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November 2019

Rare and important books & manuscripts in science and medicine, by Christian Westergaard.

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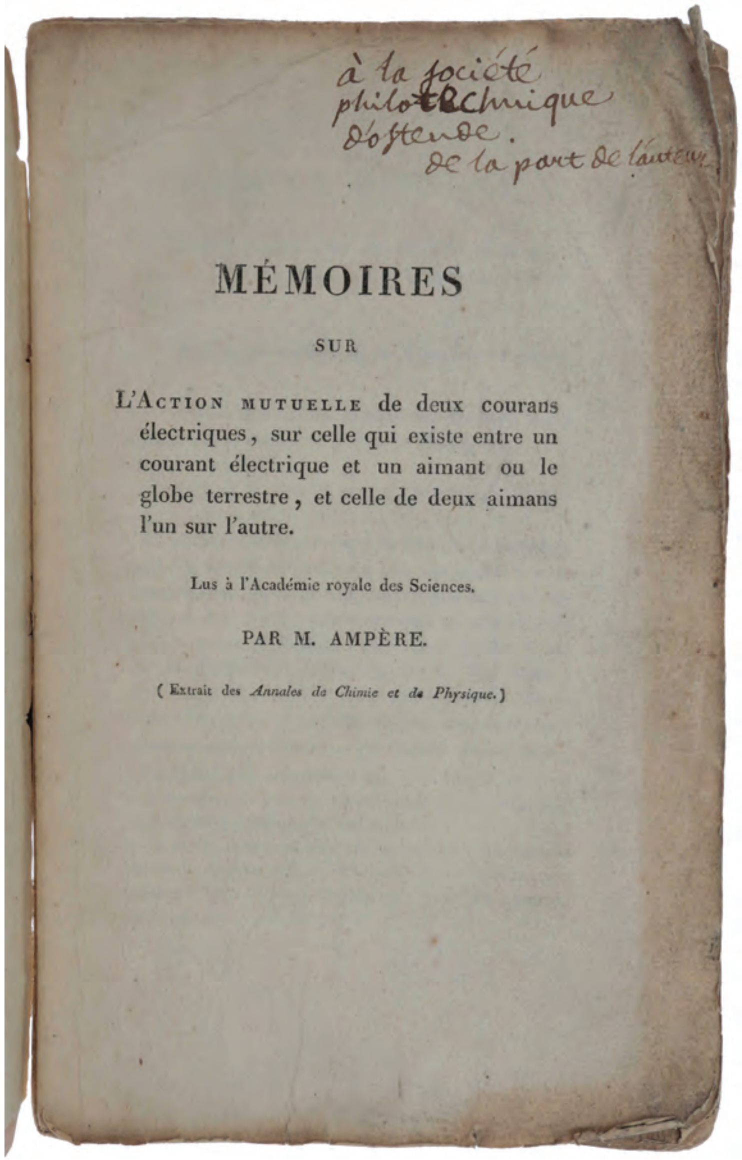
THE FOUNDATION OF ELECTRO-DYNAMICS, INSCRIBED BY AMPÈRE

AMPÈRE, Andre-Marie. *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.* [Paris: Feugeray, 1821].

$22,500

8vo (219 x 133mm), pp. [3], 4-112 with five folding engraved plates (a few faint scattered spots). Original pink wrappers, uncut (lacking backstrip, one cord partly broken with a few leaves just holding, slightly darkened, chip to corner of upper cover); modern cloth box. An untouched copy in its original state.

First edition, probable first issue, extremely rare and inscribed by Ampère, of this continually evolving collection of important memoirs on electrodynamics by Ampère and others. "Ampère had originally intended the collection to contain all the articles published on his theory of electrodynamics since 1820, but as he prepared copy new articles on the subject continued to appear, so that the fascicles, which apparently began publication in 1821, were in a constant state of revision, with at least five versions of the collection appearing between 1821 and 1823 under different titles" (Norman). The collection begins with 'Mémoires sur l'action mutuelle de deux courans électriques', Ampère's "first great memoir on electrodynamics" (DSB), representing his first response to the demonstration on 21 April 1820 by the Danish physicist Hans Christian Oersted (1777-1851) that electric currents create magnetic fields; this had been reported by François Arago (1786-1853) to an astonished Académie des Sciences on 4 September. In this article he



"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). This first paper is mostly phenomenological, but it is followed here by the important and much more mathematical sequel, 'Additions au mémoire précédent – note sur les expériences électro-magnétiques de MM. Oersted, Ampère, Arago et Biot,' in which Ampère gave the first quantitative expression for the force between current carrying conductors. Ampère attempted to explain his observations by postulating a new theory of magnetism – according to him, magnetic forces were the result of the motion of two electric fluids; permanent magnets contained these currents running in circles concentric to the axis of the magnet and in a plane perpendicular to this axis. By implication, the earth also contained currents which gave rise to its magnetism. Ampère's theory was attacked by the great Swedish chemist Jöns Jacob Berzelius in a letter to his French colleague Claude Louis Berthollet, to which Ampère replied in a letter to François Arago. These are the third and fourth items in this collection; the fifth and final part is the text of a lecture to the Académie on 2 April 1821 in which Ampère again stressed the identity of electricity and magnetism. The bibliographical complexity of this work is a direct result of Ampère's modus operandi: "His work was marked by flashes of insight, and it often happened that he would publish a paper in a journal one week, only to find the next week that he had thought of several new ideas that he felt ought to be incorporated into the paper. Since he could not change the original, he would add the revisions to the separately published reprints of the paper and even modify the revised versions later if he felt it necessary" (Norman). Only three other copies of this work listed by ABPC/RBH (all later issues). OCLC lists only one copy of this issue of the collection (University College, London), and we found no record of any earlier issue.

Provenance: Société Philotechnique d'Ostende (inscribed in the hand of Ampère).

The collection opens with the 'Premier Mémoire' [1] (numbering as in the list of contents, below), first published in Arago's Annales de Chimie et de Physique at the end of 1820 (Series 2, Tome 15, pp. 59-76 in the October issue & 170-218 in November, read 18 & 25 September). "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 an-

other 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.

"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).

The first quantitative expression for the force between current carrying conductors appeared in Ampère's less well-known 'Note sur les expériences électro-magnétiques' [2], which originally appeared in the Annales des Mines(Series 1, Tome 5, pp. 535-553). Ampère stated, without proof, that, if two infinitely small portions of electric current A and B, with intensities g and h, separated by a distance r, set at angles α and β to AB and in directions which created with AB two planes at an angle γ with each other, the force they exert on each other is

$$gh (\sin \alpha \sin \beta \sin \gamma + k \cos \alpha \cos \beta)/r^2,$$

where k is an unknown constant which he stated could 'conveniently' be taken to be zero. This last assumption was an error which significantly retarded his progress in the next two years before he stated correctly that k = – ½.

As far as Ampère was concerned, "The physical theory of electrodynamics was now complete. Given the concepts of the ether and the electromotive force of matter as Ampère had formulated them, all the observed effects could be explained; not only explained, but subjected to mathematical analysis. The combination was a potent one and the accuracy of Ampère's calculations and the depths of his insight led many to embrace his theory" (Williams, p. 150).

Not everyone was convinced of the identity of electricity and magnetism, however. Humphry Davy (1778-1829) expressed doubts in a letter to Ampère of 20 February 1821. Ampère's idea of magnetism created by circulating electric currents was also in direct opposition to a theory put forward by Johann Joseph von Prechtl (1778-1854), and supported by Berzelius (1779-1848), according to which electromagnetism was 'transverse magnetism' – whereas Ampère eliminated magnetism and showed how all the phenomena could be accounted for by the action of two electric fluids, Prechtl and Berzelius reduced electromagnetism to

magnetic action. Berzelius expressed this view in his letter [3]; Ampère responded in a letter to Arago [4]. Ampère again stressed the 'identity' of electricity and magnetism in a lecture to the Académie on 2 April 1821 [5].

The exceptionally complicated bibliography of this work has yet to be subjected to scholarly analysis. The text of the first article, 'Mémoires sur l'action mutuelle de deux courans électriques', appears to be essentially identical to that of the 'Extrait' from the Annales (Dibner 62; Norman 43; Sparrow 8). However, there are very significant textual differences between the Extrait and the journal article in the Annales. The journal article and Extrait begin to diverge at p. 16 of the Extrait (p. 73 of the journal): the last paragraph on p. 16, through to the end of p. 18, has been moved (with minor changes) from pp. 212-214 of the journal version. There are many other changes, too numerous to detail. The Extrait is thus not simply a separately-printed version of the journal article, and should perhaps not be regarded as an 'offprint' in the usual sense. The second article in the present collection, 'Note sur les expériences électro-magnétiques …', was first published in the Annales des Mines; the journal volume is dated 1820, but as the article was read on December 4 it seems likely to have been published early in 1821. No offprints of this article are known. Again there are many significant textual differences between the version that appears in this collection and the journal article. The first such difference occurs on p. 70 (p. 536 in the journal): the sentence ending "soit qu'ils soient enveloppés d'un papier ou introduits dans un tube de verre, afin qu'ils ne puissant communiquer avec ces filets" in the present work reads "soit qu'ils soient enveloppés d'un papier, ou introduits dans un tube de verre qui empêche leur contact direct" in the journal. The last three items in this collection are published here for the first time. It seems to us that the Extrait of the 'Mémoires sur l'action mutuelle' could be considered as the 'zeroth' version of this collection. We have found no record of any version intermediate between the Extrait and the present version of the collection, which is thus probably the first to contain any works other than the Extrait.

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Ekelof 819; Norman 44 & 45 (later issues with 278 & 360 pages, respectively); Ronalds 10; Wheeler Gift 784. Assis & Chaib, Ampère's Electrodynamics, 2015. Grattan-Guinness, Convolutions in French Mathematics, 1800-1840, 1990. Hofmann, André-Marie Ampère, 1995. Williams, Michael Faraday, 1965.

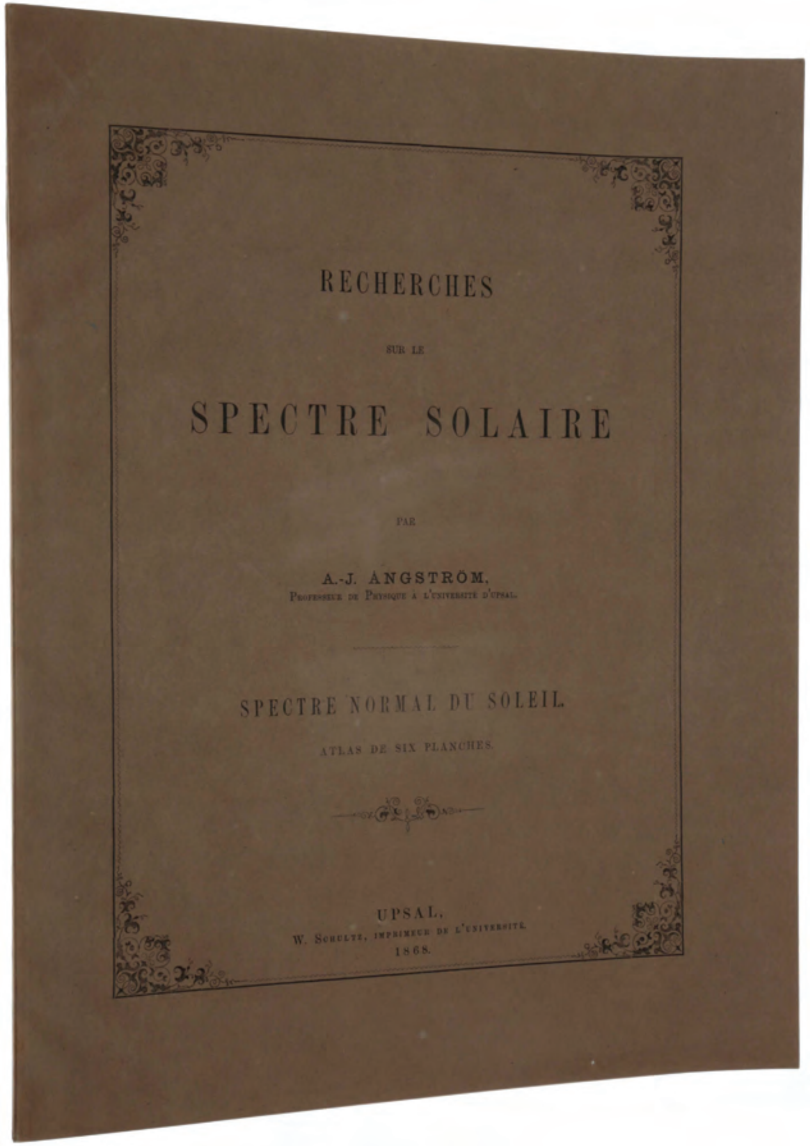
THE SOLAR SPECTRUM - AN EXCEPTIONALLY FINE SET

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

$8,500

Text: Large 4to, pp. [iv], 42, xv, [1], with lithographed frontispiece showing Ångström's spectrometer; Atlas: Oblong folio, [ii], with six plates by Robert Thalén (1827-1905). Original brown printed wrappers. Both in exceptionally fine condition.

