum physics. Within the framework of the general theory of relativity, the problem of the geometry of the real world, to which Lobachevsky had devoted so much attention, was solved; the geometry of the real world is that of variable curvature, which is on the average much closer to Lobachevsky's than to Euclid's" (ibid.).

Lobachevsky's first lecture on non-Euclidean geometry, "Exposition succincte des principes de la géométrie avec une démonstration rigoureuse du théorème des parallèles," was delivered (in French) to the Kazan department of physics and mathematics at a meeting held on 23 February 1826. French was the language



SCIENTIÆ BACCALAUREUS

VOL. I FEBRUARY, 1891. No. 3

Geometrical Researches

ON

THE THEORY OF PARALLELS,

BY

Nicolaus Lobatschewsky,

IMPERIAL RUSSIAN REAL COUNCILLOR OF STATE AND REGULAR PROFESSOR OF MATHEMATICS IN THE UNIVERSITY OF KASAN.

BERLIN, 1840.

TRANSLATED FROM THE ORIGINAL

BY

GEORGE BRUCE HALSTED.

A. M., Ph. D., Ex-Fellow of Princeton College and Johns Hopkins University, Professor of Mathematics in the University of Texas.

AUSTIN, 1891.

of scientific discourse in Russia but Lobachevsky strongly advocated the use of the Russian language and published his first four works in his native tongue: O nachalakh geometrii (1829-30); Novye nachala geometrii s polnoi teoriei parallelnykh (1835-38); Voobrazhaemaya geometriya (1835); Primenenie voobrazhaemoi geometrii k nekotorym integralam (1836). The 1835 work was revised and translated into French by Lobachevsky and published as 'Géométrie imaginaire' (Journal für die reine und angewandte Mathematik 17 (1837), 295–320). This was the first publication on non-Euclidean geometry in a Western European language, but it dealt only with Lobachevskian trigonometry, not with the underlying geometry. His other early works were not translated until the last years of the 19th century. Thus, Geometrische Untersuchungen (1840) is the first work in a Western European language to treat both the synthetic foundations of non-Euclidean geometry and the Lobachevskian trigonometry. It was translated into French in 1866 by Jules Hoüel and into English in 1891 by George Bruce Halsted, as the offered work. Lobachevsky published a final summary work in Russian, Pangeometria, as part of a volume celebrating the jubilee of the University of Kazan in 1855; it was translated into French in the following year.

The appearance of Lobachevsky's work in the Scientiae Baccalaureus is little-known, and has frequently been ignored by historians of mathematics. The first English translation is often given as that which appeared, under the same title and with the same translator, in the 'Neomonic Series' of the Bulletin of the University of Texas at Austin. The preface and main text are identical to that of the Scientiae Baccalaureus version, but its preface is dated May, 1891, and must therefore have appeared at least three months later.

"[The] Scientiae Baccalaureus, A Quarterly Journal of Scientific Research, was published under the auspices of the Missouri School of Mines [M. S. M.], and was edited by 'The Senior Classmen'. Fewer than ten libraries now own Scientiae Baccalaureus, and even at its home institution it cannot be found in the regular stacks" (Hall). Only one volume was published, in 1890-91, comprising four issues.

"The journal's greatest claim to fame came through Halsted. Vol. 1, No. 3 contains his translation of N. Lobatschewsky's [spelled here as in the journal] work on parallel lines, and Vol. 1, No. 4 contains his translation of J. Bolyai's famous appendix to the book written by his father. Their publication in Scientiae Baccalaureus was the first English language appearance of either of these seminal works on non-Euclidean geometry, in which each man, independently, showed that a consistent geometry was possible by negating Euclid's fifth postulate that exactly one parallel to a given line may be drawn through a point not on the line. E. T. Bell considers the work of Bolyai and Lobachevsky one of the major revolutions in all thought, comparable in significance to the ideas of Copernicus …

"Because of the short life and relative obscurity of the journal, and because Halsted later published his translations through the University of Texas, Scientiae Baccalaureus is not always given as a source for these translations by historians and in bibliographies. Smith credits Halsted with both translations in 1891, but does not specify where they appeared. Bonola lists Halsted's translation of Lobachevsky as "English translation by G. B. Halsted (Austin, Texas, 1891)." The … Translator's Preface, identical to the Scientiae Baccalaureus version, is dated May 1, 1891. In that Preface Halsted claims, "Of the immortal essay now first appearing in English …" The date of Vol. 1, No. 3 of Scientiae Baccalaureus is February, 1891. On the other hand, Sommerville's bibliography lists the May, 1891 Texas Univ. Bull. as the 2nd ed. of the English translation of Lobachevsky, and does cite the February publication in Scientiae Baccalaureus … knowledge of even the existence of Scientiae Baccalaureus gets buried a little deeper as the years go by. Halsted himself wrote many articles which mention Bolyai and Lobachevsky,

but he usually simply referred to 'my translation(s)' without giving any specific source. All this supports the conclusion that Halsted did not intentionally ignore Scientiae Baccalaureus, but it is reasonable to assume that when he was asked for reprints of his translations, he sent the ones published in Austin simply because those were the ones to which he had ready access. When Halsted died in 1922, his obituary notice in the [American Mathematical] Monthly [Vol. 29 (1922), p. 187] pushed Scientiae Baccalaureus even further into obscurity, saying, "His most important work was the translation of writings on non-Euclidean geometry. Lobachevski's Researches on the Theory of Parallels and Bolyai's Science Absolute of Space were first published at Austin, Texas, in 1891, as parts of the 'Neomonic Series'." As we have seen, the word 'first' in this quote is in error …

"The decision to discontinue publication of Scientiae Baccalaureus was announced in an Editorial Note in Vol. 1, No. 4, June 1891, which was signed 'W. H. E.' … The full text of Echols' editorial note follows, and gives additional evidence of the primacy of the English translations of Lobachevsky and Bolyai. 'It has been decided to discontinue the publication of this Journal and its issue ceases with this number, which closes the first volume. We close the first volume and cease the publication with considerable regret, yet with no small degree of satisfaction, believing as we do, that as a Journal of Elementary Mathematics it has accomplished fairly well the object which it had in view. Had it done nothing more than to put into English words the papers of Bolyai and Lobatschewsky its life had been well lived. We believe that the time will yet come when the seed thus sown will bear its share of fruit in the advancement of sound geometrical teaching in America'" (Hall).

"Lobachevsky was the son of Ivan Maksimovich Lobachevsky, a clerk in a land-surveying office, and Praskovia Aleksandrovna Lobachevskaya. In about 1800 the mother moved with her three sons to Kazan, where Lobachevsky and his brothers were soon enrolled in the Gymnasium on public scholarships. In 1807 Lobachevsky entered Kazan University, where he studied under the supervision of Martin Bartels, a friend of Gauss, and, in 1812, received the master's degree in physics and mathematics. In 1814 he became an adjunct in physical and mathematical sciences and began to lecture on various aspects of mathematics and mechanics. He was appointed extraordinary professor in 1814 and professor ordinarius in 1822, the same year in which he began an administrative career as a member of the committee formed to supervise the construction of the new university buildings. He was chairman of that committee in 1825, twice dean of the department of physics and mathematics (in 1820-1 and 1823-5), librarian of the university (1825-35), rector (1827-46), and assistant trustee for the whole of the Kazan educational district (1846-55).

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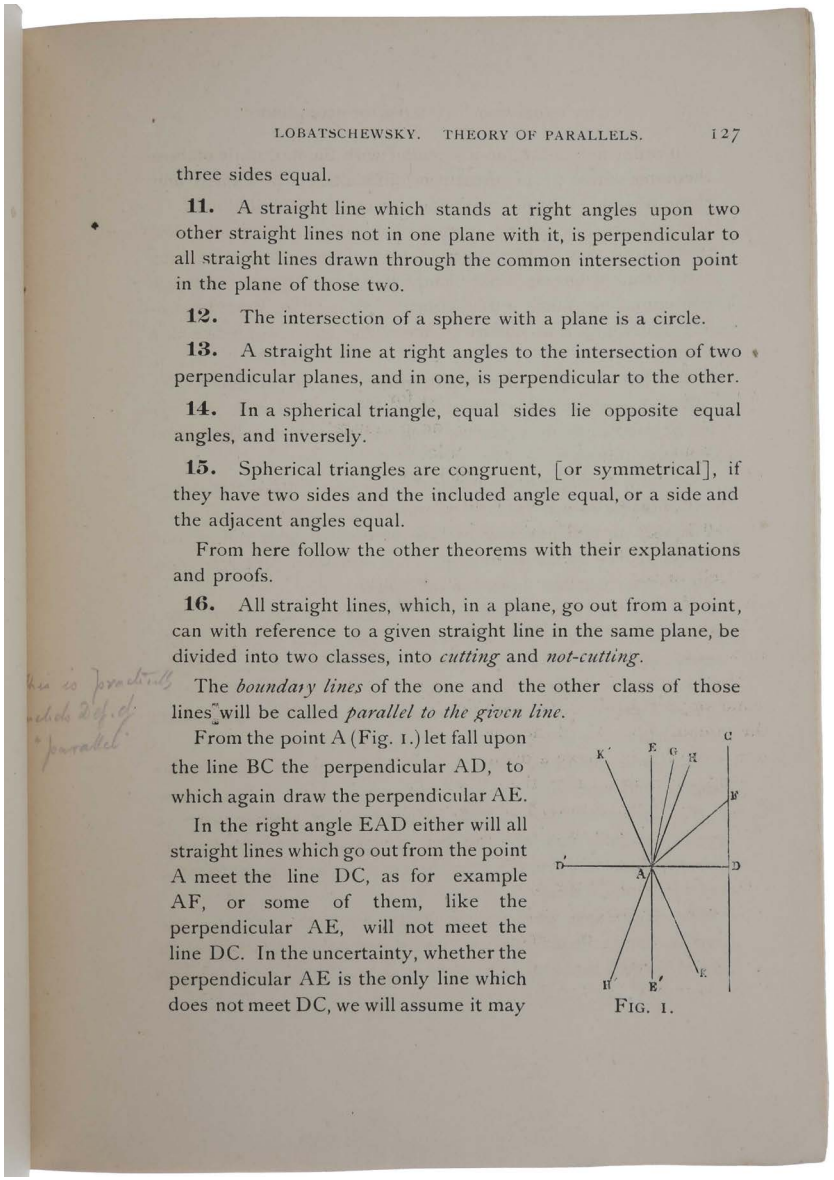
George Bruce Halsted (1853-1922) was the first student of J. J. Sylvester at Johns Hopkins University and held chairs at several universities, including the University of Texas (1894-1903) where the mathematicians L. E. Dickson and R. L. Moore were among his students. "In a period when American mathematics had few distinguished names, the eccentric and sometimes spectacular Halsted established himself as an internationally known scholar, creative teacher, and promoter and popularizer of mathematics. He was a member of and active

participant in the major mathematical societies of the United States, England, Italy, Spain, France, Germany and Russia. His activities penetrated deeply in three main fields: translations and commentaries on the works of Nikolai Lobachevski, János Bolyai, Girolamo Saccheri, and Henri Poincaré; studies in the foundations of geometry; and criticisms of the slipshod presentations of the mathematical textbooks of the day" (DSB).

PMM 293; Norman I, 1379; Sommerville p. 100. E. T. Bell, The Development of Mathematics (1945). R. Bonola, Non-Euclidean Geometry (1955). L. M. Hall, 'A Forgotten Nineteenth Century Mathematics Journal,' Missouri Journal of Mathematical Sciences 16 (2004), pp. 159-167. S. Piccard, Lobatchevsky. Grand Mathématicien Russe. Sa Vie, son Oeuvre (1957). D. E. Smith, A Source Book in Mathematics (1920). D. M. Y. Sommerville, Bibliography of Non-Euclidean Geometry (1911).



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three sides equal.

11. A straight line which stands at right angles upon two other straight lines not in one plane with it, is perpendicular to all straight lines drawn through the common intersection point in the plane of those two.

12. The intersection of a sphere with a plane is a circle.

13. A straight line at right angles to the intersection of two perpendicular planes, and in one, is perpendicular to the other.

14. In a spherical triangle, equal sides lie opposite equal angles, and inversely.

15. Spherical triangles are congruent, [or symmetrical], if they have two sides and the included angle equal, or a side and the adjacent angles equal.

From here follow the other theorems with their explanations and proofs.

16. All straight lines, which, in a plane, go out from a point, can with reference to a given straight line in the same plane, be divided into two classes, into *cutting* and *not-cutting*.

The *boundary lines* of the one and the other class of those lines will be called *parallel to the given line*.

From the point A (Fig. 1.) let fall upon the line BC the perpendicular AD, to which again draw the perpendicular AE.

In the right angle EAD either will all straight lines which go out from the point A meet the line DC, as for example AF, or some of them, like the perpendicular AE, will not meet the line DC. In the uncertainty, whether the perpendicular AE is the only line which does not meet DC, we will assume it may

FIG. 1.

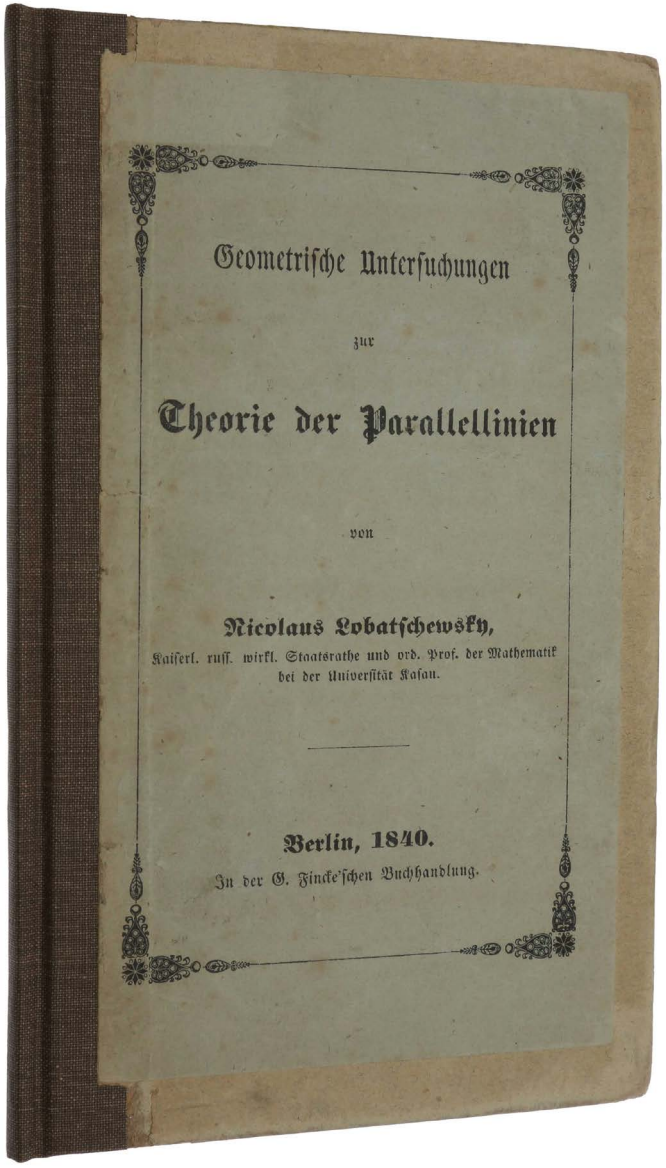
UN VRAI CHEF-D'OEUVRE DE LA LITTÉRATURE MATHÉMATIQUE

LOBACHEVSKY, Nikolai Ivanovich. *Geometrische Untersuchungen zur Theorie der Parallellinien.* Berlin: G. Fincke, 1840.

$40,000

8vo, pp. [ii], 61, [1], with two folding lithographed plates. Old boards with cloth spine, original front printed wrapper mounted on front cover. Preserved in a cloth-covered clamshell case with lettering-piece on spine.

First edition, very rare, of the first complete account of Lobachevsky's revolutionary discovery of non-Euclidean geometry to be published in a Western European language. It was through this book that the mathematical world outside Russia became aware of Lobachevsky's work. "Geometrische Untersuchungen zur Theorie der Parallellinien, which he published in Berlin in 1840, is the best exposition of his new geometry; following its publication, in 1842, Lobachevsky was, on the recommendation of Gauss, elected to the Göttingen Gesellschaft der Wissenschaften" (DSB). "Cet ouvrage est un vrai chef-d'oeuvre de la littérature mathématique" (Piccard, p. 19). Lobachevsky sent Gauss a copy of the present work, and Gauss's opinion of it is recorded in a letter, dated 28 November 1846, written to his colleague the astronomer H. C. Schumacher: "In developing the subject, the author followed a road different from the one I took myself; Lobachevsky carried out the task in a masterly fashion and in a truly geometric spirit. I consider it a duty to call your attention to this book, since I have no doubt that it will give you a tremendous pleasure." "The researches that culminated in the discovery of non-Euclidean geometry arose from unsuccessful attempts to



'prove' the axiom of parallels in Euclidean geometry. This postulate asserts that through any point there can be drawn one and only one straight line parallel to a given straight line. Although this statement was not regarded as self-evident and its derivation from the other axioms of geometry was repeatedly sought, no one openly challenged it as an accepted truth of the universe until Lobatchewsky published the first non-Euclidean geometry … In Lobatchewsky's geometry an infinity of parallels can be drawn through a given point that never intersect a given straight line … His fundamental paper was read to his colleagues in Kazan in 1826 but he did not publish the results until 1829-30 when a series of five papers appeared in the Kazan University Courier … He amplified his findings (still in Russian) in 1836-8 under the title 'New Elements of Geometry, with a Complete Theory of Parallels'. In 1840 he published a brief summary in Berlin under the title Geometrische Untersuchungen zur Theorie der Parallellinien" (PMM). The present work, like all of Lobachevsky's publications, is very rare. OCLC lists just seven copies in the US. No copies on ABPC/RBH.

"In his early lectures on geometry, Lobachevsky himself attempted to prove the fifth postulate [i.e., the axiom of parallels]; his own geometry is derived from his later insight that a geometry in which all of Euclid's axioms except the fifth postulate hold true is not in itself contradictory. He called such a system "imaginary geometry," proceeding from an analogy with imaginary numbers. If imaginary numbers are the most general numbers for which the laws of arithmetic of real numbers prove justifiable, then imaginary geometry is the most general geometrical system. It was Lobachevsky's merit to refute the uniqueness of Euclid's geometry, and to consider it as a special case of a more general system.

"In Lobachevskian geometry, given a line a and a point A not on it, one can draw through A more than one coplanar line not intersecting a. It follows that one can draw infinitely many such lines which, taken together, constitute an angle of which the vertex is A. The two lines, b and c, bordering that angle are called parallels to a and the lines contained between them are called ultraparallels, or diverging lines; all other lines through A intersect a. If one measures the distance between two parallel lines on a secant equally inclined to each, then, as Lobachevsky proved, that distance decreases indefinitely, tending to zero, as one moves farther out from A … A comparison of Euclidean and Lobachevskian geometry yields several immediate and interesting contrasts [notably that] for all triangles in the Lobachevskian plane the sum of the angles is less than two right angles" (DSB). In Euclidean geometry the sum of the angles of a triangle equals two right angles, and in spherical geometry it is always greater.

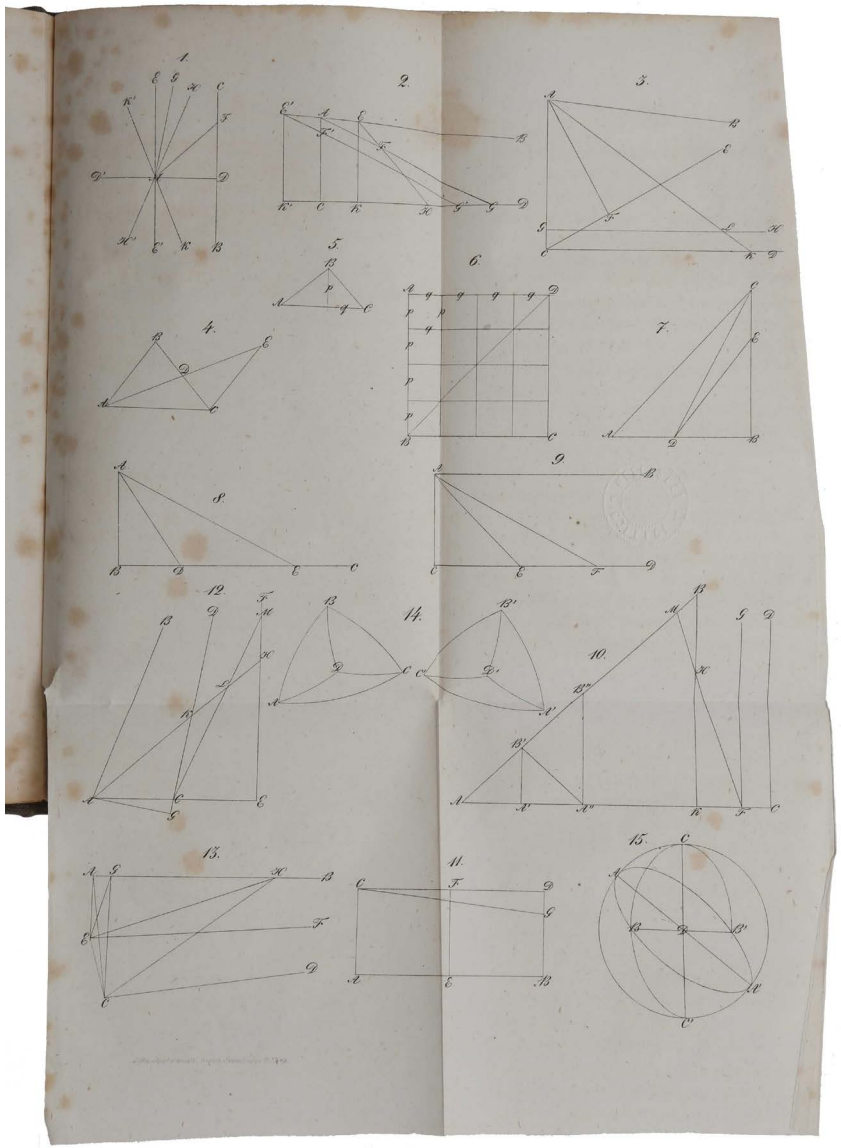
Of particular interest are the curves called 'horocycles.' These are the limiting curves of the circles that share a tangent at a given point, as the radius of the circles tends to infinity. In Euclidean geometry this limiting curve would be a straight line, but in Lobachevskian geometry it is a new kind of curve. By rotating the horocyle around the line perpendicular to the tangent, Lobachevsky obtained a 'horosphere' and he proved the remarkable fact that the geometry on a horosphere is Euclidean, so that Euclidean geometry is in a sense contained within non-Euclidean geometry.

"Working from the geometry (and, hence, trigonometry) of the Euclidean plane on horospheres, Lobachevsky derived trigonometric formulas for triangles in the Lobachevskian plane … Comparing these formulas with those of spherical trigonometry on a sphere of radius r, … Lobachevsky discovered that the formulas of trigonometry in the space he defined can be derived from formulas of spherical trigonometry if the sides of triangles are regarded as purely imaginary numbers or, put another way, if the radius r of the sphere is considered as purely imaginary … In this Lobachevsky saw evidence of the non-contradictory nature of the geometry he had discovered" (ibid.). Further evidence of the 'imaginary' nature of Lobachevskian geometry was found in the formula for the area A of a

triangle in terms of the sum S of its angles: A = r2(π – S). If r is replaced by r√-1, this becomes the formula for the area of a triangle on a sphere of radius r: A = r2(S - π).

"Lobachevsky recognized the universal character of his new geometry in naming it "pangeometry." He nevertheless thought it necessary to establish experimentally which geometry—his or Euclid's— actually occurs in the real world. To this end he made a series of calculations of the sums of the angles of triangles of which the vertices are two diametrically opposed points on the orbit of the earth and one of the fixed stars Sirius, Rigel, or 28 Eridani. Having established that the deviation of these sums from π is no greater than might be due to errors in observation, he concluded that the geometry of the real world might be considered as Euclidean, whence he also found "a rigorous proof of the theorem of parallels" as set out in his work of 1826. In explaining his calculations (in O nachalakh geometrii ["On the Principles of Geometry"]), Lobachevsky noted that it is possible to find experimentally the deviation from π of the sum of the angles of cosmic triangles of great size; in a later work (Novye nachala geometrii s polnoi teoriei parallelnykh ["New Principles of Geometry With a Complete Theory of Parallels"]) he moved to the opposite scale and suggested that his geometry might find application in the "intimate sphere of molecular attractions" …

"In [these] earlier papers Lobachevsky had defined imaginary geometry on an a priori basis, beginning with the supposition that Euclid's fifth postulate does not hold true and explaining the principle tenets of his new geometry without defining it (although he did describe the results of his experiment to prove his theorem of parallels). In Voobrazhaemaya geometriya ("Imaginary Geometry"), however, he built up the new geometry analytically, proceeding from its inherent trigonometrical formulas and considering the derivation of these formulas from spherical trigonometry to guarantee its internal consistency. In the sequel



to that paper, Primenenie voobrazhaemoi geometrii k nekotorym integralam ("Application of Imaginary Geometry to Certain Integrals"), he applied geometrical considerations in Lobachevskian space to the calculation of known integrals (in order to make sure that their application led to valid results), then to new, previously uncalculated integrals" …

"Lobachevsky's work was little heralded during his lifetime. M. V. Ostrogradsky, the most famous mathematician of the St. Petersburg Academy, for one, did not understand Lobachevsky's achievement, and published an uncomplimentary review of O nachalakh geometrii ("On the Principles of Geometry"); the magazine Syn otechestva soon followed his lead, and in 1834 issued a pamphlet ridiculing Lobachevsky's paper. Although Gauss, who had received a copy of the Geometrische Untersuchungen from Lobachevsky, spoke to him flatteringly of the book, studied Russian especially to read his works in their original language, and supported his election to the Göttingen Gesellschaft der Wissenschaften, he never publicly commented on Lobachevsky's discovery" …

"[An] important development of Lobachevsky's geometry came from Riemann, whose address of 1854, Uber die Hypothesen, welche der Geometrie zu Grunde liegen, was published in 1866. Developing Gauss's idea of intrinsic geometry of a surface, Riemann presented a notion of multidimensional curved space (now called Riemannian space) … It subsequently, then, became clear that Lobachevskian space is a Riemannian space of constant negative curvature …

"The presence of specifically Lobachevskian geometry is felt in modern physics in the isomorphism of the group of motions of Lobachevskian space and the Lorentz group. This isomorphism opens the possibility of applying Lobachevskian geometry to the solution of a number of problems of relativistic quantum physics. Within the framework of the general theory of relativity, the problem of the geometry of the real world, to which Lobachevsky had devoted so much attention, was solved; the geometry of the real world is that of variable curvature, which is on the average much closer to Lobachevsky's than to Euclid's" (ibid.).

Lobachevsky's first lecture on non-Euclidean geometry, "Exposition succincte des principes de la géométrie avec une démonstration rigoureuse du théorème des parallèles," was delivered (in French) to the Kazan department of physics and mathematics at a meeting held on 23 February 1826. French was the language of scientific discourse in Russia but Lobachevsky strongly advocated the use of the Russian language and published his first four works in his native tongue: O nachalakh geometrii (1829-30); Novye nachala geometrii s polnoi teoriei parallelnykh (1835-38); Voobrazhaemaya geometriya (1835); Primenenie voobrazhaemoi geometrii k nekotorym integralam (1836). The 1835 work was revised and translated into French by Lobachevsky and published as 'Géométrie imaginaire' (Journal für die reine und angewandte Mathematik 17 (1837), 295–320). This was the first publication on non-Euclidean geometry in a Western European language, but it dealt only with Lobachevskian trigonometry, not with the underlying geometry. His other early works were not translated until the last years of the 19th century. Thus, Geometrische Untersuchungen (1840) is the first work in a Western European language to treat both the synthetic foundations of non-Euclidean geometry and the Lobachevskian trigonometry. It was translated into French in 1866 by Jules Hoüel and into English in 1891 by George Bruce Halsted. Lobachevsky published a final summary work in Russian, Pangeometria, as part of a volume celebrating the jubilee of the University of Kazan in 1855; it was translated into French in the following year.

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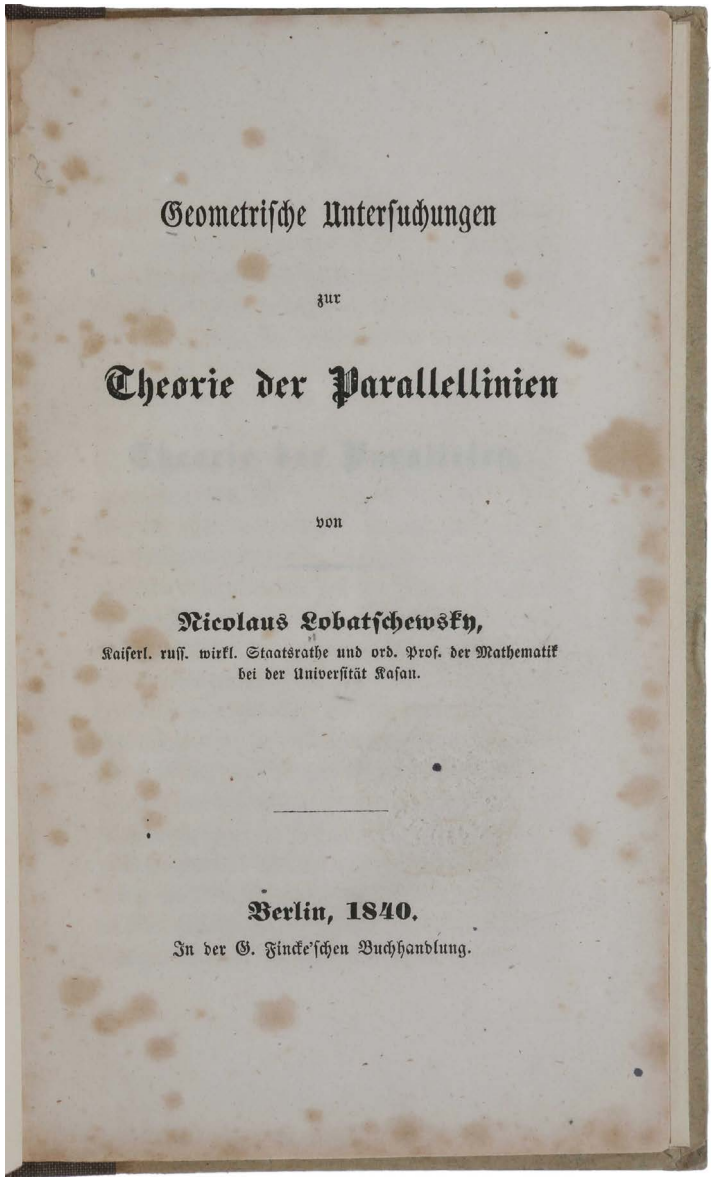
"In recognition of his work Lobachevsky was in 1837 raised to the hereditary nobility; he designed his own familial device (which is reproduced on his tombstone), depicting Solomon's seal, a bee, an arrow, and a horseshoe, to symbolize wisdom, diligence, alacrity, and happiness, respectively. He had in 1832 made a wealthy marriage, to Lady Varvara Aleksivna Moisieva, but his family of seven children and the cost of technological improvements for his estate left him with little money upon his retirement from the university, although he received a modest pension. A worsening sclerotic condition progressively affected his eyesight, and he was blind in his last years" (ibid.).

PMM 293; Engel 13; [O nachalakh geometrii]; Poggendorff I, 1482; Sommerville p. 29. Piccard, Lobatchevsky. Grand Mathématicien Russe. Sa Vie, son Oeuvre (1957).



Geometrische Untersuchungen

zur

Theorie der Parallellinien

von

Nicolaus Lobatschewsky,

Kaiserl. russ. wirkl. Staatsrathe und ord. Prof. der Mathematik bei der Universität Kasan.

Berlin, 1840.

In der G. Fincke'schen Buchhandlung.

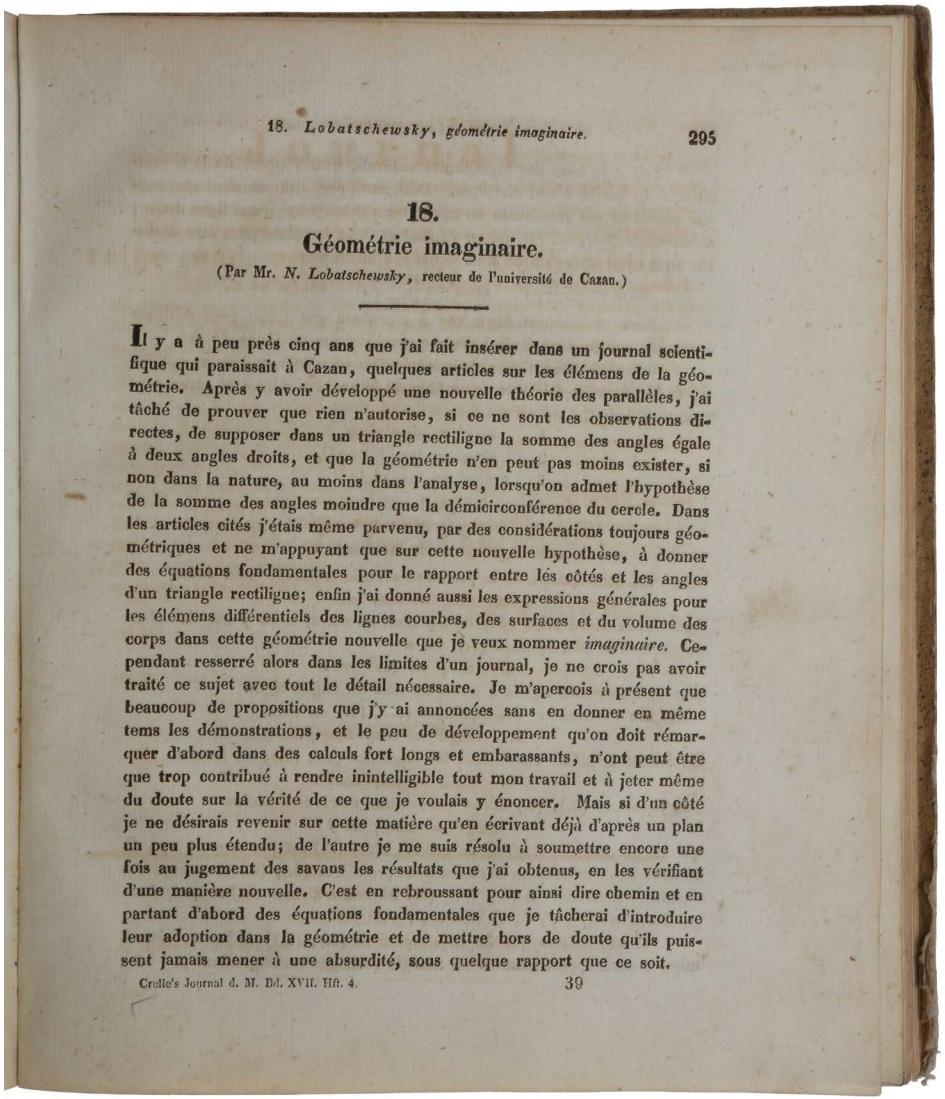
FIRST WESTERN ACCOUNT OF NON-EUCLIDEAN GEOMETRY

LOBACHEVSKY, Nikolai Ivanovich. *Géométrie imaginaire.* Berlin: Reimer, 1837.

$3,500

Pp. 295-320 and Plate II in Journal für die reine und angewandte Mathematik, Bd. 17. 4to (250 x 218 mm), pp. iv, 394 with three folding plates. Contemporary mottled boards with leather lettering-piece on spine (extremities a little rubbed).

First edition, journal issue, of the first account of any part of Lobachevsky's revolutionary discovery of non-Euclidean geometry to be published in a Western European language. "The researches that culminated in the discovery of non-Euclidean geometry arose from unsuccessful attempts to 'prove' the axiom of parallels in Euclidean geometry. This postulate asserts that through any point there can be drawn one and only one straight line parallel to a given straight line. Although this statement was not regarded as self-evident and its derivation from the other axioms of geometry was repeatedly sought, no one openly challenged it as an accepted truth of the universe until Lobatchewsky published the first non-Euclidean geometry … In Lobatchewsky's geometry an infinity of parallels can be drawn through a given point that never intersect a given straight line … His fundamental paper was read to his colleagues in Kazan in 1826 but he did not publish the results until 1829-30 when a series of five papers appeared [in Russian] in the Kazan University Courier [O nachalakh geometrii, 1829-30]" (PMM). A work with the same title, Voobrazhaemaya geometriya, was published (in Russian) in 1835, but according to Sommerville (p. 28) this French version was "Written previous to the Russian paper bearing the same title, 1835". In



18. *Lobatschewsky, géométrie imaginaire.* 295

18.

Géométrie imaginaire.

(Par Mr. *N. Lobatschewsky*, recteur de l'université de Cazan.)

Il y a à peu près cinq ans que j'ai fait insérer dans un journal scientifique qui paraissait à Cazan, quelques articles sur les élémens de la géométrie. Après y avoir développé une nouvelle théorie des parallèles, j'ai tâché de prouver que rien n'autorise, si ce ne sont les observations directes, de supposer dans un triangle rectiligne la somme des angles égale à deux angles droits, et que la géométrie n'en peut pas moins exister, si non dans la nature, au moins dans l'analyse, lorsqu'on admet l'hypothèse de la somme des angles moindre que la démicirconférence du cercle. Dans les articles cités j'étais même parvenu, par des considérations toujours géométriques et ne m'appuyant que sur cette nouvelle hypothèse, à donner des équations fondamentales pour le rapport entre les côtés et les angles d'un triangle rectiligne; enfin j'ai donné aussi les expressions générales pour les élémens différentiels des lignes courbes, des surfaces et du volume des corps dans cette géométrie nouvelle que je veux nommer *imaginaire.* Cependant resserré alors dans les limites d'un journal, je ne crois pas avoir traité ce sujet avec tout le détail nécessaire. Je m'apercois à présent que beaucoup de propositions que j'y ai annoncées sans en donner en même tems les démonstrations, et le peu de développement qu'on doit rémarquer d'abord dans des calculs fort longs et embarassants, n'ont peut être que trop contribué à rendre inintelligible tout mon travail et à jeter même du doute sur la vérité de ce que je voulais y énoncer. Mais si d'un côté je ne désirais revenir sur cette matière qu'en écrivant déjà d'après un plan un peu plus étendu; de l'autre je me suis résolu à soumettre encore une fois au jugement des savans les résultats que j'ai obtenus, en les vérifiant d'une manière nouvelle. C'est en rebroussant pour ainsi dire chemin et en partant d'abord des équations fondamentales que je tâcherai d'introduire leur adoption dans la géométrie et de mettre hors de doute qu'ils puissent jamais mener à une absurdité, sous quelque rapport que ce soit.

Crelle's Journal d. M. Bd. XVII. Hft. 4. 39

it, "he built up the new geometry analytically, proceeding from its inherent trigonometrical formulas and considering the derivation of these formulas from spherical trigonometry to guarantee its internal consistency" (DSB). Lobachevsky shows that all the analytical and geometrical theorems in non-Euclidean geometry follow from these formulas.

"In his early lectures on geometry, Lobachevsky himself attempted to prove the fifth postulate [i.e., the axiom of parallels]; his own geometry is derived from his later insight that a geometry in which all of Euclid's axioms except the fifth postulate hold true is not in itself contradictory. He called such a system "imaginary geometry," proceeding from an analogy with imaginary numbers. If imaginary numbers are the most general numbers for which the laws of arithmetic of real numbers prove justifiable, then imaginary geometry is the most general geometrical system. It was Lobachevsky's merit to refute the uniqueness of Euclid's geometry, and to consider it as a special case of a more general system.

"In Lobachevskian geometry, given a line a and a point A not on it, one can draw through A more than one coplanar line not intersecting a. It follows that one can draw infinitely many such lines which, taken together, constitute an angle of which the vertex is A. The two lines, b and c, bordering that angle are called parallels to a and the lines contained between them are called ultraparallels, or diverging lines; all other lines through A intersect a. If one measures the distance between two parallel lines on a secant equally inclined to each, then, as Lobachevsky proved, that distance decreases indefinitely, tending to zero, as one moves farther out from A … A comparison of Euclidean and Lobachevskian geometry yields several immediate and interesting contrasts [notably that] for all triangles in the Lobachevskian plane the sum of the angles is less than two right angles" (DSB). In Euclidean geometry the sum of the angles of a triangle equals two right angles, and in spherical geometry it is always greater.

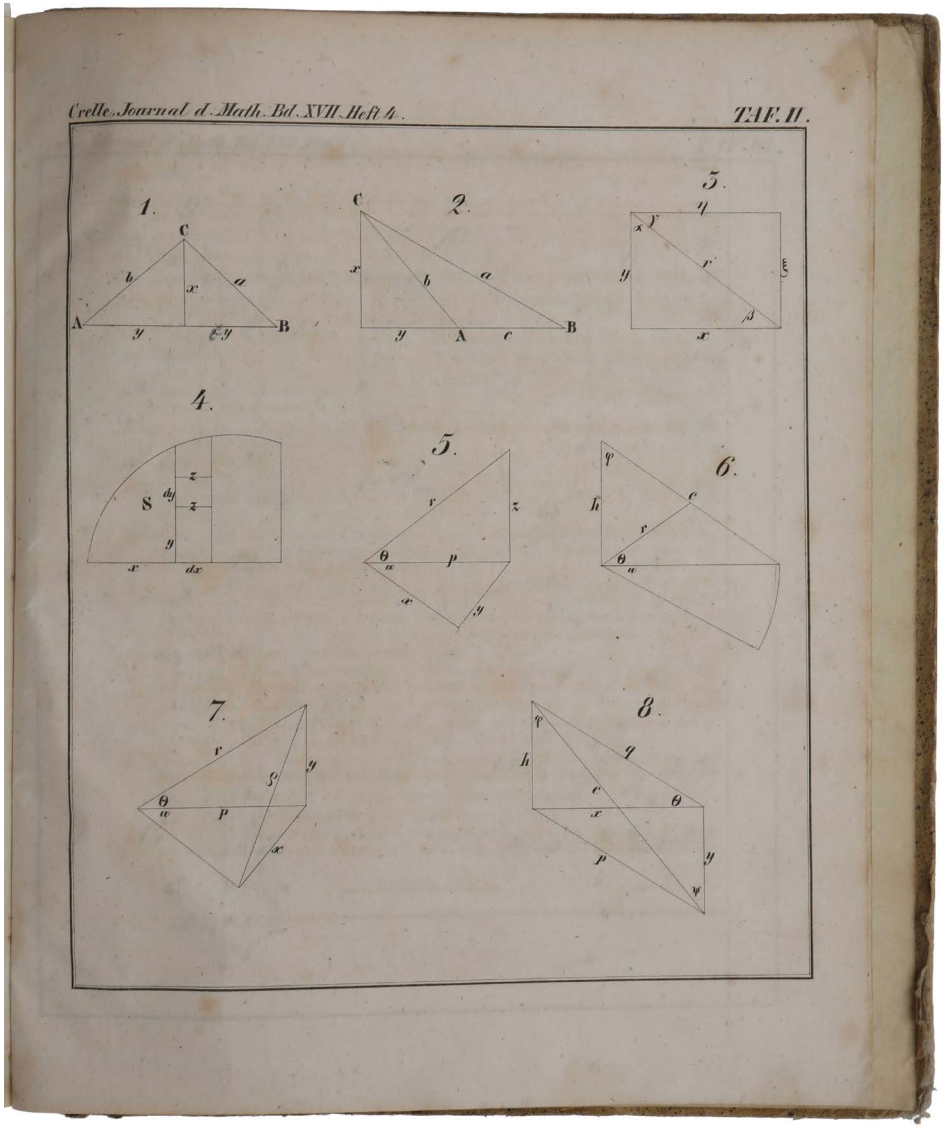
Of particular interest are the curves called 'horocycles.' These are the limiting curves of the circles that share a tangent at a given point, as the radius of the circles tends to infinity. In Euclidean geometry this limiting curve would be a straight line, but in Lobachevskian geometry it is a new kind of curve. By rotating the horocyle around the line perpendicular to the tangent, Lobachevsky obtained a 'horosphere' and he proved the remarkable fact that the geometry on a horosphere is Euclidean, so that Euclidean geometry is in a sense contained within non-Euclidean geometry.

"Working from the geometry (and, hence, trigonometry) of the Euclidean plane on horospheres, Lobachevsky derived trigonometric formulas for triangles in the Lobachevskian plane … Comparing these formulas with those of spherical trigonometry on a sphere of radius r, … Lobachevsky discovered that the formulas of trigonometry in the space he defined can be derived from formulas of spherical trigonometry if the sides of triangles are regarded as purely imaginary numbers or, put another way, if the radius r of the sphere is considered as purely imaginary … In this Lobachevsky saw evidence of the non-contradictory nature of the geometry he had discovered" (ibid.).

Lobachevsky's first lecture on non-Euclidean geometry, "Exposition succincte des principes de la géométrie avec une démonstration rigoureuse du théorème des parallèles," was delivered (in French) to the Kazan department of physics and mathematics at a meeting held on 23 February 1826. French was the language of scientific discourse in Russia but Lobachevsky strongly advocated the use of the Russian language and published his first four works in his native tongue: O nachalakh geometrii (1829-30); Novye nachala geometrii s polnoi teoriei parallelnykh (1835-38); Voobrazhaemaya geometriya (1835); Primenenie voobrazhaemoi geometrii k nekotorym integralam (1836). With the exception of Voobrazhaemaya geometriya, these early Russian works on non-Euclidean

geometry were not translated until the last years of the 19th century. Lobachevsky published a final summary work in Russian, Pangeometria, to mark the jubilee of the University of Kazan in 1855; this was translated into French in the following year.

As we have already noted, according to Sommerville the offered work was written before Voobrazhaemaya geometriya. A comparison of this work with 'Géométrie imaginaire' shows that the former is not a straightforward translation of the latter, although the content of the two works is very similar. They deal only with Lobachevskian trigonometry, omitting the underlying synthetic geometry which had been treated in detail in O nachalakh geometrii, but giving applications to the computation of definite integrals. Lobachevsky's integrals represent areas of surfaces and volumes of bodies in two- and three-dimensional non-Euclidean space. Computing the area or the volume of the same object in different manners turns out to be an efficient way of finding attractive formulae for some definite integrals. But besides the intrinsic value of the results obtained, there are several reasons why Lobachevsky worked out these computations. First of all, using non-Euclidean geometry for the computation of integrals was a way of showing the usefulness of non-Euclidean geometry in another branch of mathematics, namely analysis. At another level, drawing consequences of the new axiom system, like finding values of known integrals using these new methods was a way of checking that the new geometric system was not self-contradictory. This was a major concern for Lobachevsky, which he addresses in the introduction to 'Géométrie imaginaire'. He writes that he feels that he was unable to deal with the subject in the necessary detail in O nachalakh geometrii. Many results were stated there without proof, and this may have led some readers to doubt the truth of his work. In this work he therefore retraces the path taken in the earlier work, but this time starting with the fundamental equations of Lobachevskian trigonometry and deriving their consequences in more detail than before. By doing so he hopes



to put to rest any doubt that the assumptions on which his geometry rests could ever lead to a contradiction. "Lobachevsky's work was little heralded during his lifetime. M. V. Ostrogradsky, the most famous mathematician of the St. Petersburg Academy, for one, did not understand Lobachevsky's achievement, and published an uncomplimentary review of O nachalakh geometrii ("On the Principles of Geometry"); the magazine Syn otechestva soon followed his lead, and in 1834 issued a pamphlet ridiculing Lobachevsky's paper" (ibid.).