First edition, a very fine set in unrestored original printed wrappers, and rare thus, 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. Since the plates of the atlas were simply laid in to the printed wrappers (and not sewn), the wrappers were often lost or damaged; it is rare to find these wrappers present and in fine condition as in our copy. In his Nobel Prize lecture, Arthur Schawlow (who shared the 1981 prize for his contributions to the development of laser spectroscopy) wrote: "Fraunhofer had charted the dark lines in the spectrum of the Sun, and had measured their wavelengths. But it was Ångström who first identified some of these lines as corresponding to bright lines emitted by particular substances … Most importantly, he showed the red line of hydrogen." "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). Spectroscopic studies were crucial to Max Planck's explanation of blackbody radiation, Albert Einstein's explanation of the photoelectric effect, and Niels Bohr's explanation of atomic structure. Spectra are used to detect, identify and quantify information about the chemical composition of substances in the laboratory, as well as in astronomy where they enable the determination of the chemical composition and physical properties of celestial objects.

"By the time that Ångström began his studies on spectral analysis at Uppsala, a fair amount of information was known, more experimentally than theoretically, about the solar spectrum. Optics had become a subject of intensive study during the first half of the nineteenth century, but there was little interest in identifying the cause of the lines on the spectra and in appreciating their structural implications … no significant progress had been made in the eighteenth century since Newton's classic investigations on sunlight and his experiments with prisms and reflecting telescopes … Newton failed to note that the light from the sun is not perfectly homogeneous; instead it was [William Hyde] Wollaston (1766-1828) who first discovered this effect by observing the rays of sunlight admitted through a narrow slit in a window blind. Wollaston's initial observation of seven dark lines, followed by [Joseph von] Fraunhofer's work, which included a greater number of lines in the solar spectrum, lies at the root of all subsequent works, including that of Ångström. The studies of these pioneers had shown that whereas sunlight differed from ordinary white light in having a spectrum of dark lines, colored light differed from the same white light in having a spectrum in which bright lines could be seen. Ångström familiarized himself with all of this work …" (Reif-Acherman).

"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 [and his map of the solar spectrum] around 1860, Ångström and his collaborator at Uppsala University, Robert Thalén (1827-1905), 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" (*Biographical Encyclopedia of Astronomers*).

"In 1868, Ångström published his most important work, 'Recherches sur le Spectre Solaire', in Uppsala. The essay, a compendium of all of his experiments, received considerable international attention and became the standard of spectroscopy for at least a quarter of a century … Because of its considerable greater dispersive power [i.e., that of Ångström's spectrometer], the information included in Ångström's map surpassed the information found in Kirchhoff's map, and the number of visible bands rose accordingly … Several dark bands on Kirchhoff's map resolved themselves into arrays of tightly packed lines … The measurements give the places of and map the solar lines in … the entire visible spectrum, and the wavelengths are expressed in ten-millionths of a millimetre with two decimal places. Each line was rendered in ink as it appeared to the eye, with coloration that ranged from pale grey to coal black. The work included an atlas of close to a thousand spectral lines, with, for example, 390 more iron-lines than were previously known" (Reif-Acherman).

"For this work a high quality spectrometer with collimator and viewing telescope manufactured by the Berlin firm of Pistor and Martins was used. The prism was replaced with a transmission [diffraction] grating ruled by F. A. Norbert, of which Ångström had two available, with respectively 4501 and 2701 grooves ruled in glass … Ångström calibrated his wavelength scale by carefully measuring nine principal solar lines, using measurements in several orders of diffraction … Over 1000 solar lines were measured and tied to this scale" (Hearnshaw, p. 102). Ångström's spectrometer is illustrated on the frontispiece of the text volume of the present work.

"The gratings were measured with a dividing engine, which allowed Ångström to determine the grating space by comparison with the standard meter possessed by the Uppsala Institute … Certain doubts that Ångström had about a major systematic error [that had] crept into his work, as a result of calibrating his meter,

were confirmed years later. In 1872, Georg Lindhagen (1819-1906) checked several meter standards and found that the earlier calibration of the Uppsala meter was incorrect, having a length of 999.94 mm instead of the 999.81 mm used by Ångström in his measurements. This deviation resulted in a clear systematic error of approximately 0.013% in all of the wavelengths previously measured. Ångström did not rush to revise his work, perhaps because he never thought that the differences in wavelengths regarding this error were as significant as were later confirmed, and he only commissioned this labor to his assistant Thalén in 1874. Ångström unexpectedly died that year with the calculations scarcely begun, leaving this problem unresolved" (Reif-Acherman).

"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 …

"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 …

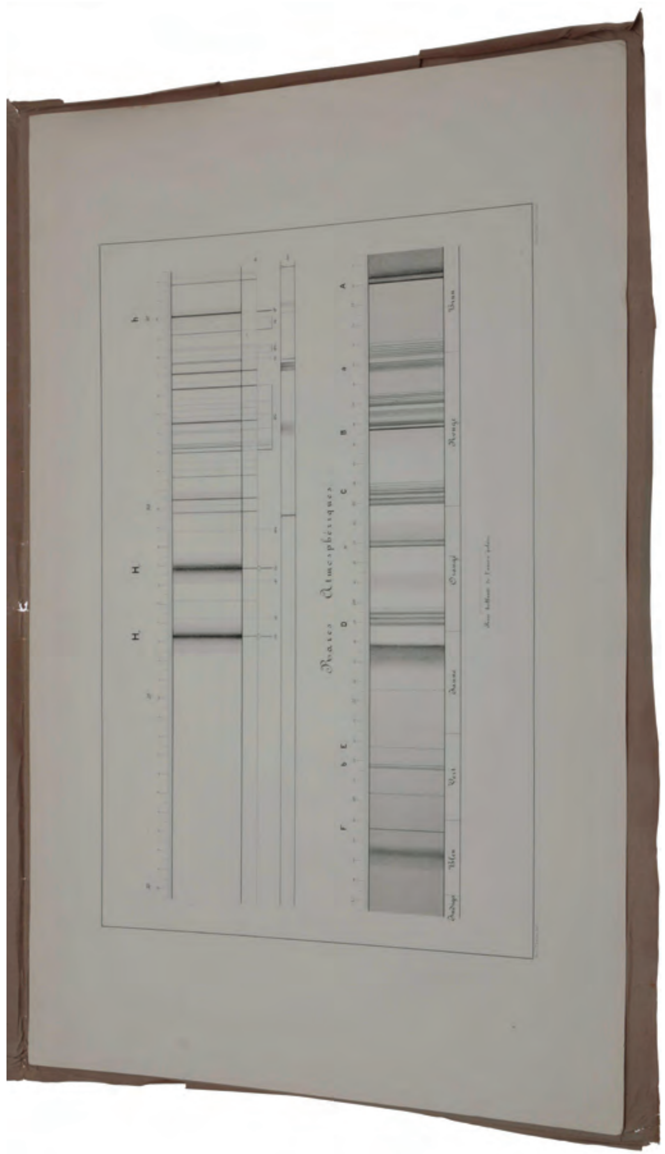
"Ångström became a member of the Royal Swedish Academy of Sciences in 1850, of the Prussian Academy of Sciences in 1867, of the Royal Society in 1870, and of the French Academy of Sciences in 1873. He was elected a member of several other Swedish and foreign scientific societies as well.

"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*).

It is sometimes said that a few copies of the atlas have a further two plates showing the ultraviolet spectrum (although the title of the atlas clearly states *Atlas de six Planches*). All copies we have located in auction records have only six plates, except for a presentation copy offered by Sotheby's, Paris in 2011, described as "Exemplaire bien complet de l'Atlas auquel sont ajoutées deux planches figurant le spectre de l'ultra-violet, d'après A. Cornu." These extra plates do not belong to the work but were added later from Cornu's 'Sur le spectre normale du soleil, partie ultra-violette', *Annales Scientifiques de l'École Normale Supérieure*, Sér. 2, T.

9 (1880), pp. 21-106, which included two plates of the ultraviolet spectrum. "The French scientist Marie Alfred Cornu (1841-1902), chair of physics at the École Polytechnique, extended Ångström's atlas to the ultraviolet with comparable accuracy by using photographic methods and similar diffraction gratings" (Reif-Acherman).

DSB I, p. 166; Norman 56; Honeyman 96. Hearnshaw, *Astronomical Spectrographs and their History*, 2009. Reif-Acherman, 'Anders Jonas Ångström and the foundation of spectroscopy – Commemorative article on the second centenary of is birth,' *Spectrochimica Acta*, Part B, vol. 102 (2014), pp. 12-23.



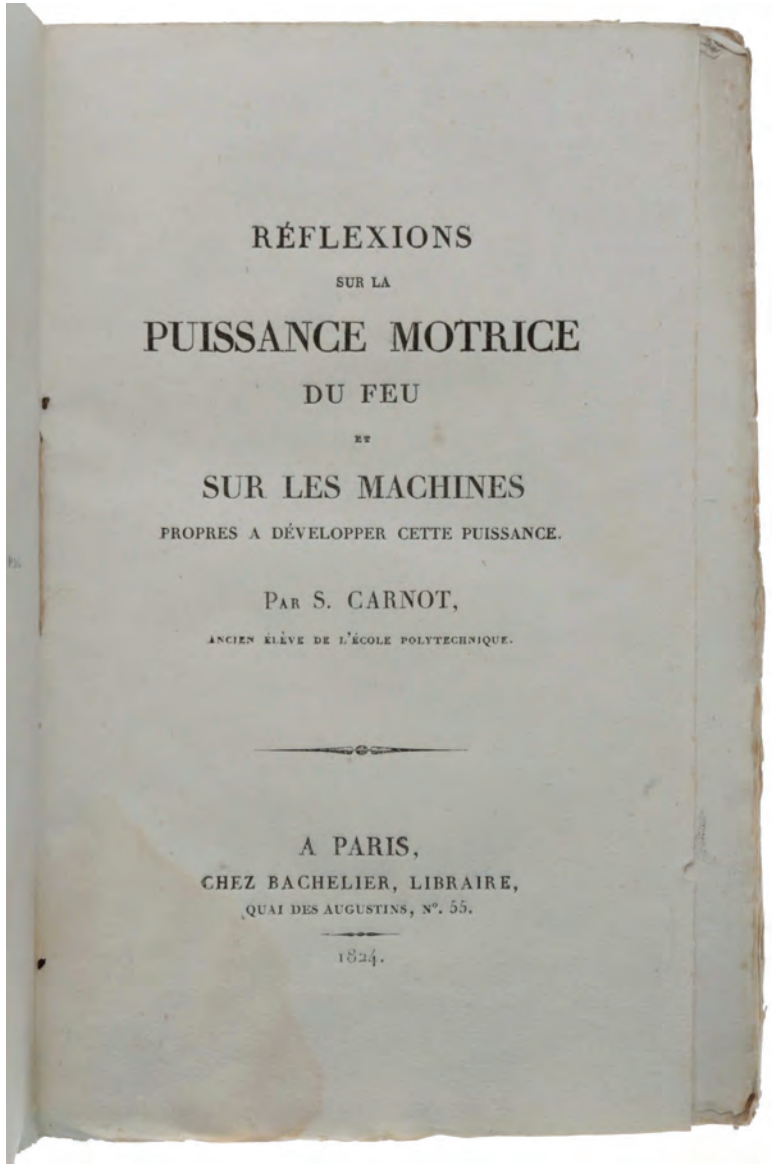
PMM 285 - THE MECHANICAL EQUIVALENT OF HEAT

CARNOT, Nicolas Leonard Sadi. *Réflexions sur la puissance motrice du feu et sur les machines propres à développer cette puissance. Atlas.* Paris: Giraudet for Bachelier, 1824.

$60,000

8vo (220 x 143 mm), pp. [iv], [1], 2-118, with one folding engraved plate. Contemporary marbled wrappers, uncut and mostly unopened, a very faint damp stain to lower left margin, an exceptional copy preserved in its original state. Custom morocco box.

First edition, very rare, and a fine copy in original state, of Carnot's only published work, which led directly to the first and second laws of thermodynamics. "Carnot, one of the most original thinkers among physical scientists, applied himself to the analysis of the cyclical operation of [heat] engines" (Dibner). "Carnot's treatise on the motive power of heat, which contains the first (albeit imperfect) statement of the second law of thermodynamics, was written to address a practical engineering problem that had occupied French physicists since 1815 – namely, how heat could be used most economically in the production of motive power … Carnot's originality lay in his recognition that the motive power of a heat engine was independent of the nature of the substance generating it – that it was a function, instead, of the transfer of heat from a warmer to a colder body. He also introduced the fundamental thermodynamic concept of completeness of cycle, in which the engine and working substance return to their original conditions. Carnot's



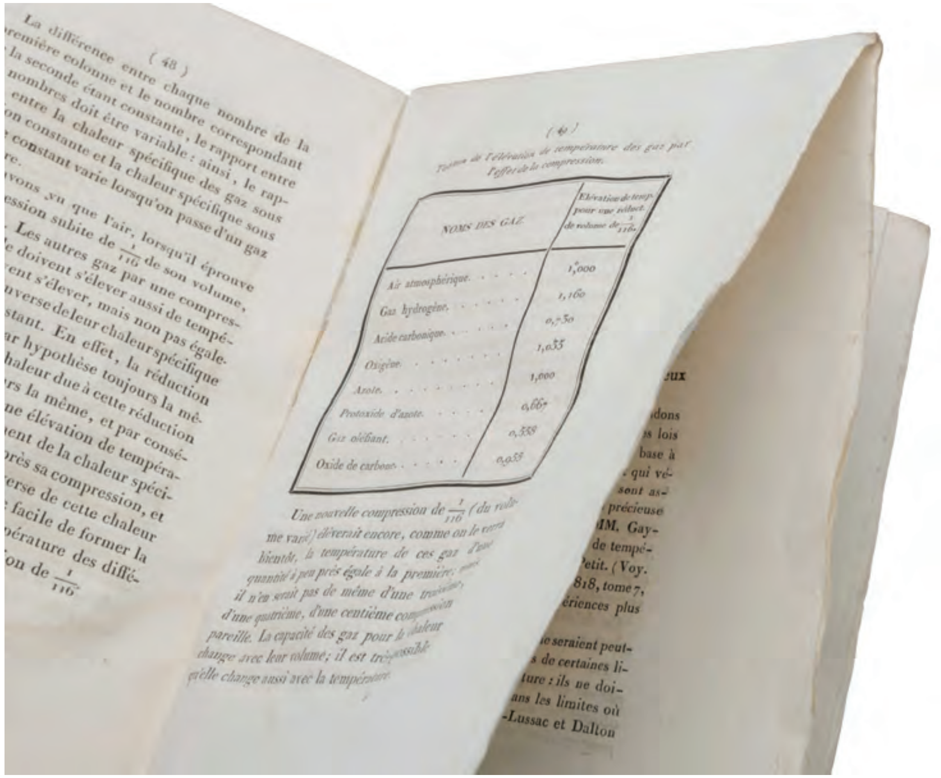
achievement was largely ignored by his contemporaries, and the *Réflexions* remained forgotten until rediscovered by William Thomson (Lord Kelvin) in the 1840s; Kelvin, one of the founders of modern thermodynamics, said of Carnot's work that 'nothing in the whole range of natural philosophy is more remarkable than the establishment of general laws by such a process of reasoning' (quoted in Fox, p. 1). The first edition of *Réflexions* was published in an edition of six hundred copies (see Fox, p. 23, illustrating the printer's bill)" (Norman). The *Réflexions* is now regarded as one of the great rarities of 19th-century science, and copies such as ours in completely original state are extremely difficult to find.

"After a concise review of the industrial, political, and economic importance of the steam engine, Carnot [in the *Réflexions*] raised two problems that he felt prevented further development of both the utility and the theory of steam engines. Does there exist an assignable limit to the motive power of heat, and hence to the improvement of steam engines? Are there agents preferable to steam in producing this motive power? As Carnot conceived it, the *Réflexions* was nothing more, nor less, than a 'deliberate examination' of these questions. Both were timely problems and, although French engineers had investigated them for a decade, no generally accepted solutions had been reached. In the absence of a clear concept of efficiency, proposed steam-engine designs were judged largely on practicality, safety, and fuel economy … The usual approach to these problems was either an empirical study of the fuel input and the work output of individual engines or the application of the mathematical theory of gases to the abstract operations of a specific type of engine. In his choice of problems Carnot was firmly in this engineering tradition; his method of attacking them, however, was radically new and is the essence of his contribution to the science of heat.

"Previous work on steam engines, as Carnot saw it, had failed for want of a sufficiently general theory, applicable to all imaginable heat engines and based on established principles. As the foundations for his study Carnot carefully set out three premises. The first was the impossibility of perpetual motion, a principle that had long been assumed in mechanics and had recently played an important role in the work of Lazare Carnot [Sadi's father]. As his second premise Carnot used the caloric theory of heat, which, in spite of some opposition, was the most accepted and most developed theory of heat available. In the *Réflexions*, heat (*calorique*) was always treated as a weightless fluid that could neither be created nor be destroyed in any process. As an element in Carnot's demonstrations this assumption asserted that the quantity of heat absorbed or released by a body in any process depends only on the initial and final states of the body. The final premise was that motive power can be produced whenever there exists a temperature difference. The production of motive power was due 'not to an actual consumption of caloric, but to its transportation from a warm body to a cold body.' Making the analogy with a waterwheel, Carnot observed that this motive power must depend on both the amount of caloric employed and the size of the temperature interval through which it falls. In his concept of reversibility Carnot also implicitly assumed the converse of this premise, that the expenditure of motive power will return caloric from the cold body to the warm body.

"The analysis of heat engines began … with an abstract, three-stage steam-engine cycle. The incompleteness of this cycle proved troublesome, and Carnot pushed the abstraction one step further, producing the ideal heat engine and the cycle that now bear his name. The 'Carnot engine' consisted simply of a cylinder and piston, a working substance that he assumed to be a perfect gas, and two heat reservoirs maintained at different temperatures. The new cycle incorporated the isothermal and adiabatic expansions and the isothermal compression of the steam engine, but Carnot added a final adiabatic compression in which motive power was consumed to heat the gas to its original, boiler temperature. In describing the engine's properties, Carnot introduced two fundamental thermodynamic concepts, completeness and reversibility. At the end of each cycle the engine and the working substance returned to their original conditions. This complete cycle not only provided an unambiguous definition of the input and output of the engine, but also rendered superfluous the detailed examination of each stage of the cycle. With each cycle the engine transferred a certain quantity of caloric from the high-temperature reservoir to the low-temperature reservoir and thereby produced a certain amount of motive power. Since each stage of the cycle could be reversed, the entire engine was reversible. Running backward, the engine consumed as much motive power as it produced running forward and returned an equal amount of caloric to the high-temperature reservoir. Joined together but operating in opposite directions, two engines would therefore produce no net effect.

"Carnot then postulated the existence of an engine that, by virtue of design or working substance, would produce more motive power than a 'Carnot engine' operating over the same temperature interval and with the same amount of caloric. A reversed 'Carnot engine' would be able to return to the boiler all of the caloric transported to the condenser by the hypothetical engine. Yet the reversed 'Carnot engine' would consume only a portion of the motive power produced by the hypothetical engine, leaving the remainder available for external work. Together these two engines would form a larger engine whose only net effect was the production of motive power in unlimited quantities. Since such a perpetual motion machine violated his first premise, Carnot concluded that no engine whatsoever produced more motive power than a 'Carnot engine.' Formulating the result now known as 'Carnot's theorem', he stated that 'the motive power of heat is independent of the agents employed to realize it; its quantity is fixed solely by the temperatures of the bodies between which is effected, finally, the transfer of caloric.'



"To elucidate further the motive power of heat, Carnot turned his attention to the physical properties of gases, a subject in which there had been considerable activity for over a decade. By 1823 a sizable body of experimental data on adiabatic and isothermal processes and on specific heats had been assimilated into the caloric theory of heat and mathematized by Laplace and Poisson. Combining the results of this activity with the concepts involved in his fundamental theorem, Carnot derived a series of seven theorems. With the exception of a long footnote in which he attempted to cast his results in algebraic form, Carnot developed his theorems in a synthetic, geometric manner that, although clear and logically rigorous, was in sharp contrast with the mathematical analysis dominant in the scientific community. Nonetheless, at least three of the theorems represented major advances. The first, that the quantity of heat absorbed or released in isothermal changes is the same for all gases, was experimentally established by Dulong in 1828, but without any reference to Carnot. In a very subtle verbal argument, Carnot also demonstrated that 'the difference between specific heat under constant pressure and specific heat under constant volume is the same for all gases.' The final theorem proved that the fall of caloric produces more motive power when the temperature interval is located lower rather than higher on the temperature scale. Although aware of the uncertainties introduced by some assumptions and experimental data for specific heat changes, Carnot was able to calculate motive power values and to verify the theorem …

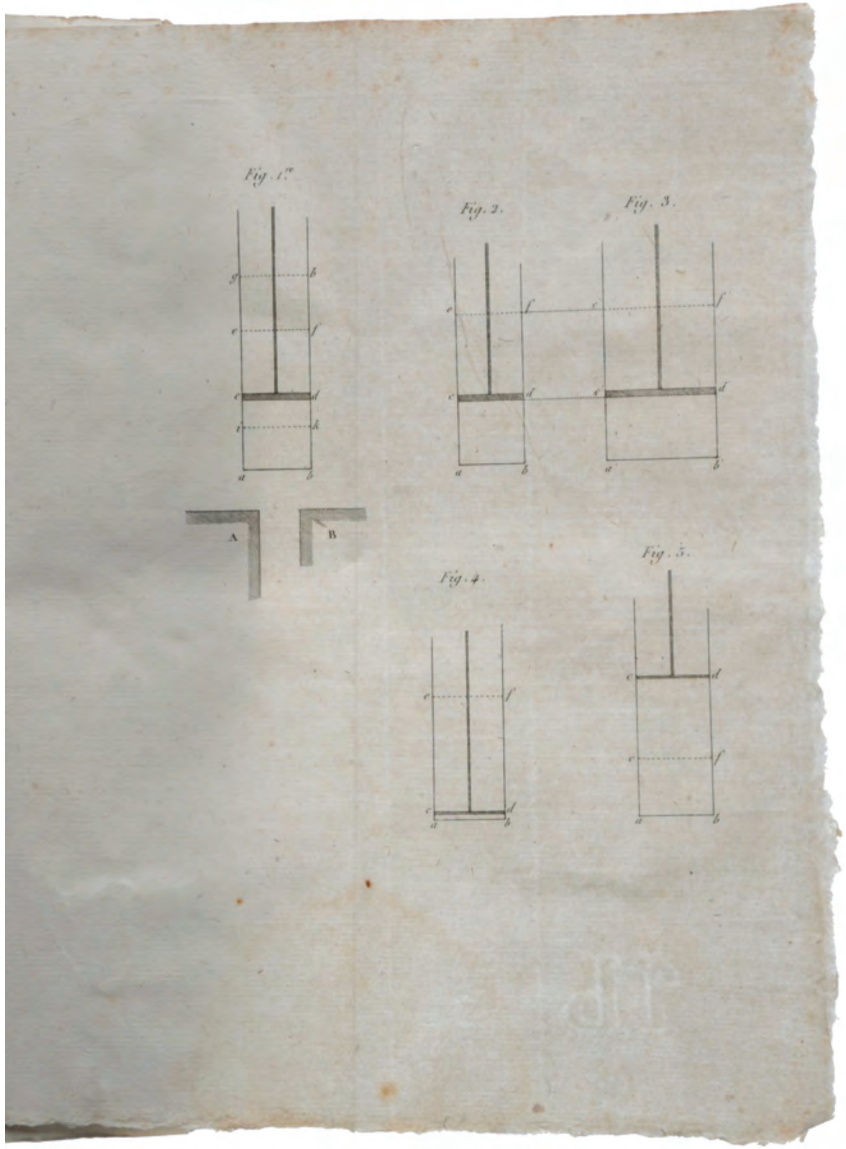
"In the final section of the *Réflexions*, Carnot returned to his original questions on steam engines. With experimental data taken from the current literature he verified that all gases produce the same amount of motive power and was able to estimate the ideal limit for its production. In a review of the most common types of steam engines, Carnot sought to apply his findings to the practical questions of steam-engine design and operation. His contributions, however, fell short of his original goal. His conclusions—that steam ought to be used expansively (adiabatically), over a large temperature interval, and without conduction losses—were already widely recognized by engineers of his time. Because of difficulties in engine construction even the problem of the best working substance was not conclusively answered.

"Although the *Réflexions* was regarded by contemporaries as primarily an essay on steam engines, Carnot's most important innovations lay in a new approach to the study of heat. While he accepted, and in some theorems furthered, the theory of heat developed by Laplace and Poisson, Carnot also shifted the emphasis from the microscopic to the macroscopic. Rather than build upon the notion of gas particles surrounded by atmospheres of caloric, he began with the directly measurable entities of volume, pressure, temperature, and work …

"Although the exact reasons are impossible to determine, the *Réflexions* had almost no influence on contemporary science. The original edition had not sold out by 1835; by 1845 booksellers had forgotten it completely. Aside from the reviews in 1824 and the references in obituaries, Carnot's work was mentioned only twice between 1824 and 1834. Clément recommended the book in his 1824–1825 lectures and Poncelet, writing sometime before 1830, cited it in his *Introduction à la mécanique industrielle* (Paris, 1839). In Carnot's obituary Robelin attributed the neglect of the *Réflexions* to its difficulty, an explanation that would have applied only to engineers and craftsmen unfamiliar with contemporary physics and mathematics. Another explanation points to the failure of the *Réflexions* to reach conclusions of real value to steam engineers. The silence on the part of physicists like Dulong, who later retraced portions of Carnot's work, is more difficult to explain. One probable factor, however, was Carnot's use of the caloric theory and of experimental results such as Clément's law of saturated vapors. His work was thus especially vulnerable, as he realized himself, when Clément's law was disproved in 1827 and when problems of radiant heat initiated a period of ‘agnosticism’ concerning the nature of heat.