"Lobachevsky was the son of Ivan Maksimovich Lobachevsky, a clerk in a land-surveying office, and Praskovia Aleksandrovna Lobachevskaya. In about 1800 the mother moved with her three sons to Kazan, where Lobachevsky and his brothers were soon enrolled in the Gymnasium on public scholarships. In 1807 Lobachevsky entered Kazan University, where he studied under the supervision of Martin Bartels, a friend of Gauss, and, in 1812, received the master's degree in physics and mathematics. In 1814 he became an adjunct in physical and mathematical sciences and began to lecture on various aspects of mathematics and mechanics. He was appointed extraordinary professor in 1814 and professor ordinarius in 1822, the same year in which he began an administrative career as a member of the committee formed to supervise the construction of the new university buildings. He was chairman of that committee in 1825, twice dean of the department of physics and mathematics (in 1820-1 and 1823-5), librarian of the university (1825-35), rector (1827-46), and assistant trustee for the whole of the Kazan educational district (1846-55).

"In recognition of his work Lobachevsky was in 1837 raised to the hereditary nobility; he designed his own familial device (which is reproduced on his tombstone), depicting Solomon's seal, a bee, an arrow, and a horseshoe, to symbolize wisdom, diligence, alacrity, and happiness, respectively. He had in 1832 made a wealthy marriage, to Lady Varvara Aleksivna Moisieva, but his family of seven children and the cost of technological improvements for his estate left him with little money upon his retirement from the university, although he received a modest pension. A worsening sclerotic condition progressively affected his eyesight, and he was blind in his last years" (ibid.).

See PMM 293; Norman I, 1379 [O nachalakh geometrii]; Sommerville p. 28.

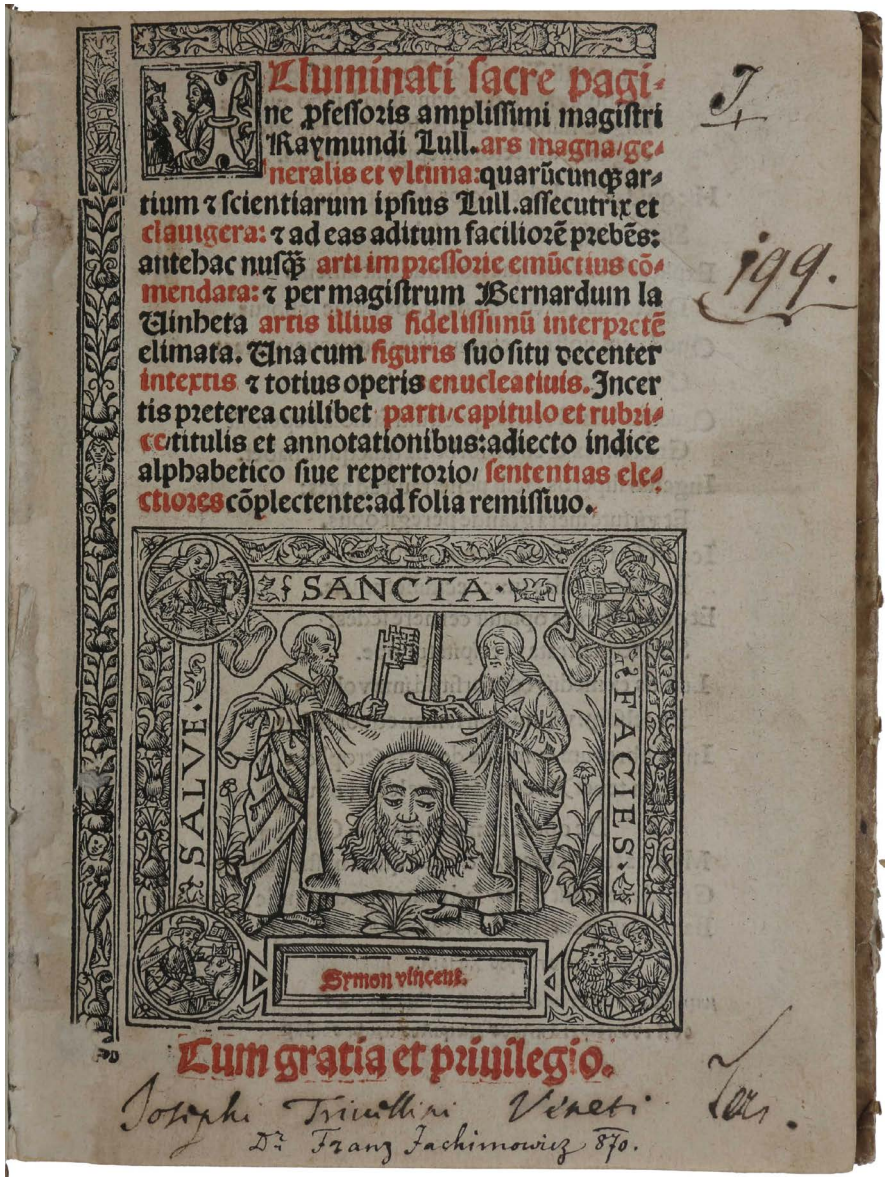
PERHAPS THE FIRST WORK IN COMPUTER SCIENCE

LULL, Ramon. *Ars magna generalis et ultima quaruncunque artium et scientiarum assecutrix et clauigera et ad eas aditum faciliorem praebens antehac nusquam arti impressorie emunctius commendata.* [Edited by Bernard de la Vinheta]. Lyons: Jacob Marechal for Simon Vincent, 5 May 1517.

$25,000

Small 4to, ff. [4], 124; gothic letter; title and last preliminary leaf printed in red and black, title with large woodcut printer's device and strip ornament on two sides, 5 large woodcut diagrams, one with volvelles printed on a separate sheet intended to be cut out and mounted, smaller printer's device at end. 18th century German boards.

Third edition, the first edited by the Lullist Bernard de Lavinheta (d. ca. 1530), of the Ars Magna, Lull's greatest work, now recognised as perhaps the first work in computer science. All early editions are all of the greatest rarity. "Lull invented an 'art of finding truth' which inspired Leibniz's dream of a universal algebra four centuries later … The most distinctive characteristic of [his] Art is clearly its combinatory nature, which led to both the use of complex semi-mechanical techniques that sometimes required figures with separately revolving concentric wheels – 'volvelles', in bibliographical parlance – and to the symbolic notation of its alphabet. These features justify its classification among the forerunners of both modern symbolic logic and computer science, with its systematically exhaustive consideration of all possible combinations of the material under



examination, reduced to a symbolic coding … The Art's function as a means of unifying all knowledge into a single system remained viable throughout the Renaissance and well into the seventeenth century. As a system of logical inquiry, its method of proceeding from basic sets of pre-established concepts by the systematic exploration of their combinations – in connection with any question on any conceivable subject – can be succinctly stated in terms taken from the Dissertatio de arte combinatoria (1666) of Leibniz, which was inspired by the Lullian Art: 'A proposition is made up of subject and predicate; hence all propositions are combinations. Hence the logic of inventing [discovering] propositions involves solving this problem: 1. Given a subject, [finding] the predicates; 2. Given a predicate, finding the subjects [to which it may] apply, whether by way of affirmation or negation'" (DSB). "From today's point of view Llull made contributions to the following areas: 1. The idea of a formal language, 2. The idea of a logical rule, 3. The computation of combinations, 4. The use of binary and ternary relations, 5. The use of symbols for variables, 6. The idea of substitution for a variable, 7. The use of a machine for logic. He also pioneered the integration of ontology with logic. Leibniz (1646–1716) mentions Llull by name several times and explicitly uses his ideas in [Ars combinatoria]. Leibniz learnt of Llull's ideas through Kircher [Ars magna sciendi, 1669]. This work led directly into the development of computing machines. So Llull contributed ideas that are fundamental to the modern disciplines of computer science and computer engineering" (Crossley, p. 40). ABPC/RBH list, in the last 35 years, no complete copy of the first edition (Venice, 1480), only the Honeyman copy of the second (Barcelona, 1501) (Sotheby's, May 13, 1980, lot 2063, £1,900 = $4,378, "modern vellum, marginally repaired, wormed"), and none of the third.

Running throughout Lull's immense oeuvre "is a leitmotif that enables one to arrive at an overall, if not unitary, view, that leitmotif being the Ars lulliana or Lullian Art: a philosophico-theological system that makes use of common basic concepts from the three monotheistic religions of its day, subjecting them to discussion with a view to convincing Muslims (and Jews) via rational argument of the truth of the Christian mysteries of faith. By revising his Art and extending it to all fields of human knowledge, Ramon Llull succeeded in creating a universal science, based on the algebraic notation of its basic concepts and their combination by means of mechanical figures …

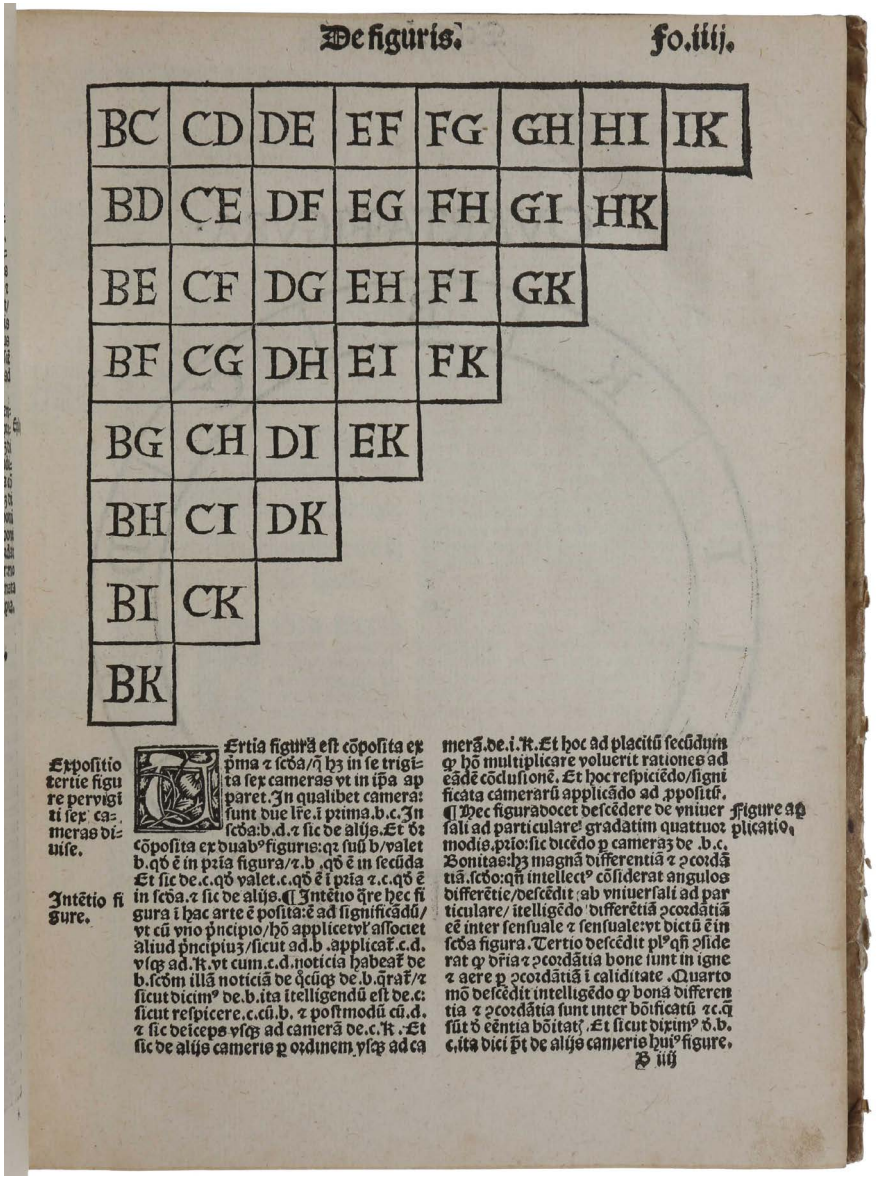
"From a more abstract point of view, Llull's combinatorial Art can be described as a process of elementary analysis and of reconstruction. On the one hand, it resolves the historical religions into their most primitive elements; on the other, it represents these elements by letters (from B to K), in order to recombine these letters and the elements of the different religions that they designate until, through these combinations, a vision of the world is reached that is as consistent as possible: this will correspond to truth. Undoubtedly, this process which Llull applied to all kinds of question – not just religious controversies – is a key ingredient of modern thought. One only has to think of Gottfried Wilhelm Leibniz's characteristica universalis: thus, in his Dissertatio de arte combinatoria, in 1666, the young Leibniz, clearly inspired by Llull, had already outlined the project of a reconstruction of the whole of reality based on a definite number of basic notions. Leibniz criticizes the basic notions of the Lullian "alphabet" as too limited and proposes another alternative and broader alphabet. In contradistinction to Llull, Leibniz does not represent these basic notions with letters but rather uses numbers. Thus, the basic notion of "space" is represented by the number 2, the basic notion of "between" by the number 3, and the basic notion of "the whole" by the number 10. Consequently, according to Leibniz, a complex concept such as, for instance, "interval" can be formulated as 2.3.10, that is, "space between the whole". Leibniz was convinced that in this way all questions could be reduced to mathematical problems and that, in order to solve any problem, we only have to set about calculating. This is the meaning of Leibniz's famous "Calculemus!"

"It is through Leibniz that Llull's influence also became decisive for more recent developments such as formal logic, as developed by Gottlob Frege in the late 19th

century. According to Frege, Leibniz's characteristica, in its later evolution, limited itself to different fields, such as arithmetic, geometry, chemistry and so on, but did not become universal as Leibniz, in fact, had wished. This is why Frege, in his famous

Begriffsschrift from 1879, intended to create an elementary language that would unify the different formal languages which, after Leibniz, had been established in the different natural sciences. This language developed into the formal logic that until now has dominated the philosophical discourse and which was an important step in the journey towards the creation of computing languages. What characterizes this kind of logic is its formal notation, using variables and symbols to represent the different logical propositions and operations. Based on this notation, Frege developed the so-called logical calculus. Although the language reached by this formal logic differs from that of the Art, Llull can be considered as the forerunner of this project, insofar as in his thought one can already find the idea of an elementary language that follows logical rules and uses variables while operating with the principle of substitution of these variables" (Fidora & Sierra, pp. 1-2).

Lull's work begins "with an 'Alphabet' giving the meaning of nine letters, in which he says, 'B signifies goodness, difference, whether?, God, justice, and avarice. C signifies …', and so on, all of which can best be set out in a table. He then sets out the components of the first column in his First Figure, or Figure A (f. iv). Notice first of all, as always with Llull, the letters don't represent variables, but constants. Here they're connected by lines to show that in the Divinity these attributes are mutually convertible. That is to say that God's goodness is great, God's greatness



De figuris. fo.iiij.

BC	CD	DE	EF	FG	GH	HI	IK
BD	CE	DF	EG	FH	GI	HK	
BE	CF	DG	EH	FI	GK		
BF	CG	DH	EI	FK			
BG	CH	DI	EK				
BH	CI	DK					
BI	CK						
BK							

Expositio tertie figure pervigiti sex cameras diuise.

Intētio figure.

Figure applicatio.

Ertia figurā est cōposita ex pma ⁊ scōa/q̄ hz in se trigīta sex cameras vt in ipa apparet. In qualibet camera: sunt due lre. i pma. b. c. In scōa: b. d. ⁊ sic de alijs. Et ōz cōposita ex duab⁹ figuris: qz suū b/valet b. qō ē in pria figura/⁊. b. qō ē in secūda Et sic de. c. qō valet. c. qō ē i pria ⁊. c. qō ē in scōa. ⁊ sic de alijs. ¶ Intētio ōre hec figura i hac arte ē posita: ē ad significādū/ vt cū vno pncipio/hō applicetur associet aliud pncipiuz/sicut ad. b. applicat. c. d. vsqz ad. k. vt cum. c. d. noticia habeat de b. scōm illā noticiā de ḡcūqz de. b. ḡrat/⁊ sicut dicim⁹ de. b. ita itelligendū est de. c: sicut respicere. c. cū. b. ⁊ postmodū cū. d. ⁊ sic deiceps vsqz ad camerā de. c. k. Et sic de alijs cameris p ordinem vsqz ad camerā. de. i. k. Et hoc ad placitū secūdum q̄ hō multiplicare voluerit rationes ad eādē cōclusionē. Et hoc respiciēdo/significata camerarū applicādo ad ppositū. ¶ Hec figura docet descēdere de vniuersali ad particulare' gradatim quattuor modis. pzio: sic dicēdo p cameraz de. b. c. Bonitas: hz magnā differentiā ⁊ pcordātiā. scōo: qñ intellect⁹ cōsiderat angulos differētie/descēdit ab vniuersali ad particulare/ itelligēdo differētiā pcordātiā eē inter sensuale ⁊ sensuale: vt dictū ē in scōa figura. Tertio descēdit pl⁹ qñ pside rat q̄ dria ⁊ pcordātia bone sunt in igne ⁊ aere p pcordātiā i caliditate. Quarto mō descēdit intelligēdo q̄ bona differentia ⁊ pcordātia sunt inter bōificatū ⁊c. q̄ fūt ō eēntia bōitat'. Et sicut dixim⁹ ō. b. c, ita dici pt de alijs cameris hui⁹ figure.

P iiij

is good, etc. This, in turn was one of Llull's definitions of God, because in the created world, as we all know too well, people's goodness is not always great, nor their greatness particularly good, etc. Now such a system of vertices connected by lines is what, as mathematicians, you will of course recognize as a graph. This might seem to be of purely anecdotal interest, but as we shall see in a moment, the relational nature of Llull's system is fundamental to his idea of an Ars combinatoria.

The components of the second column are set out in a Second Figure, or Figure T (f. iiv). Here we have a series of relational principles related among themselves in three groups of three, hence the triangular graphs. The first triangle has difference, concordance, and contrariety; the second beginning, middle, and end; and the third majority, equality, and minority. The concentric circles between the triangles and the outer letters show the areas in which these relations can be applied …

"The Third Figure (f. iiiir) combines the first two: Here Llull explains that B C, for instance, implies four concepts: goodness and greatness (from Figure A), and difference and concordance (from Figure T), permitting us to analyze a phrase such as 'Goodness has great difference and concordance' in terms of its applicability in the areas of sensual/sensual, sensual/intellectual, and intellectual/intellectual. It furthermore, as he points out, permits us to do this systematically throughout the entire alphabet. This is important, because one of the ways in which Llull conceived his Art as 'general' was precisely in its capacity to explore all the possible combinations of its components. Now as mathematicians, you will recognize this figure as a half matrix, and you will also see that, in relation to the graph of the First Figure, it is an adjacency matrix. Because such a matrix is symmetrical (in Llull's case this means he makes no distinction between B-C and C-B), he saw no reason to reproduce the other half; and because his graph admits no loops (that is, omits relations such as B-B), he could also omit the principal diagonal.

"If the Third Figure explores all possible binary combinations, the Fourth Figure (f. iiiiv) does the same for ternary combinations. In medieval manuscripts, the outside circle is normally drawn on the page, and the two inner ones are separate pieces of parchment or paper held in place on top of it by a little piece of string, permitting them to rotate in relation to each other and to the larger circle" (Bonner, pp. 9-14). In the present printed version of the work, the parts of the volvelle illustrating the ternary combinations were printed on a separate sheet and intended to be cut out and mounted on the Fourth Figure. In this copy the volvelle has never been assembled; the sheet on which the parts of the volvelle are printed is bound at the end of the book.

"Binary relations are worked out more extensively in a section he calls 'The Evacuation of the Third Figure'. For the 'compartment', as he calls it, of B C, he not only uses 'goodness' and 'greatness' from the First Figure, and 'difference' and 'concordance' from the Second Figure, but also the first two questions of the third column of the alphabet, those also corresponding to the letters B C, which are 'whether?' and 'what?'. This means that for the combination of 'goodness' and 'greatness' one has three possibilities, a statement and two questions:

Goodness is great.
Whether goodness is great?
What is great goodness?

and so on for 'goodness' and 'difference', 'goodness' and 'concordance', for a total of 12 propositions and 24 questions.

"Ternary relations are worked out in a Table based on the Fourth Figure … the full form of the Ars generalis ultima has 84 [columns]!" (Bonner, p. 14). These ternary relations are listed in a twelve-page table (ff. viiiv – xiiir), and then a short interpretation is given for each entry.

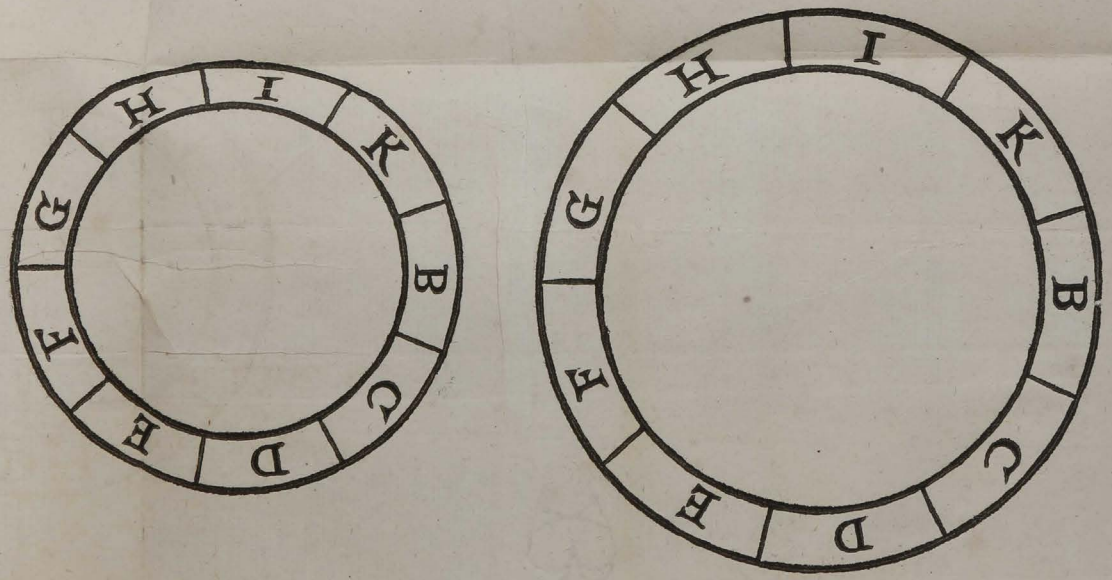
Llull's ideas would be developed further by Giovanni de la Fontana (1395-1455) and Nicholas of Cusa (1401-64) in the 15th century (in his work De conjecturis Nicholas developed his method ars generalis conjecturandi, in which he describes a way of making conjectures, illustrated by wheel charts and symbols that much resemble those of Llull), and Giordano Bruno (1548-1600) in the 16th century (Bruno used the rotating figures of the Lullist system as instruments of a system of artificial memory, and attempted to apply Lullian mnemotechnics to different modes of rhetorical discourse).

Ramon Llull (1232/3–1315/6) "was born on Majorca around 1232, only two or three years after the King of Aragon and Catalonia had recovered the island from the Muslims. This meant that Llull grew up in an island that was still strongly multicultural. Muslims continued to represent perhaps a third of the population, and Jews, although a much smaller minority, were an important economic and cultural force on the island. So when at the age of thirty he was converted from a profligate youth and he decided to devote his life to the service of the Church, it seemed only logical to do so by trying to convert these 'infidels', as they were then called. And he decided to do this in three ways: (1) to develop a system that his adversaries would find difficult to refute (which is what we'll see in a moment), and to try to persuade them of the truth of Christianity instead of just trying to refute their own doctrines, as his predecessors had done; (2) to be willing to risk his life in proselytizing among Muslims and Jews (he in fact made three trips to North Africa); and (3) to try to persuade Kings and Popes of the need for setting up language schools for missionaries, for which purpose he travelled many times throughout France and Italy. He lived to 83 or 84, an incredible age when the average life-span was around 40, dying in 1316" (Bonner, p. 5).

The editor of this edition, the Franciscan Bernard de Lavinheta (d. c. 1530), was the greatest Lullist of the early 16th century. "Almost nothing is known of [his] background, nor even whether he was Spanish or French. We only know that before coming to Paris he taught at Salamanca. The brand of Lullism he brought there was that of the Lullist school of Barcelona and its interest in the Art. He was the first, as a trained theologian, to teach the Art at the University of Paris, thereby giving it the official sanction it had lacked for a century and a half. His publication of Lullian works at Lyon, Paris and Cologne in 1514-18 was very influential throughout Europe" (Bonner, Selected Works of Ramon Llull, vol. 1, p. 80).

Palau 143693; Rogent & Duran 65; Tomash L142. Alexander Fidora & Carles Sierra (eds.), Ramon Llull: From the Ars Magna to Artificial Intelligence, 2011 (http://www.iiia.csic.es/library). Anthony Bonner, 'What was Llull up to?', pp. 5-24 in Fidora & Sierra (ibid.). John N. Crossley, 'Ramon Llull's contribution to computer science,' pp. 39-60 in ibid.

¶ Aduertendum est q̃ minor circulus istorum poni debet intra maiorem circulum volubiliter: sicut rota in axe: et ābo ponuntur intra quartam figuram: etiam volubiliter: in folio quarto in secunda parte.



b c d t	b c e t	b c f t	b c g t	b c h t	b c i t	b c k t
b c t b	b c t b	b c t b	b c t b	b c t b	b c t b	b c t b
b c t c	b c t c	b c t c	b c t c	b c t c	b c t c	b c t c
b c t d	b c t e	b c t f	b c t g	b c t h	b c t i	b c t k
b d t b	b e t b	b f t b	b g t b	b h t b	b e t b	b k t b
b d t c	b e t c	b f t c	b g t c	b h t c	b i t c	b k t c
b d t d	b e t e	b f t f	b g t g	b h t h	b i t i	b k t k
b t b c	b t b c	b t b c	b t b c	b t b c	b t b c	b t b c
b t b d	b t b e	b t b f	b t b g	b t b h	b t b i	b t b k

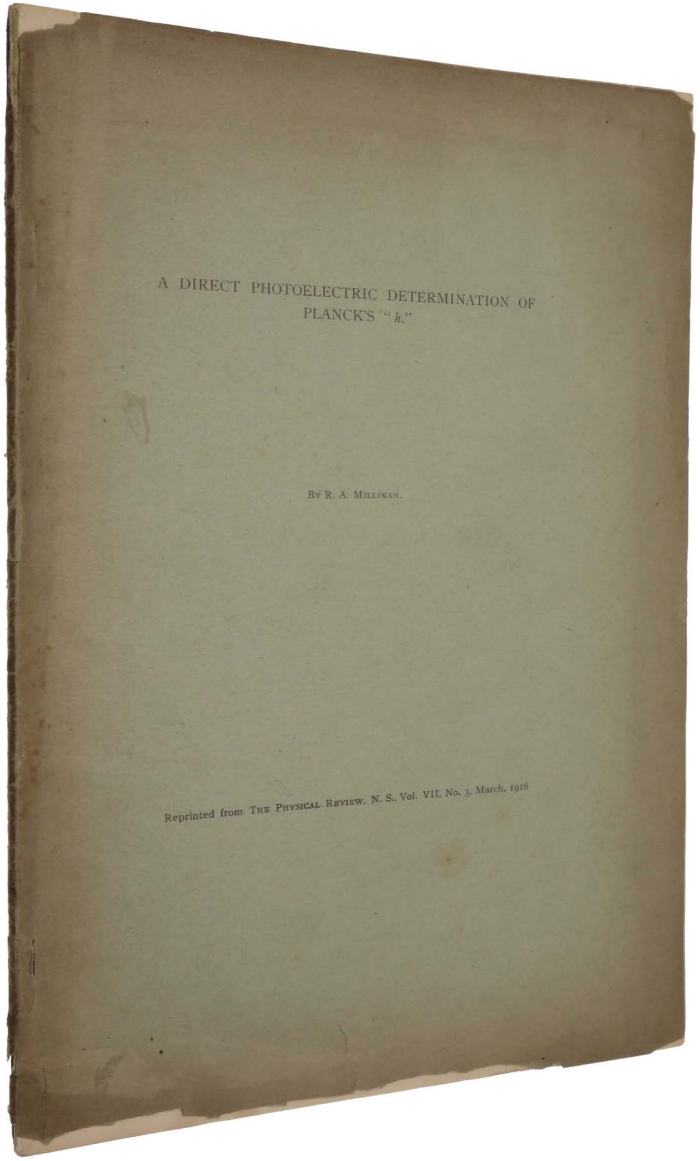
EXPERIMENTAL CONFIRMATION OF EINSTEIN'S QUANTUM HYPOTHESIS

MILLIKAN, Robert Andrews. *A Direct Photoelectric Determination of Planck's "h".* [Lancaster: American Physical Society], 1916.

$9,500

Offprint from: The Physical Review, vol. 7, no. 3, 3 March, 1916, 8vo (255 x 182 mm), pp. 355-388, original green printed wrappers (extremeties with some chipping, spine strip with wear, margins with light smudging), in all a very fine copy of the rare offprint issue in its original state.

First edition, rare offprint issue, of Millikan's dramatic experimental confirmation of Einstein's quantum theory of the photoelectric effect, for which Einstein was awarded the Nobel Prize in Physics 1921. The Nobel Prize in Physics 1923 was awarded to Robert A. Millikan "for his work on the elementary charge of electricity and on the photoelectric effect". "While it had been known for a long time that light falling on metal surfaces may eject electrons from them (the photoelectric effect), Millikan was the first to determine with great accuracy that the maximum kinetic energy of the ejected electrons obey the equation Einstein had proposed in 1905: namely, ½mv2 = hf – P, where h is Planck's constant, f the frequency of the incident light, and P is, in Millikan's words, "the work necessary to get the electron out of the metal." Millikan determined h to have the value 6.57x10−27 erg-sec to "a precision of about 0.5 per cent," a value far better than had been obtained in any previous attempt" (Gerald Holton, APS Focus, April 22, 1999). Ironically, Millikan did not believe in the hypothesis that was the basis of Einstein's equation, namely, the photon theory of light, in which electromagnetic radiation came in



corpuscles or quanta. "This hypothesis may well be called reckless first because an electromagnetic disturbance which remains localized in space seems a violation of the very conception of an electromagnetic disturbance, and second because it flies in the face of the thoroughly established facts of interference … We are confronted, however, by the astonishing situation that these facts [the observations on the photoelectric effect] were correctly and exactly predicted nine years ago by a form of quantum theory [the photon hypothesis] which has now pretty generally been abandoned (p. 355)." Only later did Millikan come to accept the quantum hypothesis. OCLC list one copy (Princeton). No copies in auction records.

"About 1912, now aware of Einstein's interpretation of [the photoelectric effect, Millikan] began an intensive experimental study of the phenomenon, with the aim of testing the formula relating the frequency of the incident light to the retarding potential which cut off the photocurrent. No experimentalist had yet succeeded in proving or disproving the validity of the equation. Millikan took great care to avoid the mistakes that he and other physicists had previously made. Since a spark source of ultraviolet light induced spurious voltages in the apparatus, he used a high-pressure mercury-quartz lamp arranged to suppress stray light, especially on the short wavelength side. To extend the range of test well into the visible region, he made targets of alkali metals which were photosensitive up to 6,000 Å. Where others had adulterated their results by using photosensitive materials as the reference for the cut-off voltage, Millikan employed a Faraday cage of well-oxidized copper netting which was not photosensitive in the range of his incident radiation. Finally, he sought to reduce the inaccuracies introduced when the photocurrent near the cut-off point was too low to measure with precision. Having noticed that this current was highest when the metal was fresh, he fashioned his targets into thick cylinders and rigged up an electromagnetically operated knife to shave off the ends of the blocks.

"By 1915, as the result of these meticulous investigations, Millikan had confirmed the validity of Einstein's equation in every detail. He not only demonstrated the linear relationship between the cut-off potential and the frequency of the incident light but also showed that the intercept of the graphed data on the voltage axis equaled the contact electromotive force, or work potential, of the target metal, a quantity which he had measured independently, to within 0.5 percent. In addition Millikan proved that the slope of the line equaled the ratio of Planck's constant to the electronic charge, and his work provided the best measure of h then available. Despite the conclusiveness of these results, Millikan did not believe that he had confirmed Einstein's theory of light quanta but only his equation for the photoelectric effect. In the face of all the evidence for the wave nature of light, he was convinced, as were most other physicists of the day, that the equation had to be based on a false, albeit evidently quite fruitful, hypothesis" (DSB).

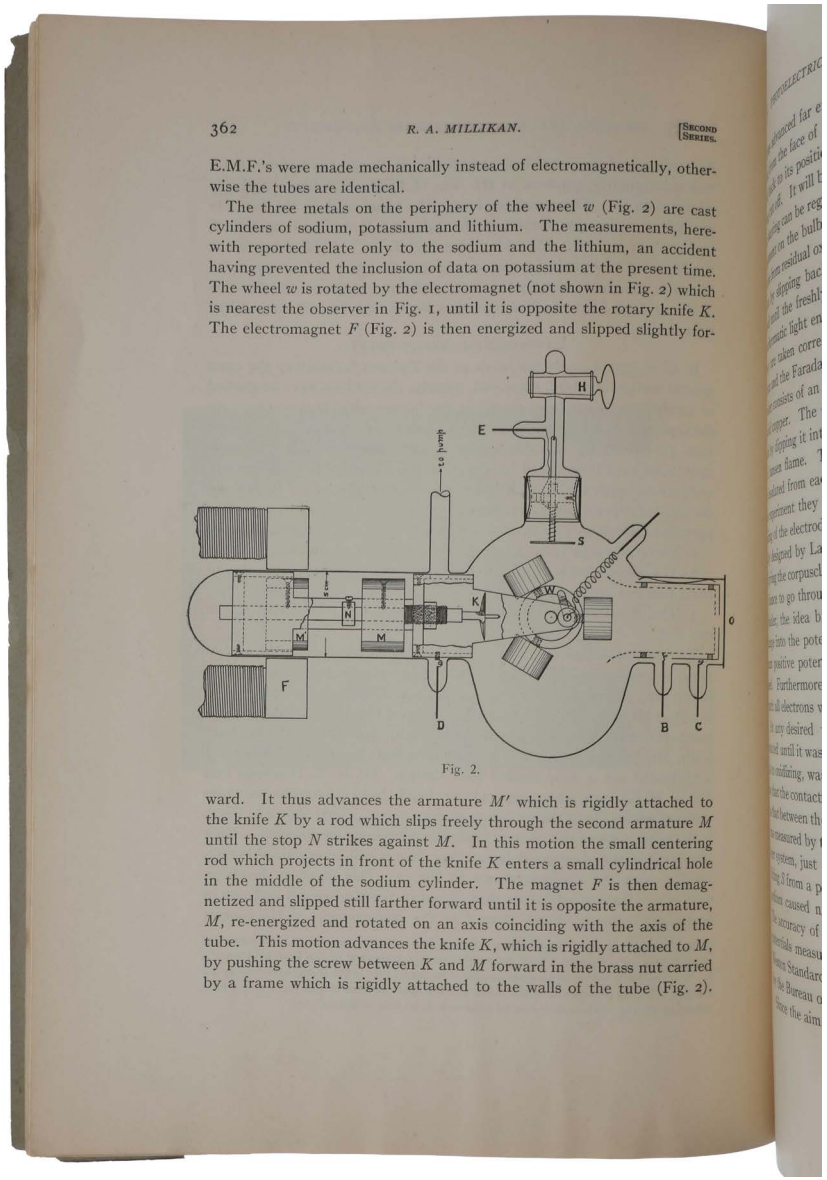
"Millikan's success was above all attributable to an ingenious device he termed "a machine shop in vacuo." A rotating sharp knife, controlled from outside the evacuated glass container by electromagnetic means, would clean off the surface of the metal used before exposing it to the beam of monochromatic light. The kinetic energy of the photoelectrons were found by measuring the potential energy of the electric field needed to stop them–here Millikan was able to confidently use the uniquely accurate value for the charge e of the electron he had established with his oil drop experiment in 1913.

"Shining through it all are Millikan's typical characteristics as experimenter and person: his penchant for experimenting in an area involving the hottest question of the day, his energetic persistence (this paper was the culmination of work he had begun in 1905), and his passion for obtaining results of great precision. In short, Millikan's experiment was a triumphant work, of highest importance in its day, and richly deserving to be cited as part of his Nobel Prize award in 1923, given "for his work on the elementary charge of electricity and the photoelectric effect."

"To the historian, the volume in which Millikan's paper appeared shows that physics in America was still a mixed bag. Other papers show that the main attention at that time is the experimental part of science, in which Americans were long regarded as most interested and most competent. But the volume as a whole indicates that a good deal of the work going on in physics in this country in the early years of this century was still narrow and unambitious, even tending, for example, to descend to lengthy descriptions of improvements in basic equipment.

"In an earlier paper (January 1916) in the same volume, Millikan writes in the very first sentence that "Einstein's photoelectric equation…cannot in my judgment be looked upon at present as resting upon any sort of a satisfactory theoretical foundation," even though "it actually represents very accurately the behavior" of photoelectricity. Indeed, Millikan's paper on Planck's constant shows clearly that he is emphatically distancing himself throughout from Einstein's 1905 attempt to couple photo effects with a form of quantum theory. What we now call the photon was, in Millikan's view, "[the] bold, not to say the reckless, hypothesis"– reckless because it was contrary to such classical concepts as light being a wave propagation phenomenon. So Millikan's paper is not at all, as we would now expect, an experimental proof of the quantum theory of light.

"In 1912 Millikan gave a lecture at the Cleveland meeting of the American Association for the Advancement of Science, meeting jointly with the American Physical Society, in which he clearly regarded himself as the proper presenter of Planck's theory of radiation. With his usual self-confidence, Millikan confessed that a corpuscular theory of light was for him "quite unthinkable," unreconcilable, as he saw it, with the phenomena of diffraction and interference. In short, Millikan's classic 1916 paper was purely intended to be the verification of Einstein's equation for the photoelectric effect and the determination of h, without accepting any of



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E.M.F.'s were made mechanically instead of electromagnetically, otherwise the tubes are identical.

The three metals on the periphery of the wheel w (Fig. 2) are cast cylinders of sodium, potassium and lithium. The measurements, herewith reported relate only to the sodium and the lithium, an accident having prevented the inclusion of data on potassium at the present time. The wheel w is rotated by the electromagnet (not shown in Fig. 2) which is nearest the observer in Fig. 1, until it is opposite the rotary knife K. The electromagnet F (Fig. 2) is then energized and slipped slightly forward. It thus advances the armature M' which is rigidly attached to the knife K by a rod which slips freely through the second armature M until the stop N strikes against M. In this motion the small centering rod which projects in front of the knife K enters a small cylindrical hole in the middle of the sodium cylinder. The magnet F is then demagnetized and slipped still farther forward until it is opposite the armature, M, re-energized and rotated on an axis coinciding with the axis of the tube. This motion advances the knife K, which is rigidly attached to M, by pushing the screw between K and M forward in the brass nut carried by a frame which is rigidly attached to the walls of the tube (Fig. 2).

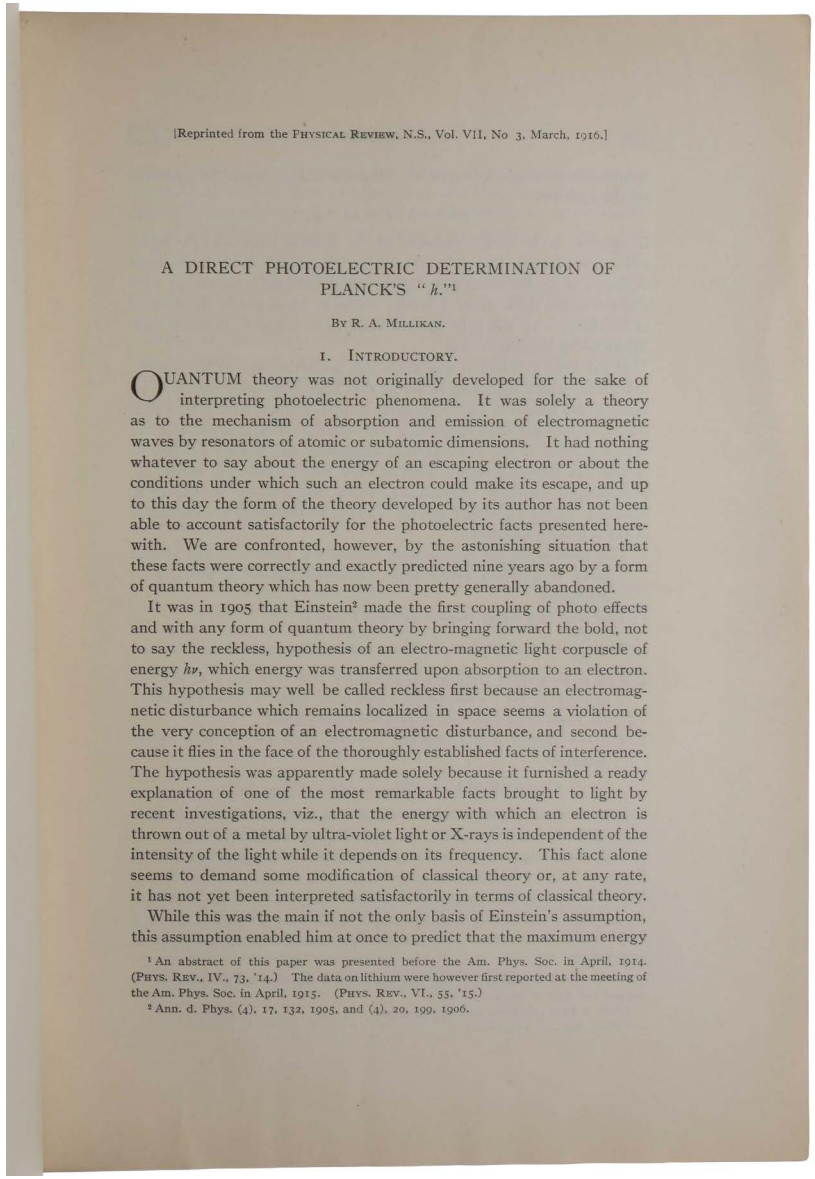
Fig. 2.

the "radical" implications which today seem so natural.
"When Millikan's Nobel Prize came to pass, his Nobel address contained passages that showed his continuing struggle with the meaning of his own achievement: "This work resulted, contrary to my own expectation, in the first direct experimental proof…of the Einstein equation and the first direct photo-electric determination of Planck's h."

"Yet it is difficult to find any published basis in Millikan's experimental papers of that struggle with his own expectations. His internal conflict was of a somewhat different sort; while Millikan conceded that Einstein's photoelectric equation was "experimentally established…the conception of localized light-quanta out of which Einstein got his equation must still be regarded as far from being established." Ironically, it had been Millikan's experiment which convinced the experimentalist-inclined committee in Stockholm to admit Einstein to that select circle in 1922.

"One final irony: In 1950, at age 82, Millikan published his Autobiography, with Chapter 9 entitled simply "The Experimental Proof of the Existence of the Photon–Einstein's Photoelectric Equation." By then, Millikan had of course come to terms with the photon. Moreover, he had evidently changed his mind about what he had done around 1916, for now he wrote that as the experimental data became clear in his lab, they "proved simply and irrefutably, I thought, that the emitted electron that escapes with the energy hf gets that energy by the direct transfer of hf units of energy from the light to the electron, and hence scarcely permits of any other interpretation than that which Einstein had originally suggested, namely that of the semi-corpuscular or photon theory of light itself."

"In the end, Millikan re-imagined the complex personal history of his splendid experiment to fit the simple story told in so many of our physics textbooks" (Holton).



[Reprinted from the PHYSICAL REVIEW, N.S., Vol. VII, No 3, March, 1916.]

A DIRECT PHOTOELECTRIC DETERMINATION OF PLANCK'S "*h*."[1]

BY R. A. MILLIKAN.

1. INTRODUCTORY.

QUANTUM theory was not originally developed for the sake of interpreting photoelectric phenomena. It was solely a theory as to the mechanism of absorption and emission of electromagnetic waves by resonators of atomic or subatomic dimensions. It had nothing whatever to say about the energy of an escaping electron or about the conditions under which such an electron could make its escape, and up to this day the form of the theory developed by its author has not been able to account satisfactorily for the photoelectric facts presented herewith. We are confronted, however, by the astonishing situation that these facts were correctly and exactly predicted nine years ago by a form of quantum theory which has now been pretty generally abandoned.

It was in 1905 that Einstein[2] made the first coupling of photo effects and with any form of quantum theory by bringing forward the bold, not to say the reckless, hypothesis of an electro-magnetic light corpuscle of energy $h\nu$, which energy was transferred upon absorption to an electron. This hypothesis may well be called reckless first because an electromagnetic disturbance which remains localized in space seems a violation of the very conception of an electromagnetic disturbance, and second because it flies in the face of the thoroughly established facts of interference. The hypothesis was apparently made solely because it furnished a ready explanation of one of the most remarkable facts brought to light by recent investigations, viz., that the energy with which an electron is thrown out of a metal by ultra-violet light or X-rays is independent of the intensity of the light while it depends on its frequency. This fact alone seems to demand some modification of classical theory or, at any rate, it has not yet been interpreted satisfactorily in terms of classical theory.

While this was the main if not the only basis of Einstein's assumption, this assumption enabled him at once to predict that the maximum energy

[1] An abstract of this paper was presented before the Am. Phys. Soc. in April, 1914. (PHYS. REV., IV., 73, '14.) The data on lithium were however first reported at the meeting of the Am. Phys. Soc. in April, 1915. (PHYS. REV., VI., 55, '15.)

[2] Ann. d. Phys. (4), 17, 132, 1905, and (4), 20, 199, 1906.

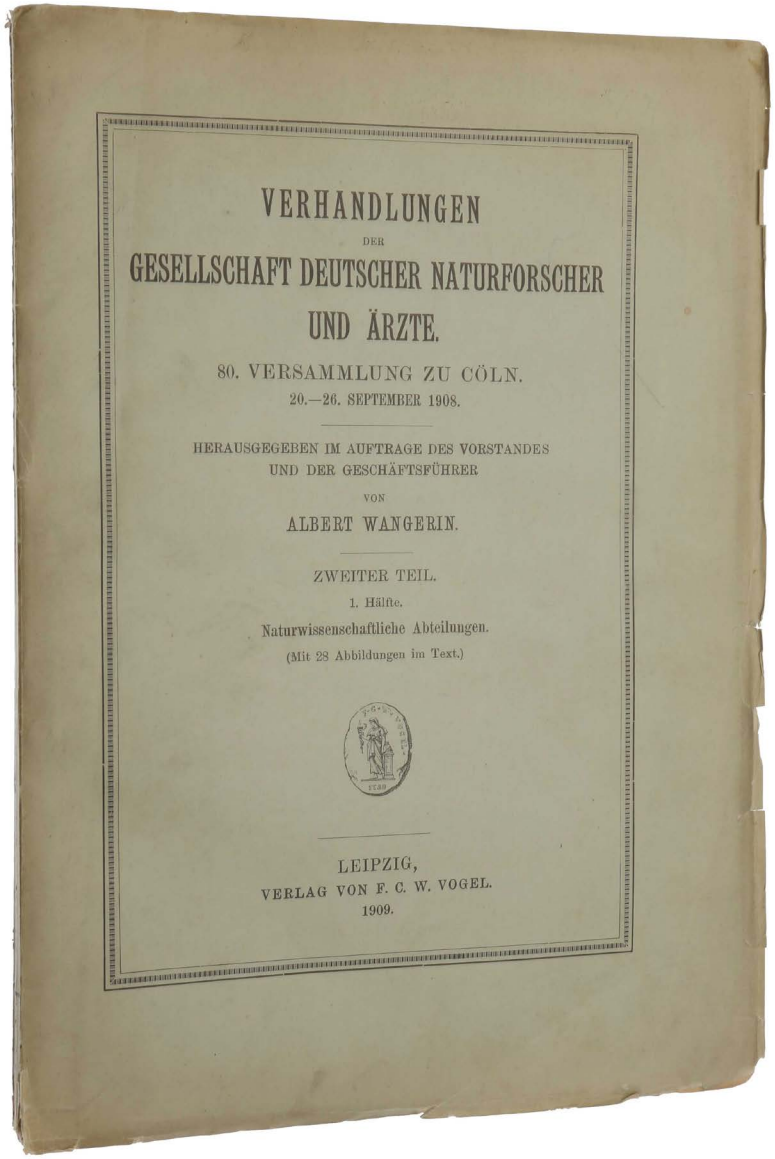
FOUR-DIMENSIONAL SPACE-TIME DIAGRAMS

MINKOWSKI, Hermann. *Raum und Zeit.* Leipzig: F.W. Vogel, 1909.

$2,800

Pp. 4-9 in Verhandlungen der Gesellschaft Deutscher Naturforscher und Ärzte. 80 Versammlung zu Cöln. 20. – 28. September 1908. Zweiter Theil, 1. Häfte. Large 8vo (255 x 180 mm), pp. x, 218. Original printed wrappers, uncut, spine strip and corners with light wear. A fine copy. Custom cloth box.

First edition of Minkowski's famous lecture on his theory of four-dimensional space-time, the first published account of this theory to be illustrated with 'spacetime diagrams', which were widely used later by Stephen Hawking, Roger Penrose and others in the development of general relativity. "In 'Space and Time', read by Minkowski in Cologne only a few months before his death, he introduced the notion that made possible the expansion of the relativity theory of Einstein from its specific (1905) to its general form (1916). Minkowski's space-time hypothesis was in effect a restatement of Einstein's basic principle in a form that greatly enhanced its plausibility and also introduced important new developments. Hitherto natural phenomena had been thought to occur in a space of three dimensions and to flow uniformly through time. Minkowski maintained that the separation of time and space is a false conception; that time itself is a dimension, comparable to length, breadth, and height; and that therefore the true conception of reality was constituted by a space-time continuum possessing these four dimensions" (Printing and the Mind of Man). "The laws of physics were written in the language of the geometry of this four dimensional space ... Physics



VERHANDLUNGEN
DER
GESELLSCHAFT DEUTSCHER NATURFORSCHER
UND ÄRZTE.

80. VERSAMMLUNG ZU CÖLN.
20.—26. SEPTEMBER 1908.

HERAUSGEGEBEN IM AUFTRAGE DES VORSTANDES
UND DER GESCHÄFTSFÜHRER
VON
ALBERT WANGERIN.

ZWEITER TEIL.
1. Hälfte.
Naturwissenschaftliche Abteilungen.
(Mit 28 Abbildungen im Text.)

LEIPZIG,
VERLAG VON F. C. W. VOGEL.
1909.

had become geometry, and it was a stunning achievement" (Gerber, pp. 358-9). "'Raum und Zeit' was originally published in Vol. II (1909) of the Verhandlungen of the Deutscher Naturforscher und Aertze [offered here]; it was later reprinted in the Jahresbericht der Deutschen Mathematiker Vereinigung" (Norman). It is most commonly encountered in the separate (and later) printing from the Jahresbericht published as a tribute after Minkowski's sudden and tragic death. An outstanding copy of the first printing of this landmark lecture, rare in the original printed wrappers.

Barchas 1440 (Verhandlungen journal issue); Honeyman 2231 & Norman 1514 (both listing separate printing from Jahresbericht); PMM 401 (Jahresbericht journal issue). Einstein, 'Autobiographical notes,' in Albert Einstein: Philosopher-Scientist. Paul A. Schilpp ed. (1969), pp. 1-94. Gerber, The Language of Physics: The Calculus and the Development of Theoretical Physics in Europe, 1750-1914, 1988; Gray, The Symbolic Universe: Geometry and Physics 1890-1930, 1999; Petkov, Space, Time, and Spacetime. Physical and Philosophical Implications of Minkowski's Unification of Space and Time, 2010; Walter, 'The Historical Origins of Spacetime,' pp. 27-38 in Ashtekar & Petkov (eds.), The Springer Handbook of Spacetime, 2014.

"Minkowski's … 'Space and Time' lecture given in Cologne in 1908, began with these words: 'The views of space and time which I wish to lay before you have sprung from the soil of experimental physics, and therein lies their strength. They are radical. Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality.' He ended as follows: 'The validity without exception of the world postulate [i.e., the relativity postulate], I like to think, is the true nucleus of an electromagnetic image of the world, which, discovered by Lorentz, and further revealed by Einstein, now lies open in the full light of day'. It is hardly surprising that these opening and closing statements caused a tremendous stir among his listeners, though probably few of them followed the lucid remarks he made in the body of the speech. Minkowski did not live to see his lecture appear in print. In January 1909 he died of appendicitis. [David] Hilbert called him 'a gift of heaven' when he spoke in his memory" (Pais, p. 152).

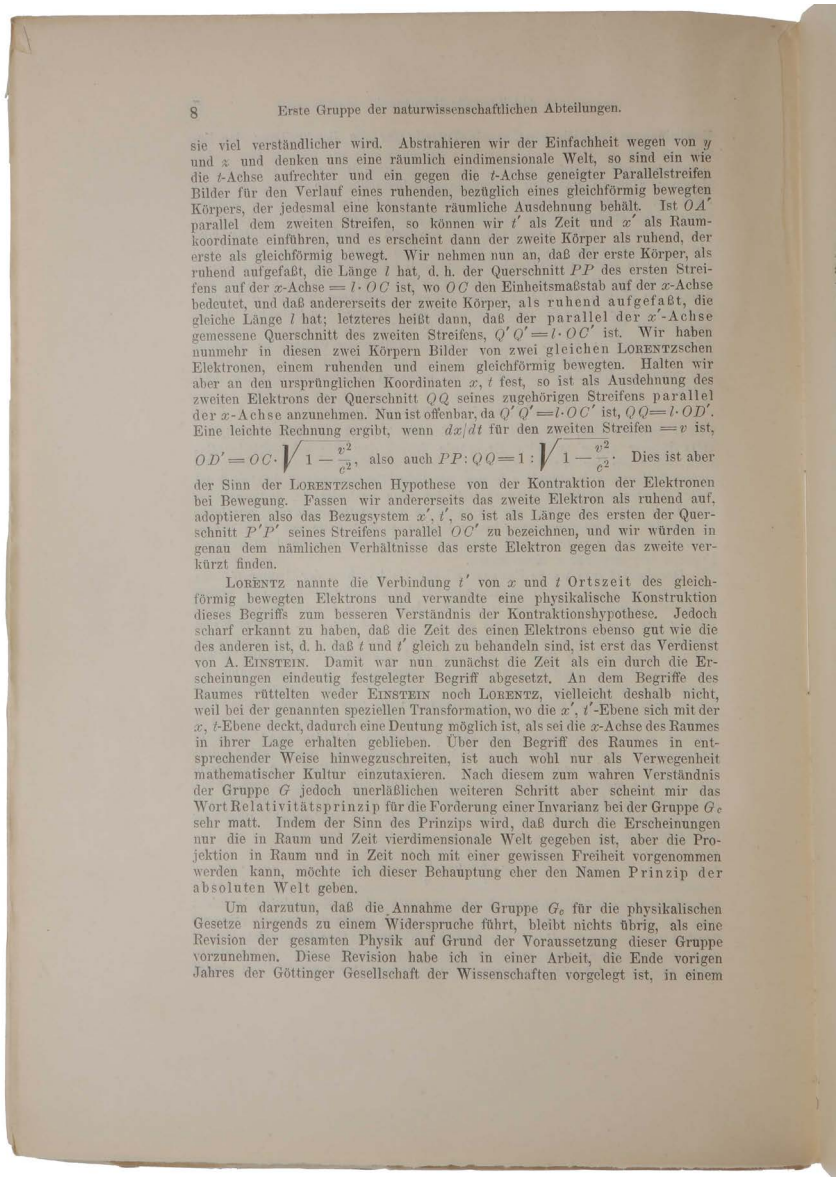
"The impact of Minkowski's ideas on the twentieth century physics has been so immense that one cannot imagine modern physics without the notion of spacetime. It would hardly be an exaggeration to say that spacetime has been the greatest discovery in physics of all times. The only other discovery that comes close to spacetime is Einstein's general relativity, which revealed that gravity is a manifestation of the curvature of spacetime. But it was the discovery of spacetime which paved the way for this deep understanding of what gravity really is" (Petkov, p. v).

Initially, Einstein was not impressed by Minkowski's four-dimensional formulation of special relativity, describing it as 'überflüssige Gelehrsamkeit' (superfluous learnedness), but he soon realized that his theory of gravity would be impossible without Minkowski's revolutionary contributions. At the beginning of his 1916 paper on general relativity Einstein wrote: "The generalization of the theory of relativity has been facilitated considerably by Minkowski, a mathematician who was the first one to recognize the formal equivalence of space coordinates and the time coordinate, and utilize this in the construction of the theory." In 1946 in his 'Autobiographical Notes' (p. 59), Einstein summarized Minkowski's main contribution: "Minkowski's important contribution to the theory lies in the following: Before Minkowski's investigation it was necessary to carry out a Lorentz transformation on a law in order to test its invariance under such transformations; he, on the other hand, succeeded in introducing a formalism such that the mathematical form of the law itself guarantees its invariance under Lorentz transformations. By creating a four-dimensional tensor-calculus

he achieved the same thing for the four-dimensional space which the ordinary vector calculus achieves for the three spatial dimensions. He also showed that the Lorentz transformation (apart from a different algebraic sign due to the special character of time) is nothing but a rotation of the coordinate system in the four-dimensional space."

Hermann Minkowski was born on 22 June 1864, in Alexotas, then in Russia, now the city of Kaunas in Lithuania. His parents were German and, from Alexotas, they moved back to their native land, in fact to Königsberg (now Kaliningrad in Russia), when he was only 8 years old. After attending the Gymnasium in Königsberg he attended the University there from which he was to receive his doctorate in 1885. As a young student, in 1883, he won the Grand Prix of the Academy of Sciences in Paris for his work on number theory. From Königsberg, he moved to a position at the University of Bonn in 1887. In 1896, after a brief return to Königsberg, he was appointed to a position at the Polytechnic in Zürich where he was one of Einstein's teachers. Einstein apparently thought Minkowski an excellent teacher of mathematics. Minkowski's view of Einstein, at the time, was less kind. In 1902 he moved again, this time to a chair at the University of Göttingen, where he remained for the remainder of his relatively short life, dying in 1909 at the age of only 44. In Göttingen, under the influence of David Hilbert, he became interested in mathematical physics.

In October 1907 Hilbert and Minkowski conducted a joint seminar at Göttingen on the equations of electrodynamics in which they studied Einstein's 1905 paper 'Zur Elektrodynamik bewegter Körper' (Annalen der Physik 17, pp. 891-921), and Poincaré's 1906 article 'Sur la dynamique de l'électron' (Rendiconti del Circolo Matematico di Palermo 21, pp. 129-76). Participants in the seminar included Max von Laue, Max Born, Max Abraham and Arnold Sommerfeld. On November 5 Minkowski delivered a talk to the Göttingen Mathematical Society entitled 'The



8 Erste Gruppe der naturwissenschaftlichen Abteilungen.

sie viel verständlicher wird. Abstrahieren wir der Einfachheit wegen von y und z und denken uns eine räumlich eindimensionale Welt, so sind ein wie die t-Achse aufrechter und ein gegen die t-Achse geneigter Parallelstreifen Bilder für den Verlauf eines ruhenden, bezüglich eines gleichförmig bewegten Körpers, der jedesmal eine konstante räumliche Ausdehnung behält. Ist OA' parallel dem zweiten Streifen, so können wir t' als Zeit und x' als Raumkoordinate einführen, und es erscheint dann der zweite Körper als ruhend, der erste als gleichförmig bewegt. Wir nehmen nun an, daß der erste Körper, als ruhend aufgefaßt, die Länge l hat, d. h. der Querschnitt PP des ersten Streifens auf der x-Achse $= l \cdot OC$ ist, wo OC den Einheitsmaßstab auf der x-Achse bedeutet, und daß andererseits der zweite Körper, als ruhend aufgefaßt, die gleiche Länge l hat; letzteres heißt dann, daß der parallel der x'-Achse gemessene Querschnitt des zweiten Streifens, $Q'Q' = l \cdot OC'$ ist. Wir haben nunmehr in diesen zwei Körpern Bilder von zwei gleichen LORENTZschen Elektronen, einem ruhenden und einem gleichförmig bewegten. Halten wir aber an den ursprünglichen Koordinaten x, t fest, so ist als Ausdehnung des zweiten Elektrons der Querschnitt QQ seines zugehörigen Streifens parallel der x-Achse anzunehmen. Nun ist offenbar, da $Q'Q' = l \cdot OC'$ ist, $QQ = l \cdot OD'$. Eine leichte Rechnung ergibt, wenn dx/dt für den zweiten Streifen $= v$ ist, $OD' = OC \cdot \sqrt{1 - \frac{v^2}{c^2}}$, also auch $PP : QQ = 1 : \sqrt{1 - \frac{v^2}{c^2}}$. Dies ist aber der Sinn der LORENTZschen Hypothese von der Kontraktion der Elektronen bei Bewegung. Fassen wir andererseits das zweite Elektron als ruhend auf, adoptieren also das Bezugsystem x', t', so ist als Länge des ersten der Querschnitt $P'P'$ seines Streifens parallel OC' zu bezeichnen, und wir würden in genau dem nämlichen Verhältnisse das erste Elektron gegen das zweite verkürzt finden.

LORENTZ nannte die Verbindung t' von x und t Ortszeit des gleichförmig bewegten Elektrons und verwandte eine physikalische Konstruktion dieses Begriffs zum besseren Verständnis der Kontraktionshypothese. Jedoch scharf erkannt zu haben, daß die Zeit des einen Elektrons ebenso gut wie die des anderen ist, d. h. daß t und t' gleich zu behandeln sind, ist erst das Verdienst von A. EINSTEIN. Damit war nun zunächst die Zeit als ein durch die Erscheinungen eindeutig festgelegter Begriff abgesetzt. An dem Begriffe des Raumes rüttelten weder EINSTEIN noch LORENTZ, vielleicht deshalb nicht, weil bei der genannten speziellen Transformation, wo die x', t'-Ebene sich mit der x, t-Ebene deckt, dadurch eine Deutung möglich ist, als sei die x-Achse des Raumes in ihrer Lage erhalten geblieben. Über den Begriff des Raumes in entsprechender Weise hinwegzuschreiten, ist auch wohl nur als Verwegenheit mathematischer Kultur einzutaxieren. Nach diesem zum wahren Verständnis der Gruppe G jedoch unerläßlichen weiteren Schritt aber scheint mir das Wort Relativitätsprinzip für die Forderung einer Invarianz bei der Gruppe G_c sehr matt. Indem der Sinn des Prinzips wird, daß durch die Erscheinungen nur die in Raum und Zeit vierdimensionale Welt gegeben ist, aber die Projektion in Raum und in Zeit noch mit einer gewissen Freiheit vorgenommen werden kann, möchte ich dieser Behauptung eher den Namen Prinzip der absoluten Welt geben.

Um darzutun, daß die Annahme der Gruppe G_c für die physikalischen Gesetze nirgends zu einem Widerspruche führt, bleibt nichts übrig, als eine Revision der gesamten Physik auf Grund der Voraussetzung dieser Gruppe vorzunehmen. Diese Revision habe ich in einer Arbeit, die Ende vorigen Jahres der Göttinger Gesellschaft der Wissenschaften vorgelegt ist, in einem

Principle of Relativity'; it was not published until 1915 ('Das Relativitätsprinzip,' Annalen der Physik 47, pp. 927-38). Minkowski opened his talk by declaring that recent developments in the electromagnetic theory of light had given rise to a completely new conception of space and time, namely, as a four-dimensional, non-Euclidean manifold. He introduced many of the mathematical concepts and terms that have come to be associated with his name and that became standard in any discussion of relativity, but he did not treat them systematically at this stage.

Minkowski's second talk, "The Basic Equations of Electromagnetic Processes in Moving Bodies", was delivered at the meeting of the Göttingen Scientific Society on December 21, 1907, and published in April 1908. "It presented a new theory of the electrodynamics of moving media, incorporating formal insights of the relativity theories introduced earlier by Einstein, Poincaré and Planck. For example, it took over the fact that the Lorentz transformations form a group, and that Maxwell's equations are covariant under this group. Minkowski also shared Poincaré's view of the Lorentz transformation as a rotation in a four-dimensional space with one imaginary coordinate. These insights Minkowski developed and presented in an original, four-dimensional approach to the Maxwell-Lorentz vacuum equations, the electrodynamics of moving media, and in an appendix, Lorentz-covariant mechanics" (Gray, p. 102).

""The basic equations" made for challenging reading. It was packed with new notation, terminology, and calculation rules, it made scant reference to the scientific literature, and offered no figures or diagrams … Few were impressed at first by Minkowski's innovations in spacetime geometry and four-dimensional vector calculus. Shortly after "The basic equations" appeared in print, two of Minkowski's former students, Einstein and Laub, discovered what they believed to be an infelicity in Minkowski's definition of ponderomotive force density. These two young physicists were more impressed by Minkowski's electrodynamics of moving media than by the novel four-dimensional formalism in which it was couched, which seemed far too laborious. Ostensibly as a service to the community, Einstein and [Jakob] Laub re-expressed Minkowski's theory in terms of ordinary vector analysis ['Über die elektromagnetischen Grundgleichungen für bewegte Körper', Annalen der Physik 26, 1908, pp. 532-540] …

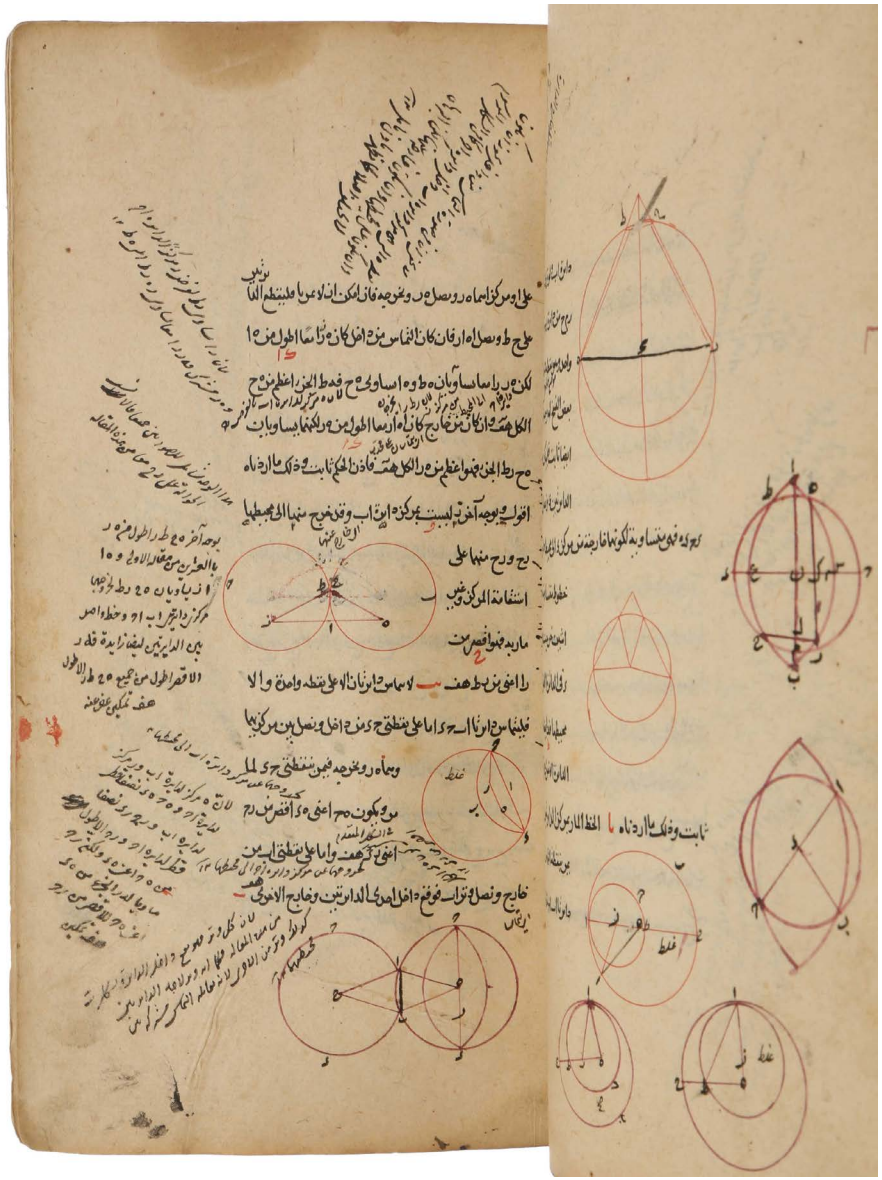
"The form Minkowski gave to his theory of moving media in "The basic equations" had been judged unwieldy by a founder of relativity theory, and in the circumstances, decisive action was called for if his formalism was not to be ignored. In September 1908, during the annual meeting of the German Association of Natural Scientists and Physicians in Cologne, Minkowski took action, by affirming the reality of the four-dimensional "world", and its necessity for physics … One way for Minkowski to persuade physicists of the value of his spacetime approach to understanding physical interactions was to appeal to their visual intuition. From the standpoint of visual aids, the contrast between Minkowski's two publications on spacetime is remarkable: where "The basic equations" is bereft of diagrams and illustrations, Minkowski's Cologne lecture makes effective use of diagrams in two and three dimensions. For instance, Minkowski employed a two-dimensional spacetime diagrams to illustrate FitzGerald-Lorentz contraction of an electron, and the lightcone structure of spacetime" (Walter, pp. 14-17).

ARABIC EUCLID MANUSCRIPT 70 YEARS BEFORE THE FIRST PRINTED EDITION

NAṢĪR AL-DĪN AL-TŪSĪ. *Kitāb tahrīr ʻuṣūl al-handasa li-ʻŪqlīdus, an exposition of Euclid's ʻElements of Geometry,' signed by ʻIzz al-Din Ahmad.* Persia, Safavid: dated 929 AH/1522-23 AD.

$38,500

A fine early sixteenth-century manuscript of al-Tūsī's recension of Euclid's Elements, written more than seven decades before the first published Arabic edition of Euclid (Rome, 1594), which was a printing of another manuscript of al-Tūsī's recension. This, together with his commentary on Ptolemy's Almagest, is al-Tūsī's most important work. The first printed edition of Euclid (Venice, 1482) was a Latin translation by Campanus of Novara (1220-96) based upon al-Tūsī's version. Probably written in 1248, the earliest extant manuscript of al-Tūsī's version of Euclid is dated 1258 (held by the British Library – see Stocks & Baker, p. 374). Naṣīr al-Dīn Abū Ja'far Muhammad ibn Muhammad ibn al-hasan al-Tūsī (1201-74 AD) was one of the greatest Islamic scholars in the fields of mathematics, astronomy, geometry and theology. "Al-Tūsī's influence, especially in eastern Islam, was immense. Probably, if we take all fields into account, he was more responsible for the revival of the Islamic sciences than any other individual. His bringing together so many competent scholars and scientists at Marāgha resulted not only in the revival of mathematics and astronomy but also in the renewal of Islamic philosophy and even theology. Al-Tūsī's works were for centuries authoritative in many fields of Islamic learning; … and his mathematical



studies affected all later Islamic mathematics … In the West al-Tūsī is known almost entirely as an astronomer and mathematician whose significance, at least in these fields, is becoming increasingly evident" (DSB). Tūsī's recension exists in two versions: one in 13 books of which only two manuscripts survive (both in the Laurenziana) – this is the version printed at the Medici Press in 1594; and another in 15 books (as here) which was first printed at Constantinople in 1801 (see Heath, pp. 77-8).

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

"In ancient times, commentaries were written by Heron of Alexandria (flourished 62 AD), Pappus of Alexandria (flourished ca. 320 AD), Proclus, and Simplicius of Cilicia (flourished c. 530 AD). The father of Hypatia, Theon of Alexandria (ca. 335–405 AD), edited the Elements with textual changes and some additions; his version quickly drove other editions out of existence, and it remained the Greek source for all subsequent Arabic and Latin translations until 1808, when an earlier edition was discovered in the Vatican.

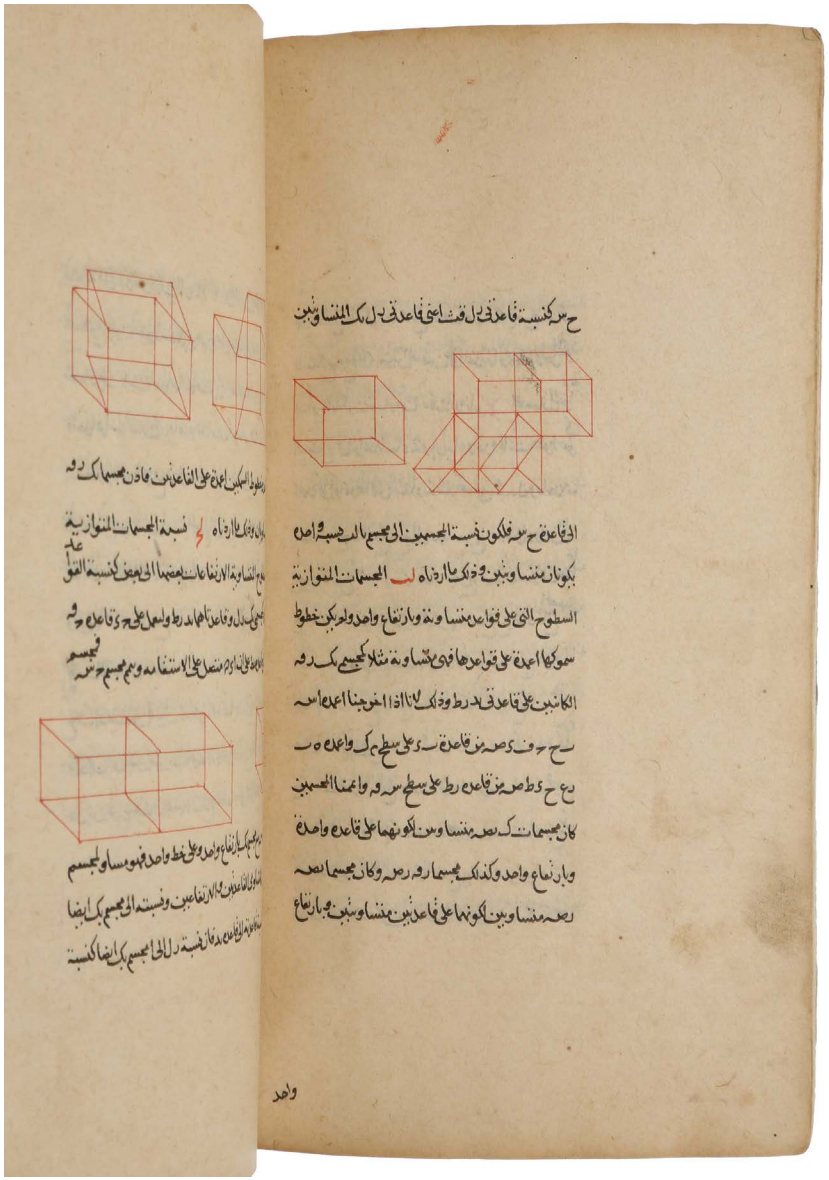
"The immense impact of the Elements on Islamic mathematics is visible through the many translations into Arabic from the 9th century forward, three of which must be mentioned: two by al-hajjāj ibn Yūsuf ibn Mahar, first for the Abbāsid caliph hārūn al-Rashīd (ruled 786–809) and again for the caliph al-Mahmūn

(ruled 813–833); and a third by Ishāq ibn hunayn (died 910), son of hunayn ibn Ishāq (808–873), which was revised by Thābit ibn Qurrah (ca. 836–901) and again by Nahīr al-Dīn al-hūsī. Euclid first became known in Europe through Latin translations of these versions.

“The first extant Latin translation of the Elements was made about 1120 by Adelard of Bath, who obtained a copy of an Arabic version in Spain, where he traveled while disguised as a Muslim student. Adelard also composed an abridged version and an edition with commentary, thus starting a Euclidean tradition of the greatest importance until the Renaissance unearthed Greek manuscripts. Incontestably the best Latin translation from Arabic was made by Gerard of Cremona (ca. 1114–87) from the Ishāq-Thābit versions …

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

“The subject of Book II has been called geometric algebra because it states algebraic identities as theorems about equivalent geometric figures. Book II contains a construction of “the section,” the division of a line into two parts such that the ratio of the larger to the smaller segment is equal to the ratio of the original line to the larger segment. (This division was renamed the golden section in the Renaissance after artists and architects rediscovered its pleasing proportions.) Book II also generalizes the Pythagorean theorem to arbitrary triangles, a result that is equivalent to the law of cosines. Book III deals with



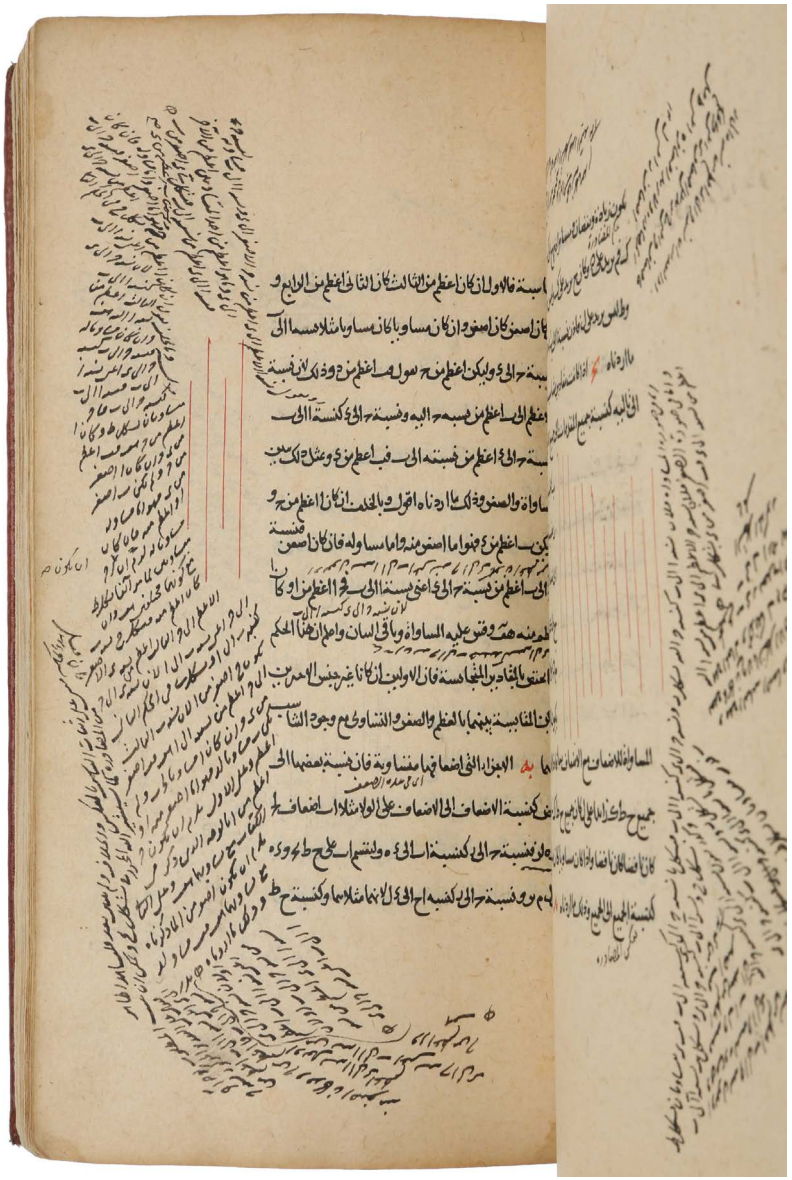
properties of circles and Book IV with the construction of regular polygons, in particular the pentagon.

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

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

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

“Books XI–XIII examine three-dimensional figures, in Greek stereometria. Book



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

"Tūsī wrote over 150 works, in Arabic and Persian, that dealt with the ancient mathematical sciences, the Greek philosophical tradition, and the religious sciences (law [fiqh], dialectical theology [kalām], and Sufism). He thereby acquired the honorific titles of khwāja (distinguished scholar and teacher), ustādh al-bashar (teacher of mankind), and al-mu'allim al-thālith (the third teacher, the first two being Aristotle and Farabi). In addition, Tūsī was the director of the first major astronomical observatory, which was located in Marāgha (Iran).

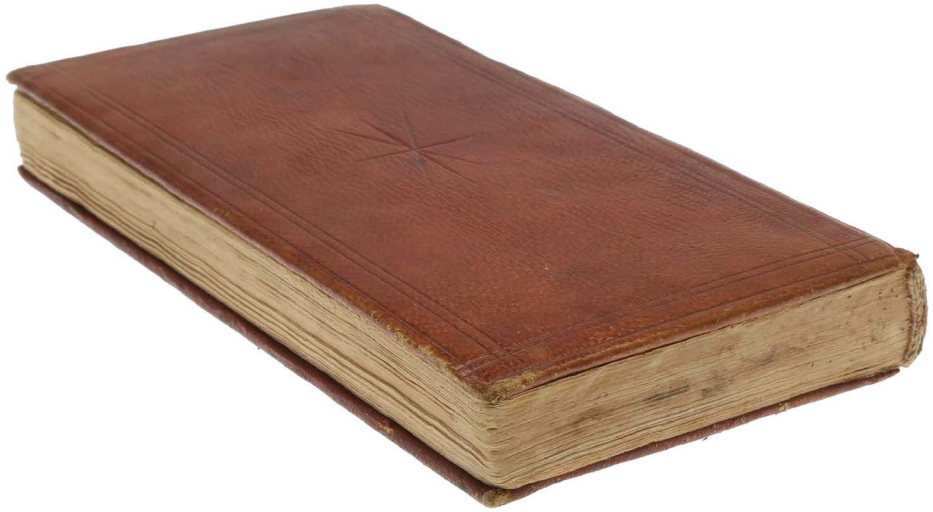
"Tūsī was born into a family of Imāmi (Twelver) Shi'a. His education began first at home; both Tūsī's father and his uncles were scholars who encouraged him to pursue al-'ulūm al-shar'iyya (the Islamic religious sciences) as well as the 'ulūm al-awā'il (the rational sciences of the ancients). He studied the branches of philosophy (hikma) and especially mathematics in Tūs [north-east Iran], but eventually traveled to Nīshāpūr (after 1213) in order to continue his education in the ancient sciences, medicine, and philosophy with several noted scholars; among the things he studied were the works of Ibn Sīnā, who became an important formative influence. Tūsī then traveled to Iraq where his studies included legal theory; in Mosul (sometime between 1223 and 1232), one of his teachers was Kamāl al-Dīn ibn Yūnus (d. 1242), a legal scholar who was also renowned for his expertise in astronomy and mathematics.

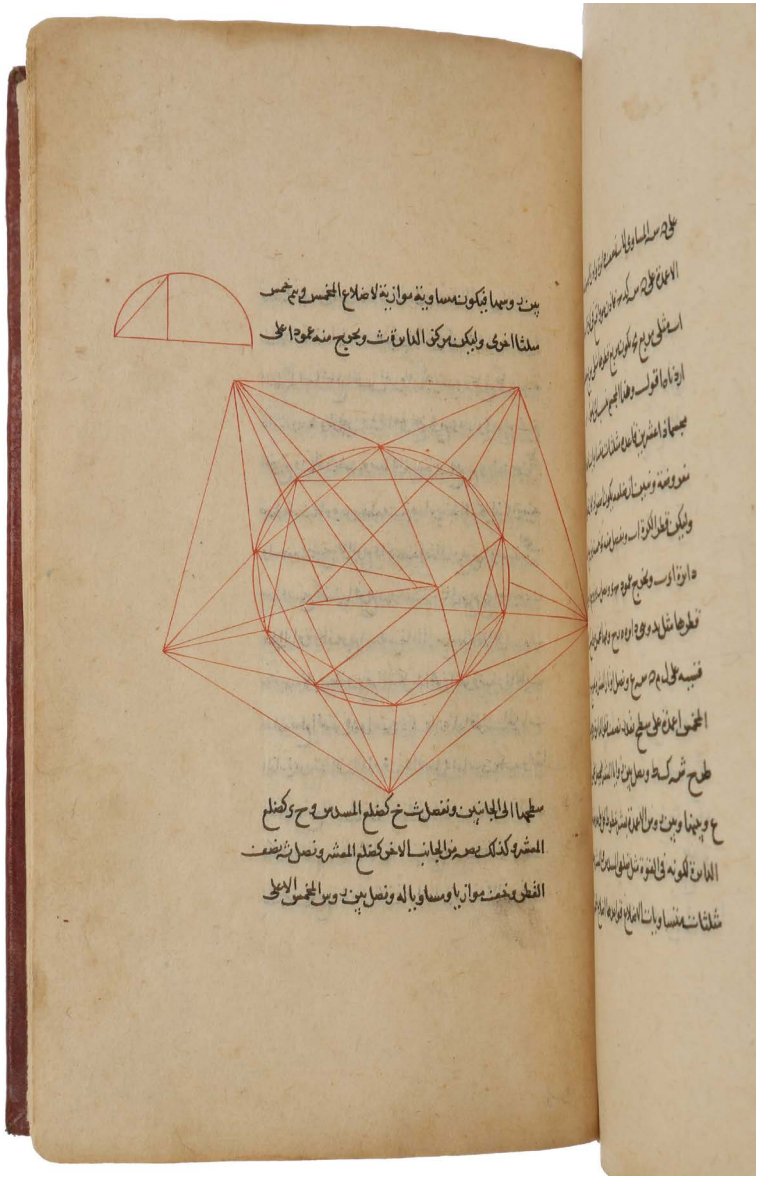
"In the early 1230s, after completing his education, Tūsī found patrons at the Ismā'īlī courts in eastern Iran; he eventually relocated to Alamūt, the Ismā'īlī capital, and witnessed its fall to the Mongols in 1256. Tūsī then served under the Mongols as an advisor to Īlkhānid ruler Hūlāgū Khan, becoming court astrologer as well as minister of religious endowments (awqāf). One major outcome was that Tūsī oversaw the construction of an astronomical observatory and its instruments in Marāgha, the Mongol headquarters in Azerbaijan, and he became its first director. The Marāgha Observatory also comprised a library and school. It was one of the most ambitious scientific institutions established up to that time and may be considered the first full-scale observatory. It attracted many famous and talented scientists and students from the Islamic world and even from as far away as China. The observatory lasted only about 50 years, but its intellectual legacy would have repercussions from China to Europe for centuries to come. Indeed, it is said that Ulugh Beg's childhood memory of visiting the remnants of the Marāgha Observatory as a youth contributed to his decision to build the Samarqand Observatory. Mughal observatories in India, such as those built by Jai Singh in the 18th century, clearly show the influence of these earlier observatories, and it has been suggested that Tycho Brahe might have been influenced by them as well. In 1274 Tūsī left Marāgha with a group of his students for Baghdad.

"Tūsī's writings are both synthetic and original. His recensions (tahārīr) of Greek and early Islamic scientific works, which included his original commentaries, became the standard in a variety of disciplines. These works included Euclid's Elements, Ptolemy's Almagest, and the so called mutawassiṭāt (the 'Intermediate Books' that were to be studied between Euclid's Elements and Ptolemy's Almagest) with treatises by Euclid, Theodosius, Hypsicles, Autolycus, Aristarchus, Archimedes, Menelaus, Thābit ibn Qurra, and the Banū Mūsā. In mathematics, Tūsī published a sophisticated 'proof' of Euclid's parallels postulate that was important for the development of non-Euclidean geometry, and he treated trigonometry as a discipline independent of astronomy, which was in many ways similar to what

was accomplished later in Europe by Johann Muller (Regiomontanus). Other important and influential works include books on logic, ethics, and a famous commentary on a philosophical work of Ibn Sīnā" (Biographical Encyclopedia of Astronomers, pp. 1153-4).

T. L. Heath, The Thirteen Books of Euclid's Elements, Vol. I (1908). P. Stocks, Subject Guide to the Arabic Manuscripts in the British Library, ed. by C. F. Baker (London: British Library, 2001). See also C. Brockelmann, Geschichte der arabischen Literatur, I. 670 – 676; suppl. I. 924-933.





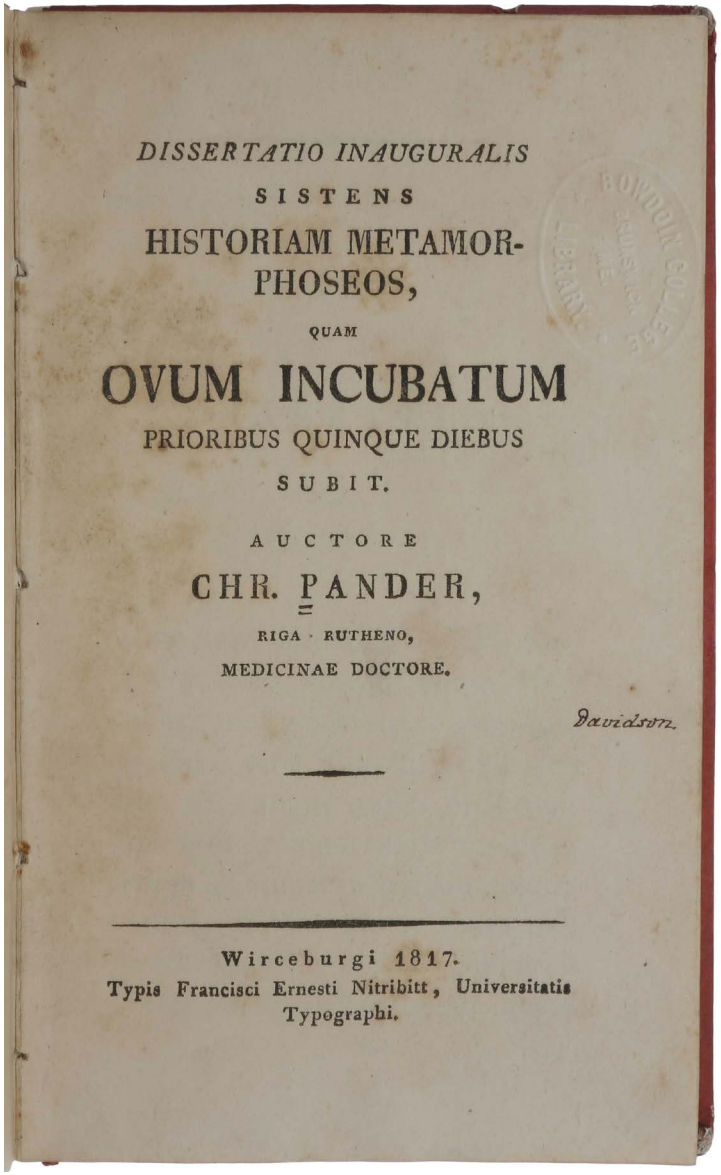
A FOUNDATION WORK OF MODERN EMBRYOLOGY

PANDER, Christian Heinrich. *Dissertatio inauguralis sistens historiam metamorphoseos, quam ovum incubatum prioribus quinque diebus subit.* Würzburg, Frans Ernst Nitribitt, 1817.

$28,000

8vo (190 x 115 mm), pp. [ii], 69. Contemporary dark orange boards with gilt-ruled spine, all edges gilt (some wear at extremities). Blind-stamp of Bowdoin College, deaccession label to front paste-down.

First edition of a fundamental work in embryology and a legendary rarity. "The original version of Pander's medical thesis, in which he announced the discovery of the trilaminar structure of the chick blastoderm, a term he coined from the Greek 'blastos' (germ) and 'derma' (skin)" (Norman). "Pander sent a copy of his thesis to Karl von Baer, who made immediate use of it for his own embryological researches. The investigations of these two biologists together represent the foundation of modern embryology and of "a new branch of comparative morphology, which . . . made possible a far more universal and extensive study of the organs in living creatures than had been conceivable before, embracing not only the present characteristics of the organs, but also their evolutional history" (Nordenskiold, The History of Biology: a Survey, New York, 1928, pp. 368-369). "In the twelfth hour of embryonic development he reported that the blastoderm consisted of two entirely separate layers: an inner layer, thick and opaque; and an outer layer thin, smooth, and transparent. Between these two a third layer developed, in which blood vessels formed and from which 'events of the greatest



DISSERTATIO INAUGURALIS
SISTENS
HISTORIAM METAMOR-
PHOSEOS,
QUAM
OVUM INCUBATUM
PRIORIBUS QUINQUE DIEBUS
SUBIT.
AUCTORE
CHR. PANDER,
RIGA · RUTHENO,
MEDICINAE DOCTORE.

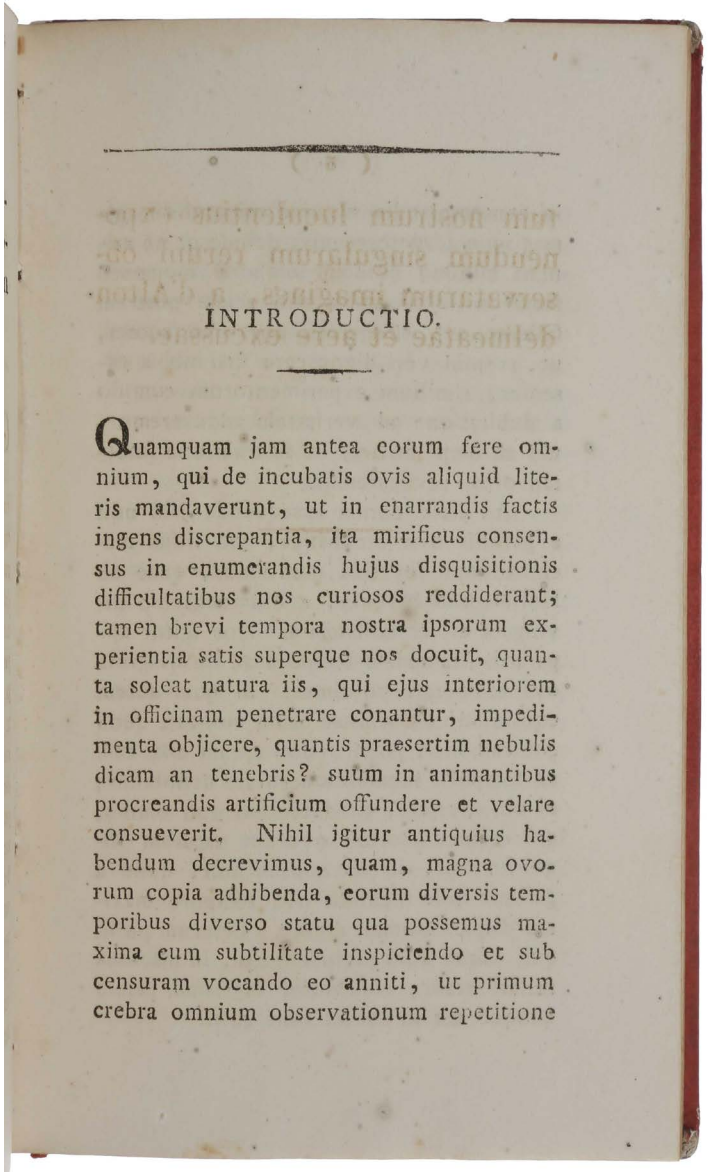
Davidson.

Wirceburgi 1817.
Typis Francisci Ernesti Nitribitt, Universitatis Typographi.

importance subsequently occur'. When Baer received a copy of Pander's work in 1818 at Königsberg University, where he was serving as prosector to his old Dorpat professor, Burdach, he began his own investigations, which ultimately revolutionized embryology. Baer's first treatise on the subject includes an introduction styled as a personal letter to Pander, explaining his differences with his old friend" (DSB). Pander did not pursue his embryological studies after the appearance of his thesis, but did finance the publication of an illustrated German-language version [Beiträge zur Entwicklungsgeschichte des Hunchens im Eye, 1817]" (Norman). ABPC/RBH record only the Norman copy (Sotheby's, 1998, lot 1232, $12,650).

Provenance: 1. The American entomologist and invertebrate embryologist Alpheus S. Packard, Jr. (1839-1905) (signature on front free endpaper dated 1885). Packard, a champion of Lamarck, published in 1901 a biography of him with English translations from his work. Packard was first at Bowdoin College, Maine, and then at Brown as professor of zoology and geology. 2. Bowdoin College (their blind stamp on title and withdrawn bookplate on front paste-down. 3. Davidson (name written in a small neat hand on title, and 'Gynaec. 283' on front paste-down in the same hand – Gynaec is Latin for women's apartment).