“In 1834 Clapeyron, with whom Carnot may have been acquainted in 1832, published an analytical reformulation of the *Réflexions*. While Clapeyron preserved the premises, the theorems, and some of the specific arguments, the emphasis and style were considerably altered. He related the Carnot cycle to the pressure-volume indicator diagram and, emphasizing Carnot’s function, translated Carnot’s synthetic work from the world of heat engines to the realm of the mathematical theory of gases. Carnot’s work attracted no further attention until C. H. A. Holtzman in 1845 and William Thomson in 1848 began working on special aspects of Clapeyron’s paper. Between 1848 and 1850 Thomson, working directly from the *Réflexions*, published a series of papers that both extended and confirmed Carnot’s results. These papers constituted a strong defence for Carnot’s work, including his use of the caloric theory, at a time when Joule, Julius Mayer, and Helmholtz were establishing the convertibility of heat and work and the principle of energy conservation. In 1850 Clausius showed that Carnot’s theorem was correct as stated but that Carnot’s proof, which assumed no heat was lost, needed modification. Clausius added the statements that in the Carnot engine a certain quantity of heat is destroyed, another quantity is transferred to the colder body, and both quantities stand in a definite relation to the work done. With these additions, which Thomson also adopted in 1851, Carnot’s theorem became the second law of thermodynamics” (DSB).

Sadi Carnot was the eldest son of Lazare Carnot (1753-1823), who, at the time of Sadi’s birth in 1796, was a member of the French Revolutionary government (1795-99). Sadi was named after the medieval Persian poet and philosopher Sa’di of Shiraz. Under his father’s tuition, Sadi Carnot showed great promise and entered the École Polytechnique at the youngest possible age of 16. After graduating in 1814, Carnot went to the École du Génie at Metz to take the two-



year course in military engineering. In 1815 Lazare was permanently exiled to Germany, and in 1819 Sadi left military service to live in Paris in his father's former apartment. He attended courses at various institutions in Paris, and became interested in industrial problems and, in particular, began to study the theory of gases. Sadi visited his father in 1821 in his exiled home in Magdeburg, where he discussed steam engines with Lazare and his brother Hippolyte, who was then living with his father. After returning to Paris, Carnot began the work which led to the mathematical theory of heat and founded the modern theory of thermodynamics. After the publication of *Réflexions* in 1824, Sadi continued with his research, and although nothing of this was published, notes that Carnot made as his ideas developed have survived. In 1827, Sadi was recalled to full time military duties, but after less than a year he retired permanently and returned to live in Paris where he aimed to continue with his research into the theory of heat. In June 1832 he became ill and had not fully regained his strength when the cholera epidemic of 1832 struck Paris. Although only 36 years of age, he died within a day of contracting the disease.

Bibliotheca Mechanica, p. 63; DSB III, pp. 79-83 ("the first edition of *Réflexions* … is very rare"); Dibner 155; Norman 404; PMM 285 ("His work led directly to the enunciation of the theory of conservation of energy by Helmholtz in 1847 … The second law of thermodynamics is also implicit in Carnot's treatise"). Fox, 'Introduction,' in Carnot, *Reflexions on the motive power of fire*, ed. Fox, pp. 1-57.

BEAUTIFUL GLOBE BOOK, WITH FOUR FOLDOUT CELESTIAL MAPS

CORONELLI, Vincenzo Maria. *Epitome cosmographica, o Compendiosa introduttione all'astronomia, geografia, & idrografia, per l'uso, dilucidatione, e fabbrica delle sfere, globi, planisferi, astrolabi, et tavole geografiche, e particolarmente degli stampati, e spegiati nelle publiche lettioni.* Colonia [Venice]: Andrea Poletti, 1693.

$22,500

8vo (192 x 140 mm), pp. [xxx] (including engraved and printed titles), 420, [16], with 37 double-page plates, one (p. 361) with 6 volvelles (two circular plates and four pointers). Contemporary paper-covered boards, manuscript title in brown ink to spine (boards slightly rubbed and dust-soiled).

First edition, rare when complete, of this sumptuously illustrated work, a uniquely valuable source for the documentation of several of the most elaborate large-scale globes and astronomical mechanisms, some now lost, constructed during the latter decades of the seventeenth century. Coronelli, a Franciscan monk, was the official cosmographer of the Venetian Republic, the greatest maker of terrestrial globes and maps during the last half of the seventeenth century, and the founder of the *Accademia Cosmografica degli Argonauti*, the first geographical society. The *Epitome* is particularly important for the information Coronelli includes on the highly decorative and massive globes he constructed for Louis XIV, one of which is illustrated in this work (they can be seen today in the Bibliothèque Nationale in Paris). The work contains four large foldout celestial maps in circular format engraved in a spectacular baroque style; they were based on the most recent

astronomical observations and were copied into the eighteenth century. Also included are two large terrestrial maps – the western and eastern hemispheres. Of the 37 double-page plates, many illustrate globes, spheres, astronomical diagrams and instruments. As a leading cosmographer, Coronelli's career bears on the history of astronomy at many points. "In the *Epitome* he listed everything that seemed to him important in astronomy and geography, describing, without making any value judgements, the systems of Ptolemy (18 lines), Tycho Brahe (17 lines), Copernicus (132 lines), and Descartes (40 lines). Not only does he give, for many constellations, an account of their history or the origins of their names, and for all the positions of the stars, … every star is accorded a number … The appearances of comets since ancient times are all listed. The chapter 'Geography' contains summary descriptions of the continents, but also of the earthquakes and volcanic eruptions recorded since ancient times. The explorers of the more important regions are listed, together with lists of writings on astronomy and geography by authors back to the ancient Greeks" (Schmidt & Bridge, p. 100). Riccioli was his main source of technical information, for the earth as well as the heavens; other celestial information, including the only telescopic stars he included, was taken from Bayer and Hevelius. Coronelli was a long-standing friend of Edmond Halley and Robert Hooke, observing a lunar eclipse with them in London in 1696. ABPC/RBH list only two complete copies in last 40 years: Christie's, 21 March 2012, £6875; Sotheby's, 7 December 1989, £2640. Although reasonably well represented in institutional collections, it is unclear how many of those copies are complete (the copy in Cambridge University Library, for example, lacks one plate; and of the two British Library copies, one lacks two of the volvelles at p. 361 and the other lacks the engraved title).

The *Epitome* is divided into three books. The first, comprising 35 chapters (pp. 1-208), begins with a discussion of spherical geometry, latitude and longitude, great circles, the tropics, winds, and climate. This is followed by a discussion of the stars and planets, their distances and number, the constellations, comets, and solar and lunar eclipses. The second book, comprising 17 chapters (pp. 209-324), is devoted to geography: the land and sea, the various regions of the earth (Europe, Asia, Africa, America, the poles), with tables of latitude and longitude of the major cities. There follow several chapters devoted to earthquakes and volcanic eruptions. The third book, divided into two parts, comprising 39 chapters, is devoted to the description of various globes, celestial and terrestrial, and how to use them, as well as information on the construction and use of popular instruments such as armillary spheres, planispheres, and astrolabes. Part one (pp. 325-342) describes and illustrates some of the most spectacular globes to be found across Europe (England, France, Germany). Chapter 1 describes the 'English Globe' produced in 1679 by the Earl of Castlemaine (1634-1705), in collaboration with Joseph Moxon (1627-91). It was an immobile globe whose sphere does not rotate but is fixed in place over a planisphere; this allowed complex calculations to be performed more easily than with a turning sphere. Chapter 2 is devoted to the great globe of Gottorp, Germany, constructed under the supervision of Adam Olearius (1599-1671), which had a map of the earth's surface on the outside and a map of star constellations with astrological and mythological symbols on the inside. Turned by water-power, it demonstrated the 'movement' of the heavens to those seated inside in candlelight – it was a predecessor of the modern planetarium. In chapter 3, Coronelli describes the 'Globus Pancosmus' of Erhardt Weigel (1625-99), which had a circumference of 9.5 metres with interior effects such as a breeze which could be made to blow from any desired quarter, various elements such as rain, hail and thunder and images of people of different nationalities. Chapter 4 is devoted to Christopher Treffler's 'Sphaericum Automatum', a self-moving celestial globe, now lost – it was for sale for the price of 8000 Talari in 1688 at the time Coronelli was passing through Augsburg, and his description of it appears to be the only surviving evidence that it existed. The fifth and final chapter in this part is devoted to the globes Coronelli constructed for Louis XIV, described below.

In part two, in 34 chapters (pp. 343-406), Coronelli describes the construction of globes in general: how the globe gores are designed and printed to represent the earth on a flat plane, how the heavens can also be represented in the plane by means of planispheres, how the globes themselves are assembled by gluing the gores onto a large ball made of wood and papier-mâché and finished with plaster, and how the information on the globes is to be presented. Coronelli was celebrated for his skill in arranging large amounts of information on his globes in a comprehensible yet stylistically appealing manner.

Born in Venice, Coronelli (1650-1718) was apprenticed to a woodcut printer in Ravenna at ten years old. On his return to Venice in 1665, he entered as a student in the convent of the minors of S. Nicolò della Lattuga. He demonstrated his precocious ingenuity as early as 1666, when he published an almanac in Venice, the *Calendario perpetuo sacro profane,*the first of almost 140 works he was to produce throughout his life.He was sent by his superiors to study in Rome, in the college of S. Bonaventura; after just three years, in 1674, he earned his doctorate in theology, excelling also in the study of astronomy and Euclid. A little before 1678 he began working as a geographer and was commissioned to make a set of globes for the Duke of Parma, each of which was 1.8 metres in diameter. They so impressed Cardinal d'Estrées, ambassador to Rome and advisor to Louis XIV, that he invited Coronelli to Paris in 1681 to construct a pair of terrestrial and celestial globes for the King. They each had a diameter of fifteen feet, and were built with trapdoors so they could be worked on from the inside.

The present volume is particularly noteworthy for the information Coronelli includes on the king's globes. Weighing two tons, these extremely ornate works remained until the 1920s the largest globes in the world. They stood on a marble base of five steps and were held by a system of four meridian-rings and a massive horizon-ring supported by eight pillars, all in bronze. Each had a wooden frame

covered with glued layers of plaster and fabric and an outer surface of very fine linen on which the information on the globe was painted and lacquered. Coronelli had made a systematic study of the history of exploration and plotted on his globes the discoveries of named navigators from the ninth century onwards. He used Portuguese sources for recent mapping of Zambesia; German for the Blue Nile; Dutch charting of Australia; and Narbrough's 1670 voyage for the Strait of Magellan. The king's terrestrial globe shows the track of the French Jesuit voyage to Siam, undertaken to establish the longitude of the capital of Siam by observing the satellites of Jupiter, using Cassini's tables. Coronelli plotted the outbound and return voyage track on his globe, annotated with measurements of magnetic declination. (This preceded by more than ten years Halley's first Atlantic voyage to measure magnetic declination and calculate the longitude of Barbados using Cassini's tables). The globe is packed with information not only about geography but also about natural history, anthropology and ethnography. The astronomer Philippe de La Hire wrote a manual that carefully avoided linking the celestial globe to any of the competing cosmologies of the day. These globes established Coronelli's fame, and also his fortune as smaller versions of the globes became an essential feature of the great houses and libraries of Europe. Coronelli was made royal cartographer to Louis XIV in 1681 as a result of making these great globes, and worked in Paris for two years. He collaborated with Jean Baptiste Nolin (1657-1708), who went on to become the French publisher for all of Coronelli's works. While in Paris, his contact with the Académie des Sciences gave him access to information about the latest French explorations, including those of La Salle in North America, and these contacts continued.

After his stay in Paris, Coronelli lived and worked in various European countries. He returned to Venice in 1683, and in the following year founded the *Accademia Cosmografica degli Argonauti* (named after Jason and the Argonauts, the adventurers who set out to find the golden fleece). Among the preliminary leaves of the *Epitome* is a list of members of this society, which reveals that it counted princes, ambassadors and cardinals amongst its members. They were to receive a minimum of six copper engravings a month, creating a guaranteed market for Coronelli's productions. By 1693 the *Accademia* had 260 members spread throughout Europe.

Having returned home to Venice, Coronelli turned his rooms in the *Gran Casa dei Frari* into one of the world's leading globe-making workshops, besides accommodating a substantial production of maps, prints, and books in what he called his 'Laboratorio dei Frari.' From 1687 until 1707, he directed a large workshop of mapmakers producing high-quality publications crucial to the evolution of cartography. Coronelli was named cosmographer of the Serene Republic of Venice, and was in charge of depicting the victorious battles fought by his compatriots during the Venetian-Turkish War of 1684-1687. Coronelli published an ambitious multi-volume atlas, the*Atlante Veneto*, in 1691, which was intended to be an extension of Blaeu's atlas in three parts, covering hydrography and ancient and modern geography. Coronelli was known as a careful scholar, and his work across Europe gave him access to the latest information. For example, he produced the first widely published European map of settlements in New Mexico, 'America Settentrionale' (1688), after being given the information by a former governor of New Mexico, Diego de Peñalosa. By the time of his death, Coronelli had published hundreds of maps, as well as the first six volumes of the *Biblioteca Universale Sacro-Profana*, considered to be the first encyclopedia with entries in alphabetical order.

In 1696 Coronelli travelled with the Venetian ambassadors to Germany, the Netherlands and England. In London he was able to present on 11 May a pair of 1½-foot globes (celestial and terrestrial) to William III, for which he was rewarded with 200 gold guineas. On 16 May, Coronelli observed a total eclipse of the moon, as well as Jupiter and its satellites, with members of the Royal Society and especially his 'long-standing friends' Robert Hooke and Edmond Halley (*Viaggi del P. Coronelli* (1697), vol. 2, p. 154). In the same month he visited the universities of Oxford and Cambridge, and was elected a fellow of the Royal Society. Coronelli brought with him to England a quantity of sheets of globe gores to sell. He was in the process of compiling a large volume, the *Libro dei Globi* (published in 1697), containing the gores of all five pairs (terrestrial and celestial) of his printed globes, ranging from 5 cm to 108 cm in diameter. He marketed this as an atlas, from which customers could have globes made if they wished.

In spite of the antagonism and opposition of most of the members of his own order, in 1701 Coronelli, a favourite of Pope Innocent XII (1691-1700), was elected Superior General of his order. In 1704, however, the new pope, Clement XI (1700-1721), removed him from office because of his unsatisfactory leadership qualities with his confrères, and following accusations of misuse of the order's funds to finance his publications. After such a scandal, Coronelli was reduced to spending his twilight years rather disagreeably in his 'Convento dei Frari' in Venice, disliked by most of his brothers and shunned by many of his former patrons and protectors. There, he mainly sold guides to Venice and reprints of his maps and views. When he died, in 1718, all the copperplates of his works were sold to pay for roofing and drain pipes, and to cover the many debts of his publishing firm.

Coronelli's was a career full of contradictions. His selling success was due to a steady advertising effort, by courting not only princes but also their librarians, and by promising to the public high-standard publications at low, convenient prices. An example of his commercial originality is his novel subscription scheme for financing his first pair of printed globes, coupled with the *Atlante Veneto*, in which subscribers to both became members of the *Accademia Cosmographica*

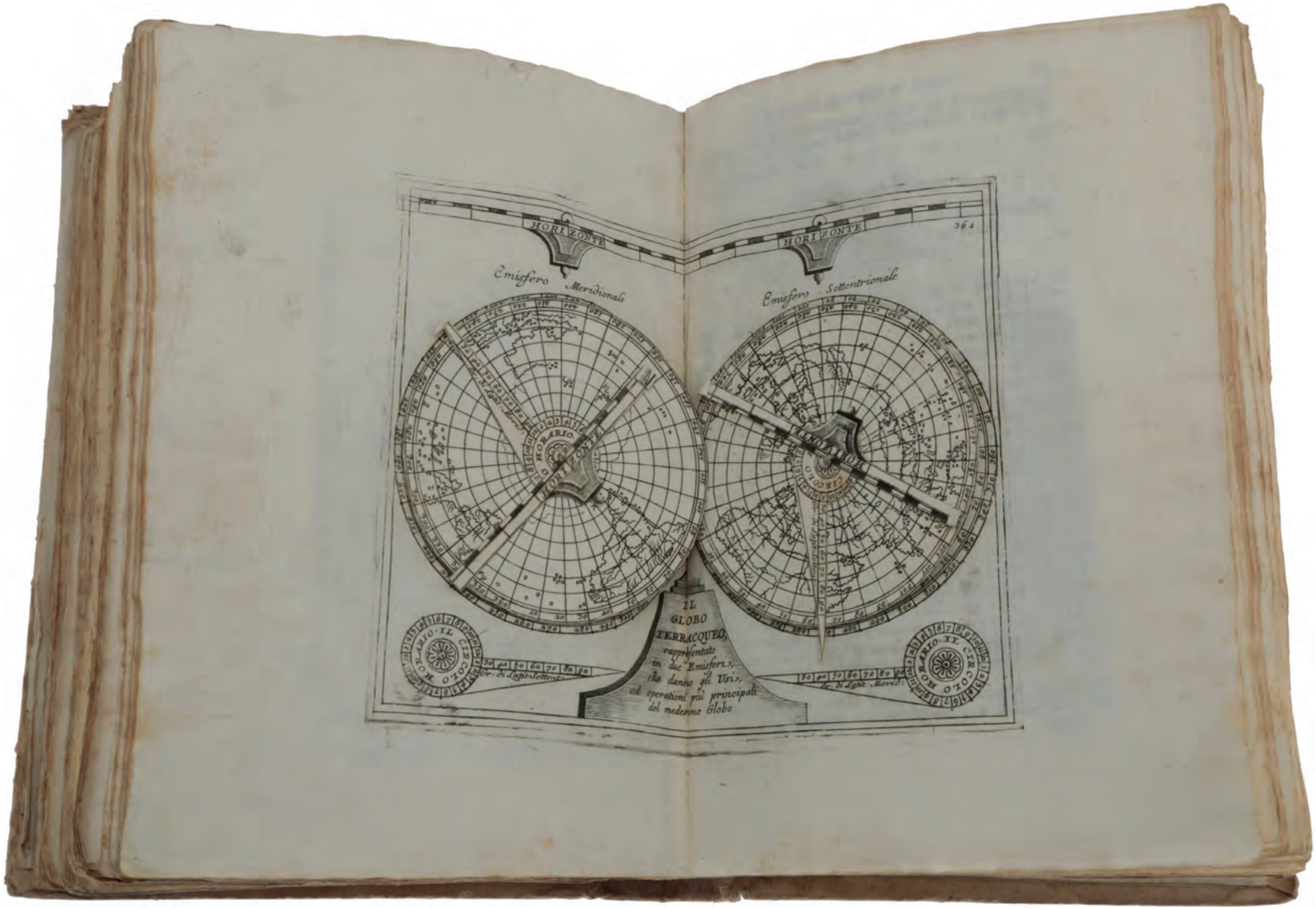
degli Argonauti. But the ground on which his success was built was not solid. Coronelli never sold enough globes, maps, prints and books to cover his editing, engraving, printing and globe-mounting expenses. He therefore asked for money from the Republic of Venice and later from his Order, which he received for twenty years.

Coronelli had skilfully obtained the favour of the Venetian ruling class by adopting the right degree of obsequiousness. But his success was more than the good fortune of an able courtier and skilful craftsman. By returning from Paris to Venice with the astronomical and geographical materials used for the king's globes and atlases, the friar had enriched the Republic with assets whose value in extolling the image of the city was evident. Thanks to his work, for a brief period, Venice once again became the capital of geographical information – something the Republic had ceased to be more than a century before, just as – at least in the eyes of the Venetians themselves – Venice was again one of the leading forces in European politics, fighting the Turks at the Pope's request, at the Emperor's side, in a war in which France had refused to take part.

When in 1705 the question was raised whether the Republic had really gained the expected advantages from the stipends bestowed on Coronelli, many people judged that the fruits produced by those payments – his maps and globes and his teaching – were rich and worthy of praise. During Coronelli's lifetime, geography was highly fashionable even at the courts, where it was admired not only for its various traditional and practical uses, especially in warfare, but also as a science for men of quality. Without any knowledge of it they would be incapable of waging war or, even more importantly, talking about it in a salon. On the other hand, a Republic whose treasury was being drained by war was in the end forced to consider this matter purely in terms of costs and benefits. When he lost external funds Coronelli could not survive as a publisher.

Most copies of the *Epitome* lack several of the plates, and sometimes the engraved or printed titles.

Riccardi I: 374-5; Houzeau & Lancaster 8006; Macclesfield 563 (lacking first quire including title, two plates & final leaf); Nordenskiold Collection 57 (28 plates only); Wardington 117 (lacking printed title); Armao, *Vincenzo Coronelli* (1944), p. 189 (collation as this copy); Warner, *The Sky Explored*, pp. 56-7. Milanesi, *Vincenzo Coronelli, Cosmographer* (*1650-1718*), 2016 (the best account of Coronelli's life and work, from which much of our description is taken). Schmidt & Bridge, 'Vincenzo Coronelli's methods of work. A supplement to the article in 'Der Globusfreund' 43/44,' *Globe Studies* No. 59/60, Papers Read at the 12th International Symposium for the Study of Globes, Jena 2011 (2014), pp. 96-111.


HORIZONTE
HORIZONTE
361
Emisfero Meridionale
Emisfero Settentrionale
IL GLOBO TERRACQUEO rappresentato in due Emisferi, che danno gli Usi, ed operationi più principali del medesmo Globo

PMM 318 - THE BEGINNINGS OF PHOTOGRAPHY

DAGUERRE, Louis-Jacques Mande. *Historique et description des procédés du daguerréotype et du diorama.* Paris: Bethune and Plon for Susse freres and Delloye: 1839.

$95,000

8vo (212 x 138 mm), pp. [iv] (half-title, notice of publisher, title page, table of contents), 79, [1], [4] (advertisements), with six lithographed plates. Original printed yellow wrappers (minor loss to ends of spine, upper right corner of front wrapper creased and slightly chipped). An excellent, unrestored copy.

First edition, second but first obtainable issue (see below), of Daguerre's exposition of his photographic process, 'the beginnings of photography" (Horblit); this is a fine copy in original printed wrappers and very rare in this condition. "Perhaps no other invention ever captured the imagination of the public to such a degree and conquered the world with such lightning rapidity as the daguerreotype" (Gernsheim, *The History of Photography*, p. 71). "The daguerreotype is a photographic image with a mirror-like surface on a silver or silver-coated copper plate. A unique photograph, the daguerreotype is not produced from a negative, and the final image appears either positive or negative depending on the angle of reflected light" (Hannavy, p. 365). Daguerre (1787-1851), a gifted set designer and creator of the famous Diorama, a picture show based on lighting effects, began experimenting in the 1820s with fixing the images of the camera obscura on silver chloride paper. His lack of success stimulated his interest in

the heliographic method invented by Nicéphore Niépce (1765-1833), who had produced the first successful photographic image in 1826 or 1827 on a pewter plate coated with bitumen of Judea dissolved in oil of lavender, and in 1829 Daguerre succeeded in persuading the reluctant Niépce to become his partner. After Niépce's death in the spring of 1835, Daguerre serendipitously discovered a quicker method of exposing and developing the Niépcian image through the application of mercury vapour. Using this method, with common table salt as the fixative, he produced his first successful permanent photographic image in 1837. On August 19, 1839 the scientist-politician François Arago (1786-1853) made a full announcement of the new process to a packed house at a joint meeting of the Académies des Sciences and des Beaux-Arts at the Institut de France. Daguerre's manual, published by order of the government, quickly sold out. A total of 39 reprints, new editions, and translations appeared in the following 18 months. The great demand accounts for the profusion of issues of the first edition: seven are recorded, all from the same basic setting of type. Of these the first four differ in the booksellers' names alone. The present copy is of the first Susse issue, the second to appear, preceded only by the Alphonse Giroux issue, published shortly after Arago's 19 August announcement, of which only three copies are known.

"Louis-Jacques Mandé Daguerre was born on 18 November 1787 at Cormeilles-en-Parisis. His childhood was spent at Orleans, where his father was employed as a clerk on the royal estate. Showing talent for drawing, the boy was apprenticed to an architect at the age of thirteen, and three years later became a pupil of Degotti, scene painter at the Paris Opera. Later he made himself independent and designed the decor for the productions of several Paris theatres. He also collaborated with Prevost on a number of large panoramas – a kind of show which enjoyed immense popularity in the last decade of the eighteenth and first half of the nineteenth centuries … In 1822 Daguerre associated himself with the painter Charles Bouton (an assistant of Prevost) in a new venture, the Diorama, a picture show with changing light effects which aroused astonishment and admiration by its perfect illusion of reality" (Gernsheim, p. 65).