"Pander (1794-1865) was the son of a wealthy banker of German descent. After studying in the local schools of Riga, he entered the University of Dorpat in 1812. Dorpat had been refounded in 1798, and its faculty was German trained. Prior to this the Baltic gentry had traditionally sent their sons to German universities. Although his father had wanted him to study medicine, Pander was more interested in natural history, but he attempted to combine the two. At Dorpat he came under the influence of the anatomist Karl Friedrich Burdach, who had also taught Karl Ernst von Baer; Baer later continued Pander's embryologic researches.



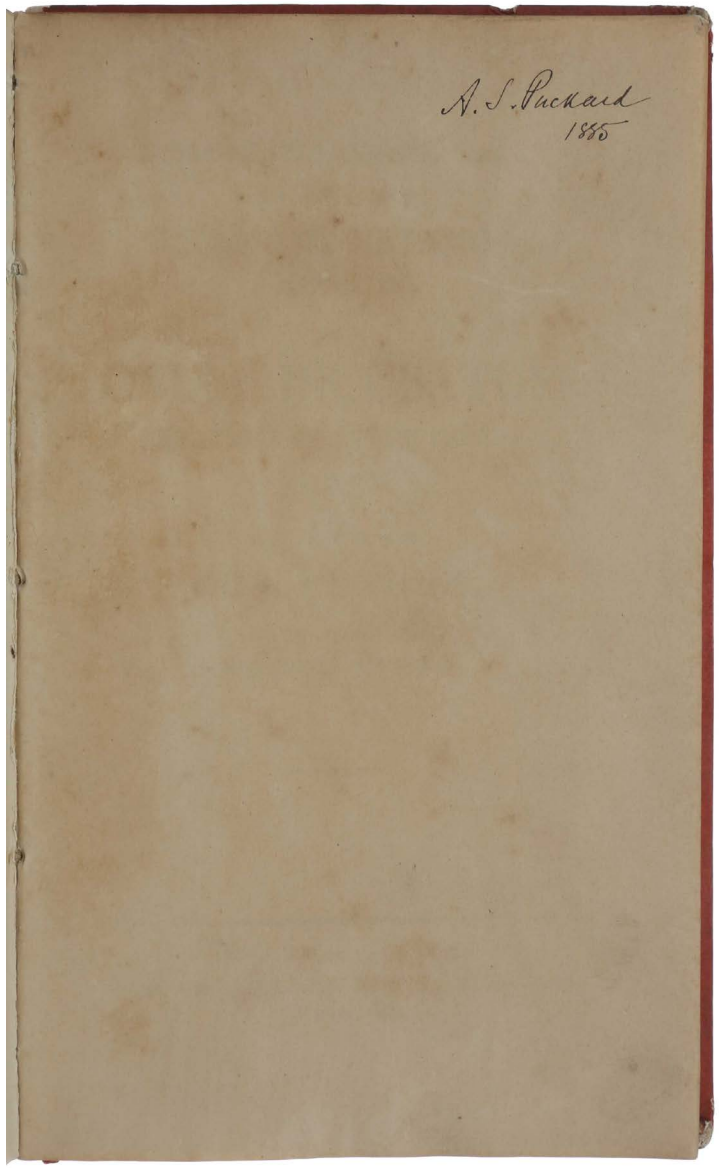
INTRODUCTIO.

Quamquam jam antea eorum fere omnium, qui de incubatis ovis aliquid literis mandaverunt, ut in enarrandis factis ingens discrepantia, ita mirificus consensus in enumerandis hujus disquisitionis difficultatibus nos curiosos reddiderant; tamen brevi tempora nostra ipsorum experientia satis superque nos docuit, quanta soleat natura iis, qui ejus interiorem in officinam penetrare conantur, impedimenta objicere, quantis praesertim nebulis dicam an tenebris? suum in animantibus procreandis artificium offundere et velare consueverit. Nihil igitur antiquius habendum decrevimus, quam, magna ovorum copia adhibenda, eorum diversis temporibus diverso statu qua possemus maxima cum subtilitate inspiciendo et sub censuram vocando eo anniti, ut primum crebra omnium observationum repetitione

"In 1814 Pander left Dorpat for Berlin and from there he went on to Göttingen. In March 1816, at a congress of Baltic students resident in Germany, he renewed his acquaintance with Baer, who persuaded him to come to the University of Würzburg and study under Ignaz Döllinger. In his autobiography Baer states that he, Pander, and Döllinger had discussed Döllinger's hope that someone would study anew the development of the chick embryo. Pander took on the task and he received his M.D. at Würzburg in 1817. His dissertation, "Historia metamorphoseos quam ovum incubatum prioribus quinque diebus subit," was amplified and then published in German (1817) with illustrations by the elder E. J. d'Alton" (DSB).

Building upon the work of Marcello Malpighi (1628-94) and Caspar Friedrich Wolff (1733-94), Pander's thesis methodically describes the different layers from which the various organs of the chicken embryo emerge. "Pander completed his studies on the chick in less than 15 months … In his narrative Pander concentrated on the structure of the undeveloped egg, the early development of the blastoderm, and the appearance of the primitive streak with its two primitive lateral folds. He described the beginning somites, which he identified as rudimentary vertebrae. He observed the blastoderm separating into two germ layers, or, as Pander expressed it, into a 'serous layer' and 'mucous layer', and he explained how soaking the germ in water for 24 hours allowed him to tease the two layers apart. He later related the appearance of a third layer, the 'vascular layer', from which, he asserted, the vascular system arose …

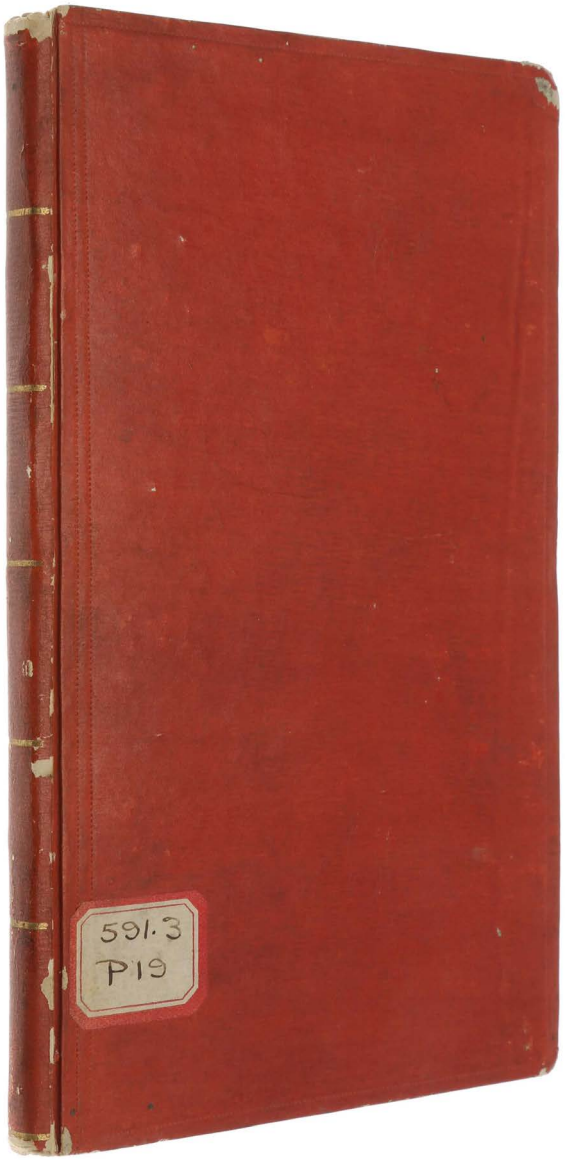
"It was not, however, in the factual details but in his conception of the germ layers that Pander made his most valuable contribution. These were antecedents to later embryonic structures. It was in their growth and interplay that embryonic form came into being. "Actually a unique metamorphosis begins in each of these three layers," Pander asserted, "and each hurries towards its goal; although each is not



yet independent enough to indicate what it truly is; it still needs the help of its sister travellers, and therefore, although already designated for different ends, all three influence each other collectively until each has reached an appropriate level." For Pander the germ layers were thus an essential precursor to later structures. In their movements and interactions was to be found the explanation for later form. Pander does not elaborate on these germ layer interactions. Whether he intended mechanical processes, or whether in some prescient way he thought of the interplay of forces or exchanges of chemicals, we are unable to determine in his statements. Both Wolff and [Albrecht von] Haller, however, had misunderstood and passed by this critical juncture of the developmental process, and it was here that Pander thought the controversy between preformation and epigenesis would be resolved" (Gilbert, A Conceptual History of Modern Embryology, vol. 7, pp. 3-4).

"Pander, for reasons that are not entirely clear, never pursued his early research, although he regarded his studies as incomplete and had expressed himself only briefly on the subsequent transformations that took place in the embryo" (DSB). After four years leisurely travel through Western Europe, Pander settled in St. Petersburg where he pursued an independent career as geologist and palaeontologist, eventually promoting a pre-Darwinian theory of evolution. He became a member of the St. Petersburg Academy of Sciences in 1826.

Garrison-Morton 474; Waller 11925; Norman 1631.



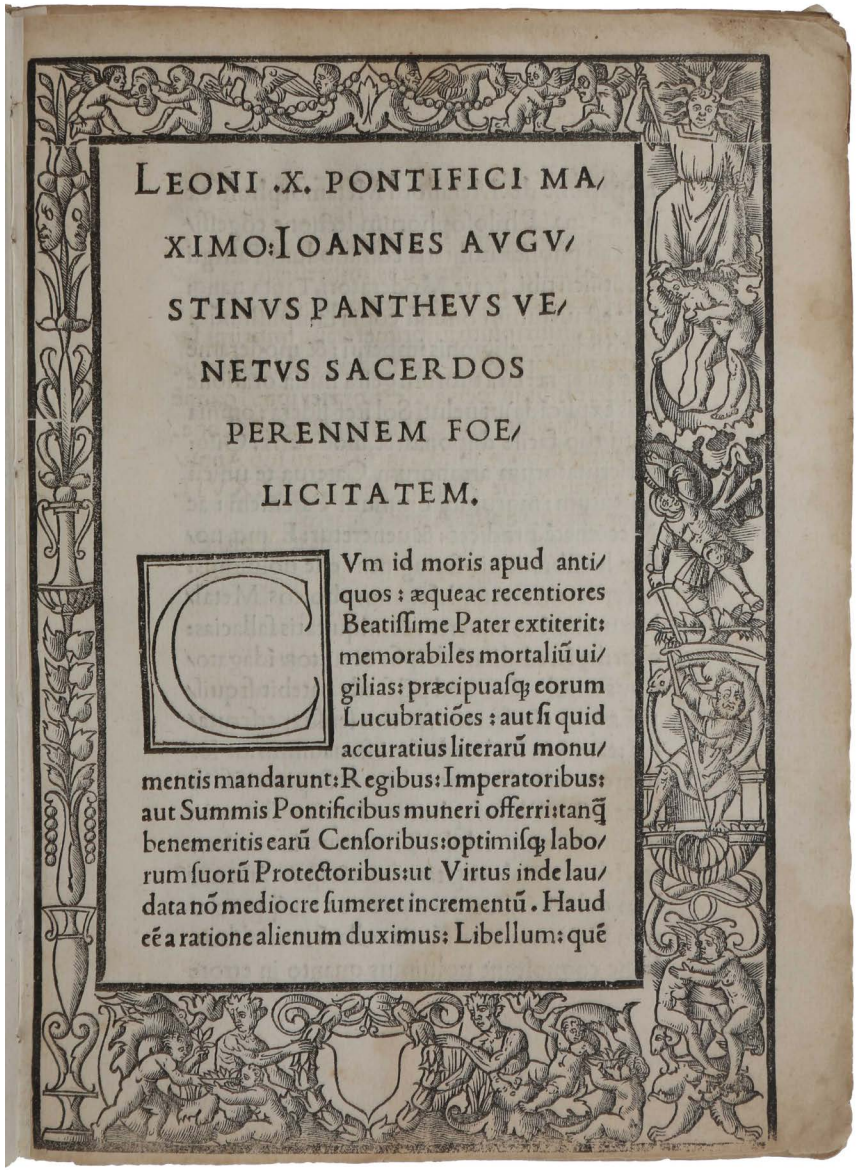
ALCHEMICAL RARITY WITH 12 LEAVES OF CONTEMPORARY MANUSCRIPT

PANTHEUS, Giovanni Agostino. *Ars Transmutationis Metallicae ... [with, as issued] Commentarium theoricae Artis Mettalicae Transmutationis.* [Venice: Tacuino, 1519 (colophon of part I dated September 1518)].

$48,000

Two parts in one volume with continuous pagination, small 4to, ff. 38 (Ars Transmutationis ff. [1]-26, Commentarivm theoricae ff. [27]-38), with several contemporary marginal annotations and 12 added blank leaves densely annotated in a contemporary hand; printed in Roman, Greek and Hebrew letters; several full-page and other smaller woodcut diagrams in the text, tables, woodcut border on 3r, woodcut initials throughout (first couple of leaves a little waterstained and reinforced at gutter, but generally very good). Eighteenth-century vellum.

First edition of one of the greatest rarities in the alchemical and chemical literature; this is an exceptionally interesting copy, bound with twelve leaves of contemporary script conveying an all-encompassing 'tree of human endeavours', charting the features and visually suggesting the relative position of human faculties and of most fields of knowledge, an extraordinarily wide-ranging array that includes disciplines such as economics and politics amongst more traditional trivium and quadrivium 'artes', and of course alchemy. "Pantheus wrote against spurious alchemy and he deals partly with the assay of gold and partly with the chemical preparation of various substances which were made at Venice in his time and were used in the arts. He describes, for example, the manufacture of white lead and of an alloy for mirrors. Pantheus was a priest at Venice, but seems



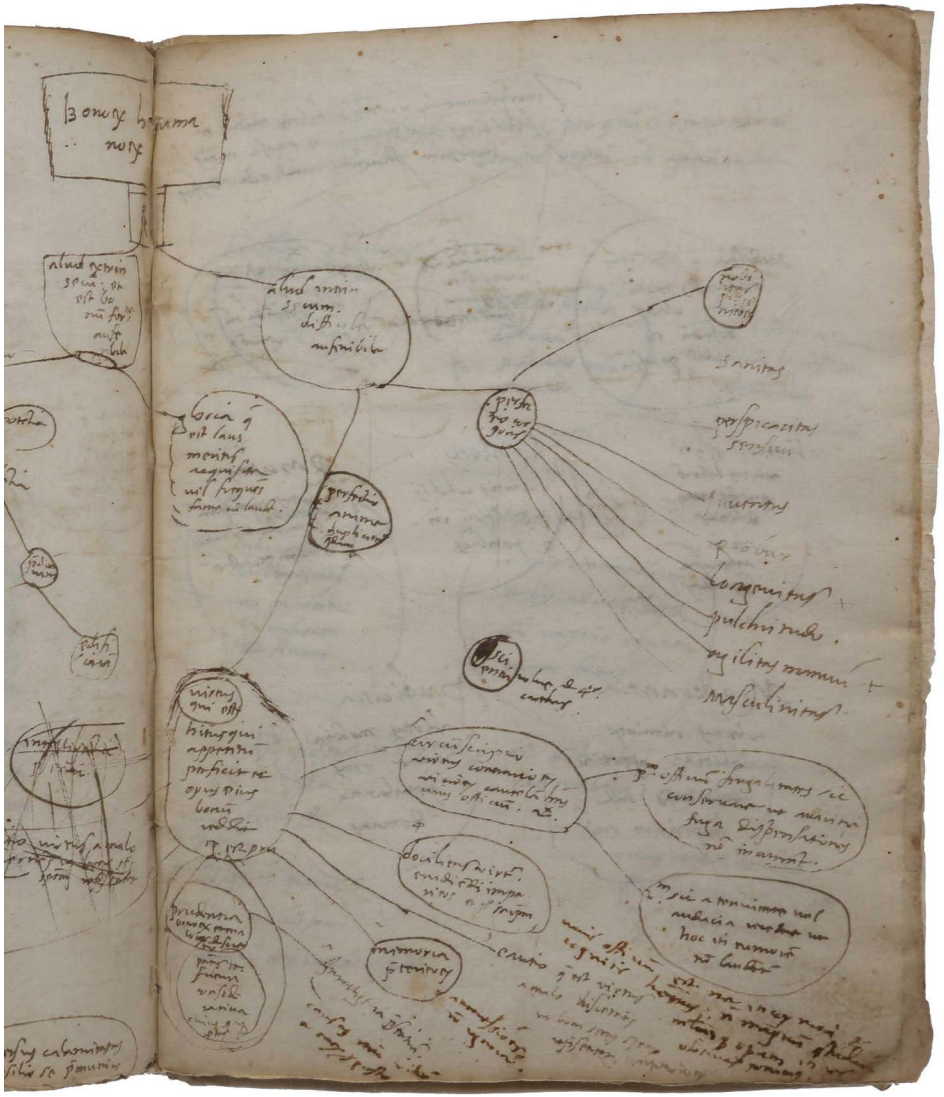
LEONI .X. PONTIFICI MA/
XIMO:IOANNES AVGV/
STINVS PANTHEVS VE/
NETVS SACERDOS
PERENNEM FOE/
LICITATEM.

CVm id moris apud anti/
quos : æqueac recentiores
Beatiſſime Pater extiterit:
memorabiles mortaliũ ui/
gilias: præcipuaſq; eorum
Lucubratiões : aut ſi quid
accuratius literarũ monu/
mentis mandarunt: Regibus: Imperatoribus:
aut Summis Pontificibus muneri offerri: tanq̃
benemeritis earũ Cenſoribus: optimiſq; labo/
rum ſuorũ Protectoribus: ut Virtus inde lau/
data nõ mediocre ſumeret incrementũ . Haud
eẽ a ratione alienum duximus: Libellum: quẽ

nevertheless to have been devoted to chemical research" (Ferguson). It has only been recognised in the present century that the Ars transmutationis metallicae is also an important, and very early, contribution to atomism, the precursor to modern atomic theory. Emil Offenbacher, the most important dealer in alchemy and chemistry books of the last century, described the first edition of this book as "almost unobtainable today" (his Cat. 37 (1985), item 137, in which he described the 1550 edition). The greatest collector of early chemistry books of the past century, Roy G. Neville, never found a copy. ABPC/RBH list just one copy in the last 80 years (and that in a modern binding). OCLC lists three copies in US (Claremont Colleges, Delaware, Madison (Wisconsin)).

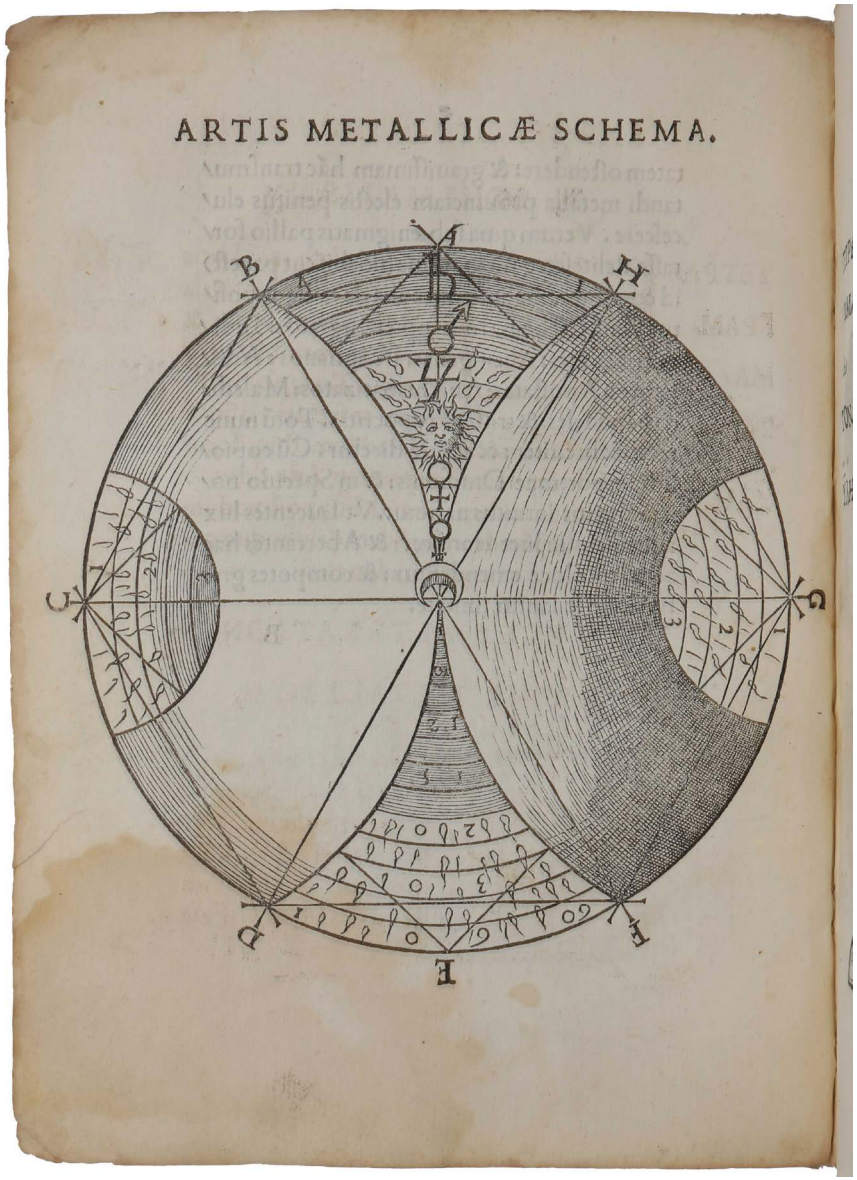
"During the course of the sixteenth century, a pronounced trend emerged toward the permeation of Christian Kabbalah with alchemical symbolism. This convergence of alchemy and Kabbalah was perhaps to be expected as both arts were concerned with knowledge of creation. Both arts, too, advocated a secret transmission of knowledge from master to pupil, with initiations, ordinations, and revelations from God and his angels. To a certain extent, the kabbalists' reduction of language to its elemental letters corresponded to the alchemists' reduction of matter to its primal state; the permutation of letters and words corresponding to the circulation and combination of elements and substances. The first known combination of alchemy and Kabbalah can be found in the works of the Venetian priest Giovanni Agostino Panteo (d. 1535), who develops a hybrid "Kabbalah of Metals" in two works: the Ars transmutationis metallicae (Art of metallic transmutation, 1519) and Voarchadumia contra alchimiam (Voarchadumia against alchemy, 1530)" (Forshaw, p. 149).

The alchemical atomist tradition may originate with the De lapide philosophorum attrinuted to one Frater Effararius or Ferrarius, a work probably of late mediaeval origin, much of which is a commentary on Geber's Summa perfectionis. Geber's



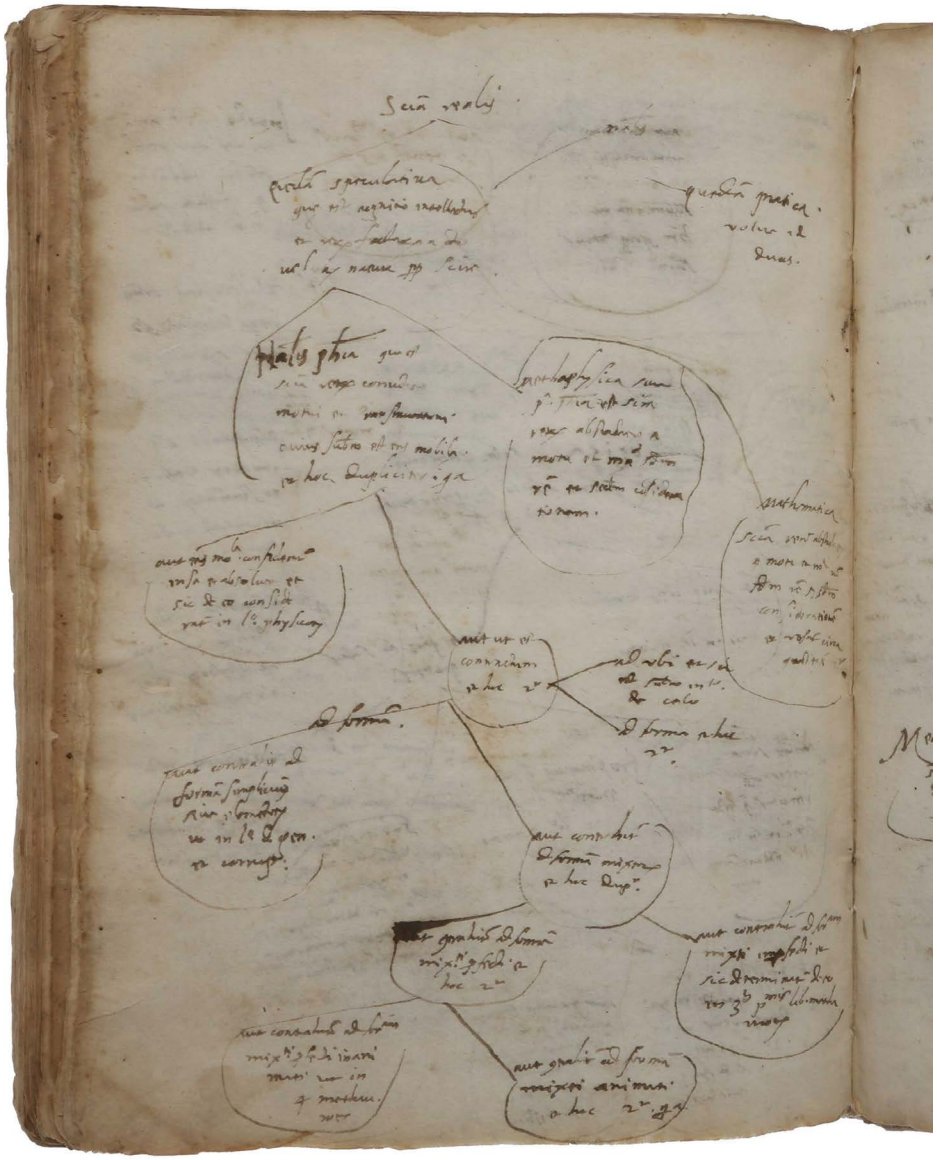
work advocates a corpuscular theory, in which the smallest parts of matter are particles resistant to decomposition by the techniques of the laboratory, but which are not absolutely indivisible as advocated by the atomists.

"Brother Effararius's atomistic interpretation of Geberian corpuscular theory received considerable expansion in the early sixteenth century, with the publication of Giovanni Agostino Pantheo's Ars et theoria transmutationis of 1518. Pantheus, a Venetian priest, was deeply interested in linking alchemy to the Cabala, but also in microstructural explanations of matter. He begins these speculations by commenting on a passage from Aristotle, … a 'heterodox' version of Aristotle's definition of mixture … In Pantheus the Aristotelian passage appears thus: "Therefore according to the Philosopher in Book One of De generatione et corruptione, 'mixture is the union of the altered miscibles conjoined per minima.' Note 'miscibilium', that is, of the elements." The genuine Aristotle had of course said in Book 1, Chapter 10 of De generatione that 'Mixture is the union of the altered miscibles.' The astonishing addition of the words per minima to this definition of mixture has the effect of turning Aristotle into an outspoken corpuscularian, or as Pantheo would have it, an atomist. For the Venetian priest immediately adds the gloss … "Likewise note 'per minima', that is, through indivisibles. For if something could be divided, then it would not be a minimum, since every part must be less than its whole. Therefore it appears that the mixture of the elements is brought about through minima, that is, per indivisibilia. And that 'element' is the smallest of existing bodies appears through its definition. For 'element' is the smallest particle of the body." After making it clear that he considers Aristotle an atomist, Pantheo then goes on to apply this theory to the alchemical process of 'putrefaction,' whereby a metal or other substance is dissolved into minimal particles. He explicitly compares the dissolution to the procedure of calcination, saying that both involve the loss of an interparticular 'glue'. Pantheo proceeds to develop an elaborate analogy between the body-to-be-putrefied and a home-to-



be-demolished: “For an integral whole (for example a house) consists of integral parts. When one part has been destroyed or removed from its place, which it had before, the form in the whole, or the essence of the whole house, is destroyed, although the stones, boards, and cement from which the house was made remain. So it also happens in our subject. For when the moisture is separated from its own place which it had in the elemental mixture, as it were an integral part in the whole, and this occurs through heat raising it, and separating it from the other parts, the form and essence of the mixture itself is annihilated wholly. But the substance of the moisture is not annihilated, nor can it be.” This interesting comparison rests on an analogy between the mortar joining the building-components of the house to the humidity glueing the elemental particles of a dissolving metal together … It is interesting to note that precisely the same house-analogy is used in Isaac Newton’s chymical notebooks to describe putrefaction” (Newman, pp. 302-4).

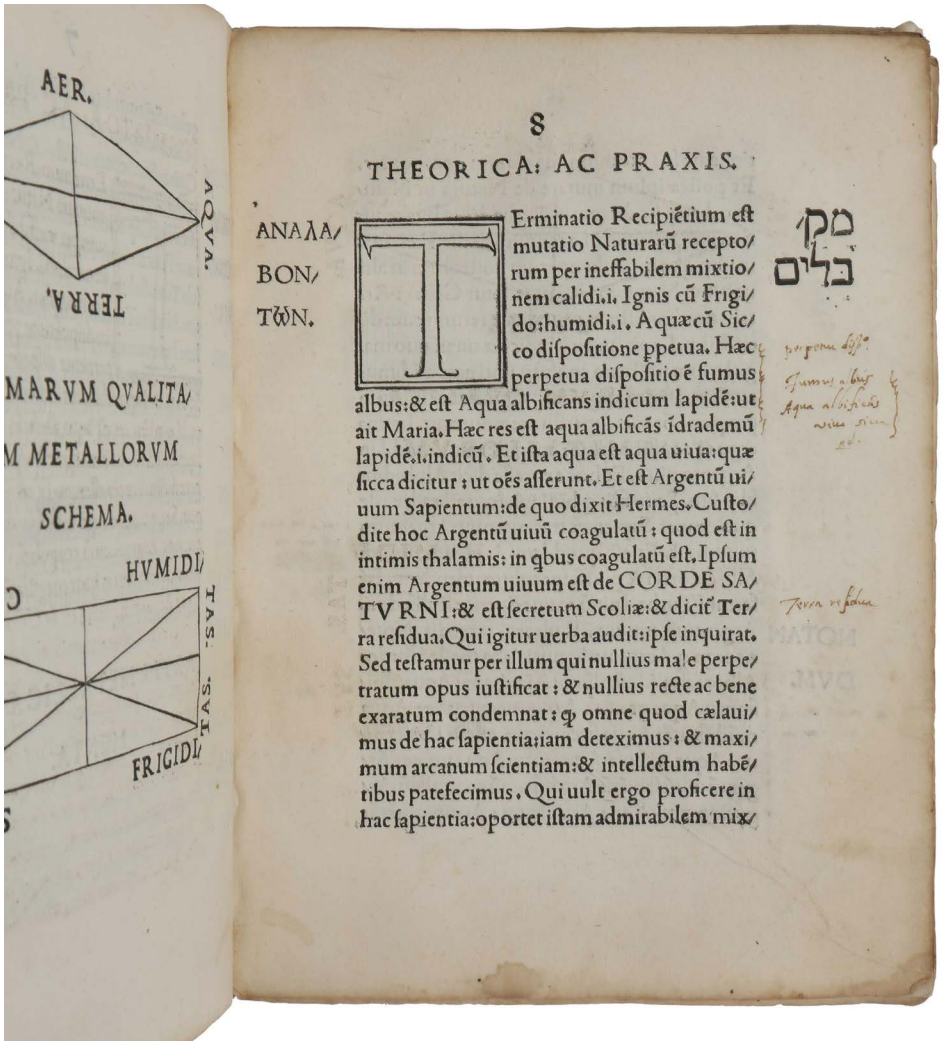
“In his preface to the pope [Leo X], Pantheus describes his booklet as very recently put together from varied reading of the philosophers. He wishes it to contain the sincere truth of the secret of transmutation, to abolish deceits and incredulity, to reveal the stone to the sons of wisdom and to conceal it from the ignorant. Reading his book would have saved those who have followed false interpretations all their time and expense. Similarly in a second preface to the reader he promises to elucidate completely this most weighty theme of the transmutation of metals. Actually he only succeeds in making the matter more mysterious by various charts, diagrams and columns of letters and numbers as well as the Tetragrammaton and Greek and Hebrew characters. After the manner of the Lullian alchemical treatises he sets letters for stages in the process of transmutation and gives diagrams of the four elements and primary qualities … Numerical equivalents are given for the different letters of the alphabet, and the totals are added up at the base of the columns …



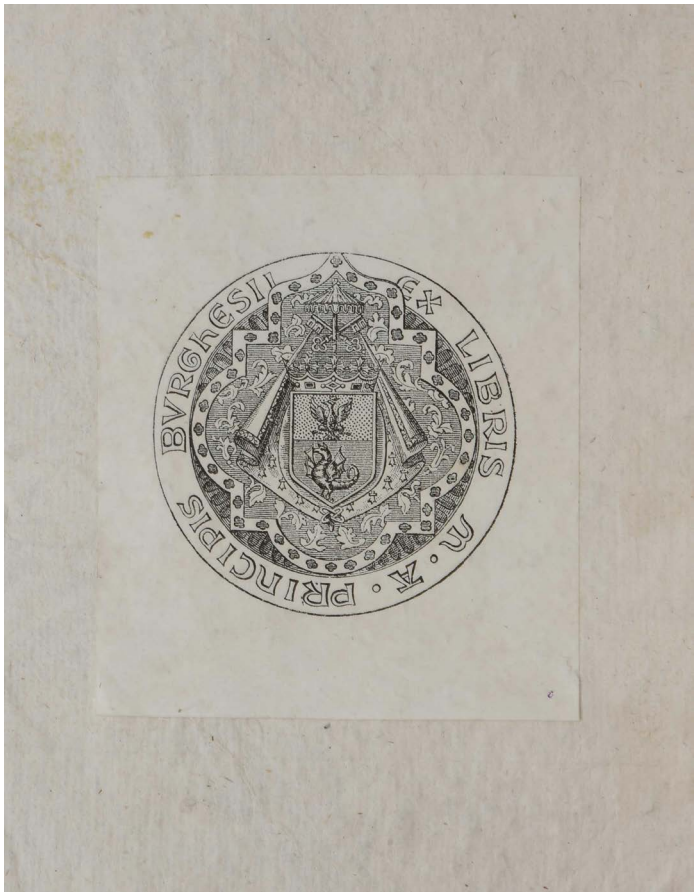
"After the Art of Metallic Transmutation ends with the date, September 7, 1518, there follows a new title page inscribed, 'Commentary of the Theory of the Metallic Art of Transmutation.' It is addressed to a noble Pole named William Hyerosky. In it Pantheus alludes to 'those Institutes of ours edited in former years' and over which he has heard that Hyerosky pores day and night. He denies that these Institutes were incomplete as published and left something for verbal interpretation like the cabala. But he now explains what his Hebrew characters represent and the numerical value of some letters. He adds some recipes, then reverts to columns of numbers in his closing pages. It is at the close of this Commentary that we find the final date of publication, December 30, 1518. It is not quite clear whether by the title, Institutes, Pantheus refers to the preceding Ars transmutationis, which may have circulated in manuscript form for some time before being printed, or to some other earlier production of his.

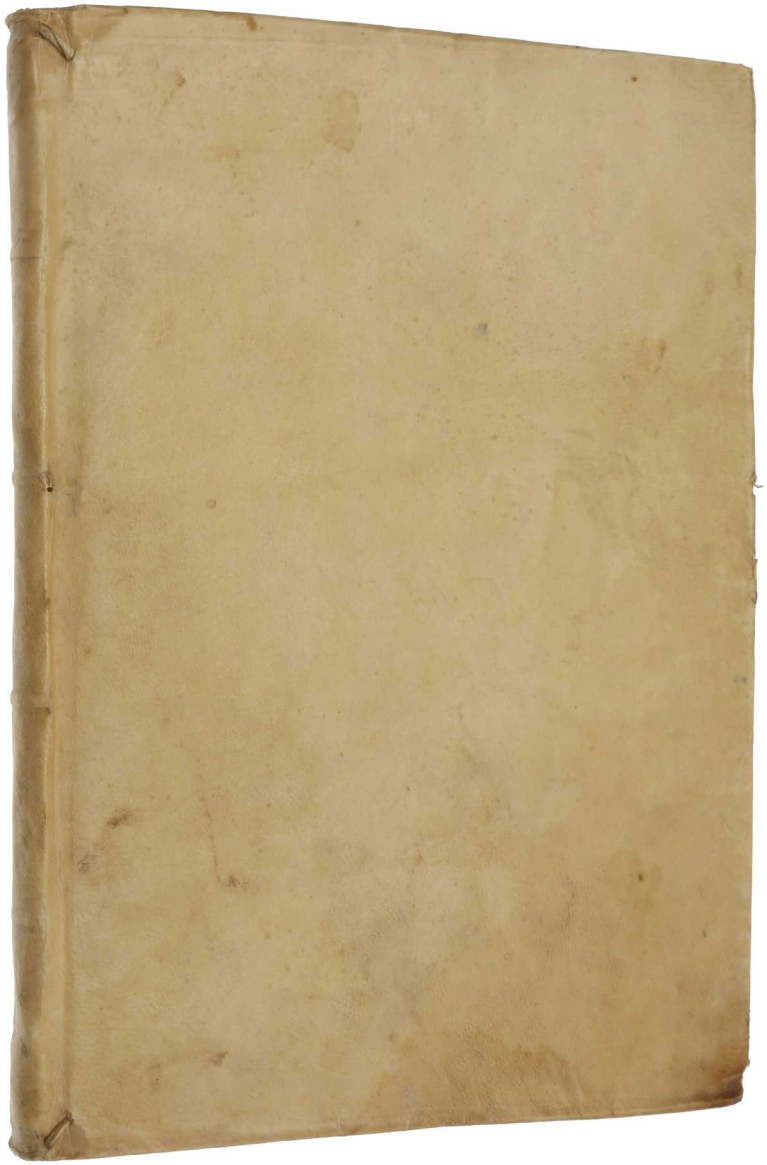
"It seems probable that, after the publication of this volume of 1518, someone called to the attention of its author or the papal court or the Venetian government the existence of a papal decretal and a decree of Venice against alchemists. For in 1530 Pantheus brought out with the same printer at Venice a book entitled, Voarchadumia contra alchimiam: ars distincta ab archimia et sophia. As this title suggests, he now professed to be writing not on alchemy but on Voarchadumia, an art distinct from alchemy. This Voarchadumia he represented as true wisdom, the very opposite of alchemy, a sort of 'cabala of metals' … Yet he repeats most of his work of 1518 in the course of the Voarchadumia … Both the Voarchadumia and the work of 1518 were reprinted together at Paris in 1550, and again, but omitting both the papal edict and the preface to the pope, in the second volume of Zetzner's Theatrum chemicum as published in 1615 and in 1659" (Thorndike, pp. 538-40).

Duveen, p. 449 ("of great rarity"); Ferguson, II, p. 167; Hoover 623; Stillwell 866.



Thorndike, V, pp. 537-40. This first edition not in Schmiedler, Geschichte der Alchemie. Newman, 'Experimental Corpuscular Theory in Aristotelian Alchemy: From Geber to Sennert,' in Lüthy, Murdoch & Newman (eds.), Late Medieval and Early Modern Corpuscular Matter Theories, 2001. On Panteo see A. Perifano, L'Alchimie a la Cour de Come 1er de Medicis: savoirs, culture et politique (Paris 1997), pp. 18-19.



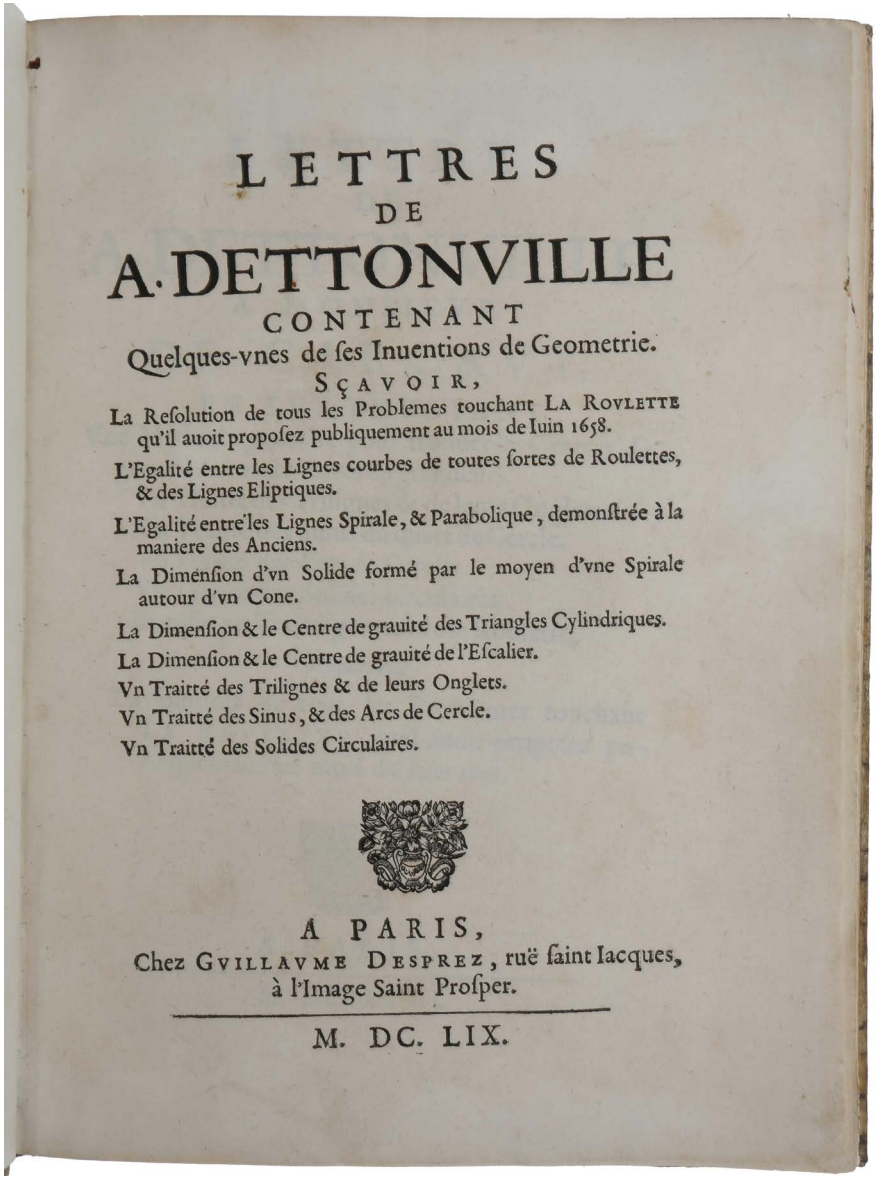


ONE OF PASCAL'S MOST BRILLIANT WORKS

PASCAL, Blaise. *Lettres de A[mos] Dettonville contenant Quelques-unes de ses Inventions de Géométrie. Sçavoir – La Résolution de tous les Problèmes touchant la Roulette qu'il avoit proposez publiquement au mois de Juin 1658. L'Égalité entre les Lignes courbes de toutes sortes de Roulettes, & des Lignes Eliptiques. L'Égalité entre les Lignes Spirale, & Parabolique, demonstrée à la manière des Anciens. La Dimension d'un Solide formé par le moyen d'une Spirale autour d'un Cone. La Dimension & le Centre de gravité des Triangles Cylindriques. La Dimension et le Centre de gravité de l'Escalier. Un Traitté des Trilignes & de leurs Onglets. Un Traitté des Sinus, et des Arcs de Cercle, Un Traitté des Solides Circulaires.* Paris: Guillaume Desprez, 1659.

$75,000

Nine parts in one volume, 4to, pp. [2] (general title dated 1659); [2] (Lettre de Monsieur de Carcavy a Monsieur Dettonville); [2, part title dated 1658 – Lettre de A. Dettonville à Monsieur de Carcavy, en lui envoyant, une Méthode générale pour trouver les Centres de gravité de toutes sortes de grandeurs. Un Traitté des Trilignes et de leurs Onglets. Un Traitté des Sinus du quart de Cercle. Un Traitté des Arcs de Cercle. Un Traitté des Solides circulaires. Et enfin un Traitté générale de la Roulette, contenant la Solution de tous les Problemes touchant la Roulette qu'il auoit proposez publiquement au mois de juin 1658]; 26 (Lettre de Monsieur Dettonville à Monsieur de Carcavy); 25 (Traité des Trilignes & de leurs Onglets); 8 (Propriétez des Sommes Simples, Triangulaires & Pyramidales); 24 (Traité des Sinus du quart de Cercle; Traite des Arcs de Cercle); 7 (Petit Traite des Solides); 10 (Traitte general de la roulette ou, Problèmes touchant la Roulette proposez publiquement & résolue par A. Dettonville); [2, part title dated 1659 – Lettre de A. Dettonville



LETTRES
DE
A·DETTONVILLE
CONTENANT
Quelques-vnes de ſes Inuentions de Geometrie.
SÇAVOIR,

La Reſolution de tous les Problemes touchant LA ROVLETTE qu'il auoit propoſez publiquement au mois de Iuin 1658.

L'Egalité entre les Lignes courbes de toutes ſortes de Roulettes, & des Lignes Eliptiques.

L'Egalité entre les Lignes Spirale, & Parabolique, demonſtrée à la maniere des Anciens.

La Dimenſion d'vn Solide formé par le moyen d'vne Spirale autour d'vn Cone.

La Dimenſion & le Centre de grauité des Triangles Cylindriques.

La Dimenſion & le Centre de grauité de l'Eſcalier.

Vn Traitté des Trilignes & de leurs Onglets.

Vn Traitté des Sinus, & des Arcs de Cercle.

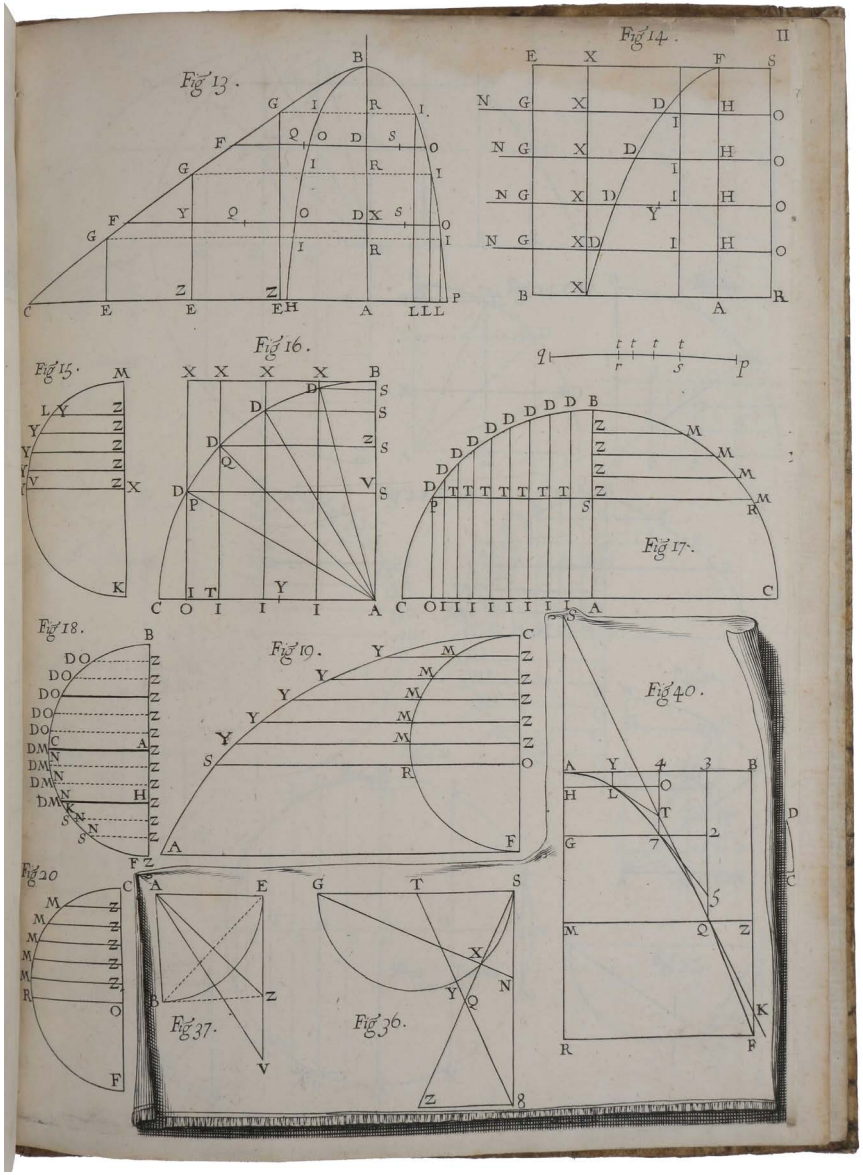
Vn Traitté des Solides Circulaires.