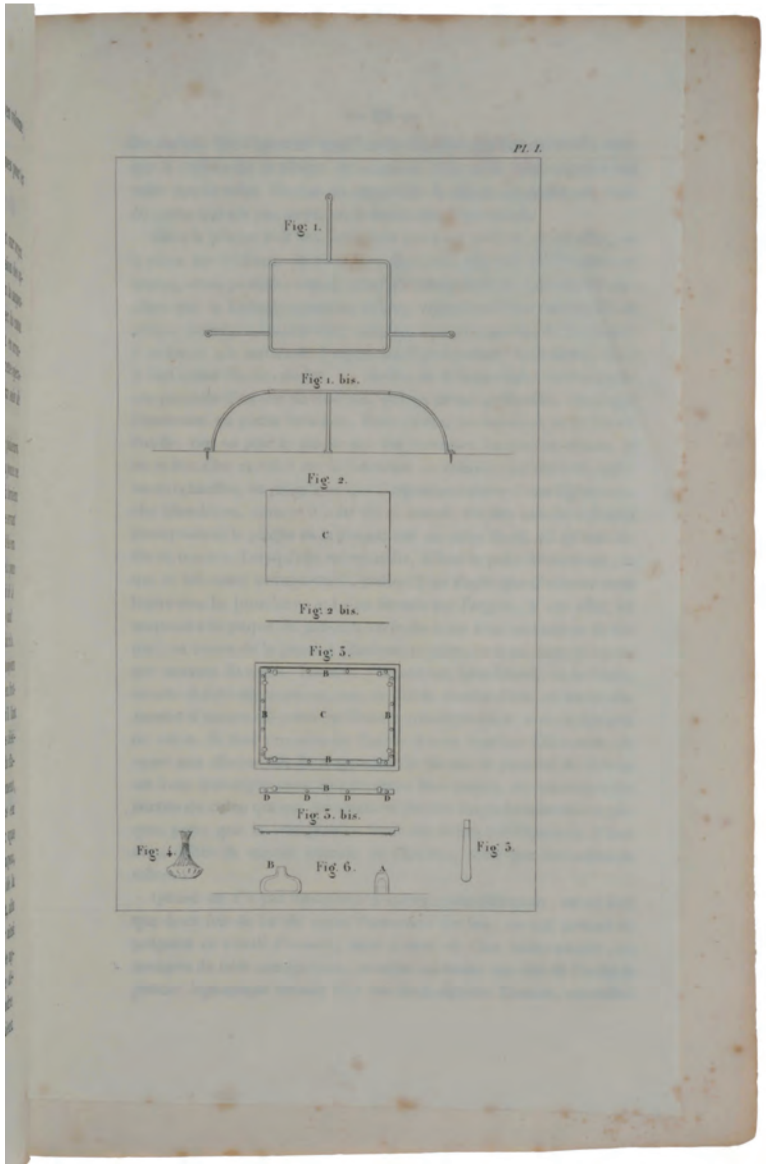
The realism of the paintings in the Diorama, which so impressed Daguerre's audiences, was achieved by tracing images projected by a camera obscura. "Was it not natural that he, like Fox Talbot some years later, should have wished to find a method by which the fugitive image, which he was so laboriously tracing, could be made to delineate itself? Obsessed by this idea, Daguerre equipped a laboratory at the Diorama near the Place de la République in Paris, and there for several years he carried out mysterious experiments, shutting himself in his workroom for days on end. The famous chemist, J. B. Dumas, relates that Madame Daguerre consulted him one day in 1827 as to whether or not he thought it possible that her husband would be able to fix the images of the camera. 'He is always at the thought; he cannot sleep at night for it. I am afraid he is out of his mind; do you, as a man of science, think it can ever be done, or is he mad?' 'In the present state of knowledge', replied Dumas, 'it cannot be done; but I cannot say it will always remain impossible, nor set the man down as mad who seeks to do it'" (*ibid*., p. 66).

Daguerre's fortunes changed only when he came into contact with Nicéphore Niépce. "Niépce's first experiments with light-sensitive materials placed in a homemade camera obscura were conducted in 1816. He succeeded in taking impressions of views out of his workroom window using paper covered with muriate (or chloride) of silver, but the images were not permanent. Moreover, they were negative images, and attempts to print them in the positive were not successful … It was at this point that he began to experiment with bitumen of Judea as a light-sensitive coating. The bitumen, he had discovered, hardened when exposed to the sun's rays, whereas parts that had not been exposed could be dissolved and washed away by oil of lavender. The result was a fine image formed where light had fallen … He began to refer to his efforts to take directly the image of nature as 'heliographic,' i.e., drawn by the sun" (Hannavy, p. 1004).

"Daguerre first contacted Niépce in January 1826, after hearing about his heliographic experimentation from the optician Vincent Chevalier. Niépce eventually visited Daguerre at the Diorama in August 1827, and the two men formed a company on 14 December 1829 in order to exploit both Niépce's invention, based on the photosensitivity of bitumen of Judea, and Daguerre's improvements to the camera obscura. After Niépce's death (5 July 1833), Daguerre signed a new contract in 1835 with Niépce's son, Isidore" (*ibid*., p. 365).

"Building upon the materials used by his partner [Niépce] – silvered copper plates, and iodine with which to strengthen the image – Daguerre discovered in 1831 the light-sensitivity of iodide of silver, which he produced by subjecting a silvered copper plate to iodine vapour, as Niépce had done for a different purpose. This silver salt, however, was not sensitive enough to produce an image, and it was not until the spring of 1835 that Daguerre was in a position to ask Isidore Niépce, who according to the contract had succeeded his father, to come to Paris to look at important results.

"Daguerre had just discovered the possibility of developing the latent image. How it happened is one of the classic legends of photography. Daguerre put away in his chemical cupboard a plate which had been exposed – apparently as unsuccessfully as usual – intending to re-polish and use it again. When, a few days later, he opened the cupboard he found, to his amazement, the under-exposed plate impressed with a distinct picture. He quickly made a number of exposures as before, put the plates in the cupboard one at a time, and by a lengthy process of elimination of the various chemicals it contained he at length established that the vapour from a few drops of spilt mercury from a broken thermometer had worked the miracle. Daguerre himself stated, however, that he had been experimenting with several



mercurial compounds, from which 'it was only a short step to the vapours of metallic mercury, and good fortune led me to take it.' This proved the solution to the whole problem, for by this means Daguerre established that a plate need receive only a comparatively short exposure of 20 minutes to half an hour and that the latent image could then be made visible by an after-process.

"Though the images were still not permanently fixed, Daguerre felt that his discovery was such an immense improvement upon Niépce's results that the firm should from now on be called Daguerre and Isidore Niépce instead of Niépce-Daguerre. Isidore, in need of money, unwillingly agreed to this, in a signed codicil to the original contract, on 9 May 1835.

"Having been brought an important stage nearer his goal, with his usual self-assurance Daguerre triumphantly but prematurely claimed that he had succeeded in fixing the image of the camera obscura, including portraits. In fact it was not until the first half of 1837 that he was really able to fix his pictures permanently, using a solution of common salt in hot water, whilst the application of the process to portraiture was left to later experimenters …

"Having at last succeeded in fixing the image of the camera obscura by quite different methods from Niépce's, Daguerre felt that his own discoveries had improved the original heliographic process out of all recognition, and in spite of the terms of the original contract, it is perhaps understandable that he should now insist on calling the invention by his name alone – daguerreotype. Isidore Niépce at first indignantly refused to sign the new contract which Daguerre had prepared, but when his partner threatened to publish his process and heliography separately, he gave way (though he protested later), for it was obvious that no one would be interested commercially in the slower process. Moreover, it was clear that Daguerre was concerned only with the honour of being considered the inventor, for he still agreed that the profits from the new process should be divided equally.

"Having settled this delicate matter to his satisfaction, Daguerre suggested to Niépce a public subscription to run from 15 March to 15 August 1838, calling for four hundred subscribers at 1,000 francs each, and stipulating that the processes of heliography and daguerreotype should not be made public unless there were at least one hundred subscribers. If sold outright, the price for the inventions should be not less than 200,000 francs (at that time about £8,000).

"During the next few months Daguerre attracted all the publicity he could by driving round Paris with the apparatus on a truck, photographing monuments and public buildings; but he failed to find buyers for the shares, or a Maecenas to put down the lump sum required. So, towards the end of 1838, he approached a number of leading scientists, including J. B. Dumas, Biot, Humboldt, and Arago, with the purpose of interesting the Government. He was fortunate in finding in François Dominique Arago an influential ally, for he was a member of the Chamber of Deputies as well as a distinguished physicist and astronomer. Soon afterwards, Arago gave the discovery official status by a brief announcement at the *Académie des Sciences*, on 7 January 1839" (Gernsheim, pp. 66-68). By this time a final contract had been signed, naming Daguerre as the sole inventor of the new process.

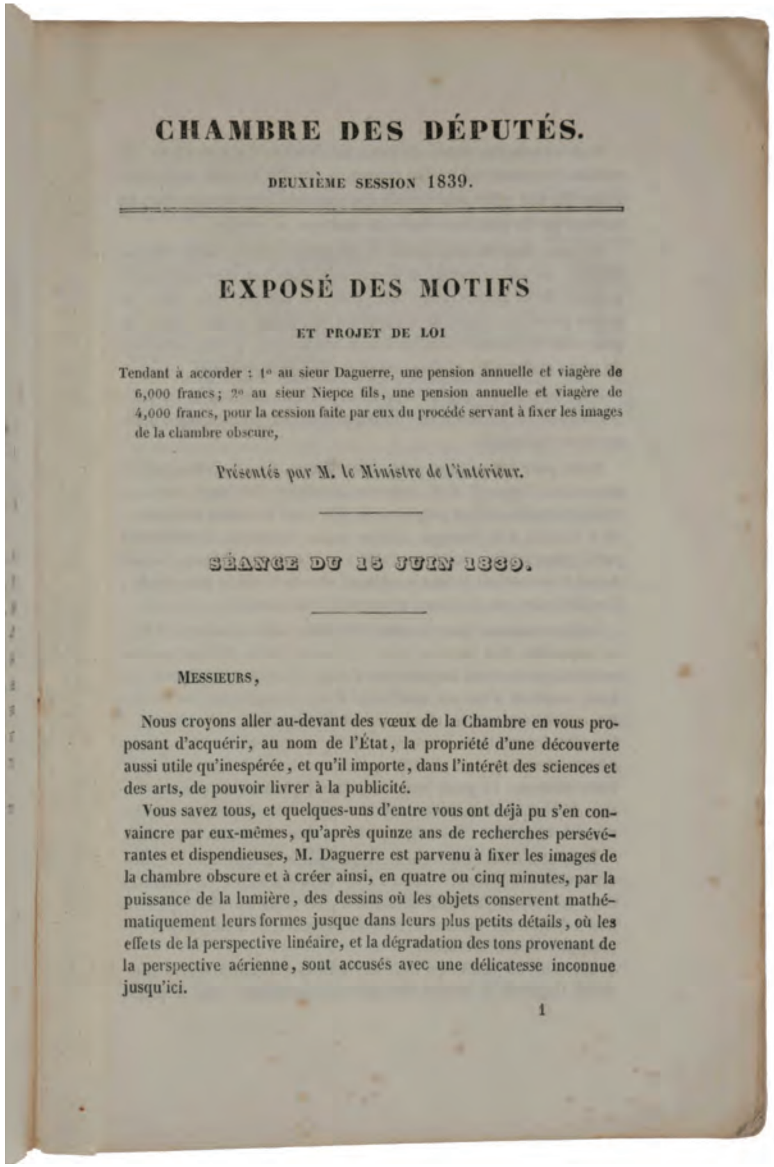
"Arago formally divulged the process to a joint meeting of the Académie des Sciences and Académie des beaux-arts on 19 August 1839, after King Louis-Philippe signed the law granting lifetime pensions to Daguerre and Isidore Niépce on 7 August 1839" (Hannavy, p. 365). Arago's report was published at Paris by Bachelier on 31 August 1839 as *Rapport sur la daguerréotype*, very shortly after the first issue of Daguerre's manual appeared. Arago regrets "that the inventor of this ingenious apparatus could not himself be responsible for presenting all of its properties before the Academy.' He then gives a technical and chemical description of the new process.

"According to the terms of the law, Daguerre was required to publish details of the daguerreotype process and techniques for painting diorama pictures. In addition to Arago's public explanation of the technical production of daguerreotypes, Daguerre produced an illustrated manual outlining the various steps of the process. Daguerre added his correspondence with Niépce, in which he suggests experimenting with the photosensitivity of silver and iodine, in order to demonstrate that the daguerreotype was indeed his own invention. His cited letters – which document the fact that Daguerre's systematic experiments with silver nitrate, and eventually mercury, led him to the discovery of his own photographic process – only revealed part of the picture. In fact, Niépce had already used iodine, but only as a kind of 'developing agent' to darken the shaded parts of his proofs. Daguerre's claims in the manual angered Niépce's son, Isidore, who responded with his own pamphlet [*Histoire de la découverte improprement nommée daguerréotype*, 1841], in which he asserted that his father invented the daguerreotype" (*ibid.*).

"Daguerre's epoch-making invention was as yet far from perfect, and it was left to other scientists, American, English, French, and Austrian, to speed up the process and make it applicable to portraiture – the great desideratum. The daguerreotype suffered from inherent disadvantages and for this reason the process must be regarded as a cul-de-sac:

(a) The mirror-like surface of the silvered copper plate makes the picture difficult to see.

(b) The picture, being a direct positive, was laterally reversed.



CHAMBRE DES DÉPUTÉS.

DEUXIÈME SESSION 1839.

EXPOSÉ DES MOTIFS

ET PROJET DE LOI

Tendant à accorder : 1° au sieur Daguerre, une pension annuelle et viagère de 6,000 francs; 2° au sieur Niepce fils, une pension annuelle et viagère de 4,000 francs, pour la cession faite par eux du procédé servant à fixer les images de la chambre obscure,

Présentés par M. le Ministre de l'intérieur.

SÉANCE DU 15 JUIN 1839.

MESSIEURS,

Nous croyons aller au-devant des vœux de la Chambre en vous proposant d'acquérir, au nom de l'État, la propriété d'une découverte aussi utile qu'inespérée, et qu'il importe, dans l'intérêt des sciences et des arts, de pouvoir livrer à la publicité.

Vous savez tous, et quelques-uns d'entre vous ont déjà pu s'en convaincre par eux-mêmes, qu'après quinze ans de recherches persévérantes et dispendieuses, M. Daguerre est parvenu à fixer les images de la chambre obscure et à créer ainsi, en quatre ou cinq minutes, par la puissance de la lumière, des dessins où les objets conservent mathématiquement leurs formes jusque dans leurs plus petits détails, où les effets de la perspective linéaire, et la dégradation des tons provenant de la perspective aérienne, sont accusés avec une délicatesse inconnue jusqu'ici.