A PARIS,
Chez GVILLAVME DESPREZ, ruë ſaint Iacques, à l'Image Saint Proſper.

M. DC. LIX.

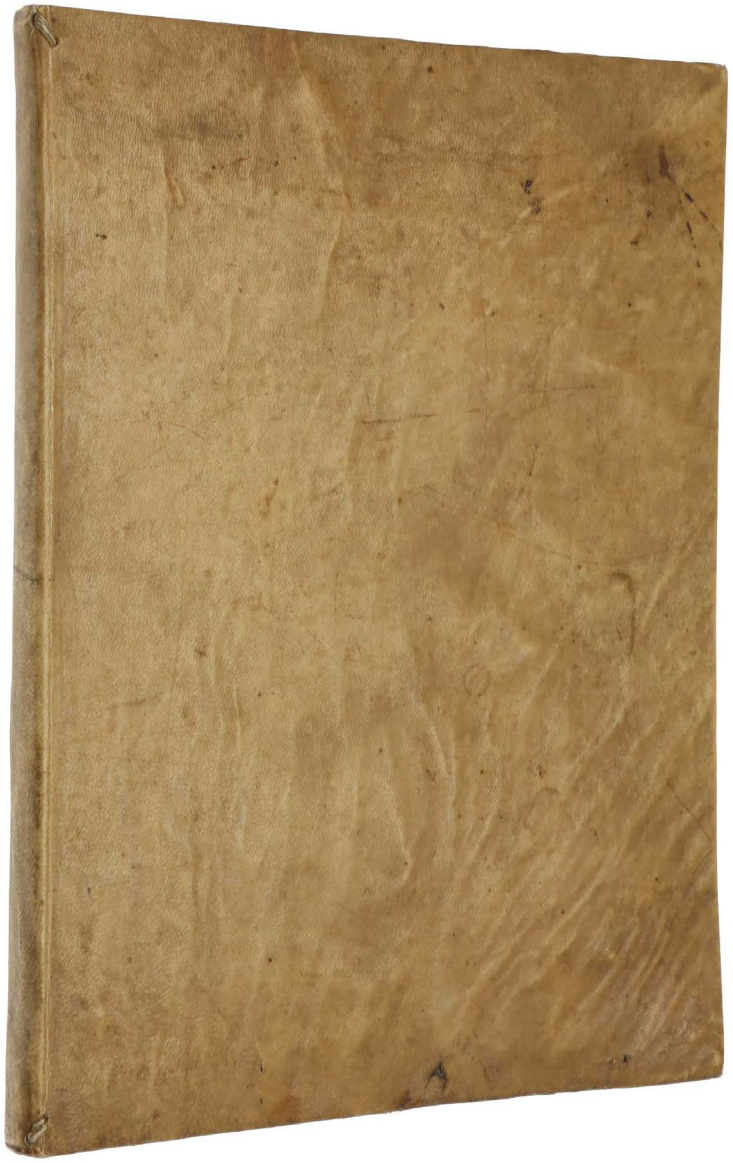
à Monsieur Huggyens de Zulichem, en lui envoyant La Dimension des Lignes de toutes sortes de Roulettes, lesquelles il montre être égales à des Lignes Elliptiques], 7 (Dimension des Lignes Courbes de toutes les Roulettes); [2, part title dated 1658 – Lettre de A. Dettonville à Monsieur De Sluze Chanoine de la Cathédrale du Liège, en lui envoyant la Dimension & le Centre de gravité de l'Escalier. La Dimension & le Centre de gravité des Triangles Cylindriques. La Dimension d'un Solide formé par le moyen d'une Spirale autour d'un Cone], 8 (De l'Escalier, des Triangles Cylindriques, & de la Spirale au tour d'un Cone; Pour la Dimension et le Centre de gravite de l'Escalier; Pour la Dimension et le Centre de gravite des Triangles Cylindriques; Dimension d'un Solide Forme par le moyen d'une Spirale autour d'un Cone); [2, part title dated 1658 – Lettre de A. Dettonville a Monsieur A. D. D. S. en lui envoyant La Démonstration à la maniere des Anciens de l'Égalité des Lignes Spirale & Parabolique], 16 (Égalité des Lignes Spirale & Parabolique). With 4 folding engraved plates of geometrical diagrams.

First edition, extremely rare (one of about 120 copies printed), of one of Pascal's most brilliant works. "Édition originale, extrêmement rare, de la dernière œuvre de Pascal, l'une des plus éclatantes de son génie" (Lucien Scheler in Tchemerzine, V, pp. 54-55). "Pascal devoted himself during 1658 and the first part of 1659 to the perfection of the "theory of indivisibles," an ancestor of the integral calculus. In 1658, after using the method of indivisibles to solve several infinitesimal problems relating to the cycloid, he proposed the problems he had solved as a challenge to other mathematicians, then announced his own superior solutions in four letters published in December 1658 and January 1659 under the pseudonym A. Dettonville. These pamphlets were collected in February 1659 under the above title. The structure of this work is very complex, with the first letter (Lettre de A. Dettonville à Monsieur de Carcavy) containing five separately paginated sections and the remaining three letters (Lettre de A. Dettonville à Monsieur A. D. D. S. …, Lettre de A. Dettonville à Monsieur De Sluze …, and Lettre de A. Dettonville



à Monsieur Huggyens ([i.e., Huygens]) appearing in inverse order of their composition" (Norman). In a diagram (fig. 26) in the treatise Traité des Sinus du quart de Cercle, Pascal introduced what Leibniz later called the 'characteristic triangle' and used to establish the differential calculus. Pascal made us of it "to determine the sum of the sines (ordinates) of a portion of the curve, that is, the area under this portion. If Pascal had at this point only been more interested in arithmetic considerations and in the problem of tangents, he might have anticipated the important concept of the limit of a quotient and have discovered the significance of this for the determination of both tangents and quadratures. Had he done this, he would have hit upon the crucial point in the calculus some seven years before Newton and about fourteen years before Leibniz" (Boyer, p. 153). "Cette édition originale ne fut donc tirée qu'à 120 exemplaires, le tirage classique de l'époque étant d'environ 3000" ('Mémoires sur la vie de M. Pascal par Marguerite Périer, sa nièce,' p. 40 in Pascal, Œuvres complètes, Bibliothèques de la Pléiade, N.R.F., 1957 and Tchémerzine, V, 55). ABPC/RBH list six copies, those of Pierre Berge, Macclesfield, Norman, Bute, Honeyman and that of Jean Jacques Amelot, Seigneur de Chaillou (1689-1749). The Berge copy made $118,262 in 2015 ("contemporary vellum, loss to lower board, plates reinforced at folds, second plate detached"). Apart from these copies, we know of two that were sold in the French trade since 2000, a copy in contemporary vellum in 2005 for €295,000 and another in 2008 for €250,000. Our copy is complete with all four part-titles and the two-page Lettre de Monsieur de Carcavy à Monsieur Dettonville, some of which are often lacking (e.g., the Bute copy had only the first of the four part-titles). OCLC lists three copies in US (Harvard, NYPL, Yale).

According to the testimony of Gilberte Périer, his sister, it was to forget very painful toothache that in 1657 Pascal suddenly resumed his mathematical research, interrupted since his religious conversion late in 1654. According to Pascal himself, the solution to the problem of finding the area of a cycloid (the

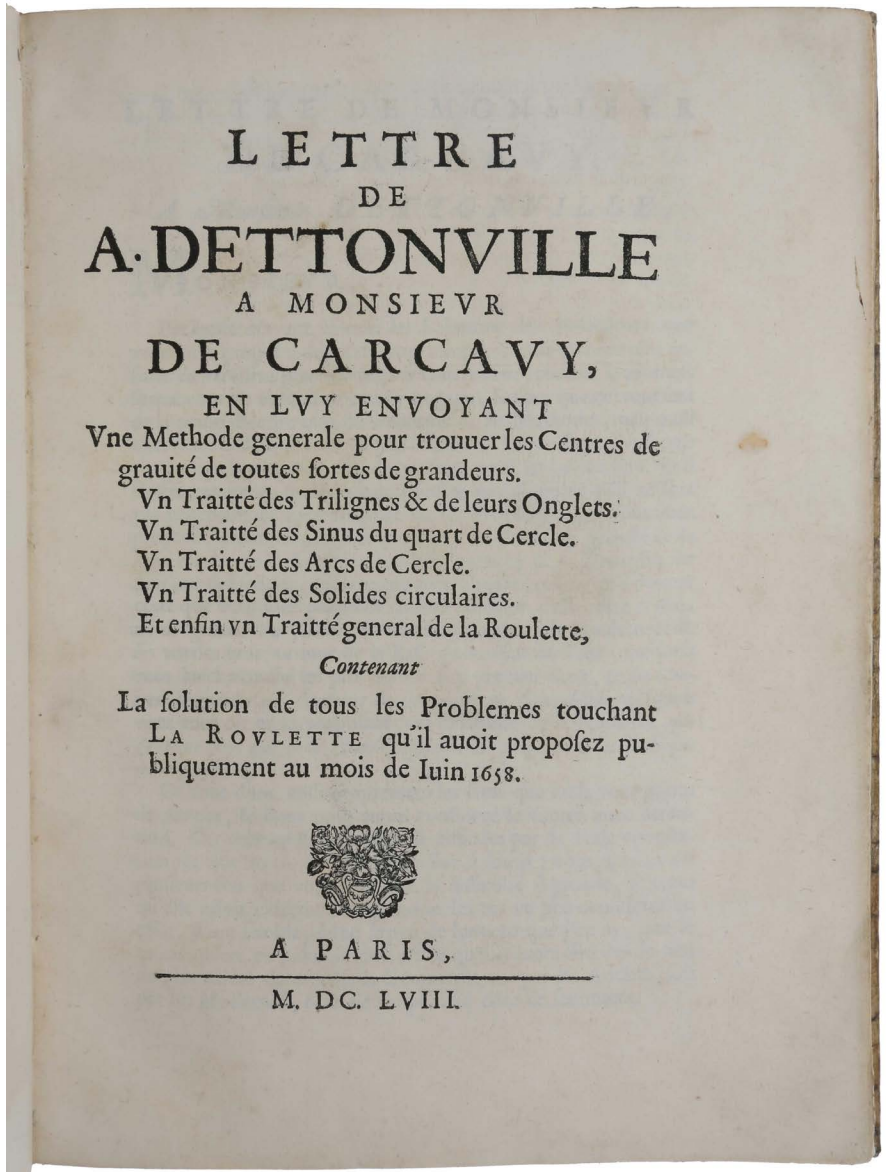


path travelled by a point on a circle as it rolls along a plane) came to him in his sleep. Initially he wrote nothing of this discovery as he regarded it as a distraction from his work on religion, but his friend the Duke of Roannez pointed out that God may have provided this vision to give more strength to his work against atheists and libertines, because by showing them the depth of his genius they would be less likely to challenge his proofs of religious doctrine.

"During 1658 and the first months of 1659 Pascal devoted most of his time to perfecting the "theory of indivisibles," a forerunner of the methods of integral calculus. This new theory enabled him to study problems involving infinitesimals: calculations of areas and volumes, determinations of centers of gravity, and rectifications of curves.

"From the end of the sixteenth century many authors, including Stevin (1586), L. Valerio (1604), and Kepler (1609 and 1615), had tried to solve these fundamental problems by using simpler and more intuitive methods than that of Archimedes, which was considered a model of virtually unattainable rigor. The publication in 1635 of Cavalieri's Geometria marked the debut of the method of indivisibles; its principles, presentation, and applications were discussed and elaborated in the later writings of Cavalieri (1647 and 1653) and in those of Galileo (1638), Torricelli (1644), Guldin (1635–1641), Gregory of Saint-Vincent (1647), and A. Tacquet (1651). (The research of Fermat and Roberval on this topic remained unpublished.) The method, which assumed various forms, constituted the initial phase of development of the basic procedures of integral calculus, with the exception of the algorithm.

"Pascal first referred to the method of indivisibles in a work on arithmetic of 1654, "Potestatum numericarum summa." He observed that the results concerning the summation of numerical powers made possible the solution of certain quadrature



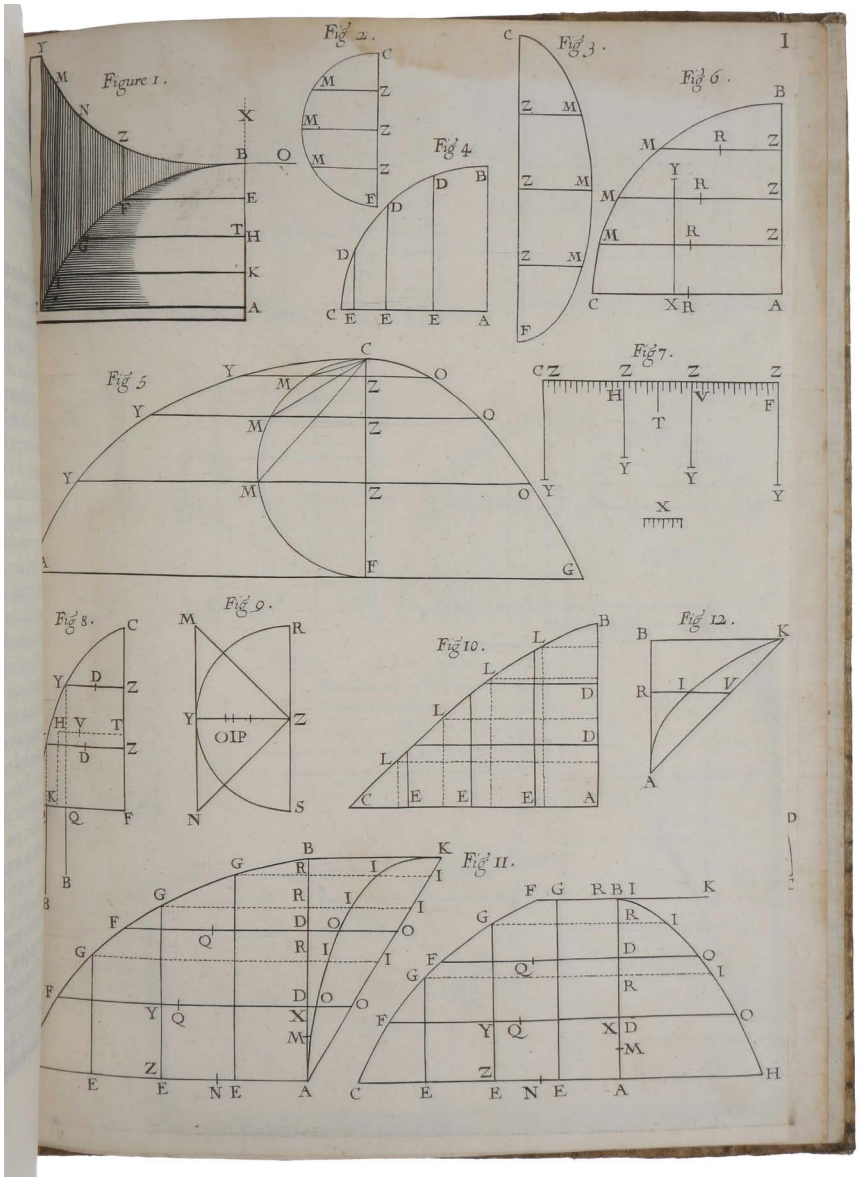
LETTRE
DE
A·DETTONVILLE
A MONSIEVR
DE CARCAVY,
EN LVY ENVOYANT
Vne Methode generale pour trouuer les Centres de grauité de toutes ſortes de grandeurs.
Vn Traitté des Trilignes & de leurs Onglets.
Vn Traitté des Sinus du quart de Cercle.
Vn Traitté des Arcs de Cercle.
Vn Traitté des Solides circulaires.
Et enfin vn Traitté general de la Roulette,
Contenant
La ſolution de tous les Problemes touchant LA ROVLETTE qu'il auoit propoſez publiquement au mois de Iuin 1658.

A PARIS,
M. DC. LVIII.

problems. As an example he stated a known result concerning the integral of xn for whole number n, in modern notation. This arithmetical interpretation of the theory of indivisibles permitted Pascal to give a sufficiently precise idea of the order of infinitude and to establish the natural relationship between "la mesure d'une grandeur continue" and "la sommation des puissances numériques." In the fragment "De l'esprit géométrique", composed in 1657, he returned to the notion of the indivisible in order to specify its relationship to the notions of the infinitely small and of the infinitely large and to refute the most widespread errors concerning it.

"At the beginning of 1658 Pascal believed that he had perfected the calculus of indivisibles by refining his method and broadening its field of application. Persuaded that in this manner he had discovered the solution to several infinitesimal problems relating to the cycloid or roulette, he decided to challenge other mathematicians to solve these problems. Although rather complicated, the history of this contest is worth a brief recounting because of its important repercussions during a crucial phase in the birth of infinitesimal calculus. In an unsigned circular distributed in June 1658, Pascal stated the conditions of the contest and set its closing date at 1 October. In further unsigned circulars and pamphlets, issued between July 1658 and January 1659, he modified or specified certain of the conditions and announced the results. He also responded to the criticism of some participants and sought to demonstrate the importance and the originality of his own solutions.

"Most of the leading mathematicians of the time followed the contest with interest, either as participants (A. de Lalouvère and J. Wallis) or as spectators working on one or several of the questions proposed by Pascal or on related problems—as did R. F. de Sluse, M. Ricci, Huygens, and Wren. Their solutions having been judged incomplete and marred by errors, Lalouvére and Wallis were eliminated.



Their heated reactions to this decision were partially justified by the bias it displayed and the commentaries that accompanied it. This bias was the source of intense polemics concerning, in particular, the importance of Torricelli's original contribution. At the end of the contest Pascal published his own solutions to some of the original problems and to certain problems that had been added in the meantime. In December 1658 and January 1659 he brought out, under the pseudonym A. Dettonville, four letters setting forth the principles of his method and its applications to various problems concerning the cycloid, as well as to such questions as the quadrature of surfaces, cubature of volumes, determination of centers of gravity, and rectification of curved lines. In February 1659 these four pamphlets were collected in Lettres de A. Dettonville contenant quelques-unes de ses inventions de géométrie ….

"This publication of some 120 pages has a very complex structure. The first of the Lettres consists of five sections with independent paginations, and the three others appear in inverse order of their composition. Thus only by returning to the original order is it possible to understand the logical sequence of the whole, follow the development of Pascal's method, and appreciate the influence on it of the new information he received and of his progress in mastering infinitesimal problems.

"When he began the contest, Pascal knew of the methods and the chief results of Stevin, Cavalieri, Torricelli, Gregory of Saint-Vincent, and Tacquet; but he was not familiar with the bulk of the unpublished research of Roberval and Fermat. Apart from this information, and in addition to the arithmetical procedures that he applied, starting in 1654, to the solution of problems of the calculus of indivisibles, Pascal possessed a new method inspired by Archimedes. It was elaborated on a geometric foundation, its point of departure being the principle of the balance and the concepts of static moment and center of gravity. Pascal learned of the importance of the results obtained by Fermat and Roberval—notably in the study of the cycloid—at the time he issued his first circular. This information led him to modify the subject of the contest and to develop his own method further. Similarly, in August 1658, when he was informed of the result of the rectification of the cycloid, Pascal extended rectification to other arcs of curves and then undertook to determine the center of gravity of these arcs, as well as the area and center of gravity of the surfaces of revolution generated by their revolution about an axis. Consequently the Lettres present a method that is in continual development, appearing increasingly complex as it becomes more precise and more firmly based. The most notable characteristics of this work, which remained unfinished, are the importance accorded to the determination of centers of gravity, the crucial role of triangular sums and statical considerations, its stylistic rigor and elegance, and the use of a clear and precise geometric language that partially compensates for the absence of algebraic symbolism. Among outstanding contributions of the work are the discovery of the equality of curvature of the generalized cycloid and the ellipse; the deepening of the concept of the indivisible; a first step toward the concept of the definite integral and the determination of its fundamental properties; and the indirect recourse to certain methods of calculation, such as integration by parts.

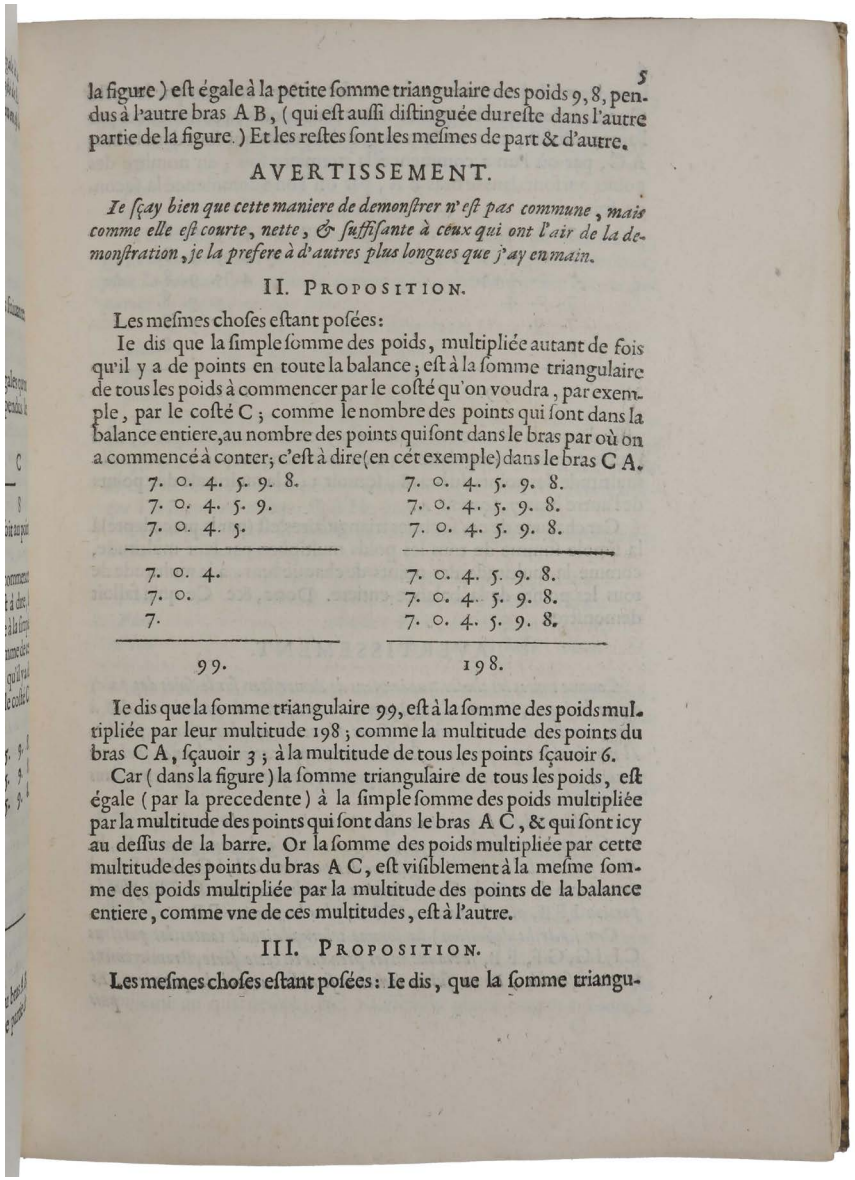
"Assimilated and exploited by Pascal's successors, these innovations contributed to the elaboration of infinitesimal methods. His most productive contribution, however, appears to have been his implicit use of the characteristic triangle. Indeed, Leibniz stated that Pascal's writings on the characteristic triangle were an especially fruitful stimulus for him. This testimony from one of the creators of infinitesimal calculus indicates that Pascal's work marked an important stage in the transition from the calculus of indivisibles to integral calculus" (DSB).

As noted in Sotheby's Macclesfield sale catalogue, the four works contained in this volume were separately printed, and the first item, Lettres … a Monsieur

Carcavy, was printed in two shops. The Lettre de M. de Carcavy, Lettre de M.D. a M. de C. and Traité générale de la roulette were printed in shop A. The Traité des trilignes rectangles, Traité des sinus, and Petit traité des solides circulaires were printed in shop B. Desprez was, as it were, the official publisher for Port Royal, and published the Pensées in 1670, the second edition, and what became the textus receptus in 1678 (see H.J. Martin, 'Guillaume Desprez, libraire de Pascal & de Port-Royal', Le Livre français sous l'ancien régime (Paris: Promodis, 1987), pp. 65-78).

Amos Dettonville is an anagram of Louis de Montalte, the pen name used by Pascal in Les Provinciales (1657).

Macclesfield 1599; Norman 1649; Sotheran, 1, 3475; Tchemerzine, V, pp. 54-55. Boyer, The History of the Calculus and its Conceptual Development, 1949.



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la figure) eſt égale à la petite ſomme triangulaire des poids 9, 8, pendus à l'autre bras A B, (qui eſt auſſi diſtinguée du reſte dans l'autre partie de la figure.) Et les reſtes ſont les meſmes de part & d'autre.

AVERTISSEMENT.

Ie ſçay bien que cette maniere de demonſtrer n'eſt pas commune, mais comme elle eſt courte, nette, & ſuffiſante à ceux qui ont l'air de la demonſtration, je la prefere à d'autres plus longues que j'ay en main.

II. PROPOSITION.

Les meſmes choſes eſtant poſées:

Ie dis que la ſimple ſomme des poids, multipliée autant de fois qu'il y a de points en toute la balance; eſt à la ſomme triangulaire de tous les poids à commencer par le coſté qu'on voudra, par exemple, par le coſté C; comme le nombre des points qui ſont dans la balance entiere, au nombre des points qui ſont dans le bras par où on a commencé à conter; c'eſt à dire (en cét exemple) dans le bras C A.

7. 0. 4. 5. 9. 8.	7. 0. 4. 5. 9. 8.
7. 0. 4. 5. 9.	7. 0. 4. 5. 9. 8.
7. 0. 4. 5.	7. 0. 4. 5. 9. 8.
7. 0. 4.	7. 0. 4. 5. 9. 8.
7. 0.	7. 0. 4. 5. 9. 8.
7.	7. 0. 4. 5. 9. 8.
99.	198.

Ie dis que la ſomme triangulaire 99, eſt à la ſomme des poids multipliée par leur multitude 198; comme la multitude des points du bras C A, ſçauoir 3; à la multitude de tous les points ſçauoir 6.

Car (dans la figure) la ſomme triangulaire de tous les poids, eſt égale (par la precedente) à la ſimple ſomme des poids multipliée par la multitude des points qui ſont dans le bras A C, & qui ſont icy au deſſus de la barre. Or la ſomme des poids multipliée par cette multitude des points du bras A C, eſt viſiblement à la meſme ſomme des poids multipliée par la multitude des points de la balance entiere, comme vne de ces multitudes, eſt à l'autre.

III. PROPOSITION.

Les meſmes choſes eſtant poſées: Ie dis, que la ſomme triangu-

PALERMO OBSERVATORY HANDBOOK

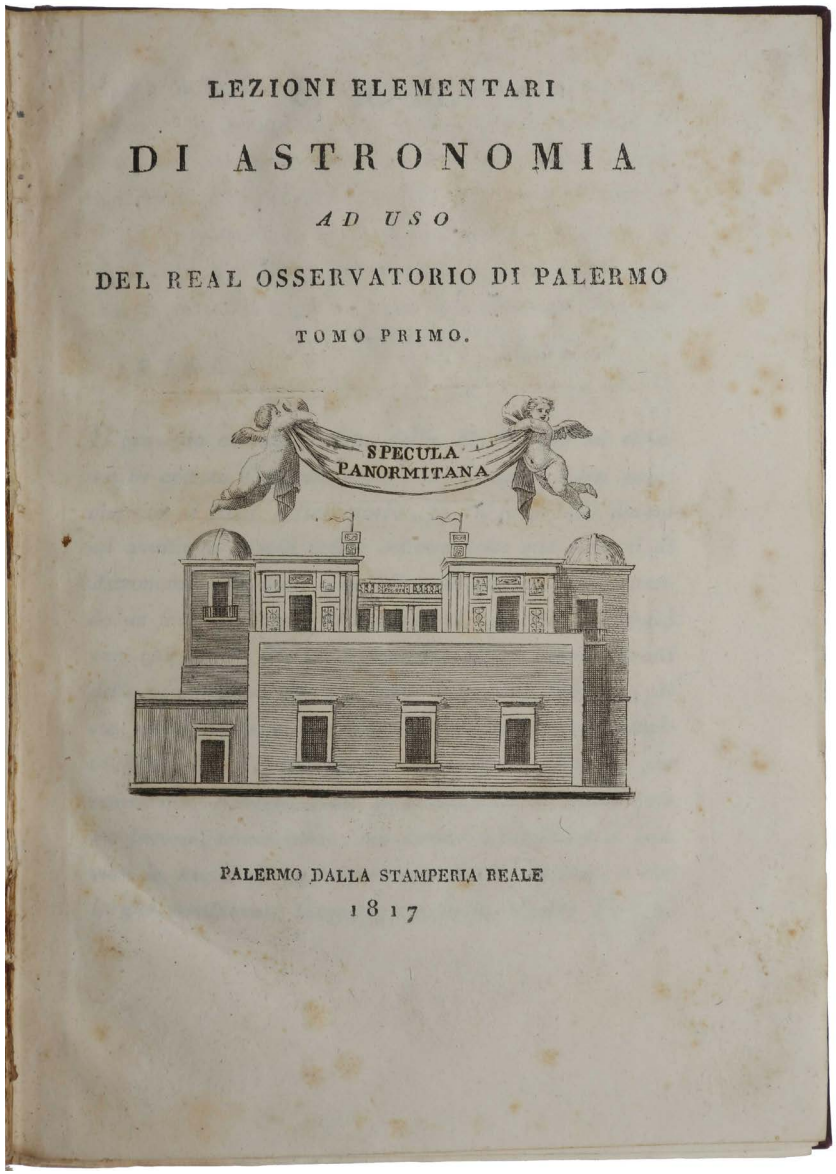
PIAZZI, Giuseppe. *Lezioni elementari di astronomia ad uso del Osservatorio d. Palermo.* Palermo, Stamperia Reale, 1817.

$4,500

Two vols., large 8vo, pp. xviii, [1], 240; xxvi, 446, with 11 engraved plates (9 folding). Engraved illustration of the observatory on each title. Contemporary half-calf.

First edition, very rare, of Piazzi's Palermo Observatory handbook. Having obtained a grant from the Viceroy of Sicily, Piazzi set up the observatory in 1789; as the southern-most European observatory, it offered unequalled access to the southern skies. Piazzi was able to acquire a great masterpiece of 18th century technology, the five-foot vertical circle completed for him by the English instrument maker Jesse Ramsden, for the observatory (illustrated on Tav. II of the present work). It was here that Piazzi discovered the first minor planet, Ceres, between the orbits of Mars and Jupiter. This, together with the great star catalogue he published at Palermo in 1803 (Praecipuarum Stellarum Inerrantium Positiones), listing 6,748 stars, established his reputation. Using this catalogue, he was able to show that the majority of stars exhibit proper motions relative to the Sun. The present work, a detailed technical handbook intended for the use of astronomers at the Palermo Observatory, became the leading astronomical textbook of the period, considered sufficiently important to be translated into German with a preface by Carl Friedrich Gauss (Lehrbuch der Astronomie, Berlin: 1822). This is a very rare book: no copies are listed on ABPC/RBH.

"Gioacchino Giuseppe Maria Ubaldo Nicolò Piazzi (1746-1826) was born in



LEZIONI ELEMENTARI

DI ASTRONOMIA

AD USO

DEL REAL OSSERVATORIO DI PALERMO

TOMO PRIMO.

SPECULA PANORMITANA

PALERMO DALLA STAMPERIA REALE

1817

Ponte, Valtellina, July 16, 1746, to one of the wealthiest families of the region. The penultimate of 10 sons, most of whom died as children, his parents worried about his health and for this reason quickly baptized him at home. The register of baptisms of St. Maurizio Church clearly specifies "ob imminens vitae periculum," or "because of impending danger of death".

"Following the tradition that encouraged younger children of wealthy and noble families to take holy orders, Giuseppe joined the Teatine order at the age of 19. We do not have firsthand documents about his early studies, but we know from documents preserved in the Archive of the Palermo Observatory that between 1770 and 1780 he was requested by his superiors to teach philosophy and mathematics in many different Italian cities, including Rome, Genoa, and Ravenna. In 1781, he was appointed to the Chair of Mathematics in the newly established Accademia dei Regi Studi of Palermo (which became the University of Palermo in 1806); a few years later, in 1787, he was named to the Chair of Astronomy even though he was not yet even an amateur astronomer. In a matter of only a few years, however, he was to become one of the most respected astronomers of his time.

"In March 1787, soon after he was charged with overseeing the construction of a new observatory at Palermo, Piazzi departed for a three-year stay at the major astronomical centers of Paris and London. During his travels he gained the esteem and friendship of some of the most reputed astronomers of the time, including Lalande, Messier, Mechain, Cassini, Maskelyne, and Herschel. Moreover, he succeeded in securing for the new observatory a unique instrument: the famous 5-foot circular-scale altazimuth telescope made by Jesse Ramsden of London. Returning to Palermo in November 1789, Piazzi was able, in a matter of months, to have the new observatory built on top of the tower of Santa Ninfa at the Royal Palace.

"Encouraged by the possession of the 5-foot Palermo Circle, whose accuracy was regarded to be much superior to that of any other existing instrument, Piazzi centered his scientific program on the accurate measurements of stellar positions. His observational technique required that each star had to be observed for at least four nights before its position could be established. This painstaking work resulted in the publication in 1803 of his first star catalog. For this highly regarded work, he was awarded the prize for mathematics and physics at the Institut National de France, Fondation Lalande, and was elected a fellow of the Royal Society. It was while working on this catalog that Piazzi, on January 1, 1801, unexpectedly discovered Ceres, the "missing planet" between the orbits of Mars and Jupiter" (Serio et al, pp. 17-18).

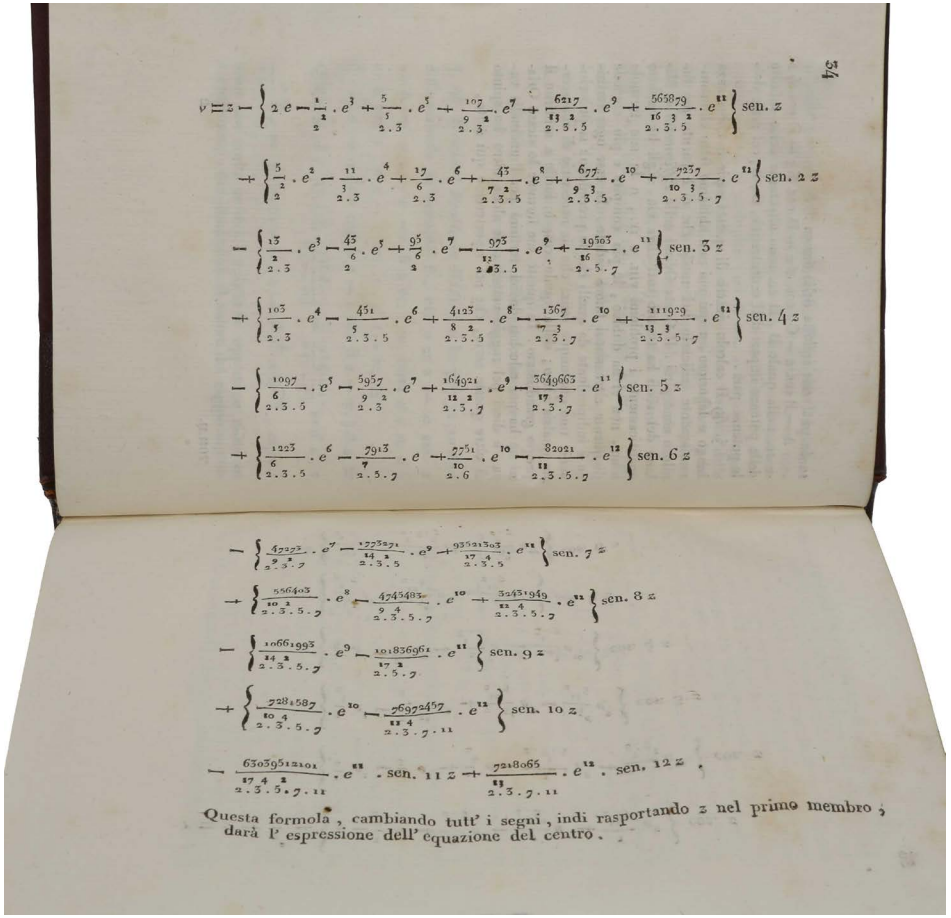
On the night of 1 January 1801, Piazzi was engrossed in updating a star catalogue by the English astronomer Francis Wollaston which was replete with inaccuracies. The catalogue had to be checked star by star, a task Piazzi was performing with a 1.5-metre vertical circle to determine star positions. At 8.43pm he saw a 'star' in Taurus that was not in the catalogue. The next night, he found that the star had shifted position about 4 minutes of arc to the west and slightly less to the north. He saw it again on 3 and 4 January, and continued following its movement until 11 February. His next step to notify the astronomical community of the discovery was to send letters to just two astronomers on 24 January, his closest friend Barnaba Oriani and the German astronomer Johann Bode. These letters gave no times of observation, a vague reference to a change of direction from retrograde to direct, and the wrong declination for 1 January. Piazzi was heavily criticized for not sharing his full set of observations with other astronomers, who were unable to locate the planet. Over the next few months Piazzi developed a corrected set of observational data, which he sent to the French astronomer Jérôme Lalande on 11 April. These new observations reached Baron Franz von Zach, editor of the world's only astronomical journal, on 6 June. They were finally published in the September issue of the Monatliche Correspondenz where, shortly after their appearance, they were seen by the 24-year old Gauss, who set himself the task of

computing the orbit of Ceres from Piazzi's observations. This he accomplished in a little more than a month, and on 7 December von Zach directed his telescope at the position predicted by Gauss and immediately observed the planet. This sensational result established Gauss's reputation as a universal genius.

After the Ceres affair, Piazzi undertook to determine the right ascension of a number of basic stars, relating them directly to the sun, in order to improve on earlier observations (including those made at Greenwich). Since he was at that time in poor health, he enlisted the aid of Niccolo Cacciatore as his collaborator. Piazzi's new star catalogue, published at Palermo in 1813, catalogued the mean position of 7,646 stars. It was widely esteemed among astronomers, and the Institut de France again awarded Piazzi a prize. Following the publication of the present wor in 1817, Piazzi was summoned to Naples by King Ferdinand I, who wished him to supervise the completion of the observatory already under construction on the hill at Capodimonte. He was appointed director general of the observatories of both Sicily and Naples, and Piazzi subsequently divided his time between the two. Piazzi returned to settle in Naples in 1824, his health weakened, and he died there two years later.

Piazzi's Lezioni consists of seven chapters: Vol. I: First observations and results; Basic facts of modern astronomy; On stars; Vol. II: Theory of the motion of the planets; The solar system; Eclipses; Comets. Detailed information about the discovery and orbit of Ceres is included in vol. II (pp. 198-204). Many problems with their solutions are included to assist the reader.

Foderà Serio, G., Manara, A. & Sicoli, P. 'Giuseppe Piazzi and the Discovery of Ceres', pp. 17-24 in W. F. Bottke Jr., A. Cellino, P. Paolicchi & R. P. Binzel. Asteroids III. Tucson, Arizona: University of Arizona Press, 2002. Houzeau & Lancaster 9275.



24

$$v = z - \left\{ 2e - \frac{1}{2}\cdot e^3 + \frac{5}{2.3}\cdot e^5 - \frac{107}{2.3^2}\cdot e^7 + \frac{6217}{2.5.5^2}\cdot e^9 + \frac{568879}{2.3.5^3}\cdot e^{11} \right\} \text{sen. } z$$

$$+ \left\{ \frac{5}{2}\cdot e^2 - \frac{11}{2.3}\cdot e^4 + \frac{17}{2.3^6}\cdot e^6 + \frac{43}{2.3.5^2}\cdot e^8 + \frac{677}{2.3.5^3}\cdot e^{10} + \frac{7237}{2.3.5.7^3}\cdot e^{12} \right\} \text{sen. } 2z$$

$$- \left\{ \frac{13}{2.3}\cdot e^3 - \frac{43}{2^6}\cdot e^5 + \frac{95}{2^6}\cdot e^7 - \frac{923}{2.3.5^2}\cdot e^9 + \frac{19563}{2.5.7^6}\cdot e^{11} \right\} \text{sen. } 3z$$

$$+ \left\{ \frac{103}{2.3^5}\cdot e^4 - \frac{451}{2.3.5^5}\cdot e^6 + \frac{4123}{2.3.5^8}\cdot e^8 - \frac{1367}{2.3.7^3}\cdot e^{10} + \frac{111929}{2.3.5.7^3}\cdot e^{12} \right\} \text{sen. } 4z$$

$$- \left\{ \frac{1097}{2.3.5^6}\cdot e^5 - \frac{5957}{2.3^9}\cdot e^7 + \frac{164921}{2.3.7^{12}}\cdot e^9 - \frac{3649663}{2.3.7^{17}}\cdot e^{11} \right\} \text{sen. } 5z$$

$$+ \left\{ \frac{1223}{2.3.5^6}\cdot e^6 - \frac{7913}{2.3.7^7}\cdot e + \frac{7751}{2.6^{10}}\cdot e^{10} - \frac{82021}{2.3.5.7^{12}}\cdot e^{12} \right\} \text{sen. } 6z$$

$$- \left\{ \frac{42223}{2.3^2.7^9}\cdot e^7 - \frac{1273321}{2.3.5^{14}}\cdot e^9 + \frac{93521303}{2.3.5^{17}}\cdot e^{11} \right\} \text{sen. } 7z$$

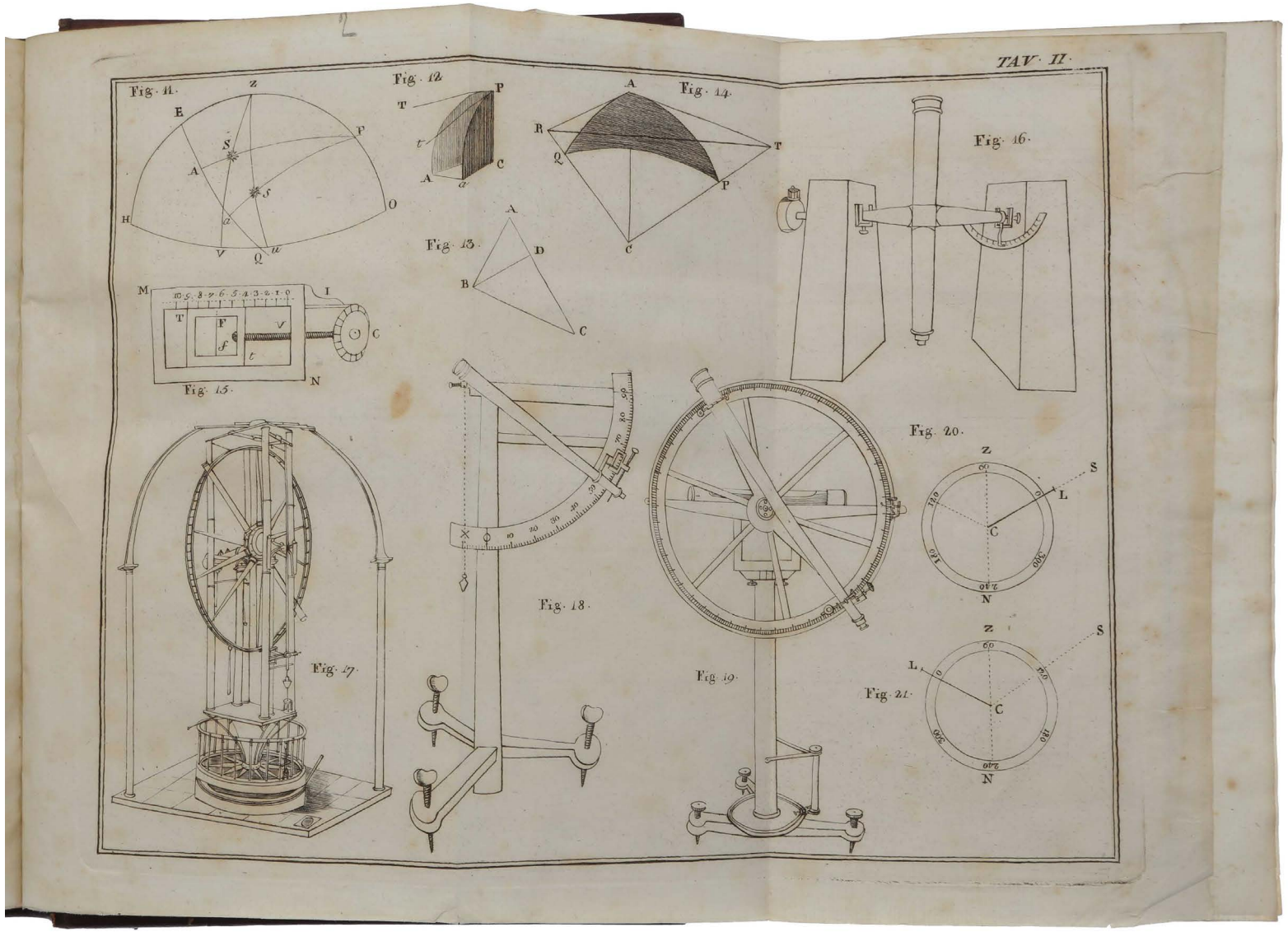
$$+ \left\{ \frac{556403}{2.3.5.7^{10}}\cdot e^8 - \frac{4745483}{2.3.5.7^9}\cdot e^{10} + \frac{32431949}{2.3.5.7^{22}}\cdot e^{12} \right\} \text{sen. } 8z$$

$$- \left\{ \frac{10661993}{2.3.5.7^{14}}\cdot e^9 - \frac{10183696i}{2.5.7^{17}}\cdot e^{11} \right\} \text{sen. } 9z$$

$$+ \left\{ \frac{7281587}{2.3.5.7^{10}}\cdot e^{10} - \frac{26972457}{2.3.7.11^{22}}\cdot e^{12} \right\} \text{sen. } 10z$$

$$- \frac{63039512101}{2.3.5.7.11^{17}}\cdot e^{11} \cdot \text{sen. } 11z + \frac{7218065}{2.3.7.11^{22}}\cdot e^{12} \cdot \text{sen. } 12z \,.$$

Questa formola, cambiando tutt' i segni, indi rasportando z nel primo membro, darà l' espressione dell' equazione del centro.


TAV. II.
Fig. 11.
Fig. 12.
Fig. 13.
Fig. 14.
Fig. 15.
Fig. 16.
Fig. 17.
Fig. 18.
Fig. 19.
Fig. 20.
Fig. 21.

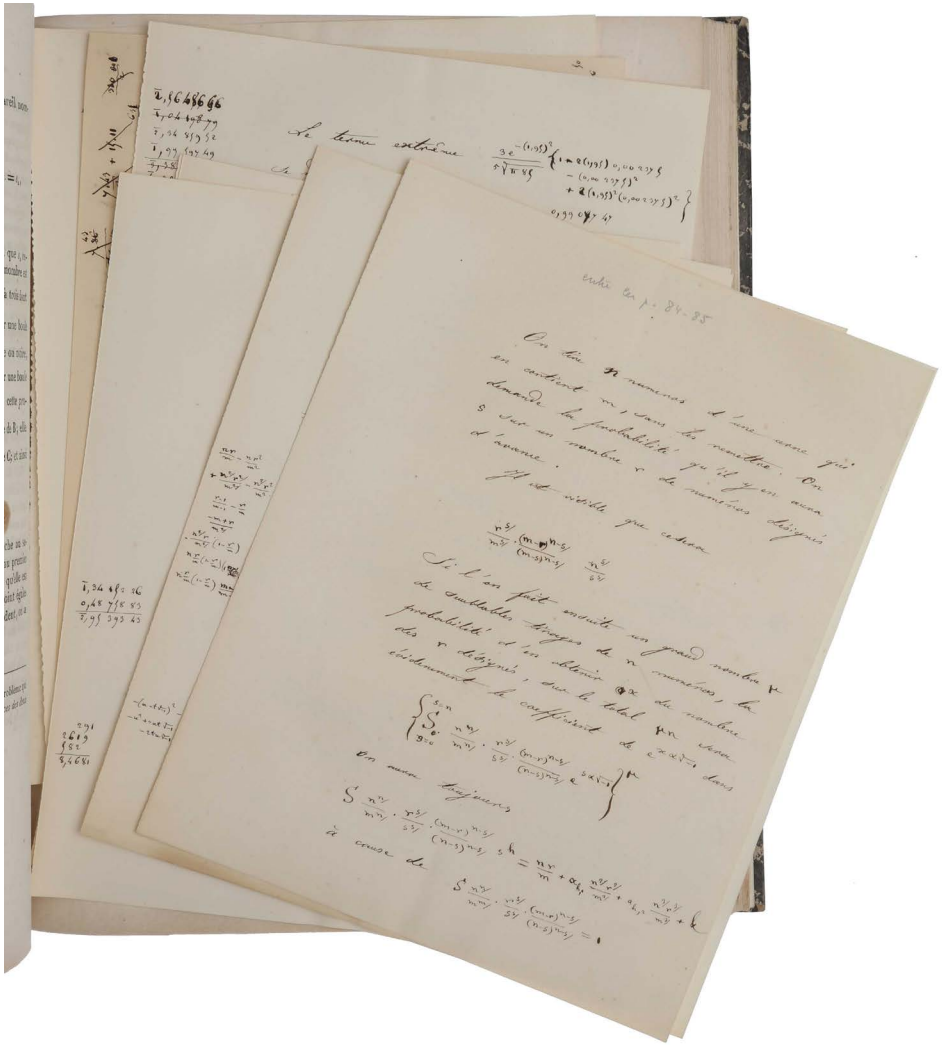
HEAVILY ANNOTATED BY ONE OF POISSON'S FIERCEST CRITICS

POISSON, Siméon-Denis. [BIENAYMÉ, Irenée-Jules]. *Recherches sur la Probabilité des Jugements en Matière criminelle et en Matière civile, précédées des Règles générales du Calcul des Probabilités.* Paris: Bachelier, 1837.

$5,500

4to (270 x 216 mm), pp. [iv], ix, [1], [2, errata], 415. Later half-calf and marbled boards, spine decorated in blind and ruled and lettered in gilt, marbled endpapers.

First edition, an extraordinary association copy, having belonged to the brilliant and highly original French statistician Jules Bienaymé, a fierce critic of Poisson, with more than 40 inserted pages of manuscript notes and mathematical proofs and calculations in his hand, as well as numerous marginal pencil annotations. Bienaymé published very little, making the mathematical manuscripts inserted in this volume particularly precious. "In Recherches sur la probabilité des jugements en matière criminelle et en matière civile (1837; "Research on the Probability of Criminal and Civil Verdicts"), an important investigation of probability, the Poisson distribution appears for the first and only time in his work [on p. 206]. Poisson's contributions to the law of large numbers (for independent random variables with a common distribution, the average value for a sample tends to the mean as sample size increases) also appeared therein. Although originally derived as merely an approximation to the binomial distribution (obtained by repeated, independent trials that have only one of two possible outcomes), the Poisson distribution is now fundamental in the analysis of problems concerning radioactivity, traffic, and the random occurrence of events in time or space"



(Britannica). "Recherches sur la probabilité des jugements is significant for the author's participation in an important contemporary debate. The legitimacy of the application of the calculus to areas relating to the moral order, that is to say within the broad area of what is now called the humanistic sciences, was bitterly disputed beginning in 1820 in politically conservative circles … Poisson was bold enough to take pen in hand to defend the universality of the probabilistic thesis and to demonstrate the conformability to the order of nature of the regularities that the calculus of probability, without recourse to hidden causes, reveals when things are subjected to a great number of observations" (DSB). "Poisson's major mathematical interests were in mechanics, mathematical analysis, and physics … It was towards the end of his life, roughly from 1835 on, that Poisson's interests began to focus on probability and statistics. His major work in this area, essentially a treatise on probability limit theory with applications to juridical statistics, appeared in 1837 [as the present work]. Bienaymé was later to dispute various results in this work, particularly a version of the Law of Large Numbers" (Heyde & Seneta, p. 5).

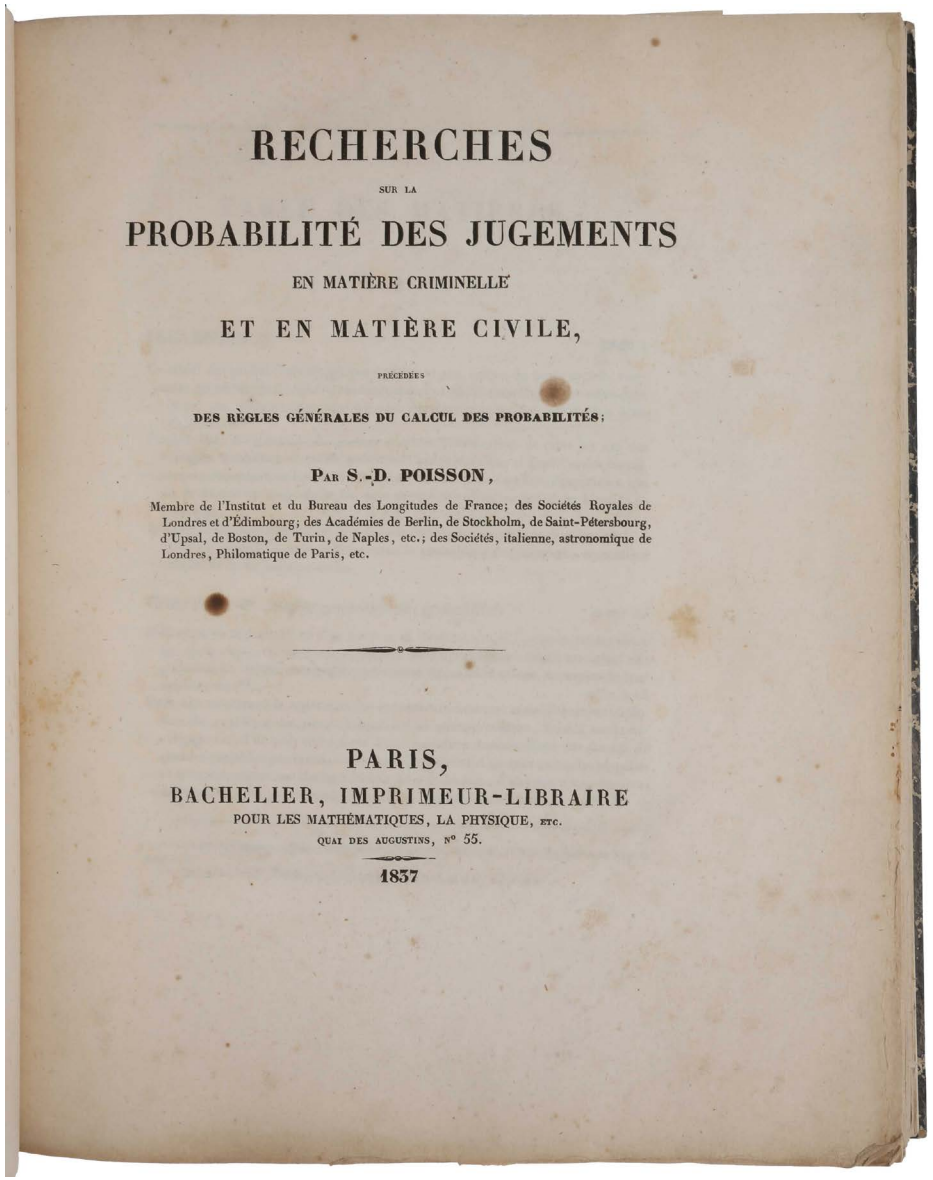
Provenance: Jules Bienaymé (1796-1878), with marginal annotations and inserted manuscript notes in his hand on pp. 7 (one page), 43 (8 pages), 85 (12 pages), 123 (2 pages), 205 (2 pages), 247 (4 pages), 251 (5 pages), 348 (one page), 377 (one page), 387 (5 pages), and 415 (2 pages). Bienaymé was under-appreciated until relatively recently, and published only 23 articles, many of which appeared in obscure journals. "Contributing to his being largely forgotten were the facts that Bienaymé was modest as regards his own achievements, made no great efforts to assert his priority, and was ahead of his time in mathematical statistics. He left no disciples, not being in academia; and wrote no book" (Seneta, p. 293). "Much of Bienaymé's work was concerned with extending or defending Laplacian positions, for example, in connection with least squares, judicial statistics, and insurance. He also worked along established lines in the areas of demography and social statistics. He did not lack innovative ability, however, and produced in particular the so-called Bienaymé-Chebyshev Inequality 15 years before Chebyshev, a recognition of the concept now known as sufficiency 80 years before its formulation by Fisher, and the Criticality Theorem for branching processes 28 years before its incomplete rediscovery by Galton and Watson" (Heyde & Seneta, p. 5).

"Poisson's major work on probability was a book, Recherches sur la probabilité des jugements, published in 1837. The book was in large part a treatise on probability theory after the manner of Laplace, with an emphasis on the behavior of means of large numbers of measurements. The latter portion (pp. 318-415) dealt with the subject matter of the title. Some of this material was taken from memoirs Poisson published in the two preceding years. Only a charitable modern reading could identify a new concept in the work; yet the book contains the germ of the two things now most commonly associated with the Poisson's name. The first of these is the probability distribution now commonly called the Poisson distribution. In a section of the book concerned with the form of the binomial distribution for large numbers of trials, Poisson does in fact derive this distribution in its cumulative form, as a limit to the binomial distribution when the chance of a success is very small. The distribution appears on only one page in all of Poisson's work [p. 206 of the present work]. The second most common appearance of Poisson's name in modern literature is in connection with a generalization of the Bernoulli law of large numbers" (Stigler, pp. 182-3).

Leibniz, Bernoulli, and Condorcet were among Poisson's predecessors in his work on judicial statistics, but the most significant was Laplace, who published in the first supplement (1816) to his Théorie analytique des Probabilités (1812) a calculation of the probability that a verdict will be correct as a function of the majority obtained. Laplace's formula became a source of argument when the jury system in France was in something of a state of flux. His reasoning was sharply

criticised by Poisson in 1835, who argued that any chain of reasoning in this type of problem should be based on observations consequent on the operation of the Law of Large Numbers. A year later he attacked the problem from its foundations and examined the texts of laws as well as court records. Poisson's reasoning was met with some scepticism but, in spite of this controversy, which even now is not entirely resolved, when Poisson summarized his work in Recherches sur la probabilité des jugements, it stimulated a surge of activity in the area. "This work, although designed as a contribution to juridical theory, contains so much important preliminary material of a purely probabilistic nature that it must be regarded as a work of probability theory with applications based on the legal system" (Heyde & Seneta, pp. 31-32).

Bienaymé entered this controversy with his paper ['Probabilité des jugements et des témoignages. Sur les erreurs de la method suivie dans le calcul de la probabilité des témoignages et des jugements,' Société Philomathique de Paris-Extraits, Ser. 5, 93-96], which was concerned with supporting Laplace's conclusions against criticisms from Arago. "Surprisingly, the paper itself does not explicitly mention Poisson, whose major work had appeared the preceding year. This is all the more noticeable since Bienaymé is concerned with commenting on the work of Laplace, whose conclusions had been rejected by Poisson. Bienaymé, intriguingly, does say in his last paragraph, however, that various (unspecified) reasons had prevented him from publishing his results earlier but that they had been communicated to various people, among whom was Liouville, who had mentioned them several times at a meeting of the Society … The paper also contains a comment that is relevant to his criticism of Poisson's ideas on the Law of Large Numbers: 'It is still understood here that all these variables are only arithmetical means of the opinions of all jurors. It can easily be shown that, as Jacob Bernoulli said, a multiplicity of causes produce the same effect as a single cause corresponding to the arithmetical mean of the possibilities which result from all these causes.'"



RECHERCHES

SUR LA

PROBABILITÉ DES JUGEMENTS

EN MATIÈRE CRIMINELLE

ET EN MATIÈRE CIVILE,

PRÉCÉDÉES

DES RÈGLES GÉNÉRALES DU CALCUL DES PROBABILITÉS;

PAR S.-D. POISSON,

Membre de l'Institut et du Bureau des Longitudes de France; des Sociétés Royales de Londres et d'Édimbourg; des Académies de Berlin, de Stockholm, de Saint-Pétersbourg, d'Upsal, de Boston, de Turin, de Naples, etc.; des Sociétés, italienne, astronomique de Londres, Philomatique de Paris, etc.

PARIS,

BACHELIER, IMPRIMEUR-LIBRAIRE

POUR LES MATHÉMATIQUES, LA PHYSIQUE, ETC.

QUAI DES AUGUSTINS, N° 55.

1837

Much of the inserted manuscript material in this volume appears to relate to Bienaymé's criticism of Poisson's ideas on the Law of Large Numbers. Two of the most substantial additions, at pages 247 and 251, are precisely where Poisson deduces the Law of Large Numbers from Laplace's central limit theorem for non-identically distributed summands (this occupies pp. 246-254 – see Stigler, p. 183). "In a discussion of the properties of relative frequencies Bienaymé ('Sur un principe que M. Poisson avait cru decouvrir et qu'il avait appele Loi des grand nombres,' Comptes Rendus de l'Académie des Sciences Morales et Politiques, Sér. 3, t. 11 (1855), 379-389) says that it is an illusion to think that Poisson's law of large numbers contains anything new, it is just a restatement of Bernoulli's theorem … He also criticizes the name of Poisson's law because of the vagueness of the term 'large numbers'. He points out that Poisson has shown that the number of observations necessary to obtain a confidence interval of given length may vary from one problem to another even if the average relative frequency is nearly the same" (Hald, p. 582). The extensive insertions at pages 43 and 85 appear to relate to what is now called the 'mixed binomial distribution,' which he published in 1839 ('Théorème sur la probabilité des résultats moyens des observations,' Société Philomathique de Paris-Extraits, Sér. 5, 42-44) – see Hald, pp. 580-581. In this paper, he "noted that the fluctuation of the mean statistical indicators was often greater than it should have been in accordance with the Bernoulli law, and he suggested a possible reason: some causes acting on the studied events, as he thought, remained constant within a given series of trials but essentially changed from one series to the next one" (Sheynin).

"Bienaymé was born in Paris on 28 August 1796, but began secondary education at the lycée in Bruges, then part of the French Empire. His father held a senior administrative position in the rule of Napoleon before moving to Paris in 1811 … Bienaymé entered the École Polytechnique in 1815, but the institution was closed in 1816, due to the fall of the Empire and the return of the Bourbons. With the death of his father in 1816, he entered the ministry of finances and rose to the rank of Inspector General in 1836. In 1819, seeking employment more suitable to his dreams of a scientific career, he applied for a teaching post in the special school attached to the St. Cyr military academy. He was placed first of five candidates, by Poisson himself, not without reservations, but his service there lasted only from 1 November 1819 to 10 February 1820, "not having found conditions compatible with his tastes and habits." This episode may be responsible, although it is not yet clear how, for Bienaymé's hostility to Poisson subsequently.

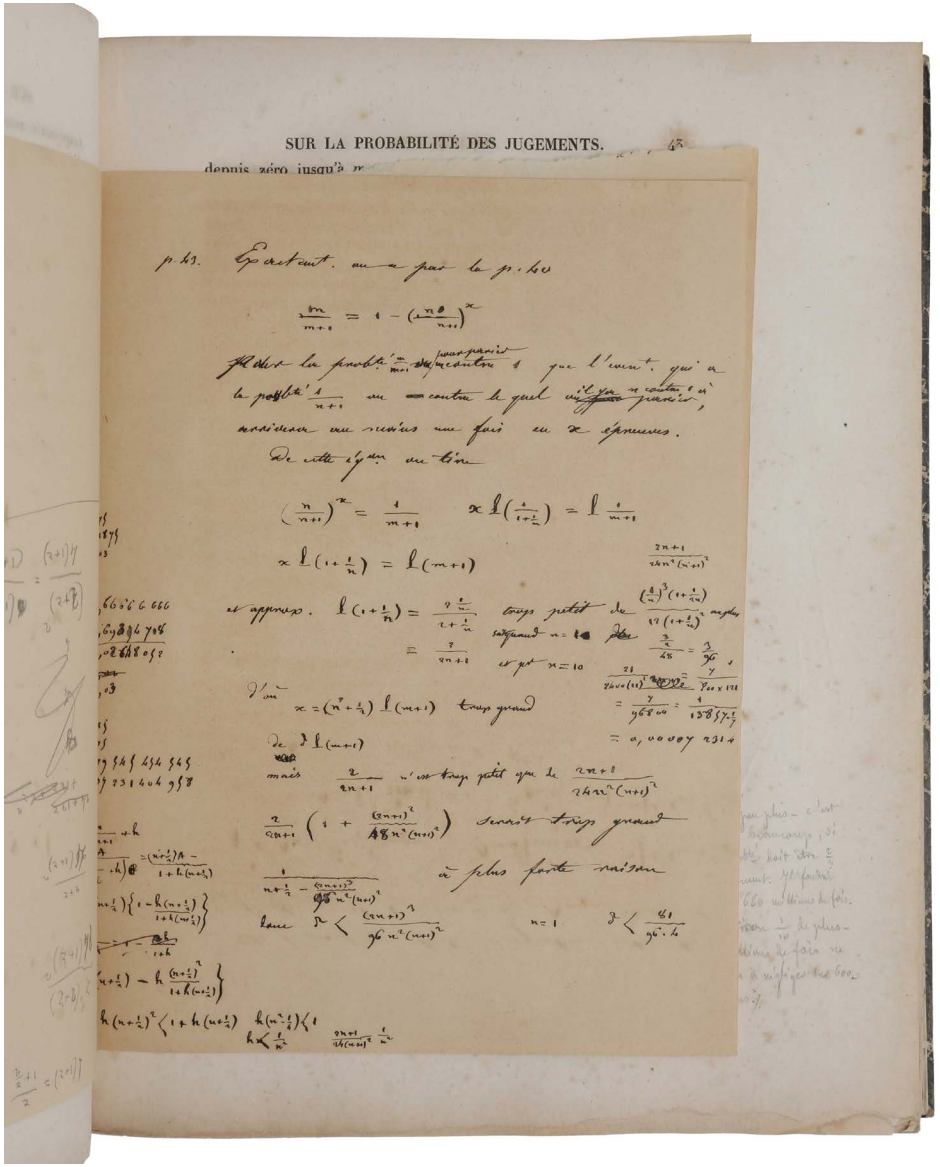
"Bienaymé was elected to the Société Philomathique de Paris in January 1838 and was active in its affairs. His contributions to its meetings were reported in the now-obscure newspaper-journal L'Institut, being reprinted at the end of the year in the collection Procès-Verbaux de la Société Philomathique de Paris-Extraits. Most of his publications in the period 1837 to 1845 appear in this medium, and are characterized, to the frustration of the reader, by lack of mathematical proofs for assertions sometimes far ahead of their time … In a letter to Quetelet of 21 April 1846, Bienaymé confides that his everyday work and the state of his health do nor permit him to complete the preparation of his writings for publication, and that he works seriously on applications which are of interest to both of them. His ill-health, especially his trembling hands, were to plague him to the end of his life.

"In 1848 Bienaymé lost his job in the Ministry of Finances for political reasons associated with changes of regime. Shortly afterwards he was asked to give some lectures on probability at the Faculté des Sciences, Paris. Again due largely to politics the Chair for probabilities was finally given to Lamé who began his course on 23 November 1850, and spoke in high praise of his friend Bienaymé at its recommencement on 26 April 1851. Bienaymé was reinstalled in August 1850 as "Inspecteur général des finances, chargé du service des retraites pour la vieillesse

et des sociétés des secours mutuels," but finally resigned in April 1852. He was elected as "académician libre" to the Paris Academy of Sciences in July 1852, and first met Pafnuty Lvovich Chebyschev (1821-94) on 12 October 1852.

"For Bienaymé, Laplace's Théorie analytique des Probabilités of 1812 was a guiding light (its dedication to Napoleon would have reinforced for him its scientific brilliance), and much of Bienaymé's work is concerned with elaborating, generalizing and defending Laplacian positions. He was passionate in the defence of scientific truth (as he perceived it), and of his friends such as Cournot, to the extent of attacking Cauchy, Poisson and J. Bertrand … [Bienaymé] died on 19 October 1878" (Seneta, pp. 292-3).

Dodge, The Concise Encyclopedia of Statistics (2008), p. 427. Hald, A History of Mathematical Statistics from 1750 to 1930 (1998). Stigler, The History of Statistics (1986). Heyde & Seneta, I J Bienaymé: Statistical theory anticipated (1977). Seneta, 'I J Bienaymé [1796-1878]: Criticality, Inequality, and Internationalization,' International Statistical Review 66 (1998), pp. 291-301. Sheynin, Theory of Probability. A Historical Essay (2009).



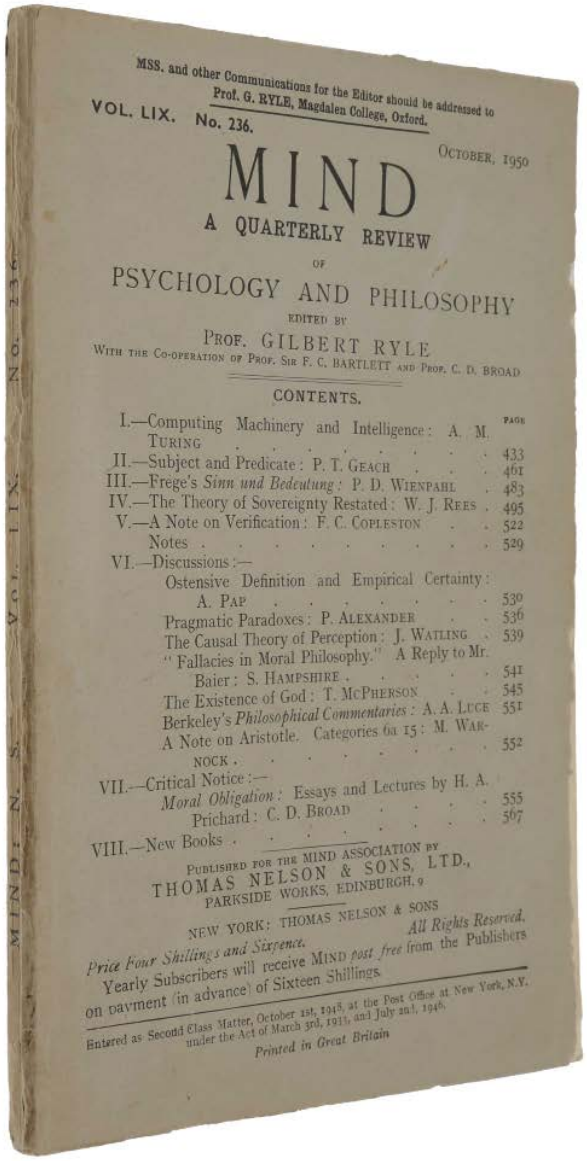
CAN MACHINES THINK?

TURING, Alan Mathison. *Computing Machinery and Intelligence.* Edinburgh: Thomas Nelson, 1950.

$4,500

Pp. 433-460 in: Mind, A Quarterly Review of Psychology and Philosophy, New Series, Vol. 59, No. 236, October, 1950. 8vo (213 x 140 mm), pp. [x], 433-576. Original printed wrappers (a few marginal chips and tears to the wrappers).

First edition, journal issue in original printed wrappers, of Alan Turing's landmark explanation of what would become known as the 'Turing test' to determine whether a machine can 'think'. In 'Computing Machinery and Intelligence,' "Turing sidestepped the debate about exactly how to define thinking by means of a very practical, albeit subjective, test: if a computer acts, reacts, and interacts like a sentient being, then call it sentient. To avoid prejudicial rejection of evidence of machine intelligence, Turing suggested the 'imitation game,' now known as the Turing test: a remote human interrogator, within a fixed time frame, must distinguish between a computer and a human subject based on their replies to various questions posed by the interrogator. By means of a series of such tests, a computer's success at 'thinking' can be measured by its probability of being misidentified as the human subject. Turing predicted that by the year 2000 a computer 'would be able to play the imitation game so well that an average interrogator will not have more than a 70-percent chance of making the right identification (machine or human) after five minutes of questioning.' No computer has come close to this standard" (Britannica). Together with 'On computable numbers', 'Computing machinery and intelligence' forms Turing's



MSS. and other Communications for the Editor should be addressed to Prof. G. RYLE, Magdalen College, Oxford.

VOL. LIX. No. 236. OCTOBER, 1950

MIND

A QUARTERLY REVIEW OF PSYCHOLOGY AND PHILOSOPHY

EDITED BY PROF. GILBERT RYLE

WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

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Printed in Great Britain

best-known work. This elegant and sometimes amusing essay was originally published in 1950 in the leading philosophy journal Mind. Turing's friend Robin Gandy (like Turing a mathematical logician) said that 'Computing Machinery and Intelligence' 'was intended not so much as a penetrating contribution to philosophy but as propaganda. Turing thought the time had come for philosophers and mathematicians and scientists to take seriously the fact that computers were not merely calculating engines but were capable of behaviour which must be accounted as intelligent; he sought to persuade people that this was so. He wrote this paper – unlike his mathematical papers – quickly and with enjoyment. I can remember him reading aloud to me some of the passages – always with a smile, sometimes with a giggle.' The quality and originality of 'Computing Machinery and Intelligence' have earned it a place among the classics of philosophy of mind" (Copeland, The Essential Turing, p. 433).

"'Computing Machinery and Intelligence' contains Turing's principal exposition of the famous 'imitation game' or Turing test … The imitation game involves three participants: a computer, a human interrogator, and a human 'foil'. The interrogator attempts to determine, by asking questions of the other two participants, which of them is the computer. All communication is via keyboard and screen, or an equivalent arrangement (Turing suggested a teleprinter link). The interrogator may ask questions as penetrating and wide-ranging as he or she likes, and the computer is permitted to do everything possible to force a wrong identification. (So the computer might answer 'No' in response to 'Are you a computer?' and might follow a request to multiply one large number by another with a long pause and a plausibly incorrect answer.) The foil must help the interrogator to make a correct identification.

"The ability to play the imitation game successfully is Turing's proposed 'criterion for thinking'. He gives two examples of the sort of exchange that might occur between an interrogator and a machine that plays successfully. The following is from pp. 446-7.

Interrogator: In the first line of your sonnet which reads 'Shall I compare thee to a summer's day', would not 'a spring day' do as well or better?

Machine: It wouldn't scan.

Interrogator: How about 'a winter's day?' That would scan all right.

Machine: Yes, but nobody wants to be compared to a winter's day.

Interrogator: Would you say Mr Pickwick reminded you of Christmas?

Machine: In a way.

Interrogator: Yet Christmas is a winter's day, and I do not think Mr Pickwick would mind the comparison.

Machine: I don't think you're serious. By a winter's day one means a typical winter's day, rather than a special one like Christmas.

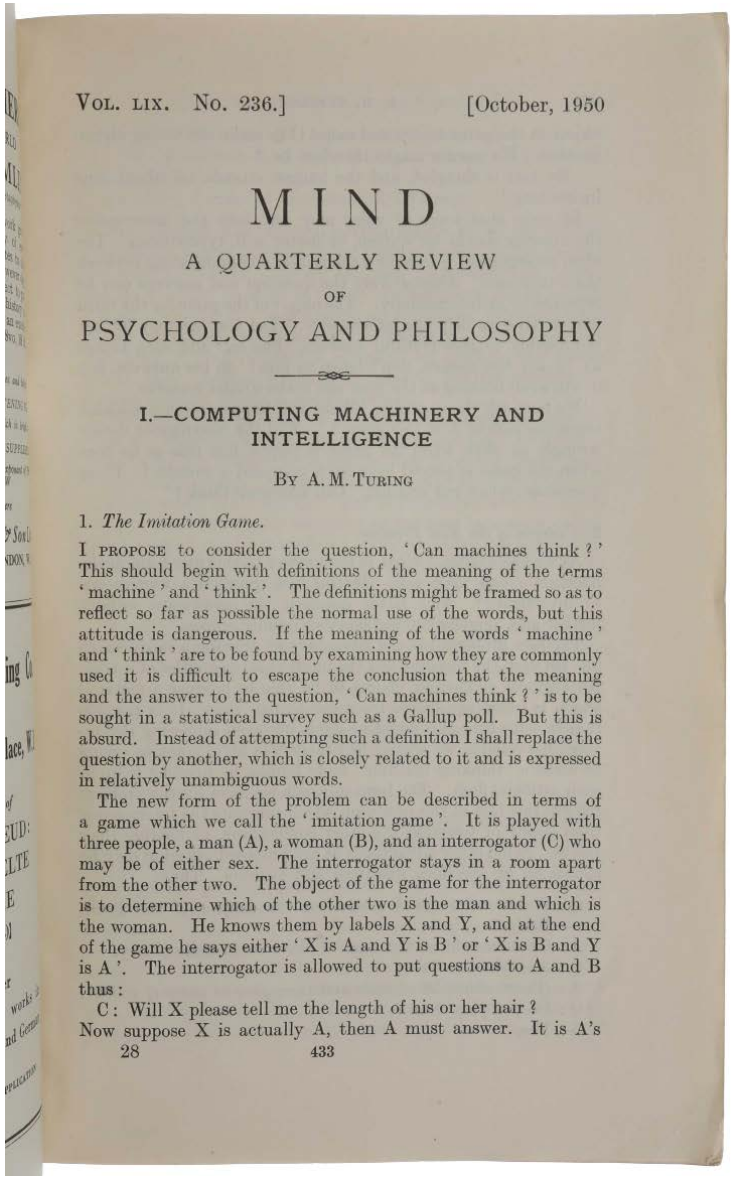
"Turing is sometimes said to have proposed a definition of 'thinking' or 'intelligence'; and sometimes his supposed definition is said to be an 'operational' or 'behaviourist' definition … There is no textual evidence to support this interpretation of Turing, however. In 'Computing Machinery and Intelligence' Turing claimed to be offering only a criterion for 'thinking' (emphasis added) … In fact, Turing made it plain in 'Computing Machinery and Intelligence' that his intention was not to offer a definition, for he said: 'The game may perhaps be criticised on the ground that the odds are weighted too heavily against the

machine. If the man were to try and pretend to be the machine he would clearly make a very poor showing. He would be given away at once by slowness and inaccuracy in arithmetic. May not machines carry out something which ought to be described as thinking but which is very different from what a man does?' (p. 442).

"A computer carrying out something that 'ought to be described as thinking' would nevertheless fail the Turing test if for any reason it stood out in conversation as very different from a man. It follows that 'thinking' cannot be defined in terms of success in the imitation game. Success in the game is arguably a sufficient condition for thinking; but success in the imitation game is not also a necessary condition for thinking. (Someone's breathing spontaneously is a sufficient condition for their being alive, but it is not also a necessary condition, for someone may be alive without breathing spontaneously.)

"Turing introduced his criterion for 'thinking' by first describing an imitation game involving a human interrogator and two human subjects, one male (A) and one female (B). The interrogator must determine, by question and answer, which of A and B is the man. A's object in the game is to try to cause the interrogator to make the wrong identification. Having introduced the imitation game in this way, Turing said: 'We now ask the question, 'What will happen when a machine takes the part of A in this game?' Will the interrogator decide wrongly as often when the game is played like this as he does when the game is played between a man and a woman? These questions replace our original, 'Can machines think?' (p. 434) …

"Section 6 of 'Computing Machinery and Intelligence', entitled 'Contrary Views on the Main Question', occupies nearly half of the article. It contains no fewer than nine objections to Turing's position, together with Turing's rebuttal of



Vol. lix. No. 236.] [October, 1950

MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—COMPUTING MACHINERY AND INTELLIGENCE

By A. M. Turing

1. *The Imitation Game.*

I propose to consider the question, 'Can machines think?' This should begin with definitions of the meaning of the terms 'machine' and 'think'. The definitions might be framed so as to reflect so far as possible the normal use of the words, but this attitude is dangerous. If the meaning of the words 'machine' and 'think' are to be found by examining how they are commonly used it is difficult to escape the conclusion that the meaning and the answer to the question, 'Can machines think?' is to be sought in a statistical survey such as a Gallup poll. But this is absurd. Instead of attempting such a definition I shall replace the question by another, which is closely related to it and is expressed in relatively unambiguous words.

The new form of the problem can be described in terms of a game which we call the 'imitation game'. It is played with three people, a man (A), a woman (B), and an interrogator (C) who may be of either sex. The interrogator stays in a room apart from the other two. The object of the game for the interrogator is to determine which of the other two is the man and which is the woman. He knows them by labels X and Y, and at the end of the game he says either 'X is A and Y is B' or 'X is B and Y is A'. The interrogator is allowed to put questions to A and B thus:

C: Will X please tell me the length of his or her hair?

Now suppose X is actually A, then A must answer. It is A's

28 433

each" (Copeland, pp. 433-6). The most famous of them, which Turing terms the 'Mathematical Objection', can be stated as follows: 'Recently the theorem of Gödel and related results … have shown that if one tries to use machines for such purposes as determining the truth or falsity of mathematical theorems and one is not willing to tolerate an occasional wrong result, then any given machine will in some cases be unable to give an answer at all. On the other hand the human intelligence seems to be able to find methods of ever-increasing power for dealing with such problems 'transcending' the methods available to machines'. This objection has become known over the years as the 'Gödel argument'.

"Turing says (p. 445): 'The short answer to this argument is that although it is established that there are limitations to the powers of any particular machine, it has only been stated, without any sort of proof, that no such limitations apply to the human intellect.' This remark might appear to cut to the heart of the matter. However, Turing expresses dissatisfaction with it, saying that the Mathematical Objection cannot 'be dismissed so lightly'. He goes on to broach a further line of attack on the argument, pointing out that humans 'often give wrong answers to questions'" (Copeland, p. 469).

"Section 7 of 'Computing Machinery and Intelligence' [is] entitled 'Learning Machines'. Turing poses the rhetorical question: 'Instead of trying to produce a programme to simulate the adult mind, why not rather try to produce one which simulates the child's?' (p. 456). The child's mind may contain 'so little mechanism' that 'something like it can be easily programmed'. If this child-machine 'were then subjected to an appropriate course of education one would obtain the adult brain' … Turing mentions in Section 7 that he has 'done some experiments with one such child-machine, and succeeded in teaching it a few things, but the teaching method was too unorthodox for the experiment to be considered really successful' (p. 457) … "AI [Artificial Intelligence] traditionally has attempted to build disembodied intelligences carrying out abstract activities – e.g. chess-playing and whose only way of interacting with the world is by means of a screen or printer. An alternative approach now called 'situated AI' aims at building embodied intelligences situated in the real world. 'Computing Machinery and Intelligence' ends with a characteristically farsighted statement in which Turing sketches each of these two approaches to AI. He contrasts research that focuses on 'abstract activity, like the playing of chess' with research aiming 'to provide the machine with the best sense organs that money can buy, and then teach it to understand and speak English' (p. 460). Turing recommended that 'both approaches should be tried' (ibid.)" (Copeland, pp. 438-9).

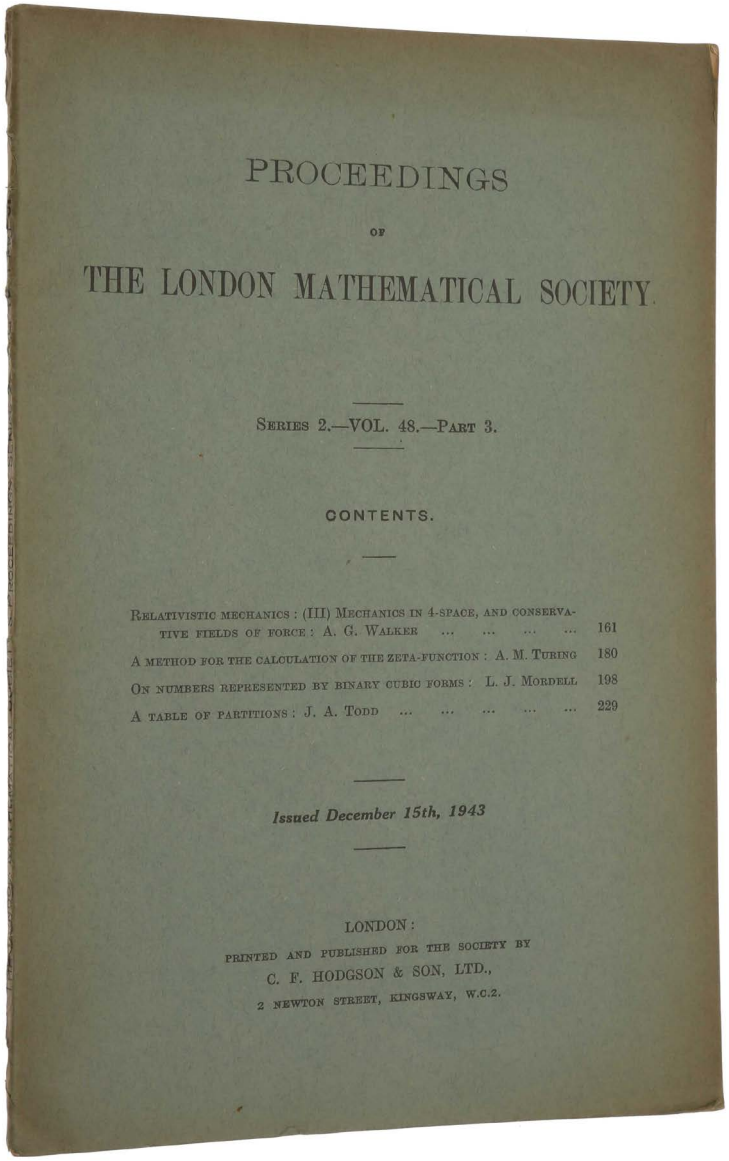
TURING DESIGNS A COMPUTER TO CHECK THE RIEMANN HYPOTHESIS

TURING, Alan Mathison. *A Method for the Calculation of the Zeta-Function.* London: C. F. Hodgson and Son, 1943.

$7,500

Pp. 180-197 in Proceedings of the London Mathematical Society, Series 2, Vol. 48, Part 3, December 15, 1943. Large 8vo (260 x 172 mm), pp. 161-240. Original printed wrappers (tiny chip from upper right corner of front wrapper, edges of wrappers lightly browned).

First edition, journal issue in the original printed wrappers, of Turing's ground-breaking work outlining a method (which he hoped could be performed by a machine) to decide the most famous open problem in mathematics, the so-called Riemann hypothesis. This is a conjecture about the location of the zeros of the 'Riemann zeta function' – it asserts that, apart from some 'trivial' zeros, they all lie on a certain 'critical line.' If true, this would have enormous implications for the study of prime numbers. Turing had worked on the zeta function since 1939 and he aimed to calculate the zeros using a mechanical computer. "The Turing archive contains a sketch of a proposal, in 1939, to build an analog computer that would calculate approximate values for the Riemann zeta-function on the critical line. His ingenious method was published in 1943 [as the present work]" (Downey, p. 11). Although he received a grant to build his zeta-function machine, the outbreak of World War II and Turing's role in it as cryptanalyst postponed the work, and it was not until four years later that he was actually able to publish his first paper on this important subject. Rare on the market in unrestored original



PROCEEDINGS

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SERIES 2.—VOL. 48.—PART 3.

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printed wrappers (we know of only one copy at auction, in the Weinreb Computer Collection, Bloomsbury Book Auctions, 28 October 1999).

The Riemann zeta function is defined as the sum of an infinite series

$\zeta(s) = 1/1^s + 1/2^s + 1/3^s + 1/4^s + \ldots..$

This actually makes sense when s is any complex number (except s = 1, when the sum is infinite). It is known that $\zeta(s) = 0$ when $s = -2, -4, -6, \ldots$ – these are called the 'trivial zeros'. The Riemann hypothesis (RH) is the assertion that all the non-trivial zeros are complex numbers of the form $s = ½ + t\sqrt{-1}$, where t is a real number – these complex numbers form a line in the complex plane, called the 'critical line'. The RH, first put forward by Bernhard Riemann in 1859, is known to be true for the first 1013 non-trivial zeros, but remains unproven. "The RH is widely regarded as the most famous unsolved problem in mathematics. It was one of the 23 famous problems selected by [David] Hilbert in 1900 as among the most important in mathematics, and it is one of the seven Millennium Problems selected by the Clay Mathematics Institute in 2000 as the most important for the 21st century" (Hedjhal & Odlyzko, p. 266).

"The first computations of zeros of the zeta function were performed by Riemann, and likely played an important role in his posing of the RH as a result likely to be true. His computations were carried out by hand, using an advanced method that is known today as the Riemann-Siegel formula. Both the method and Riemann's computations that utilized it remained unknown to the world-at-large until the early 1930s, when they were found in Riemann's unpublished papers by C. L. Siegel … In the mid-1930s, after Siegel's publication of the Riemann-Siegel formula, [the Oxford mathematician] E. C. Titchmarsh obtained a grant for a larger computation. With the assistance of L. J. Comrie, tabulating machines, some 'computers' (as the mostly female operators of such machinery were called in those days), and the recently published algorithm, Titchmarsh established that the 1041 nontrivial zero with $0 < t < 1468$ all satisfied the RH" (ibid., p. 268).

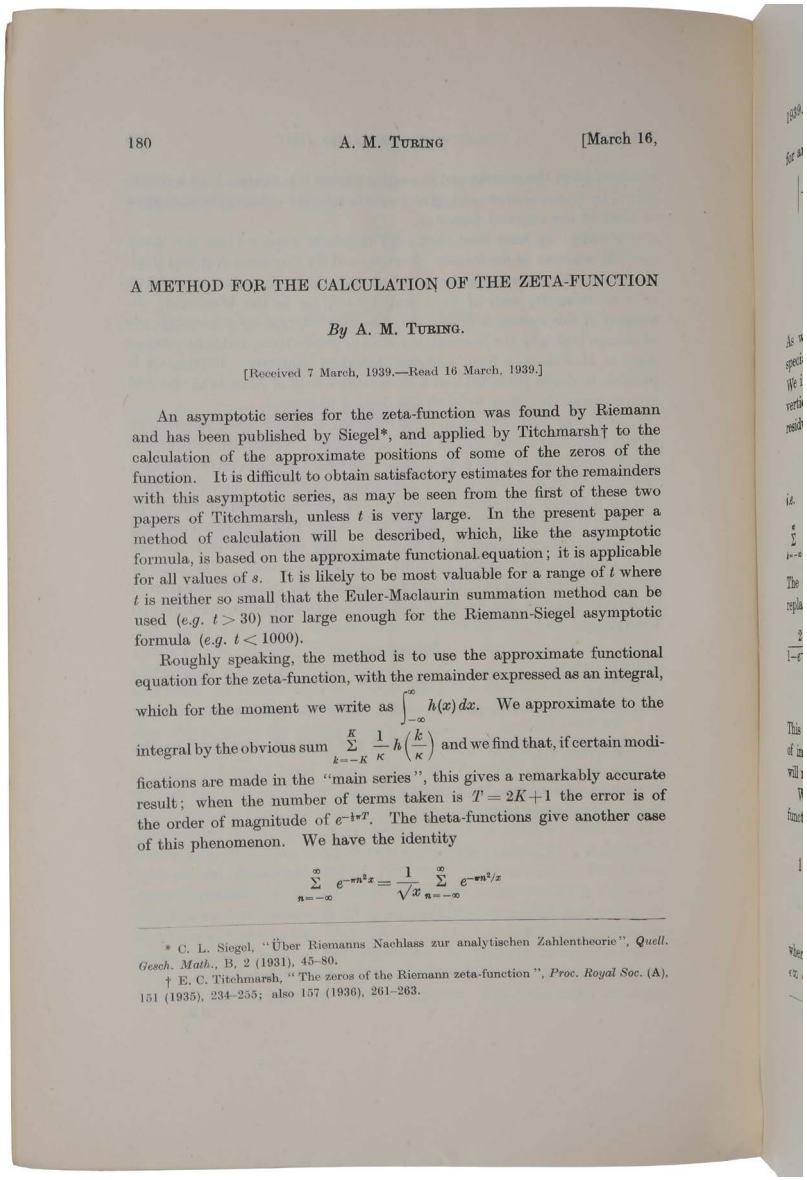
"Turing encountered the Riemann zeta function as a student, and developed a life-long fascination with it. Though his research in this area was not a major thrust of his career, he did make a number of pioneering contributions" (ibid., p. 266). "Apparently he had decided that the Riemann hypothesis was probably false, if only because such great efforts had failed to prove it. Its falsity would mean that the zeta-function did take the value zero at some point which was off the special line, in which case this point could be located by brute force, just by calculating enough values of the zeta-function … There were two aspects to the problem. Riemann's zeta-function was defined as the sum of an infinite number of terms, and although this sum could be re-expressed in many different ways, any attempt to evaluate it would in some way involve making an approximation. It was for the mathematician to find a good approximation, and to prove that it was good: that the error involved was sufficiently small. Such work did not involve computation with numbers, but required highly technical work with the calculus of complex numbers. Titchmarsh had employed a certain approximation which – rather romantically – had been exhumed from Riemann's own papers at Göttingen where it had lain for seventy years. But for extending the calculation to thousands of new zeroes a fresh approximation was required; and this Alan set out to find and to justify.

"The second problem, quite different, was the 'dull and elementary' one of actually doing the computation, with numbers substituted into the approximate formula, and worked out for thousands of different entries. It so happened that the formula was rather like those which occurred in plotting the positions of the planets, because it was of the form of a sum of circular functions with different frequencies. It was for this reason that Titchmarsh had contrived to

have the dull repetitive work of addition, multiplication, and of looking up of entries in cosine tables done by the same punched-card methods that were used in planetary astronomy. But it occurred to Alan that the problem was very similar to another kind of computation which was also done on a large practical scale – that of tide prediction. Tides could also be regarded as the sum of a number of waves of different periods: daily, monthly, yearly oscillations of rise and fall. At Liverpool there was a machine which performed the summation automatically, by generating circular motions of the right frequencies and adding them up. It was a simple analogue machine; that is, it created a physical analogue of the mathematical function that had to be calculated. This was a quite different idea from that of the Turing machine, which would work away on a finite, discrete, set of symbols. This tide-predicting machine, like a slide rule, depended not on symbols, but on the measurement of lengths. Such a machine, Alan had realised, could be used on the zeta-function calculation, to save the dreary work of adding, multiplying, and looking up of cosines.

"Alan must have described this idea to Titchmarsh, for a letter from him dated 1 December 1937 approved of this programme of extending the calculation, and mentioned: 'I have seen the tide-predicting machine at Liverpool, but it did not occur to me to use it in this way' (Hodges, pp. 140-2).

"Alan was not the only person to be thinking about mechanical computation in 1939. There were a number of ideas and initiatives, reflecting the growth of new electrical industries. Several projects were on hand in the United States. One of these was the 'differential analyser' that the American engineer Vannevar Bush had designed at the Massachusetts Institute of Technology in 1930. This could set up physical analogues of certain differential equations – the class of problem of most interest in physics and engineering. A similar machine had then been built by the British physicist D.R. Hartree out of Meccano components at Manchester



180 A. M. TURING [March 16,

A METHOD FOR THE CALCULATION OF THE ZETA-FUNCTION

By A. M. TURING.