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(c) Being on a plate of solid metal, it could not be used as a negative to print copies; each picture was unique. To overcome this, numerous processes were evolved to convert daguerreotypes into printing plates, but the procedures were too complicated for general use" (Gernsheim, p. 73).

Although Daguerre's manual was preceded in publication by William Henry Fox Talbot's account in February 1839 of his radically different process of 'Photogenic Drawing', with the appearance of the daguerreotype process it became possible to obtain mirror–like images of the world of considerable evocative impact, not approached by the rather primitive techniques of photography on paper which were still effectively limited to photogram production (widely known, though lacking a solution to the problem of preservation, long before January 1839) without the use of a developable latent image. The first daguerreotypes rightly had very considerable impact. Although the product itself was not capable of further evolution it did imaginatively open the eyes of the first practitioners to a quality of picture that could be obtained with a camera and provided them from the very first a method of working in which an image although still invisible after exposure ('invisible et seulement latente') could be revealed by later chemical treatment.

According to Beaumont Newhall, the first issue was released on or about 20 August 1839 and bears the imprint of Giroux et Cie, and Delloye. The printing was performed by Béthune et Plon. Of this, only three copies are known to have survived. On 14 September, copies of this first printing by Béthune et Plon, were released for sale with the imprint of Susse Freres, Editeurs, and Delloye. It is identical to the first issue with the exception of the imprint, and the three pages of advertisements which follow the text (for this reason, this second issue is sometimes referred to as the first issue, second imprint).

Dibner 183; *En français dans le texte* 255; Grolier/Horblit 21a (4th issue); Norman 569; PMM 318b; Roosens and Salu 2778a; Sparrow 46. Beaumont Newhall, *An historical & descriptive account of the various processes of the Daguerreotype & Diorama by Daguerre*, 1971 (see pp. 269-277 for a chronological listing of the numerous printings). Gernsheim, *The history of photography*, 1969. Hannavy (ed.), *Encyclopedia of nineteenth-century photography*, 2013.

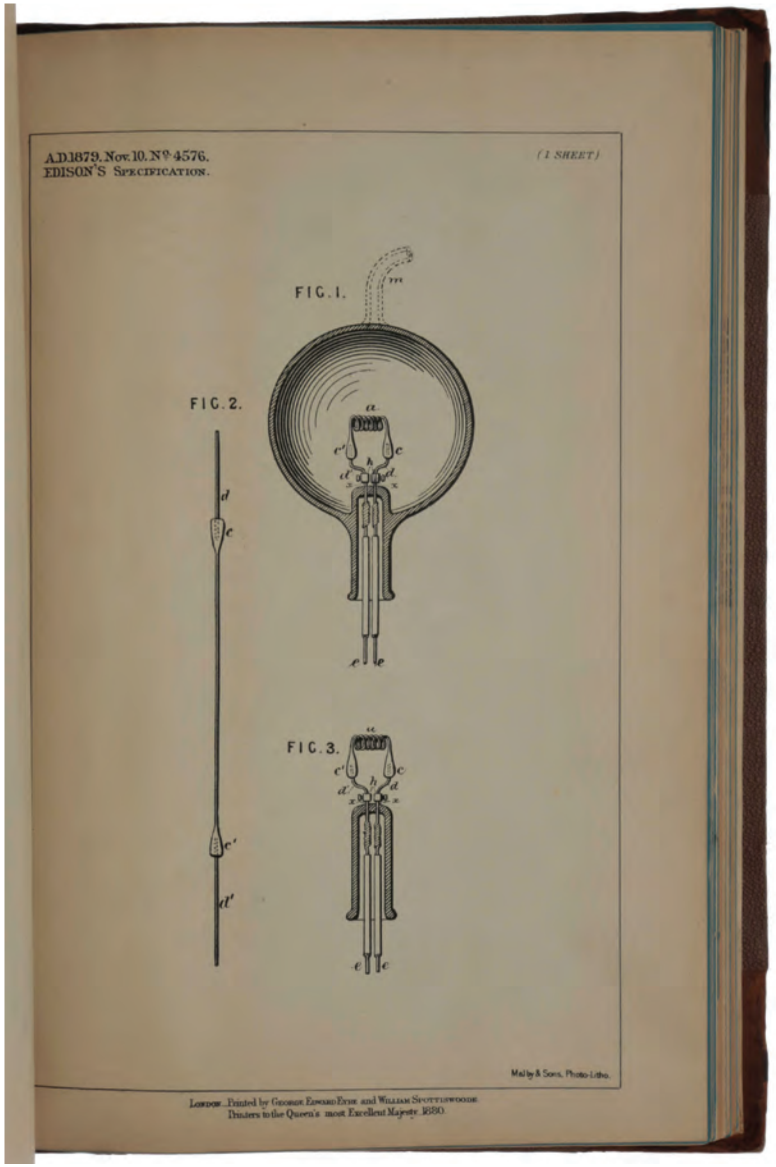
EDISON'S ELECTRIC LAMP - THE COPY OF CHARLES BATCHELOR

EDISON, Thomas Alva. *Specification of Thomas Alva Edison. Electric lamps. No. 4576, 10 November, 1879.* [Bound with thirteen other patents, including eight more by Edison, most concerned with electric lighting, the design and manufacture of light bulbs, and the supply of electricity]. London: Published and sold at the Commissioners of Patents Sale Department [British Patent Office], 1880.

$27,500

Contemporary half-calf (a little rubbed, some well done leather restoration to spine), spine label titled 'Edison's Electric Light – British Patents 1878-80.' Charles Batchelor's library stamp on front paste-down, two labels from a later institutional library on rear endpapers.

First edition, of the greatest rarity, of the first British patent granted to Edison for his most famous invention, that of an incandescent electric light. Perhaps more than any other, it was this invention that ushered in the modern age. This British patent, filed just six days after the US patent 223,898, enabled Edison and his English rival Joseph Swan to establish an effective monopoly on the electric lighting market in Britain until Edison's patent expired in 1893 (see below). This is the finest possible association copy, having formed part of the working library of the English-born engineer Charles Batchelor, Edison's 'right-hand man' for several decades, and his chief experimental assistant from 1873. This patent is extremely rare even in institutional collections – no copy is listed on OCLC, although there is a copy at Rutgers, which holds the bulk of Charles Batchelor's papers. We have



located no copy in auction records, and the last copy on the market was offered almost 80 years ago. The US patent has been selected as one of the 100 most important documents in the US National Archives.

"The invention that Edison is most remembered for is, by far, the electric light bulb. At the age of thirty-one, he decided to focus his energies–and the manpower of the Menlo Park facility–toward creating an electric light system. He began work in the fall of 1878, after returning from a vacation with the physicist George Barker. Barker had encouraged Edison to work on creating an electrical system and discussed ideas with him about it. Edison recognized the staggering potential of an electrical light system and decided to focus on creating one, having just finished work on the phonograph.

"At this time, gas lamps lighted most American cities. Other inventors had already done some pioneering work in the electrical light field, especially Humphrey Davy in 1802 (who first produced "incandescence," an electric current flowing through wire) and the Englishman Joseph Swan in 1860 (who produced many experimental incandescent lamps). But no one had been able to completely solve the practical problems of creating an effective and reliable lamp.

"From October 1878 until New Year's Day, 1880, Edison developed the components for a lighting system. His experience with telegraph technology assisted him as he tried to envision a system of relays and circuit breakers that would be necessary to making a lamp work. The main problems were locating the proper filament for the incandescent spiral and constructing a lamp that had enough pressure to contain the filament. He perfected new vacuum techniques for the latter problem and rejected the spiral filament in favor of a filament of carbonized thread.

"When the lamp with carbonized thread lasted for forty-five hours the staff at Menlo Park realized that they had had a breakthrough. Edison claimed, "none of us could go to bed, and there was no sleep for any of us for forty hours." In November 1879 they tried using carbonized cardboard, and soon they had created an experimental bulb that was superior to any of the others they had tested" (sparknotes.com/biography/edison/section5.rhtml)

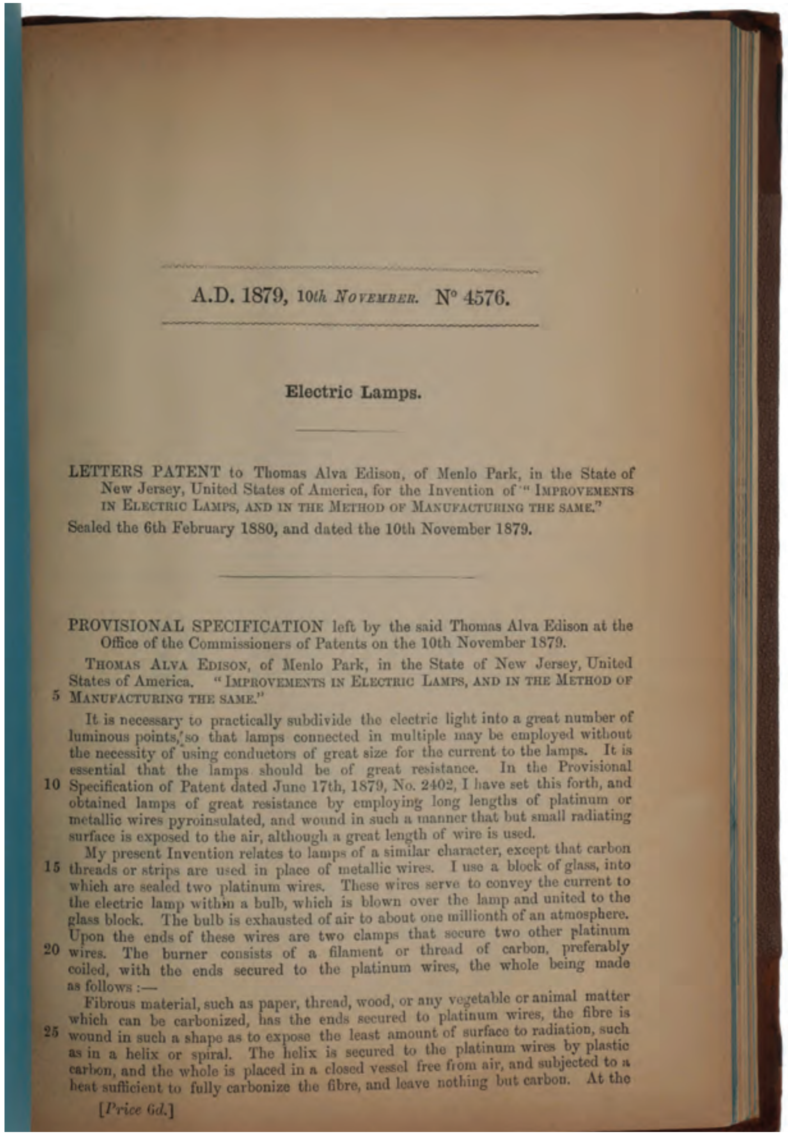
While other incandescent lamps were created before his, Edison's version was able to outstrip the others because of a combination of three factors: an effective incandescent material, a higher vacuum than others were able to achieve (by use of the Sprengel pump) and a high resistance that made power distribution from a centralized source economically viable. "The English scientist Joseph Swan (1828-1914) and Thomas Edison in the United States were the two men mainly responsible for the invention of a workable electric light bulb, a new stage in the development of electrical power. Swan demonstrated his tubular electric light bulb in Newcastle in 1878…. Edison 'inventor of inventors,' … displayed his incandescent lamp in 1879… In the same year he obtained British Patent 4576 [offered here] a few months before Swan" (Briggs & Burke, *Social History of the Media: From Gutenberg to the Internet* (2010), p. 118).

"Believing both his initial incandescent lamp lacked patentable inventions and that the technical details were practically public knowledge, Swan did not even attempt to take out a patent on any of these early activities. Yet since the UK Patent Office did not then examine for anticipation, Edison was able to file the first British patent on a carbon filament incandescent electric lamp on November 10, 1879 [the offered patent]. As Edison explained from the provisional specification, in the new patent carbon threads or strips are used in place of metallic wires. I use a block of glass onto which are sealed two platinum wires. These wires serve to convey the current to the electric lamp within a bulb, which is blown over the lamp and united to the glass block … The burner consists of a filament or thread of carbon, preferably coiled, with the ends secured to the platinum wires… this patent turned out to be of great significance for both Edison's and Swan's companies, later used to establish a monopoly in Britain…

"Since Swan had entrepreneurial experience of patenting activities and patent law, he realized that he might have made a miscalculation in not patenting carbon filament lamps, and that this could prove harmful for his efforts to manufacture them commercially. In response to Edison, Swan's first patent in 1880 was not for carbon filaments but for improvements that were needed to overcome practical problems in the production of lamps… In a practical sense, Swan's patent supplemented Edison's and together they provided the foundation patents for manufacturing carbon filament incandescent lamps…

"By 1882, the British Edison Company was established and planning active involvement in the British electric lighting market. It had plans to start developing electrification networks for public or private installations, controlling the electric lighting market by using the Edison patent of 1879. It started legal action against the Swan Company to prevent it from using or further developing carbon filament lamps that, it claimed, were produced with knowledge and practices covered in Edison's patent. However, the Edison Company realized that, although lacking a strong patent, Swan could defend his claims through evidence of his prior research and publication. As they faced the prospect of a long and costly court case with a highly uncertain outcome, negotiations soon began for an alternative, mutually beneficial solution for the Swan and Edison companies. Finally, the lawyers and directors of both proposed a merger as the Edison and Swan Electric Lighting Company.