[Received 7 March, 1939.—Read 16 March, 1939.]

An asymptotic series for the zeta-function was found by Riemann and has been published by Siegel*, and applied by Titchmarsh† to the calculation of the approximate positions of some of the zeros of the function. It is difficult to obtain satisfactory estimates for the remainders with this asymptotic series, as may be seen from the first of these two papers of Titchmarsh, unless t is very large. In the present paper a method of calculation will be described, which, like the asymptotic formula, is based on the approximate functional equation; it is applicable for all values of s. It is likely to be most valuable for a range of t where t is neither so small that the Euler-Maclaurin summation method can be used (*e.g.* $t > 30$) nor large enough for the Riemann-Siegel asymptotic formula (*e.g.* $t < 1000$).

Roughly speaking, the method is to use the approximate functional equation for the zeta-function, with the remainder expressed as an integral, which for the moment we write as $\int_{-\infty}^{\infty} h(x)\,dx$. We approximate to the integral by the obvious sum $\sum_{k=-K}^{K} \frac{1}{\kappa} h\left(\frac{k}{\kappa}\right)$ and we find that, if certain modifications are made in the "main series", this gives a remarkably accurate result; when the number of terms taken is $T = 2K+1$ the error is of the order of magnitude of $e^{-\frac{1}{2}\pi T}$. The theta-functions give another case of this phenomenon. We have the identity

$$\sum_{n=-\infty}^{\infty} e^{-\pi n^2 x} = \frac{1}{\sqrt{x}} \sum_{n=-\infty}^{\infty} e^{-\pi n^2/x}$$

* C. L. Siegel, "Über Riemanns Nachlass zur analytischen Zahlentheorie", *Quell. Gesch. Math.*, B, 2 (1931), 45–80.

† E. C. Titchmarsh, "The zeros of the Riemann zeta-function", *Proc. Royal Soc.* (A), 151 (1935), 234–255; also 157 (1936), 261–263.

University. This in turn had been followed by the commissioning of another differential analyser at Cambridge, where in 1937 the mathematical faculty had sanctioned a new Mathematical Laboratory to house it …

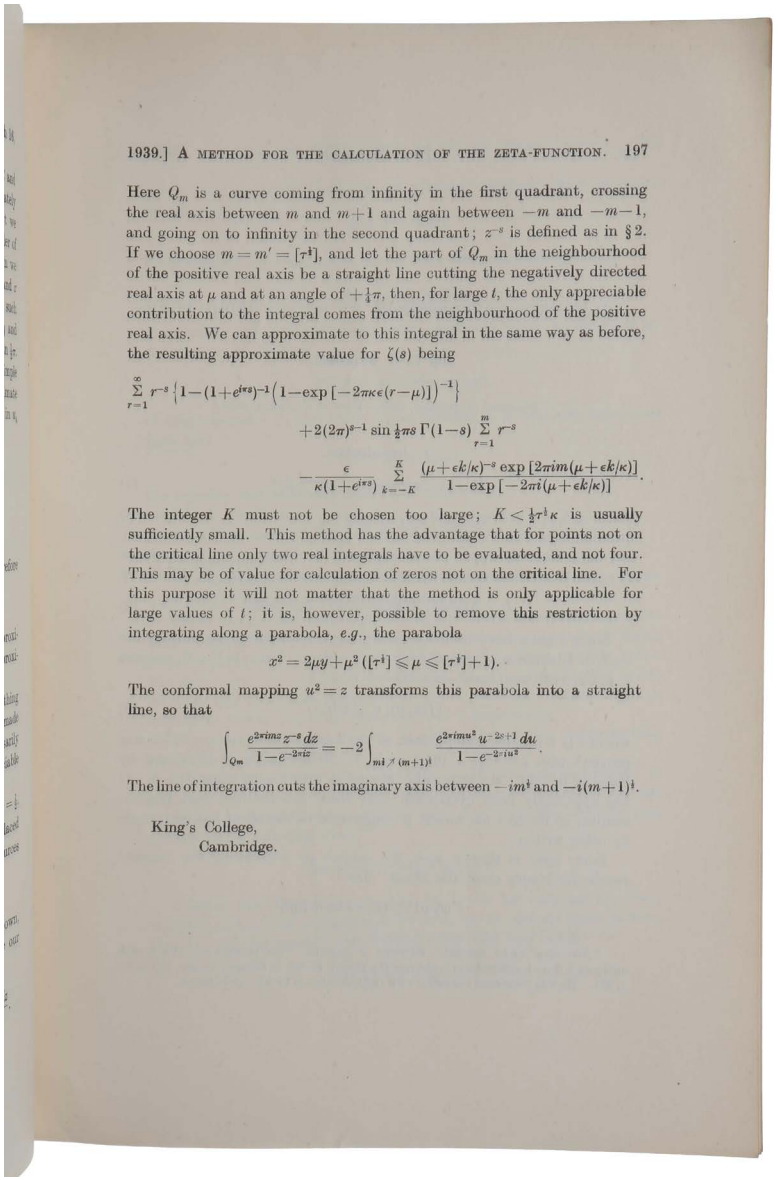
"Such a machine would have been useless for the zeta-function problem. Differential analysers could simulate only one special kind of mathematical system, and that only to a limited and very approximate extent. Similarly the Turing zeta-function machine would be entirely specific to the even more special problem on hand. It had no connection whatever with the Universal Turing Machine. It could hardly have been less universal. On 24 March he applied to the Royal Society for a grant to cover the cost of constructing it, and on their questionnaire wrote, 'Apparatus would be of little permanent value. It could be added to for the purpose of carrying out similar calculations for a wider range of t and might be used for some other investigations connected with the zeta-function. I cannot think of any applications that would not be connected with the zeta-function.' Hardy and Titchmarsh were quoted as referees for the application, which won the requested £40. The idea was that although the machine could not perform the required calculation exactly, it could locate the places where the zeta-function took a value near zero, which could then be tackled by a more exact hand computation. Alan reckoned it would reduce the amount of work by a factor of fifty. Perhaps as important, it would be a good deal more fun.

"The Liverpool tide-predicting machine made use of a system of strings and pulleys to create an analogue of the mathematical problem of adding a series of waves. The length of the string, as it wrapped itself round the pulleys, would be measured to obtain the total sum required. They started with the same idea for the zeta-function summation, but then changed to a different design. In this, a system of meshing gear wheels would rotate to simulate the circular functions required. The addition was to be done not by measuring length, but weight. There would in fact be thirty wave-like terms to be added, each simulated by the rotation of one gear wheel. Thirty weights were to be attached to the corresponding wheels, at a distance from their centres, and then the moment of the weights would vary in a wave-like way as the wheels rotated. The summation would be performed by balancing the combined effect of the weights by a single counterweight. The frequencies of the thirty waves required ran through the logarithms of the integers up to 30. To represent these irrational quantities by gear wheels they had to be approximated by fractions. Thus for instance the frequency determined by the logarithm of 3 was represented in the machine by gears giving a ratio of 34 x 31/57 x 35. This required four gear wheels, with 34, 31, 57 and 35 teeth respectively, to move each other so that one of them could act as the generator of the 'wave'. Some of the wheels could be used two or three times over, so that about 80, rather than 120 gear wheels were required in all. These wheels were ingeniously arranged in meshing groups, and mounted on a central axis in such a way that the turning of a large handle would set them in simultaneous motion. The construction of the machine demanded a great deal of highly accurate gear-cutting to make this possible.

"Donald MacPhail drew up a blueprint of the design, dated 17 July 1939. But Alan did not leave the engineering work to him. In fact his room, in the summer of 1939, was liable to be found with a sort of jigsaw puzzle of gear wheels across the floor … For Alan Turing personally, the machine was a symptom of something that could not be answered by mathematics alone. He was working within the central problems of classical number theory, and making a contribution to it, but this was not enough. The Turing machine, and the ordinal logics, formalizing the workings of the mind; Wittgenstein's enquiries; the electric multiplier and now this concatenation of gear wheels – they all spoke of making some connection between the abstract and the physical. It was not science, not 'applied mathematics', but a sort of applied logic, something that had no name" (Hodges, pp. 155-7).

We do not know how well Turing's zeta function machine would have worked, had it been built. Work on this project was interrupted by the outbreak of World War II, and this computer was never constructed. In 1950, he used the Manchester Mark 1 Electronic Computer to extend the Titchmarsh verification of the RH to the first 1104 zeros of the zeta function, the ones with $0 < t < 1540$.

Downey (ed.), Turing's Legacy: Developments from Turing's Ideas in Logic (2014); Hedjhal & Odlyzko, 'Alan Turing and the Riemann zeta function,' pp. 265-279 in Cooper & van Leeuwen (eds.), Alan Turing: His Work and Impact (2013); Hodges, Alan Turing: the Enigma (1983).



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Here Q_m is a curve coming from infinity in the first quadrant, crossing the real axis between m and $m+1$ and again between $-m$ and $-m-1$, and going on to infinity in the second quadrant; z^{-s} is defined as in § 2. If we choose $m = m' = [\tau^{\frac{1}{2}}]$, and let the part of Q_m in the neighbourhood of the positive real axis be a straight line cutting the negatively directed real axis at μ and at an angle of $+\frac{1}{4}\pi$, then, for large t, the only appreciable contribution to the integral comes from the neighbourhood of the positive real axis. We can approximate to this integral in the same way as before, the resulting approximate value for $\zeta(s)$ being

$$\sum_{r=1}^{\infty} r^{-s}\left\{1-(1+e^{i\pi s})^{-1}\left(1-\exp\left[-2\pi\kappa\epsilon(r-\mu)\right]\right)^{-1}\right\}$$
$$+2(2\pi)^{s-1}\sin\tfrac{1}{2}\pi s\,\Gamma(1-s)\sum_{r=1}^{m} r^{-s}$$
$$-\frac{\epsilon}{\kappa(1+e^{i\pi s})}\sum_{k=-K}^{K}\frac{(\mu+\epsilon k/\kappa)^{-s}\exp\left[2\pi i m(\mu+\epsilon k/\kappa)\right]}{1-\exp\left[-2\pi i(\mu+\epsilon k/\kappa)\right]}.$$

The integer K must not be chosen too large; $K < \frac{1}{2}\tau^{\frac{1}{2}}\kappa$ is usually sufficiently small. This method has the advantage that for points not on the critical line only two real integrals have to be evaluated, and not four. This may be of value for calculation of zeros not on the critical line. For this purpose it will not matter that the method is only applicable for large values of t; it is, however, possible to remove this restriction by integrating along a parabola, *e.g.*, the parabola

$$x^2 = 2\mu y + \mu^2 \quad ([\tau^{\frac{1}{2}}] \leqslant \mu \leqslant [\tau^{\frac{1}{2}}]+1).$$

The conformal mapping $u^2 = z$ transforms this parabola into a straight line, so that

$$\int_{Q_m}\frac{e^{2\pi i m z}z^{-s}\,dz}{1-e^{-2\pi i z}} = -2\int_{m^{\frac{1}{2}}\,/\,(m+1)^{\frac{1}{2}}}\frac{e^{2\pi i m u^2}u^{-2s+1}\,du}{1-e^{-2\pi i u^2}}.$$

The line of integration cuts the imaginary axis between $-im^{\frac{1}{2}}$ and $-i(m+1)^{\frac{1}{2}}$.

King's College,
Cambridge.

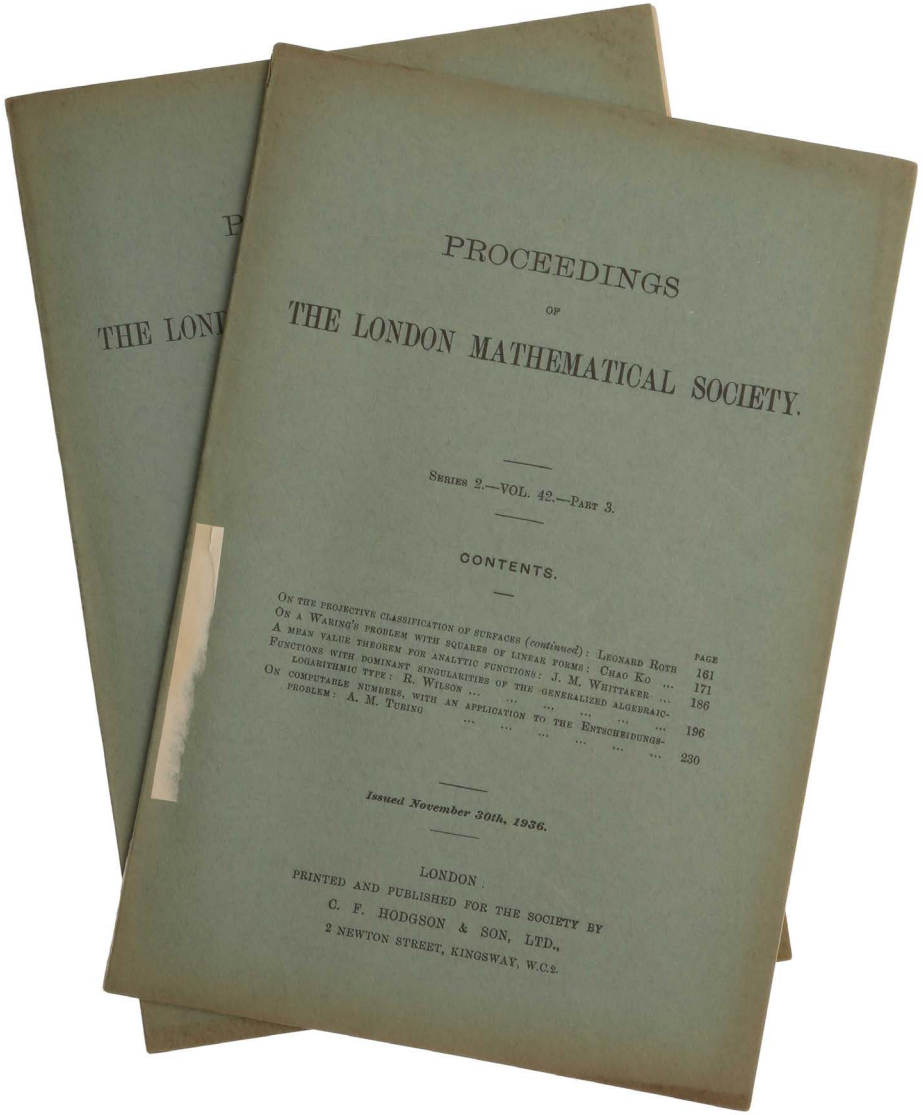
THE FOUNDATION OF MODERN COMPUTER SCIENCE

TURING, Alan Mathison. *On computable numbers, with an application to the Entscheidungsproblem; On computable numbers, with an application to the Entscheidungsproblem. A correction.* London: C. F. Hodgson and Son, 1936-37.

$120,000

Pp. 230-265 in Proceedings of the London Mathematical Society, series 2, vol. 42, part 3, November 30, 1936 & part 4, December 23, 1936; [with:] '... A correction,' pp. 544-546 in ibid., vol. 43. Three vols., large 8vo, pp. 161-240; 241-320; [iv], 546. The two journal issues in original printed wrappers, preserved in a cloth folding case (remnants of publisher's printed label on front wrapper of part 3, edges of wrappers of part 4 a little dust-soiled); the journal volume in (probably publisher's) green cloth, covers ruled in blind, spine lettered in gilt, virtually mint.

First edition, journal issue in the original printed wrappers (main paper). '"On Computable Numbers' is regarded as the founding publication of the modern science of computing. It contributed vital ideas to the development, in the 1940s, of the electronic stored-programme digital computer. 'On Computable Numbers' is the birthplace of the fundamental principle of the modern computer, the idea of controlling the machine's operations by means of a programme of coded instructions stored in the computer's memory. In addition Turing charted areas of mathematics lying beyond the scope of the Turing machine. He proved that not all precisely stated mathematical problems can be solved by computing machines. One such is the Entscheidungsproblem or 'decision problem' … In this one article, Turing ushered in both the modern computer and the mathematical study of the



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uncomputable" (Copeland, The Essential Turing, p. 6). The outstanding problem of mathematical logic at the time, the Entscheidungsproblem, posed by David Hilbert in 1928, asks whether there is an algorithm that can determine whether any given mathematical statement is true or not. "In the long view of intellectual history, I believe universal computation will stand as the single most important idea to emerge in the twentieth century. And this paper is where it first appeared with clarity" (Stephen Wolfram, in Alan Turing. His Work and Impact, p. 44). "Inspired by the human computer (i.e., the human engaged in computation), Turing described a notional machine that could read and write symbols along a segmented tape. The machine itself would be capable of assuming various internal states that, together with the input of a single symbol along the tape, could lead to a few primitive atomic actions. Based on the state and the current symbol, each configuration specifies a change (or not) of symbol, a move right or left, and a next state. Working under some straightforward assumptions about the finite and discrete nature of the machine, Turing was able to demonstrate the wide range of numbers (equivalently, the wide class of functions) that could be computed and, moreover, able to specify a single machine, the universal machine, that would be capable of simulating the computations of any such machine. Turing's characterization has come to be seen as a more compelling account of what it means to be effective, mechanical, or algorithmic than any of the various extensionally equivalent formulations offered by his contemporaries" (New Dictionary of Scientific Biography, vol. 7, p. 83). The 'Correction' was published in order to remove some formal errors made in the first paper pointed out by the Swiss mathematician Paul Bernays. ABPC/RBH list only two copies with all three parts in the original printed wrappers, the most recent being the Richard Green copy (Christie's, June 17, 2013, $182,500).

"In the spring of 1935 – at the time of von Neumann's visit to Cambridge – Turing was attending Max Newman's lectures on the foundations of mathematics when the Entscheidungsproblem first attracted his attention. Hilbert's challenge aroused Turing's instinct that mathematical questions resistant to strictly mechanical procedures could be proved to exist.

"Turing's argument was straightforward – as long as you threw out all assumptions and started fresh. "One of the facets of extreme originality is not to regard as obvious the things that lesser minds call obvious," says I. J. (Jack) Good, who served as an assistant to Turing (then referred to as "Prof") during World War II. Originality can be more important than intelligence, and according to Good, Turing constituted proof. "Henri Poincare did quite badly at an intelligence test, and Prof also was only about halfway up the undergraduate scale when he took such a test." Had Turing more closely followed the work of Alonzo Church or Emil Post, who anticipated his results, his interest might have taken a less original form. "The way in which he uses concrete objects such as exercise books and printer's ink to illustrate and control the argument is typical of his insight and originality," says colleague Robin Gandy. "Let us praise the uncluttered mind."

"A function is computable, over the domain of the natural numbers (0, 1, 2, 3, …), if there exists a finite sequence of instructions (or algorithm) that prescribes exactly how to list the value of the function at f(0) and, for any natural number n, at f(n + 1). Turing approached the question of computable functions in the opposite direction, from the point of view of the numbers produced as a result. "According to my definition," he explained, "a number is computable if its decimal can be written down by a machine."

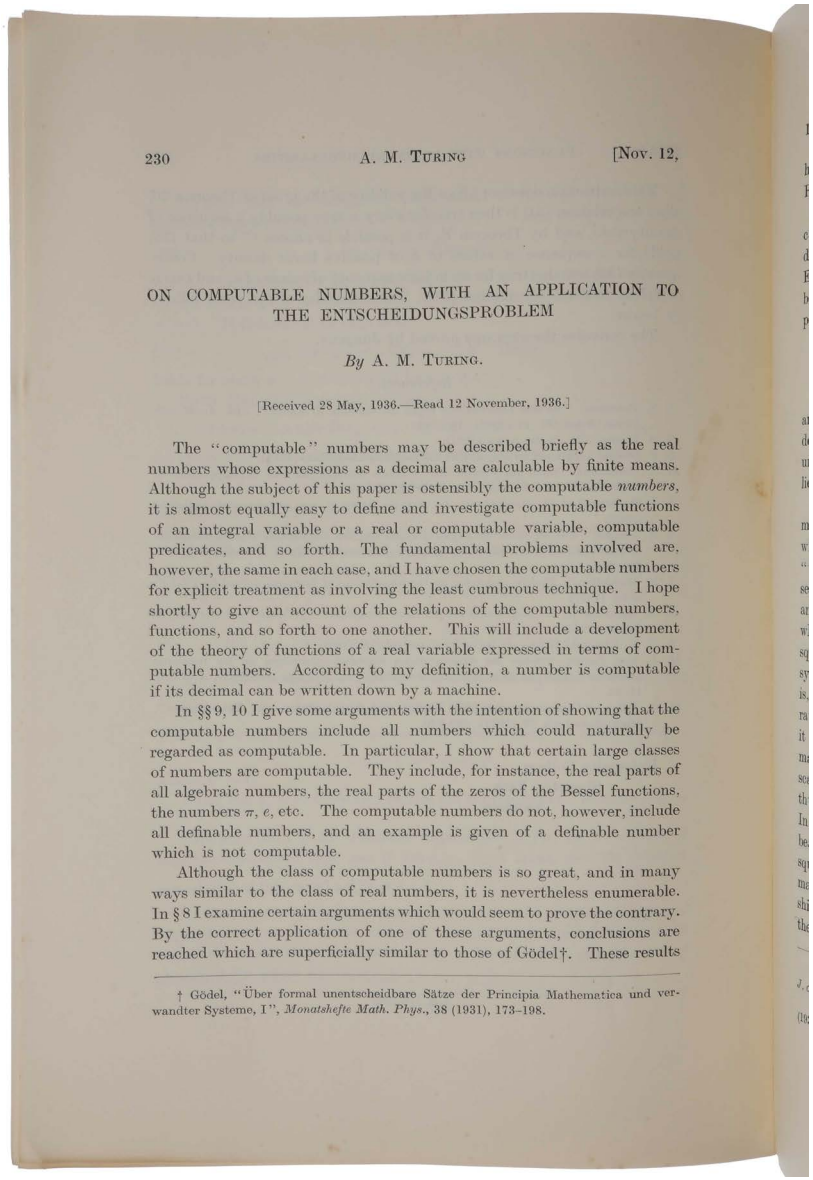
"Turing began with the informal idea of a computer – which in 1935 meant not a calculating machine but a human being, equipped with pencil, paper, and time. He then substituted unambiguous components until nothing but a formal definition of "computable" remained. Turing's machine (which he termed an LCM, or Logical Computing Machine) thus consisted of a black box (as simple as

a typewriter or as complicated as a human being) able to read and write a finite alphabet of symbols to and from a finite but unbounded length of paper tape – and capable of changing its own "m-configuration," or "state of mind."

"We may compare a man in the process of computing a real number to a machine which is only capable of a finite number of conditions … which will be called 'm-configurations'," Turing wrote. "The machine is supplied with a 'tape' (the analogue of paper) running through it, and divided into sections (called 'squares') each capable of bearing a 'symbol.' At any moment there is just one square … which is 'in the machine' … However, by altering its m-configuration the machine can effectively remember some of the symbols which it has 'seen.' … In some of the configurations in which the scanned square is blank (i.e., bears no symbol) the machine writes down a new symbol on the scanned square; in other configurations it erases the scanned symbol. The machine may also change the square which is being scanned, but only by shifting it one place to right or left. In addition to any of these operations the m-configuration may be changed."

"Turing introduced two fundamental assumptions: discreteness of time and discreteness of state of mind. To a Turing machine, time exists not as a continuum, but as a sequence of changes of state. Turing assumed a finite number of possible states at any given time. "If we admitted an infinity of states of mind, some of them will be 'arbitrarily close' and will be confused," he explained. "The restriction is not one which seriously affects computation, since the use of more complicated states of mind can be avoided by writing more symbols on the tape."

"The Turing machine thus embodies the relationship between an array of symbols in space and a sequence of events in time. All traces of intelligence were removed. The machine can do nothing more intelligent at any given moment than make a mark, erase a mark, and move the tape one square



230 A. M. TURING [Nov. 12,

ON COMPUTABLE NUMBERS, WITH AN APPLICATION TO THE ENTSCHEIDUNGSPROBLEM

By A. M. TURING.

[Received 28 May, 1936.—Read 12 November, 1936.]

The "computable" numbers may be described briefly as the real numbers whose expressions as a decimal are calculable by finite means. Although the subject of this paper is ostensibly the computable *numbers*, it is almost equally easy to define and investigate computable functions of an integral variable or a real or computable variable, computable predicates, and so forth. The fundamental problems involved are, however, the same in each case, and I have chosen the computable numbers for explicit treatment as involving the least cumbrous technique. I hope shortly to give an account of the relations of the computable numbers, functions, and so forth to one another. This will include a development of the theory of functions of a real variable expressed in terms of computable numbers. According to my definition, a number is computable if its decimal can be written down by a machine.

In §§ 9, 10 I give some arguments with the intention of showing that the computable numbers include all numbers which could naturally be regarded as computable. In particular, I show that certain large classes of numbers are computable. They include, for instance, the real parts of all algebraic numbers, the real parts of the zeros of the Bessel functions, the numbers π, e, etc. The computable numbers do not, however, include all definable numbers, and an example is given of a definable number which is not computable.

Although the class of computable numbers is so great, and in many ways similar to the class of real numbers, it is nevertheless enumerable. In § 8 I examine certain arguments which would seem to prove the contrary. By the correct application of one of these arguments, conclusions are reached which are superficially similar to those of Gödel†. These results

† Gödel, "Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme, I", *Monatshefte Math. Phys.*, 38 (1931), 173–198.

to the right or to the left. The tape is not infinite, but if more tape is needed, the supply can be counted on never to run out. Each step in the relationship between tape and Turing machine is determined by an instruction table listing all possible internal states, all possible external symbols, and, for every possible combination, what to do (write or erase a symbol, move right or left, change the internal state) in the event that combination comes up. The Turing machine follows instructions and never makes mistakes. Complicated behavior does not require complicated states of mind. By taking copious notes, the Turing machine can function with as few as two internal states. Behavioral complexity is equivalent whether embodied in complex states of mind (m-configurations) or complex symbols (or strings of simple symbols) encoded on the tape.

"It took Turing only eleven pages of 'On Computable Numbers' to arrive at what became known as Turing's Universal Machine. "It is possible to invent a single machine which can be used to compute any computable sequence," he announced. The Universal Machine, when provided with a suitably encoded description of some other machine, executes this description to produce equivalent results. All Turing machines, and therefore all computable functions, can be encoded by strings of finite length. Since the number of possible machines is countable but the number of possible functions is not, noncomputable functions (and what Turing referred to as "uncomputable numbers") must exist.

"Turing was able to construct, by a method similar to Gödel's, functions that could be given a finite description but could not be computed by finite means. One of these was the halting function: given the number of a Turing machine and the number of an input tape, it returns either the value 0 or the value 1 depending on whether the computation will ever come to a halt. Turing called the configurations that halt 'circular' and the configurations that keep going indefinitely 'circle free,' and demonstrated that the unsolvability of the halting problem implies the unsolvability of a broad class of similar problems, including the Entscheidungsproblem. Contrary to Hilbert's expectations, no mechanical procedure can be counted on to determine the provability of any given mathematical statement in a finite number of steps. This put a halt to the Hilbert program, while Hitler's purge of German universities put a halt to Göttingen's position as the mathematical center of the world, leaving a vacuum for Turing's Cambridge, and von Neumann's Princeton, to fill.

"After a full year of work, Turing gave Newman a draft of his paper in April of 1936. "Max's first sight of Alan's masterpiece must have been a breathtaking experience, and from this day forth Alan became one of Max's principle protégés," says William Newman, Max's son. Max Newman lobbied for the publication of 'On Computable Numbers, with an Application to the Entscheidungsproblem,' in the Proceedings of the London Mathematical Society, and arranged for Turing to go to Princeton to work with Alonzo Church. "This makes it all the more important that he should come into contact as soon as possible with the leading workers on this line, so that he should not develop into a confirmed solitary," Newman wrote to Church.

"Turing arrived in Princeton carrying his sextant, and stretching his resources to survive on his King's College fellowship (of £300) for the year. The page proofs of 'On Computable Numbers' arrived by mail from London on October 3. "It should not be long now before the paper comes out," he wrote to his mother on October 6. The publication of 'On Computable Numbers' (on November 30, 1936) went largely unnoticed. "I was disappointed by its reception here,' Turing wrote to his mother in February 1937, adding that "I don't much care about the idea of spending a long summer in this country." Only two requests for reprints came in. Engineers avoided Turing's paper because it appeared entirely theoretical, and theoreticians avoided it because of the references to paper tape and machines …

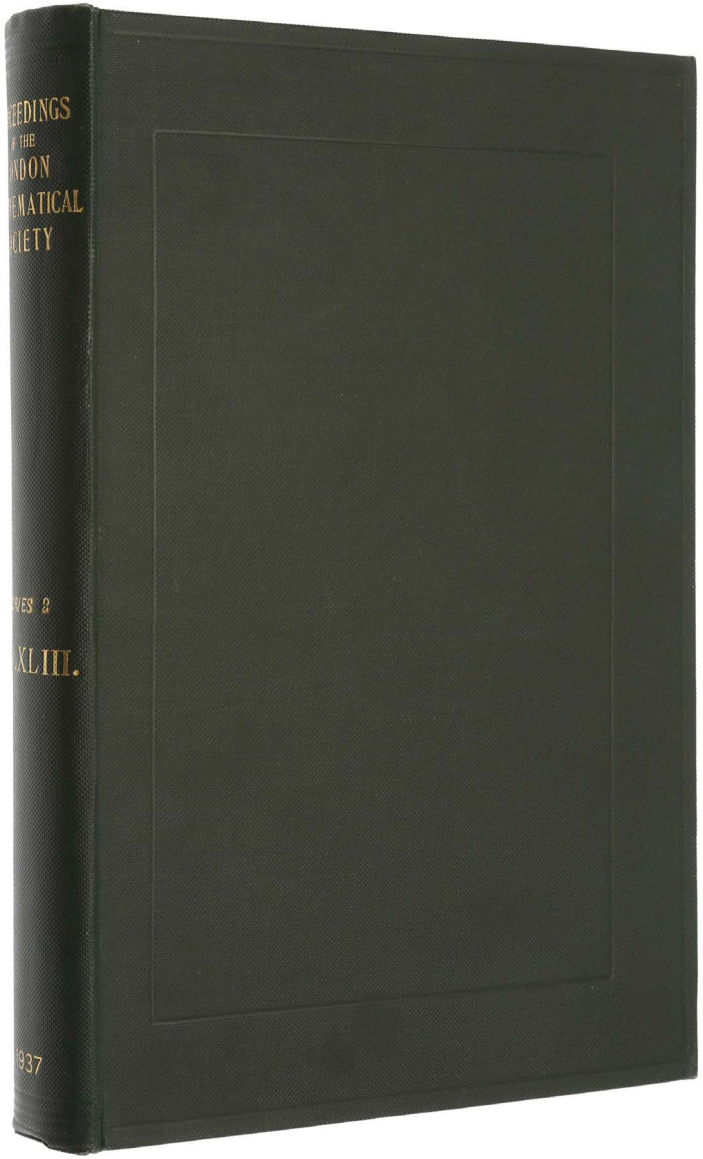
"In March of I937, Alonzo Church reviewed 'On Computable Numbers' in the

Journal of Symbolic Logic, and coined the term Turing machine. "Computability by a Turing machine," wrote Church, "has the advantage of making the identification with effectiveness in the ordinary (not explicitly defined) sense evident immediately." Church's thesis – equating computability with effective calculability – would be the Church-Turing thesis from then on.

"Even Gödel, who dismissed most attempts to strengthen his own results, recognized the Church-Turing thesis as a major advance. "With this concept one has for the first time succeeded in giving an absolute definition … not depending on the formalism chosen," he admitted in 1946. Before Church and Turing, the definition of mechanical procedure was limited by the language in which the concept was defined. "For the concept of computability however … the situation is different," Gödel observed. "By a kind of miracle it is not necessary to distinguish orders, and the diagonal procedure does not lead outside the defined notion."

"It is difficult today to realize how bold an innovation it was to introduce talk about paper tapes and patterns punched in them, into discussions of the foundations of mathematics," Max Newman recalled in 1955. For Turing, the next challenge was to introduce mathematical logic into the foundations of machines. "Turing's strong interest in all kinds of practical experiment made him even then interested in the possibility of actually constructing a machine on these lines."

"The title 'On Computable Numbers' (rather than 'On Computable Functions') signaled a fundamental shift. Before Turing, things were done to numbers. After Turing, numbers began doing things. By showing that a machine could be encoded as a number, and a number decoded as a machine, 'On Computable Numbers' led to numbers (now called 'software') that were 'computable' in a way that was entirely new" (Dyson, Turing's Cathedral, pp. 246-250).



TURING'S THESIS

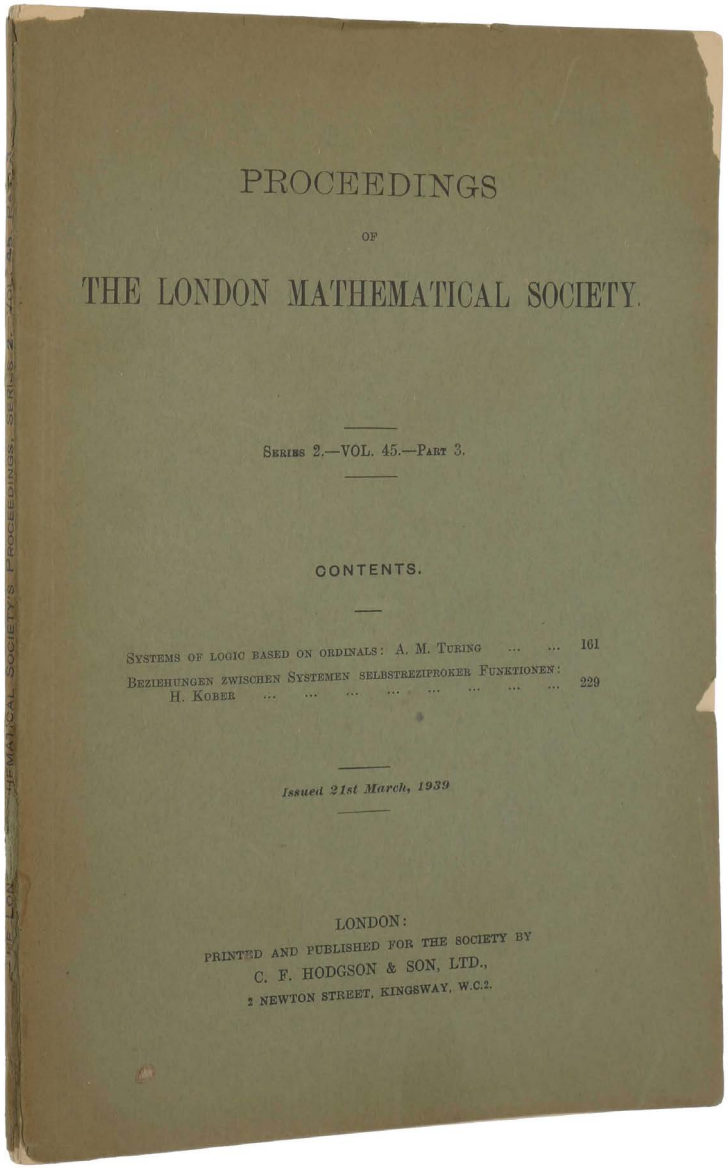
TURING, Alan Mathison. *Systems of logic based on ordinals.* London: C. F. Hodgson and Son, 1939.

$9,500

Pp. 161-228 in: Proceedings of the London Mathematical Society, Second Series, Vol. 45, No. 3, 21 March, 1939. Large 8vo (260 x 172 mm), pp. 161-240. Original printed wrappers (three chips from edges of front wrapper, one tiny chip from rear wrapper, spine ends worn and with a few letters of spine title rubbed away).

First edition, journal issue in original printed wrappers, of Turing's PhD thesis, "one of the key documents in the history of mathematics and computer science" (Appel), and perhaps Turing's most formidable paper. ""Systems of logic based on ordinals" is a profound work of first rank importance. Among its achievements are the exploration of a means of circumventing Gödel's incompleteness theorems; the introduction of the concept of an 'oracle machine,' thereby opening the field of relative computability; and, in the wake of the demolition of the Hilbert programme (by Gödel, Turing and Church), an analysis of the place of intuition in mathematics and logic" (Copeland, p. 126). Rare on the market in unrestored original printed wrappers (we know of only one copy at auction, in the Weinreb Computer Collection, Bloomsbury Book Auctions, 28 October 1999).

"Turing's 1938 Princeton PhD thesis, "Systems of logic based on ordinals," which includes his notion of an oracle machine, has had a lasting influence on computer science and mathematics... A work of philosophy as well as mathematics, Turing's thesis envisions a practical goal – a logical system to formalize mathematical



PROCEEDINGS

OF

THE LONDON MATHEMATICAL SOCIETY.

SERIES 2.—VOL. 45.—PART 3.

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BEZIEHUNGEN ZWISCHEN SYSTEMEN SELBSTREZIPROKER FUNKTIONEN: H. KOBER 229

Issued 21st March, 1939

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2 NEWTON STREET, KINGSWAY, W.C.2.

proofs so that they can be checked mechanically. If every step of a theorem could be verified mechanically, the burden on intuition would be limited to the axioms... Turing's vision of "constructive systems of logic for practical use" has become reality: in the twenty-first century, automated "formal methods" are now routine" (Appel)

Turing studied at King's College, Cambridge, becoming a Fellow in 1935. In that year he attended the logic lectures of the topologist M. H. A. Newman, from which he learned of the Entscheidungsproblem: Could there exist, at least in principle, a definite method or process by which it could be decided whether any given mathematical assertion was provable? His negative answer to this question was published in 1936 as 'On computable numbers, with an application to the Entscheidungsproblem,' shortly after Alonzo Church at Princeton had published his own solution. Turing's paper, containing the description of his 'universal machine,' is now recognized as the founding theoretical work of modern computer science. "It was only natural that the mathematician M. H. A. Newman should suggest that Turing come to Princeton to work with Church. Some of the greatest logicians in the world, thinking about the issues that in later decades became the foundation of computer science, were in Princeton's (old) Fine Hall in the 1930s: Gödel, Church, Stephen Kleene, Barkley Rosser, John von Neumann, and others. In fact, it is hard to imagine a more appropriate place for Turing to have pursued graduate study. After publishing his great result on computability, Turing spent two years (1936–38) at Princeton, writing his PhD thesis on "ordinal logics" with Church as his adviser...

"[In his PhD thesis], Turing turns his attention from computation to logic. Gödel and Church would not have called themselves computer scientists: they were mathematical logicians; and even Turing, when he got his big 1936 result "On computable numbers," was answering a question in logic posed by Hilbert in 1928. Turing's thesis, "Systems of Logic Based on Ordinals," takes Gödel's stunning incompleteness theorems as its point of departure. Gödel had shown that if a formal axiomatic system (of at least minimal expressive power) is consistent, then it cannot be complete. And not only is the system incomplete, but the formal statement of the consistency of the system is true and unprovable if the system is consistent. Thus if we already have informal or intuitive reasons for accepting the axioms of the system as true, then we ought to accept the statement of its consistency as a new axiom. And then we can apply the same considerations to the new system; that is, we can iterate the process of adding consistency statements as new axioms. In his thesis, Turing investigated that process systematically by iterating it into the constructive transfinite, taking unions of logical systems at limit ordinal notations. His main result was that one can thereby overcome incompleteness for an important class of arithmetical statements (though not for all)...

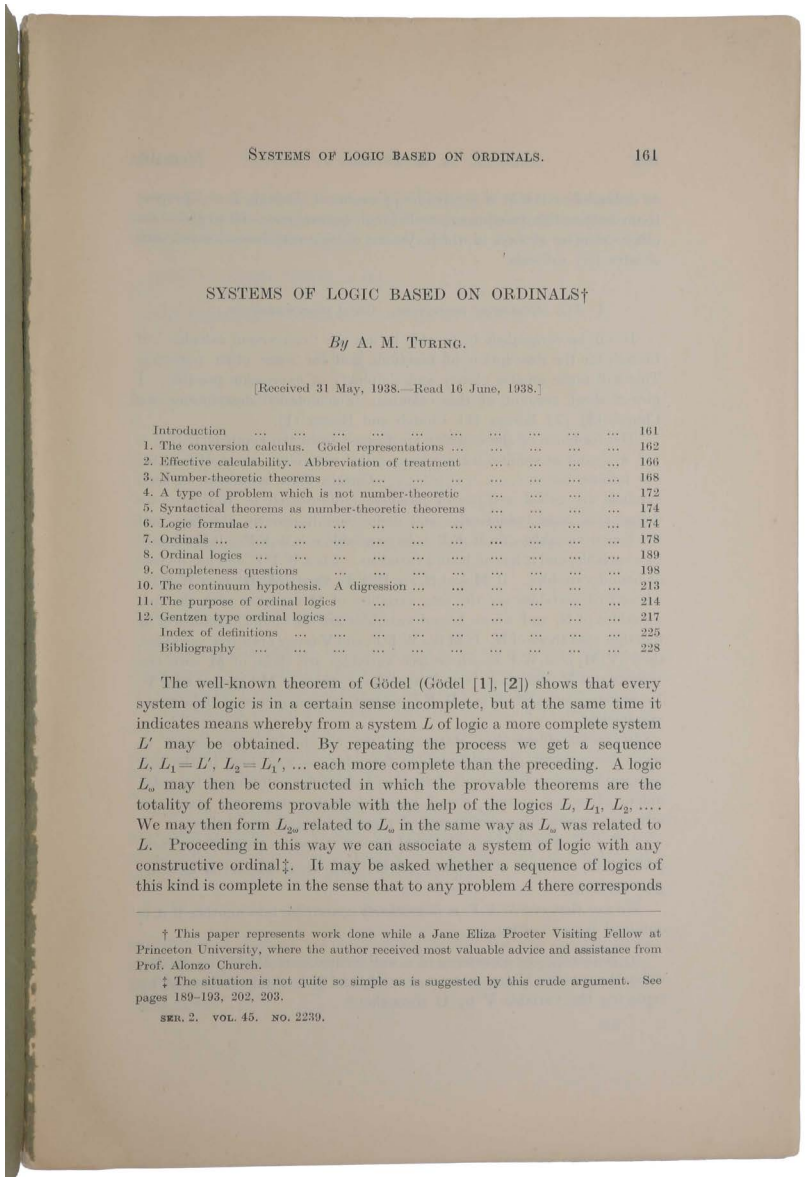
"Just as one of the strengths of Turing's great 1936 paper was its philosophical component—in which he explains the motivation for the Turing machine as a model of computation—here in the PhD thesis he is also motivated by philosophical concerns, as in section 9: 'We might hope to obtain some intellectually satisfying system of logical inference (for the proof of number theoretic theorems) with some ordinal logic. Gödel's theorem shows that such a system cannot be wholly mechanical, but with a complete ordinal logic we should be able to confine the non-mechanical steps entirely to verifications that particular formulae are ordinal formulae.'

"Turing greatly expands on these philosophical motivations in section 11 of the thesis. His program, then, is this: We wish to reason in some logic, so that our proofs can be mechanically checked (for example, by a Turing machine). Thus we don't need to trust our students and journal-referees to check our proofs. But no (sufficiently expressive) logic can be complete, as Gödel showed. If we are using

a given logic, sometimes we may want to reason about statements unprovable in that logic. Turing's proposal is to use an ordinal logic sufficiently high in the hierarchy; checking proofs in that logic will be completely mechanical, but the one "intuitive" step remains of verifying ordinal formulas...

"But the PhD thesis contains, almost as an aside, an enormously influential mathematical insight. Turing invented the notion of oracles, in which one kind of computer consults from time to time, in an explicitly axiomatized way, a more powerful kind. Oracle computations are now an important part of the tool kit of both mathematicians and computer scientists working in computability theory and computational complexity theory. This method may actually be the most significant result in Turing's PhD thesis" (Appel, Chapter 1: 'The Birth of Computer Science at Princeton in the 1930s').

The preparation of the thesis was somewhat protracted, partly because of the clash of styles between Church and Turing. Turing found Church's lectures "ponderous and excessively precise; by contrast, Turing's native style was rough-and-ready and prone to minor errors" (Feferman, p. 4). "[Turing] ended up with a draft containing the main results by Christmas of 1937. But then he wrote Philip Hall [who had been his undergraduate tutor at Cambridge] in March 1938 that the work on his thesis was "proving rather intractable, and I am always rewriting parts of it." Later he wrote that "Church made a number of suggestions which resulted in the thesis being expanded to an appalling length"" (ibid., p. 5). Church's influence may have also been partly responsible for the rather muted reception of the thesis. "One reason that the reception of Turing's [PhD thesis] may have been so limited is that (no doubt at Church's behest) it was formulated in terms of the λ-calculus, which makes expressions for the ordinals and formal systems very hard to understand" (Appel, loc.cit., p. 4). Following an oral examination in May, on which his performance was noted as "Excellent," Turing was granted his PhD in June 1938.



SYSTEMS OF LOGIC BASED ON ORDINALS. 161

SYSTEMS OF LOGIC BASED ON ORDINALS†

By A. M. TURING.

[Received 31 May, 1938.—Read 16 June, 1938.]

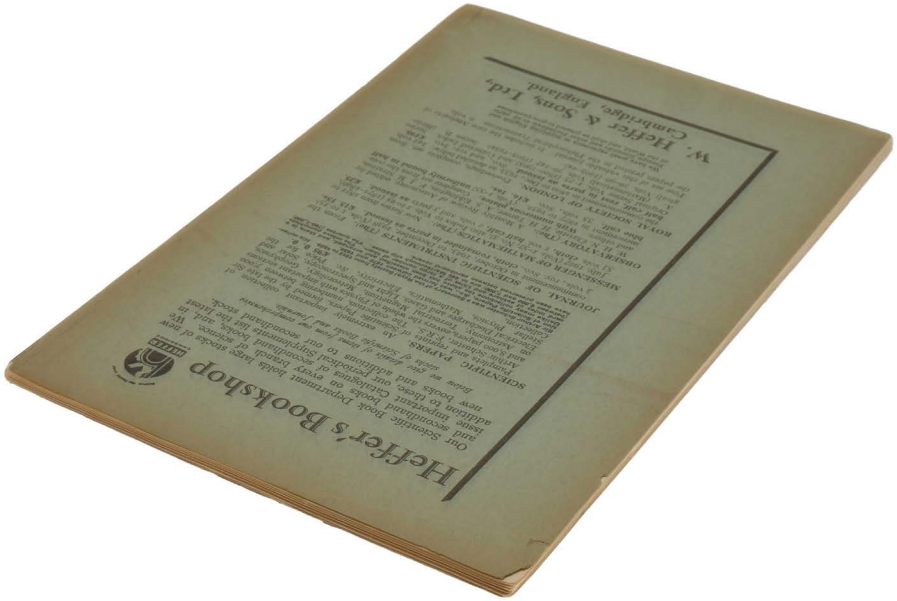
The well-known theorem of Gödel (Gödel [1], [2]) shows that every system of logic is in a certain sense incomplete, but at the same time it indicates means whereby from a system L of logic a more complete system L' may be obtained. By repeating the process we get a sequence L, $L_1 = L'$, $L_2 = L_1'$, ... each more complete than the preceding. A logic L_ω may then be constructed in which the provable theorems are the totality of theorems provable with the help of the logics L, L_1, L_2, ... We may then form $L_{2\omega}$ related to L_ω in the same way as L_ω was related to L. Proceeding in this way we can associate a system of logic with any constructive ordinal‡. It may be asked whether a sequence of logics of this kind is complete in the sense that to any problem A there corresponds

† This paper represents work done while a Jane Eliza Procter Visiting Fellow at Princeton University, where the author received most valuable advice and assistance from Prof. Alonzo Church.

‡ The situation is not quite so simple as is suggested by this crude argument. See pages 189–193, 202, 203.

SER. 2. VOL. 45. NO. 2239.

Andrew W. Appel (ed.), Alan Turing's Systems of Logic: The Princeton Thesis, Princeton University Press, 2012; B. J. Copeland, The Essential Turing, Clarendon Press, 2004; Solomon Feferman, 'Turing's Thesis,' Notices of the American Mathematical Society, Vol. 53, 2006, pp. 1-8; Andrew Hodges, Alan Turing: The Enigma, 1983, pp. 142-3.



DISCOVERY OF THE STRUCTURE OF DNA

WATSON; CRICK; WILKINS; STOKES; WILSON; FRANKLIN; GOSLING; SEEDS. *[The six milestone papers on the structure of DNA in original wrappers:] 1. WATSON, J. D. & CRICK, F. H. C. Molecular Structure of Nucleic Acids: A Structure for Deoxyribose Nucleic Acid; 2. WILKINS, M. H. F., STOKES, A. R. & WILSON, H. R. Molecular Structure of Deoxypentose Nucleic Acids; 3. FRANKLIN, R. E. & GOSLING, R. G. Molecular Configuration in Sodium Thymonucleate, pp. 737-41 in Nature, Vol. 171, No. 4356, April 25, 1953. 4. WATSON, J. D. & CRICK, F. H. C. Genetical Implications of the Structure of Deoxyribonucleic Acid, pp. 964-7 in Nature, Vol. 171, No. 4361, May 30, 1953. 5. FRANKLIN, R. E. & GOSLING, R. G. Evidence for 2-Chain Helix in Crystalline Structure of Sodium Deoxyribonucleate, pp. 156-7 in Nature, Vol. 172, No. 4369, July 25, 1953. 6. WILKINS, M. H. F., SEEDS, W. E. STOKES, A. R. & WILSON, H. R. Helical Structure of Crystalline Deoxypentose Nucleic Acid, pp. 759-62 in Nature, Vol. 172, No. 4382, October 24, 1953.* London: Justus Perthes, 1953.

$15,000

First edition, in the form in which they first appeared, of six crucial papers documenting the discovery of the structure of DNA and the mechanism of the genetic code. The first is Watson & Crick's paper 'Molecular Structure of Nucleic Acids: A Structure for Deoxyribose Nucleic Acid', which "records the discovery of the molecular structure of deoxyribonucleic acid (DNA), the main component of chromosomes and the material that transfers genetic characteristics in all life forms. Publication of this paper initiated the science of molecular biology. Forty years after Watson and Crick's discovery, so much of the basic understanding of

medicine and disease has advanced to the molecular level that their paper may be considered the most significant single contribution to biology and medicine in the twentieth century" (One Hundred Books Famous in Medicine, p. 362). Watson & Crick's paper is here accompanied by their paper published one month later, 'Genetical Implications of the Structure of Deoxyribonucleic Acid,' "in which they elaborated on their proposed DNA replication mechanism" (ibid.), together with one of the papers which provided the experimental data confirming their proposed structure, a follow up to 'Molecular Structure of Deoxypentose Nucleic Acids' by Wilkins et al. Also included is the 1961 paper 'General Nature of the Genetic Code for Proteins,' documenting Crick's team's efforts to crack the genetic code, amassing evidence suggesting that "the amino-acid sequence along the polypeptide chain of a protein is determined by the sequence of

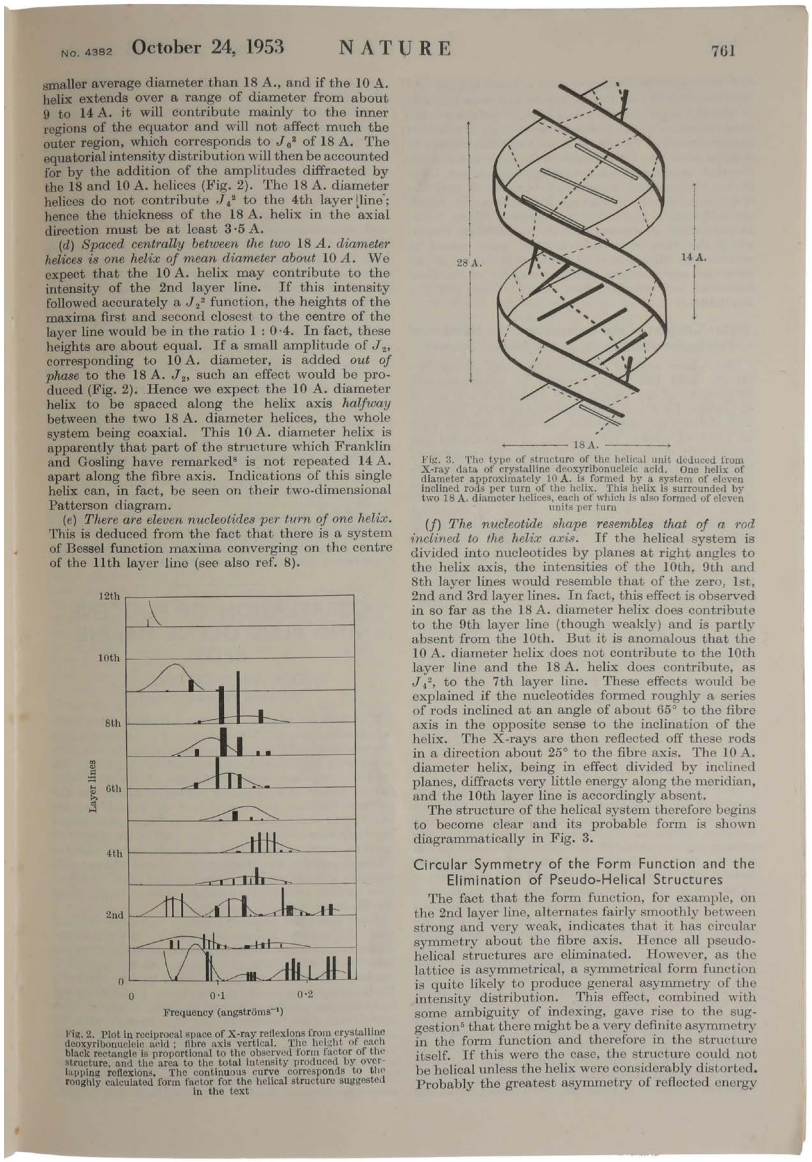
the bases along some particular part of the nucleic acid of the genetic material" (p. 1227), and that each acid was most likely coded by a group of three bases. In 1962, Watson, Crick, and Wilkins shared the Nobel Prize in Physiology or Medicine "for their discoveries concerning the molecular structure of nucleic acids and its significance for information transfer in living material." The first three papers were issued together in offprint from, but the journal issue offered here preceded the offprint and is actually rarer on the market.

DNA was first isolated by the Swiss physician Friedrich Miescher in 1869, and over the succeeding years many researchers investigated its structure and function, with some arguing that it may be involved in genetic inheritance. By the early 1950s this had become one of the most important questions in biology. Maurice Wilkins of King's College London and his colleague Rosalind Franklin were both working on DNA, with Franklin producing X-ray diffraction images of its structure. Wilkins also introduced his friend Francis Crick to the subject, and Crick and his partner James Watson began their own investigation at the Cavendish Laboratory in Cambridge, focusing on building molecular models. After one failed attempt in which they postulated a triple-helix structure, they were banned by the Cavendish from spending any additional time on the subject. But a year later, after seeing new X-ray diffraction images taken by Franklin (notably the famous 'Photo 51', which is reproduced in the third offered paper), they resumed their work and soon announced that not only had they discovered the double-helix structure of DNA, but even more importantly, that "the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material."

When Watson and Crick's paper was submitted for publication in Nature, Sir Lawrence Bragg, the director of the Cavendish Laboratory at Cambridge, and Sir John Randall of King's College agreed that the paper should be published simultaneously with those of two other groups of researches who had also prepared important papers on DNA: Maurice Wilkins, A.R. Stokes, and H.R. Wilson, authors of 'Molecular Structure of Deoxypentose Nucleic Acids,' and Rosalind Franklin and Raymond Gosling, who submitted the paper 'Molecular Configuration in Sodium Thymonucleate.' The three papers were published in Nature under the general title 'The Molecular Structure of Nucleic Acids.'

"Five weeks after Watson's and Crick's first paper in Nature, their second appeared, in which, after explaining the structure and the evidence all over again, they pursued some of the genetical implications. These flowed from the most novel, most fundamental fact of the model: "Any sequence of the pairs of the bases can fit into the structure. It follows that in a long molecule many different permutations are possible, and it therefore seems likely that the precise sequence of the bases is the code which carries the genetical information. If the actual order of the bases on one of the pair of chains were given, one could write down the exact order of the bases on the other one, because of the specific pairing." This immediately suggested, they said, how DNA duplicated itself. "Previous discussions of self-

duplication have usually involved the concept of a template, or mould. Either the template was supposed to copy itself directly or it was to produce a "negative", which in its turn was to act as a template and produce the original "positive" once again. In no case has it been explained in detail how it would do this in terms of atoms and molecules." The elucidation of the structure of DNA called for a new kind of functional explanation. "Now our model for deoxyribonucleic acid is, in effect, a pair of templates, each of which is complementary to the other. We imagine that prior to duplication the hydrogen bonds [connecting the bases in pairs] are broken, and the two chains unwind and separate. Each chain then acts as a template for the formation on to itself of a new companion chain, so that eventually we shall have two pairs of chains, where we only had one before. Moreover, the sequence of the pairs of bases will have been duplicated exactly." Yet perhaps not always exactly: the model, or rather the mistake whose correction by Donohue had cleared the way for the model, suggested for the first time a physical, molecular explanation for the central phenomenon of genetics, namely the occasional, random appearance of mutations. If the sequence of bases carried the information for the organism, then a mutation might be no more than a single change in that sequence. In particular, they wrote, "Spontaneous mutation may be due to a base occasionally occurring in one of its less likely tautomeric forms." For example, though adenine normally paired with thymine, in the rare event that one of its hydrogen atoms shifted to a particular different position at the moment the complementary chain was forming, then the base could bond with the other pyrimidine, cytosine. On the next cycle of replication, the adenine, taking its normal tautomeric form again, would pair as usual with thymine, but the cytosine would pair with guanine and so, on one of the two new double helices, a change in the sequence of bases would have appeared. This was plausible, immensely exciting speculation: proof that a change of a single base pair can cause a mutation was several years away" (Judson).



No. 4382 October 24, 1953 NATURE 761

smaller average diameter than 18 A., and if the 10 A. helix extends over a range of diameter from about 9 to 14 A. it will contribute mainly to the inner regions of the equator and will not affect much the outer region, which corresponds to J_6^2 of 18 A. The equatorial intensity distribution will then be accounted for by the addition of the amplitudes diffracted by the 18 and 10 A. helices (Fig. 2). The 18 A. diameter helices do not contribute J_4^2 to the 4th layer line; hence the thickness of the 18 A. helix in the axial direction must be at least 3·5 A.

(*d*) *Spaced centrally between the two* 18 *A. diameter helices is one helix of mean diameter about* 10 *A.* We expect that the 10 A. helix may contribute to the intensity of the 2nd layer line. If this intensity followed accurately a J_2^2 function, the heights of the maxima first and second closest to the centre of the layer line would be in the ratio 1 : 0·4. In fact, these heights are about equal. If a small amplitude of J_2, corresponding to 10 A. diameter, is added *out of phase* to the 18 A. J_2, such an effect would be produced (Fig. 2). Hence we expect the 10 A. diameter helix to be spaced along the helix axis *halfway* between the two 18 A. diameter helices, the whole system being coaxial. This 10 A. diameter helix is apparently that part of the structure which Franklin and Gosling have remarked[8] is not repeated 14 A. apart along the fibre axis. Indications of this single helix can, in fact, be seen on their two-dimensional Patterson diagram.

(*e*) *There are eleven nucleotides per turn of one helix.* This is deduced from the fact that there is a system of Bessel function maxima converging on the centre of the 11th layer line (see also ref. 8).

Fig. 2. Plot in reciprocal space of X-ray reflexions from crystalline deoxyribonucleic acid; fibre axis vertical. The height of each black rectangle is proportional to the observed form factor of the structure, and the area to the total intensity produced by overlapping reflexions. The continuous curve corresponds to the roughly calculated form factor for the helical structure suggested in the text

Fig. 3. The type of structure of the helical unit deduced from X-ray data of crystalline deoxyribonucleic acid. One helix of diameter approximately 10 A. is formed by a system of eleven inclined rods per turn of the helix. This helix is surrounded by two 18 A. diameter helices, each of which is also formed of eleven units per turn

(*f*) *The nucleotide shape resembles that of a rod inclined to the helix axis.* If the helical system is divided into nucleotides by planes at right angles to the helix axis, the intensities of the 10th, 9th and 8th layer lines would resemble that of the zero, 1st, 2nd and 3rd layer lines. In fact, this effect is observed in so far as the 18 A. diameter helix does contribute to the 9th layer line (though weakly) and is partly absent from the 10th. But it is anomalous that the 10 A. diameter helix does not contribute to the 10th layer line and the 18 A. helix does contribute, as J_4^2, to the 7th layer line. These effects would be explained if the nucleotides formed roughly a series of rods inclined at an angle of about 65° to the fibre axis in the opposite sense to the inclination of the helix. The X-rays are then reflected off these rods in a direction about 25° to the fibre axis. The 10 A. diameter helix, being in effect divided by inclined planes, diffracts very little energy along the meridian, and the 10th layer line is accordingly absent.

The structure of the helical system therefore begins to become clear and its probable form is shown diagrammatically in Fig. 3.

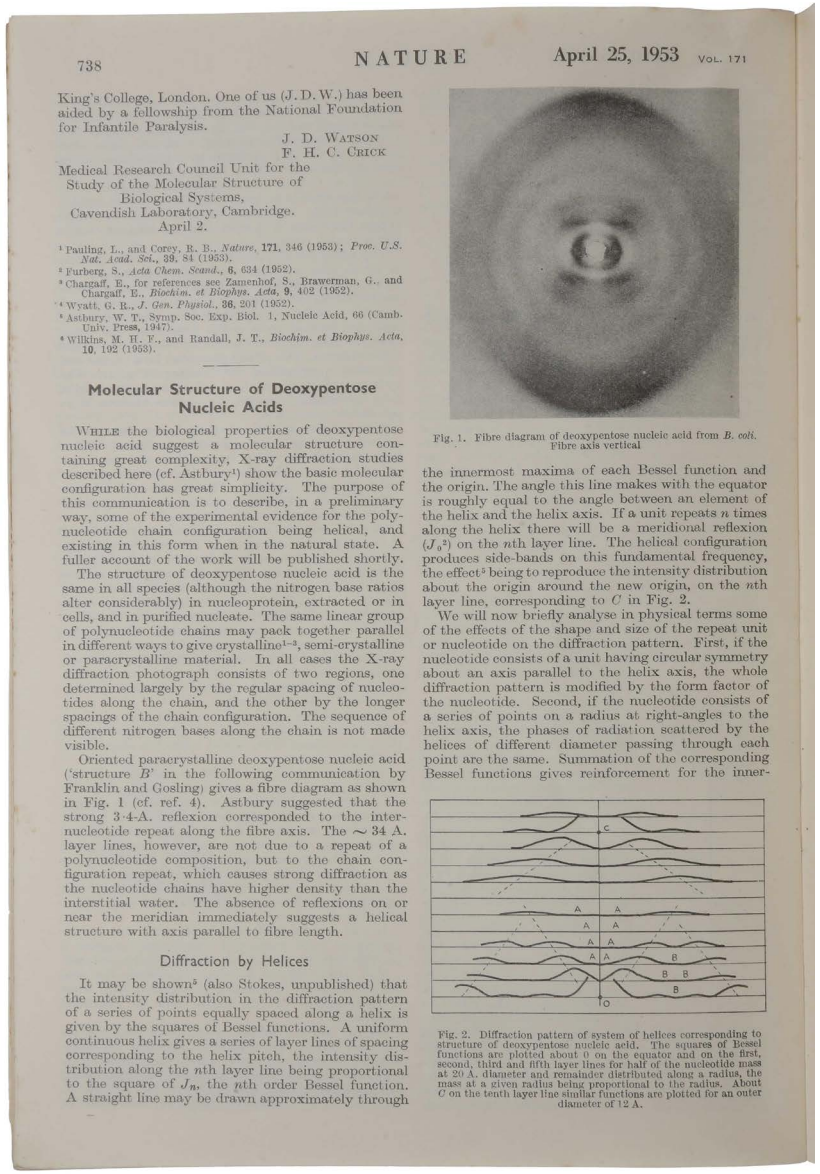
Circular Symmetry of the Form Function and the Elimination of Pseudo-Helical Structures

The fact that the form function, for example, on the 2nd layer line, alternates fairly smoothly between strong and very weak, indicates that it has circular symmetry about the fibre axis. Hence all pseudo-helical structures are eliminated. However, as the lattice is asymmetrical, a symmetrical form function is quite likely to produce general asymmetry of the intensity distribution. This effect, combined with some ambiguity of indexing, gave rise to the suggestion[5] that there might be a very definite asymmetry in the form function and therefore in the structure itself. If this were the case, the structure could not be helical unless the helix were considerably distorted. Probably the greatest asymmetry of reflected energy

"The initial description of the linear duplex structure of DNA by James Watson and Francis Crick in the early 1950s was truly a monumental advance. At that time, technology did not exist for isolating a gene, determining its nucleotide sequence, or relating such a sequence to the amino acid sequence of the corresponding protein. Messenger RNA had not been discovered, and very little was known about protein synthesis. It was evident that there were many different proteins in the cells of each organism, and it was becoming apparent that most proteins consist of a linear sequence of amino acids … how the nucleotide sequence of each gene was related to the amino acid sequence of its encoded protein remained a major unanswered question.

"In their landmark 1961 Nature paper entitled 'General Nature of the Genetic Code for Proteins,' Francis Crick, Leslie Barnett, Sydney Brenner, and Richard Watts-Tobin finally solved the riddle. They concluded correctly that the genetic code is a triplet code, the code is degenerate, triplets are not overlapping, there are no commas (although introns were subsequently discovered), and each nucleotide sequence is read from a specific starting point" (Yanofsky, 'Establishing the Triplet Nature of the Genetic Code,' Cell 128 (2007), 815-8).

Grolier Club, One Hundred Books Famous in Medicine, 99; Dibner, Heralds of Science, 200. Garrison-Morton 256.3, 256.4, 256.8, 752.1, 752.7; Judson, Eighth Day of Creation, pp. 145-56 & 184-5. Norman 534.



738 NATURE April 25, 1953 VOL. 171

King's College, London. One of us (J. D. W.) has been aided by a fellowship from the National Foundation for Infantile Paralysis.

J. D. WATSON
F. H. C. CRICK

Medical Research Council Unit for the Study of the Molecular Structure of Biological Systems,
Cavendish Laboratory, Cambridge.
April 2.

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Molecular Structure of Deoxypentose Nucleic Acids

WHILE the biological properties of deoxypentose nucleic acid suggest a molecular structure containing great complexity, X-ray diffraction studies described here (cf. Astbury[1]) show the basic molecular configuration has great simplicity. The purpose of this communication is to describe, in a preliminary way, some of the experimental evidence for the polynucleotide chain configuration being helical, and existing in this form when in the natural state. A fuller account of the work will be published shortly.

The structure of deoxypentose nucleic acid is the same in all species (although the nitrogen base ratios alter considerably) in nucleoprotein, extracted or in cells, and in purified nucleate. The same linear group of polynucleotide chains may pack together parallel in different ways to give crystalline[1-3], semi-crystalline or paracrystalline material. In all cases the X-ray diffraction photograph consists of two regions, one determined largely by the regular spacing of nucleotides along the chain, and the other by the longer spacings of the chain configuration. The sequence of different nitrogen bases along the chain is not made visible.

Oriented paracrystalline deoxypentose nucleic acid ('structure *B*' in the following communication by Franklin and Gosling) gives a fibre diagram as shown in Fig. 1 (cf. ref. 4). Astbury suggested that the strong 3·4-A. reflexion corresponded to the inter-nucleotide repeat along the fibre axis. The ~ 34 A. layer lines, however, are not due to a repeat of a polynucleotide composition, but to the chain configuration repeat, which causes strong diffraction as the nucleotide chains have higher density than the interstitial water. The absence of reflexions on or near the meridian immediately suggests a helical structure with axis parallel to fibre length.

Diffraction by Helices

It may be shown[5] (also Stokes, unpublished) that the intensity distribution in the diffraction pattern of a series of points equally spaced along a helix is given by the squares of Bessel functions. A uniform continuous helix gives a series of layer lines of spacing corresponding to the helix pitch, the intensity distribution along the nth layer line being proportional to the square of J_n, the nth order Bessel function. A straight line may be drawn approximately through

Fig. 1. Fibre diagram of deoxypentose nucleic acid from *B. coli*. Fibre axis vertical

the innermost maxima of each Bessel function and the origin. The angle this line makes with the equator is roughly equal to the angle between an element of the helix and the helix axis. If a unit repeats n times along the helix there will be a meridional reflexion (J_n^2) on the nth layer line. The helical configuration produces side-bands on this fundamental frequency, the effect[5] being to reproduce the intensity distribution about the origin around the new origin, on the nth layer line, corresponding to C in Fig. 2.

We will now briefly analyse in physical terms some of the effects of the shape and size of the repeat unit or nucleotide on the diffraction pattern. First, if the nucleotide consists of a unit having circular symmetry about an axis parallel to the helix axis, the whole diffraction pattern is modified by the form factor of the nucleotide. Second, if the nucleotide consists of a series of points on a radius at right-angles to the helix axis, the phases of radiation scattered by the helices of different diameter passing through each point are the same. Summation of the corresponding Bessel functions gives reinforcement for the inner-

Fig. 2. Diffraction pattern of system of helices corresponding to structure of deoxypentose nucleic acid. The squares of Bessel functions are plotted about 0 on the equator and on the first, second, third and fifth layer lines for half of the nucleotide mass at 20 A. diameter and remainder distributed along a radius, the mass at a given radius being proportional to the radius. About C on the tenth layer line similar functions are plotted for an outer diameter of 12 A.

CONTINENTAL DRIFT

WEGENER, Alfred. *Die Entstehung der Kontinente.* Gotha: Justus Perthes, 1912.

$5,500

Pp. 185-195, 253-256, 305-309 and one folding plate (no. 36) in three complete issues of Dr. A. Petermanns Mitteilungen aus Justus Perthes' geographischer Anstalt, Bd. 58, April, May & June 1912. 4to (277 x 230 mm), pp. [185]-248 with 8 plates (5 folding); [249]-304 with 7 folding plates; [iii], iv-xvi, [305]-314 with 6 plates (3 folding), many of the plates being coloured. Original printed wrappers, old tape repairs to hinges, extremeties slightly frayed. Rare in wrappers. Custom cloth box.

First edition, journal issues in the original printed wrappers. "Wegener is remembered today as the originator and one of the chief proponents of the theory of continental drift, which he conceived after being struck by the apparent correspondence in the shapes of the coastlines on the west and east sides of the Atlantic, and supported with extensive research on the geological and paleontological correspondences between the two sides. He postulated that 200 million years ago there existed a supercontinent ('Pangaea'), which began to break up during the Mesozoic era due to the cumulative effects of the 'Eötvös force,' which drives continents towards the equator, and the tidal attraction of the sun and moon, which drags the earth's crust westward with respect to its interior. Wegener's drift mechanism was later shown to be untenable; it has been replaced by the idea of convection currents in the earth's upper mantle. Wegener's first publication on continental drift appeared in three issues of Petermanns Mitteilung in April-June 1912; however, Wegener's theory attracted little interest until 1919, when he published the second edition of his treatise Die Entstehung

der Kontinente und Ozeane. Between 1919 and 1928 continental drift was "the focus of much controversy and debate, but the theory afterwards fell into obscurity, not to be revived until the discovery of new paleomagnetic evidence in the 1950s" (Norman). Wegener (1880-1930) died at the early age of 50 on an arctic expedition at Eismitte in Greenland. ABPC/RBH record no copies of this important paper in the original printed wrappers since the Norman copy, which was rebacked (Christie's, 29 October 1998, lot 1337, $2185).

Before Wegener put forward his revolutionary theory, it "was widely believed that continents and ocean basins are primordial features. This conviction was reinforced by global oceanographic surveys in 1872-77 demonstrating the Earth's bimodal elevation frequency, and simultaneously by gravimetric and geodetic surveys in the western U.S. and elsewhere that confirmed the principle of isostasy (i.e. an elastic crust that floats on a fluid medium). A continent can neither rise from the abyss or sink to abyssal depth spontaneously. The mass excess of its elevation is compensated by a mass deficit at depth. If it were to move sideways, it would have to drag its moorings along with it, which was thought to be absurd. Isostasy cut both ways however: it rendered physically implausible the land 'bridges' invoked by geologists to account for ancient floral and faunal similarities between continents now far apart" (Hoffmann, 'The tooth of time: Alfred Wegener,' Geoscience Canada 39 (2012), 102-111).

Wegener's interest in the problem was awakened by two chance observations. "Wegener's office mate had received a world atlas with up-to-date bathymetric maps for Christmas in 1910. They noticed that the east coast of South America appears to fit against the west coast of Africa, "as if they had once been joined". The fit is even better, Wegener continued, if the tops of the respective continental slopes are matched instead of the present coastlines. "This is an idea I'll have to pursue", but he did nothing more with it until the Fall of 1911, when he "quite accidentally" came upon a treatise on continental paleogeography (strata, flora, fauna and climate), compiled by a German high-school teacher only two years older than himself. Here, Wegener learned of the remarkable similarities in Mesozoic flora and fauna between Brazil and Gabon, and also of the concept of sunken 'land bridges' then widely invoked by geologists to account for such linkages. As a geophysicist interested in glaciology, he

was more convinced than contemporary geologists that isostasy precludes land bridges from sinking to abyssal depth. When Wladimir Köppen gently advised him not to stray too far from what he knew, Wegener wrote back (in early December) that the geological linkages require either land bridges or continental displacements, but "a continent cannot sink, for it is lighter than that upon which it is floating. Therefore, let us, just for once, take [displacement] into consideration! If such a series of astonishing

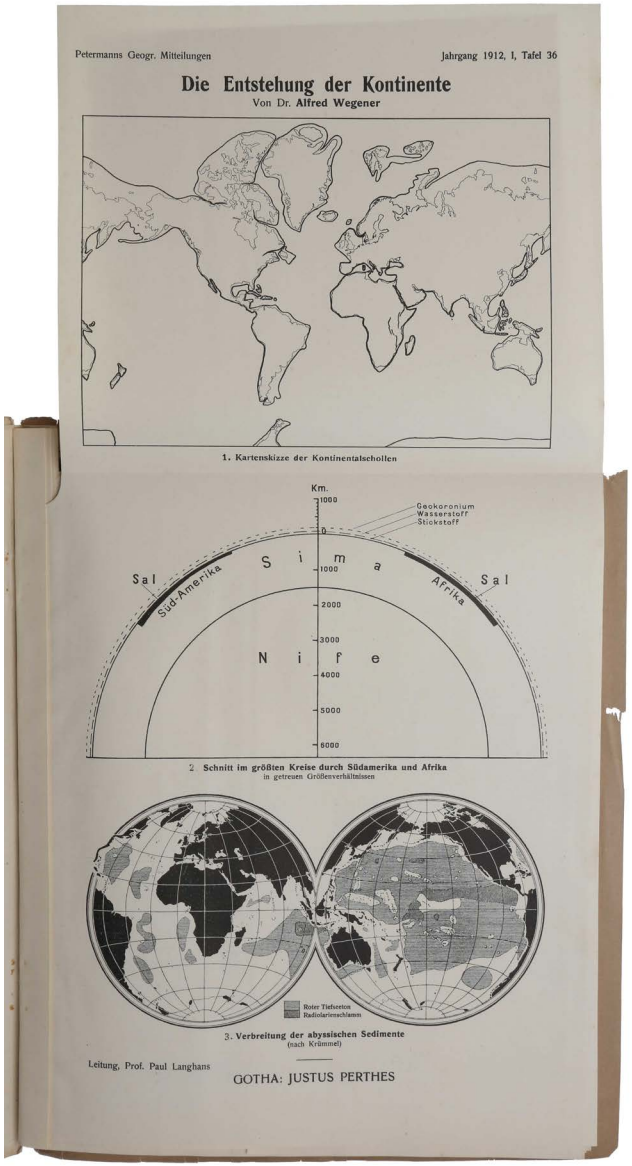
simplifications follow, and if it is shown that 'rhyme and reason' will now come to Earth history, why should we hesitate to cast the old view overboard?"" (Hoffmann).

On 6 January 1912 Wegener "presented a startling new vision of crustal history at a meeting of the recently founded Geological Association (Geologische Vereinigung) in Frankfurt. The talk did not bring pleasure to its listeners. Not yet 32, Alfred Wegener had already published in several branches of meteorology and his admired textbook, Thermodynamics of the Atmosphere (1911) showed him to be unusually skilled at synthesis. But he was unknown in geology and had only been seriously reading the geological literature for about four months. Nevertheless, so many published facts seemed inexplicable if his theory was wrong, that he submitted the text of his talk to the Geological Association under the brash title, The Origin of Continents [Die Entstehung der Kontinente]. He proposed that geological interpretations would be greatly simplified if continents

were allowed to undergo large relative horizontal displacements. The continents of today are the fragments of an ancestral landmass that rifted apart progressively in Mesozoic and Cenozoic time, allowing the Atlantic and Indian Ocean basins to grow at the expense of the Pacific. Not satisfied, he wrote an expanded version under the same title that was published in a leading geographical journal in three installments [the offered paper]. From the start, geographers were as engaged as geologists in the controversy over continental drift. But with war clouds looming over Europe and RMS Titanic hogging the headlines, it would be ten years and three editions of his subsequent book, The Origin of Continents and Oceans [Die Entstehung der Kontinente und Ozeane, 1915], before Wegener-bashing began in earnest.

"The longer 1912 paper came out in three installments: (1) geophysical arguments, (2) geological arguments, and (3) remaining geological arguments, present displacements and polar wobble. In (1) he introduces the elevation duality, gravity measurements and isostasy, thickness of the continental rafts, their composition, their plasticity in relation to that of their substrate, volcanism, and possible causes of displacement. Wegener did not distinguish between oceanic crust and mantle: the composition of the mantle was then unknown. He used [Eduard] Suess's terms, 'sial' for the continental rafts and 'sima' for the substrate, assumed to be directly covered by abyssal sediments. He uses the term 'crust' as synonymous with 'lithosphere'. He expends little space on causes, which he considers to be premature. "It will be necessary first to exactly determine the reality and the nature of the displacements before we can hope to discover their causes …

"The geological arguments are the strongest part of the paper and surprise even today. He reviews the evidence for active rifting in the Rhinegraben and the Red Sea – East African rift system. He compares the structure and geological history of his Atlantic conjugate margins, estimating the age of opening of different


Petermanns Geogr. Mitteilungen Jahrgang 1912, I, Tafel 36

Die Entstehung der Kontinente

Von Dr. Alfred Wegener

1. Kartenskizze der Kontinentalschollen

2 Schnitt im größten Kreise durch Südamerika und Afrika in getreuen Größenverhältnissen

3. Verbreitung der abyssischen Sedimente (nach Krümmel)

Leitung, Prof. Paul Langhans

GOTHA: JUSTUS PERTHES

segments and speculating on connections between South Atlantic opening and Andean contraction. His estimates are everywhere too young—Paleogene (actually Early Cretaceous) in the South Atlantic, Neogene (actually Jurassic) in the North Atlantic and Quaternary (actually Eocene) between NW Europe and Greenland. The last estimate in particular led him to predict that the separation rate between NW Europe and Greenland is ~2 meters per year and testable by geodetic experiment. His separation age being at least 100 times too young, the rate is too fast by the same multiple. In the next section, on Gondwanaland, his estimated separation ages for Africa–Madagascar, Australia–Antarctica, and Australia–India are broadly correct. Why did he insist that no ocean existed to the northwest of Europe in the Pleistocene? It is because 'steppe animals' (mammoth, woolly rhino, etc.) existed in Central Europe during Pleistocene interglacial times, but not during the Holocene. He infers a climate like southern Russia and western Siberia for Central Europe, which would be "implausible with the present ocean so close in the west". It remains a sound argument, but for the human 'overkill' hypothesis. Next he turns to the 'Permian' glaciation, represented by "indisputable ground moraines" on "typically striated pavements" in Australia, South Africa, eastern India and South America. With continents in fixed positions, Permian ice sheets occurred across most of the southern hemisphere, while in the northern hemisphere no verified Permian glacial deposit exists anywhere. This represents "a hopeless riddle for paleogeography." In Wegener's continental reconstruction, the various ice sheets are brought together into an area no larger than that occupied by the Pleistocene ice sheets. He infers that this area was centered over the south pole, which would then have been located near the southern tip of Africa. The north pole would lie in the north Pacific Ocean, taking "everything mysterious away from the phenomenon." The paper reaches its climax when Wegener contrasts the Atlantic–Indian and Pacific ocean basins, explicitly as described in the opening stanzas of [Suess's] Das Antlitz der Erde (1904). The Atlantic margins, with their "ragged shorelines and cut tablelands", follow the inner sides of older mountain belts (Appalachians, Caledonides, Mauritanides, Cape Foldbelt). The same is true for the Indian Ocean, except west of the Indus River and east of the Bay of Bengal, where the active Eurasian mountain front "spills into the ocean" in the Makran and the greater Sunda arc. In the Pacific, smooth arcuate coastlines or volcanic chains parallel fold belts that are everywhere vergent toward the ocean. "No fold belt borders the Pacific from its inner side; no platform projects into the ocean." He notes that the Pacific is on the whole deeper than the Atlantic, with correspondingly less calcareous abyssal sediments, and that Pacific volcanic rocks are less chemically evolved. These differences follow automatically from the hypothesis: "While the Atlantic opens, nearly all the Pacific margins approach towards its center; along its coasts widespread compression and convergence occur, but tension and rifting in the Atlantic". Foreshadowing the Wilson cycle he writes, "the rift that once opened to form the Pacific and to compress the primeval continent [Pangaea] from both sides, originated in oldest geological times, and the resulting motion was long extinct when the forces (that formed the Atlantic) commenced." Returning to the Atlantic, he suggests an explanation for seafloor topography. Since large areas of the seafloor are isostatically compensated, areas that are younger and hotter will be modestly elevated over those that are older and colder. "The depth variation appears also to suggest that the Mid-Atlantic Ridge should be regarded as the zone in which the floor of the Atlantic, as it keeps spreading, is continuously tearing open and making space for fresh, relatively fluid and hot sima from depth." This is not seafloor spreading as we now know it—no oceanic crust is formed by partial melting of mantle peridotite. Rather, he visualizes the sima as being exhumed in a solid state, as it does in the transition to seafloor spreading on non-volcanic margins. It is close enough to seafloor spreading, however, that one is left to wonder why Wegener subsequently abandoned such a promising lead. Had he not been deceived into thinking that the sima would readily accommodate the drift of tabular crustal bergs, would he not have tried moving the sima along with the sial? After all, he was not driven by

any particular geodynamic mechanism (he admitted he had none), he was driven by the converging lines of geological evidence.

"Wegener concludes the geological arguments with paleoclimatic (mainly floral) evidence for polar wander (i.e. true polar wander), which he assumes is as important as continental drift in accounting for observed changes in paleolatitude since the Permian glaciation. Moreover, he suggests that continental displacements were the cause of polar shifts, because "the pole of rotation must follow the pole of inertia". He considers it premature, however, to interpret the 'Lower Cambrian' glaciations in Norway, China and Australia (read Cryogenian snowball Earth) in terms of polar wander. Wise man! The final and shortest section of the paper concerns geodetic proofs (i.e. tests) of active continental displacement. He describes astronomical determinations of longitude by successive expeditions to particular sites in Greenland, and longitude differences between Europe and North America from Trans-Atlantic cables. Wegener came in for heavy criticism from geographers for suggesting that such data were consistent with displacement. A more charitable view is that Wegener was providing 'proof of concept' and a baseline for "astronomical positioning during the course of several decades." Wegener concludes with a comment on polar wobble, discovered by the American astronomer Seth Chandler in 1891 and monitored since 1899 by the International Latitude Service. He suggests that a shift in the inertial axis would cause the centre of the perturbation curve to migrate as well. He speculates that continental displacements might be the cause of the wobble itself. "This is because a perturbation once present must come to rest in spirals so that the pole of rotation and that of inertia will coincide as a consequence of the work it does in the Earth's viscous interior. If the pole of inertia shifts, the pole of rotation moves out at a right angle and follows the perturbation curve, first with a large radius, then with a smaller and smaller one until it reaches the new pole of inertia" (Hoffmann).

"The period between 1920 and 1924 marked Wegener's deepest involvement with the theory of continental displacements. A third edition of his book on the subject appeared in 1922 and was translated into English, French, Russian, Italian, Spanish, and Japanese. The theory was widely discussed and seems to have been favored more by geographers and paleoclimatologists than by geologists and geophysicists. It appealed to geologists whose fieldwork took place in the southern hemisphere much more than to those who worked in the northern hemisphere" (DSB).

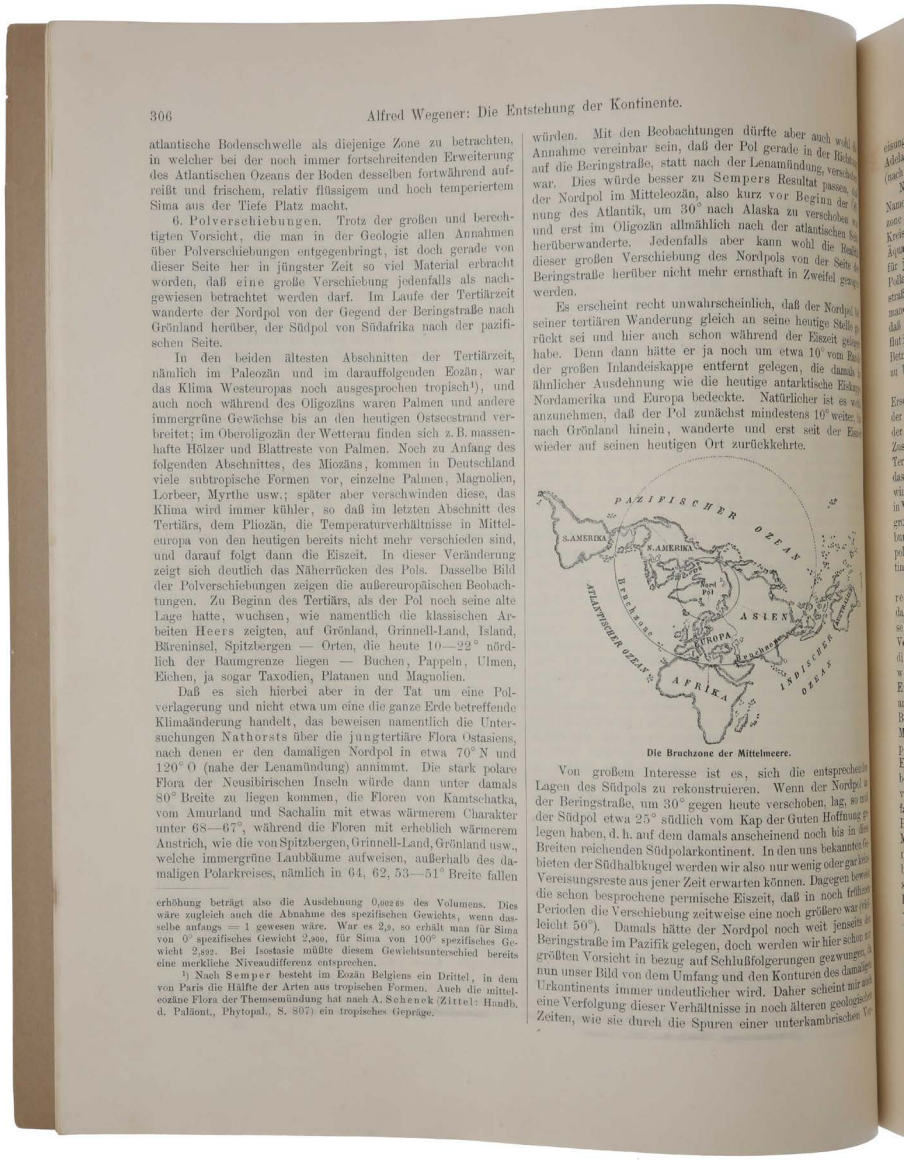
"The delayed reaction to Wegener and Köppen's theory by geologists, geographers and geophysicists took place between 1922 and 1928. Discussion meetings were held in England, South Africa and New York. Reviews of The Origin of Continents and Oceans appeared in leading international journals, starting with a highly favourable one in Nature of the 2nd edition. It concludes, "The revolution in thought, if the theory is substantiated, may be expected to resemble the change in astronomical ideas at the time of Copernicus. It is to be hoped that an English edition will soon appear." Others were less kind. The reaction to Wegener has been a focus of attention by historians (including geologists), seeking reasons for the fury of Wegener's critics" (Hoffmann).

"However, almost half a century later, with the advent of new methods and knowledge (sea floor spreading) and the discovery of paleomagnetism (1950), this concept was fully revived and fully accepted, upgraded and improved. The model of the motion of large planetary plates (continental and oceanic) gave birth to the theory of plate tectonics. Acceptance of this theory over the last 50 years has radically changed scientific knowledge about the mechanisms and types of movements that have led to global changes on the Earth (climate change, melting glaciers, creating a system of mountain, ocean circulation, earthquakes, volcanoes and other geological phenomena)" (Rundić, 'Centenary anniversary of the theory of continental drift by Alfred Wegener and its significance for geosciences and

human society,' Bulletin of the Natural History Museum 5 (2012), 21-33).

Wegener's lecture to the Geologische Vereinigung was printed in Geologische Rundschau, Bd. 3, n. 4, 9 July 1912, pp. 276-292. Although composed first, it was thus published later than the present greatly expanded work, which appeared in April-June of the same year.

Norman 2192. Marvin, Continental drift: The evolution of a concept. Washington DC: Smithsonian Institution Press, 1982 (see pp. 66-95).



306 Alfred Wegener: Die Entstehung der Kontinente.

atlantische Bodenschwelle als diejenige Zone zu betrachten, in welcher bei der noch immer fortschreitenden Erweiterung des Atlantischen Ozeans der Boden desselben fortwährend aufreißt und frischem, relativ flüssigem und hoch temperiertem Sima aus der Tiefe Platz macht.

6. Polverschiebungen. Trotz der großen und berechtigten Vorsicht, die man in der Geologie allen Annahmen über Polverschiebungen entgegenbringt, ist doch gerade von dieser Seite her in jüngster Zeit so viel Material erbracht worden, daß eine große Verschiebung jedenfalls als nachgewiesen betrachtet werden darf. Im Laufe der Tertiärzeit wanderte der Nordpol von der Gegend der Beringstraße nach Grönland herüber, der Südpol von Südafrika nach der pazifischen Seite.

In den beiden ältesten Abschnitten der Tertiärzeit, nämlich im Paleozän und im darauffolgenden Eozän, war das Klima Westeuropas noch ausgesprochen tropisch[1]), und auch noch während des Oligozäns waren Palmen und andere immergrüne Gewächse bis an den heutigen Ostseestrand verbreitet; im Oberoligozän der Wetterau finden sich z. B. massenhafte Hölzer und Blattreste von Palmen. Noch zu Anfang des folgenden Abschnittes, des Miozäns, kommen in Deutschland viele subtropische Formen vor, einzelne Palmen, Magnolien, Lorbeer, Myrthe usw.; später aber verschwinden diese, das Klima wird immer kühler, so daß im letzten Abschnitt des Tertiärs, dem Pliozän, die Temperaturverhältnisse in Mitteleuropa von den heutigen bereits nicht mehr verschieden sind, und darauf folgt dann die Eiszeit. In dieser Veränderung zeigt sich deutlich das Näherrücken des Pols. Dasselbe Bild der Polverschiebungen zeigen die außereuropäischen Beobachtungen. Zu Beginn des Tertiärs, als der Pol noch seine alte Lage hatte, wuchsen, wie namentlich die klassischen Arbeiten Heers zeigten, auf Grönland, Grinnell-Land, Island, Bäreninsel, Spitzbergen — Orten, die heute 10—22° nördlich der Baumgrenze liegen — Buchen, Pappeln, Ulmen, Eichen, ja sogar Taxodien, Platanen und Magnolien.

Daß es sich hierbei aber in der Tat um eine Polverlagerung und nicht etwa um eine die ganze Erde betreffende Klimaänderung handelt, das beweisen namentlich die Untersuchungen Nathorsts über die jungtertiäre Flora Ostasiens, nach denen er den damaligen Nordpol in etwa 70° N und 120° O (nahe der Lenamündung) annimmt. Die stark polare Flora der Neusibirischen Inseln würde dann unter damals 80° Breite zu liegen kommen, die Floren von Kamtschatka, vom Amurland und Sachalin mit etwas wärmerem Charakter unter 68—67°, während die Floren mit erheblich wärmerem Anstrich, wie die von Spitzbergen, Grinnell-Land, Grönland usw., welche immergrüne Laubbäume aufweisen, außerhalb des damaligen Polarkreises, nämlich in 64, 62, 53—51° Breite fallen

erhöhung beträgt also die Ausdehnung $0{,}00289$ des Volumens. Dies wäre zugleich auch die Abnahme des spezifischen Gewichts, wenn dasselbe anfangs = 1 gewesen wäre. War es 2,9, so erhält man für Sima von 0° spezifisches Gewicht 2,900, für Sima von 100° spezifisches Gewicht 2,892. Bei Isostasie müßte diesem Gewichtsunterschied bereits eine merkliche Niveaudifferenz entsprechen.

1) Nach Semper besteht im Eozän Belgiens ein Drittel, in dem von Paris die Hälfte der Arten aus tropischen Formen. Auch die mitteleozäne Flora der Themsemündung hat nach A. Schenck (Zittel: Handb. d. Paläont., Phytopal., S. 807) ein tropisches Gepräge.

würden. Mit den Beobachtungen dürfte aber auch wohl d Annahme vereinbar sein, daß der Pol gerade in der Richtu auf die Beringstraße, statt nach der Lenamündung, verschob war. Dies würde besser zu Sempers Resultat passen, d der Nordpol im Mitteleozän, also kurz vor Beginn der E nung des Atlantik, um 30° nach Alaska zu verschoben w und erst im Oligozän allmählich nach der atlantischen Sei herüberwanderte. Jedenfalls aber kann wohl die Hypo dieser großen Verschiebung des Nordpols von der Seite d Beringstraße herüber nicht mehr ernsthaft in Zweifel gezog werden.

Es erscheint recht unwahrscheinlich, daß der Nordpol b seiner tertiären Wanderung gleich an seine heutige Stelle g rückt sei und hier auch schon während der Eiszeit gelege habe. Denn dann hätte er ja noch um etwa 10° vom Rand der großen Inlandeiskappe entfernt gelegen, die damals ähnlicher Ausdehnung wie die heutige antarktische Eiskap Nordamerika und Europa bedeckte. Natürlicher ist es, wie anzunehmen, daß der Pol zunächst mindestens 10° weiter nach Grönland hinein, wanderte und erst seit der Eiszeit wieder auf seinen heutigen Ort zurückkehrte.

Die Bruchzone der Mittelmeere.

Von großem Interesse ist es, sich die entsprechenden Lagen des Südpols zu rekonstruieren. Wenn der Nordpol in der Beringstraße, um 30° gegen heute verschoben, lag, so muß der Südpol etwa 25° südlich vom Kap der Guten Hoffnung gelegen haben, d. h. auf dem damals anscheinend noch bis in diese Breiten reichenden Südpolarkontinent. In den uns bekannten Gebieten der Südhalbkugel werden wir also nur wenig oder gar keine Vereisungsreste aus jener Zeit erwarten können. Dagegen beweist die schon besprochene permische Eiszeit, daß in noch früheren Perioden die Verschiebung zeitweise eine noch größere war (etwa leicht 50°). Damals hätte der Nordpol noch weit jenseits der Beringstraße im Pazifik gelegen, doch werden wir hier schon größten Vorsicht in bezug auf Schlußfolgerungen gezwungen, da nun unser Bild von dem Umfang und den Konturen des damaligen Urkontinents immer undeutlicher wird. Daher scheint mir auch eine Verfolgung dieser Verhältnisse in noch älteren geologischen Zeiten, wie sie durch die Spuren einer unterkambrischen Ver