"The combined Edison-Swan Company capitalized on the Edison patent of 1879 to establish a monopoly that, practically speaking, lasted until Edison's



A.D. 1879, 10*th* *November*. N° 4576.

Electric Lamps.

LETTERS PATENT to Thomas Alva Edison, of Menlo Park, in the State of New Jersey, United States of America, for the Invention of "IMPROVEMENTS IN ELECTRIC LAMPS, AND IN THE METHOD OF MANUFACTURING THE SAME."

Sealed the 6th February 1880, and dated the 10th November 1879.

PROVISIONAL SPECIFICATION left by the said Thomas Alva Edison at the Office of the Commissioners of Patents on the 10th November 1879.

THOMAS ALVA EDISON, of Menlo Park, in the State of New Jersey, United States of America. "IMPROVEMENTS IN ELECTRIC LAMPS, AND IN THE METHOD OF MANUFACTURING THE SAME."

It is necessary to practically subdivide the electric light into a great number of luminous points, so that lamps connected in multiple may be employed without the necessity of using conductors of great size for the current to the lamps. It is essential that the lamps should be of great resistance. In the Provisional Specification of Patent dated June 17th, 1879, No. 2402, I have set this forth, and obtained lamps of great resistance by employing long lengths of platinum or metallic wires pyroinsulated, and wound in such a manner that but small radiating surface is exposed to the air, although a great length of wire is used.

My present Invention relates to lamps of a similar character, except that carbon threads or strips are used in place of metallic wires. I use a block of glass, into which are sealed two platinum wires. These wires serve to convey the current to the electric lamp within a bulb, which is blown over the lamp and united to the glass block. The bulb is exhausted of air to about one millionth of an atmosphere. Upon the ends of these wires are two clamps that secure two other platinum wires. The burner consists of a filament or thread of carbon, preferably coiled, with the ends secured to the platinum wires, the whole being made as follows:—

Fibrous material, such as paper, thread, wood, or any vegetable or animal matter which can be carbonized, has the ends secured to platinum wires, the fibre is wound in such a shape as to expose the least amount of surface to radiation, such as in a helix or spiral. The helix is secured to the platinum wires by plastic carbon, and the whole is placed in a closed vessel free from air, and subjected to a heat sufficient to fully carbonize the fibre, and leave nothing but carbon. At the

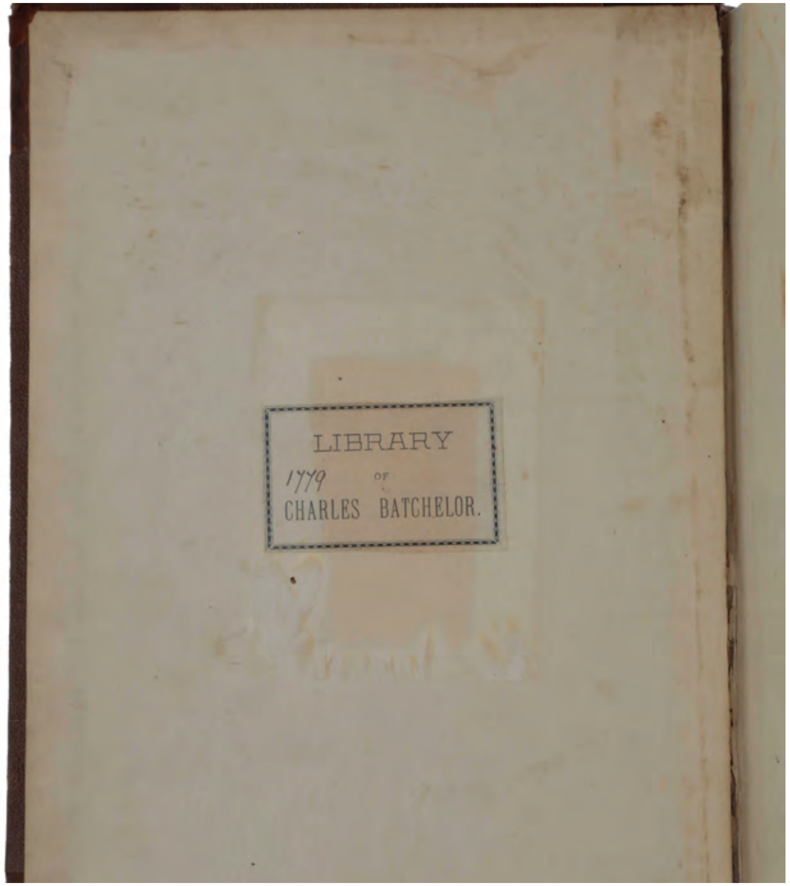
[*Price 6d.*]

patent expired in 1893" (Arapostathis & Gooday, *Patently Contestable: Electrical Technologies and Inventor Identities on Trial in Britain* (2013), pp. 178-183).

"Charles Batchelor is famous as being Thomas Edison's "right hand man" during much of Edison's long and prolific career. In his rarefied position Batchelor was involved in some of the greatest inventions and technological developments in history.

"Batchelor was born on Christmas Day, 1845, and raised in Manchester, England, then the heart of the British textile industry. The textile industry employed many engineers and mechanics, including Batchelor. He was apparently working for a textile equipment manufacturer in 1870 when he was sent to the United States to install some equipment in a Newark, New Jersey textile factory. At this time, Thomas Edison's main laboratory and shop were also located in Newark. Edison met the 25-year-old Brit, and the two formed a working relationship that would last for years. While Edison basked in the spotlight of fame, the self-effacing Batchelor made his contributions behind the scenes. Over the course of several decades, Batchelor assisted Edison with some of his most important projects in the fields of telegraphy, telephony, the phonograph, and electric lighting. In 1873 Edison named "Batch" his chief experimental assistant. Together Batchelor and Edison would come up with prospective products. Edison also frequently entrusted him with responsibility for special projects, such as setting up a demonstration lighting system at the International Electrical Exposition in Paris in 1881. In fact, Batchelor stayed in Paris for the next three years as manager of the Edison electric light companies that were established there.

"It was Edison's practice to give his key assistants shares in his companies and to let them invest in the business ventures that resulted from their inventive activity. Batchelor received shares in the Edison Electric Light Company (found in 1878) and later invested in the Edison Lamp Company (founded in 1880), the Edison Machine Works (1881), and the Edison General Electric Company (1888). He even managed the Machine Works for several years. Eventually, he was named treasurer of Edison General Electric when it was created in 1892" (*IEEE Global History Network*, ieeeghn.org/wiki/index.php/Charles_Batchelor). Dibner 70 (for the US patent).



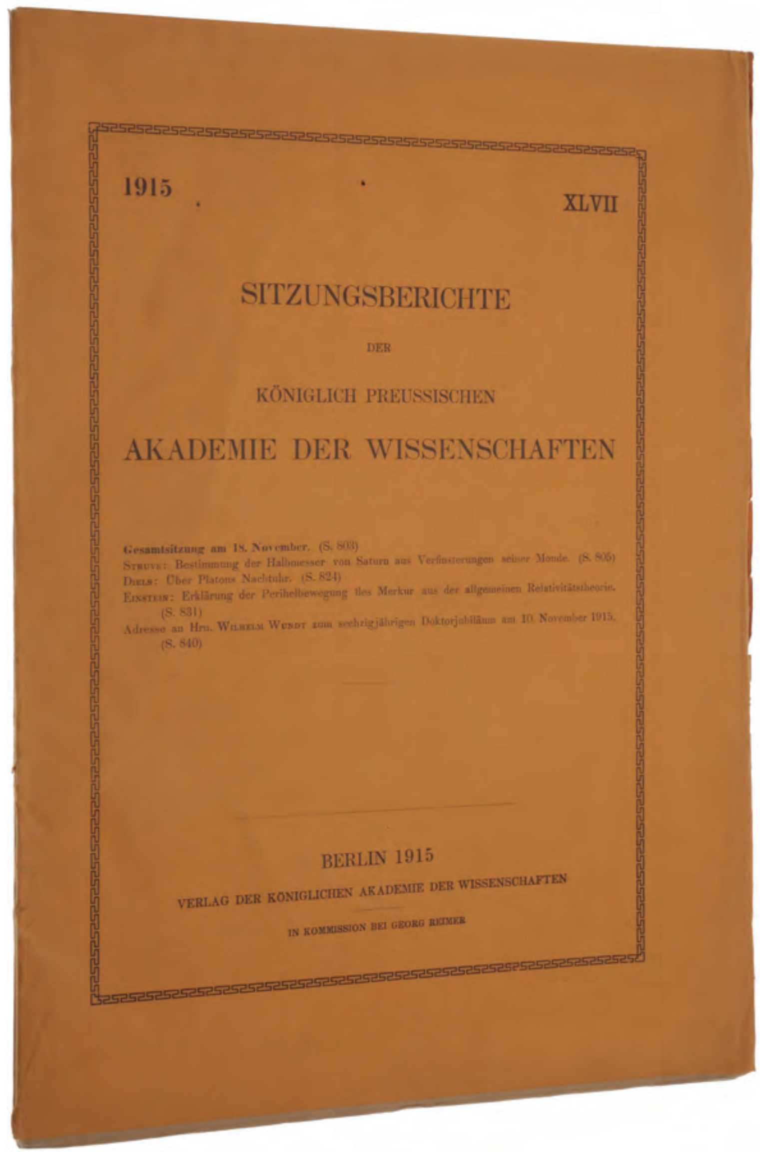
THE FIRST OBSERVATIONAL VERIFICATION OF GENERAL RELATIVITY

EINSTEIN, Albert. *Erklärung der Perihelbewegung des Merkur aus der allgemeinen Relativitätstheorie.* Berlin: Königlichen Akademie der Wissenschaften, 1915.

$8,500

Large 8vo (268 x 190 mm). In: Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften, XLVII, pp.831-839. The entire issue (pp.803-842) offered here, in original printed wrappers. A very fine and unopened copy. Rare in such good condition.

First edition, journal issue in original printed wrappers, of one of Einstein's most important papers, in which "he presents two of his greatest discoveries. Each of these changed his life" (Pais, p. 253). "In the fall of 1915, Einstein came to the painful realization that the 'Entwurf' field equations are untenable. Casting about for new field equations, he fortuitously found his way back to equations of broad covariance that he had reluctantly abandoned three years earlier. He had learned enough in the meantime to see that they were physically viable after all … and on November 4, 1915, presented the rediscovered old equations to the Berlin Academy. He returned a week later with an important modification, and two weeks after that with a further modification. In between these two appearances before his learned colleagues, he presented yet another paper showing that his new theory explains the anomalous advance of the perihelion of Mercury. Fortunately, this result was not affected by the final modification of the field equations presented the following week" (Janssen, pp. 59-60). "The first result [reported in the present



paper] was that his theory [of general relativity] 'explains … quantitatively … the secular rotation of the orbit of Mercury, discovered by Le Verrier, … without the need of any special hypothesis.' This discovery was, I believe, by far the strongest emotional experience in Einstein's scientific life, perhaps in all his life. Nature had spoken to him. He had to be right. 'For a few days, I was beside myself with joyous excitement'. Later, he told Fokker that his discovery had given him palpitations of the heart. What he told de Haas is even more profoundly significant: when he saw that his calculations agreed with the unexplained astronomical observations, he had the feeling that something actually snapped in him" (Pais, p. 253). "Einstein devoted only half a page to his second discovery: the bending of light [by gravity] is twice as large as he had found earlier. 'A light ray passing the sun should suffer a deflection of 1".7 (instead of 0".85)'" (Pais, p. 255). The confirmation of this prediction four years later by Dyson and Eddington not only confirmed Einstein's theory, but also made Einstein world famous.

"Einstein's discovery resolved a difficulty that was known for more than sixty years. Urbain Jean Joseph Le Verrier had been the first to find evidence for an anomaly in the orbit of Mercury and also the first to attempt to explain this effect. On September 12, 1859, he submitted to the Academy of Sciences in Paris the text of a letter to Herve Faye in which he recorded his findings. The perihelion of Mercury advances by thirty-eight seconds per century due to 'some as yet unknown action on which no light has been thrown … a grave difficulty, worthy of attention by astronomers.' The only way to explain the effect in terms of known bodies would be (he noted) to increase the mass of Venus by at least 10 per cent, an inadmissible modification. He strongly doubted that an intramercurial planet, as yet unobserved, might be the cause. A swarm of intramercurial asteroids was not ruled out, he believed. 'Here then, *mon cher confrere,* is a new complication which manifests itself in the neighborhood of the sun.' Perihelion precessions of Mercury and other bodies have been the subject of experimental study from 1850 up to the present. The value 43 seconds per century for Mercury, obtained in 1882 by Simon Newcomb, has not changed. The present best value is 43".11 + 0.45. The experimental number quoted by Einstein on November 18, 1915, was 45" ± 5.

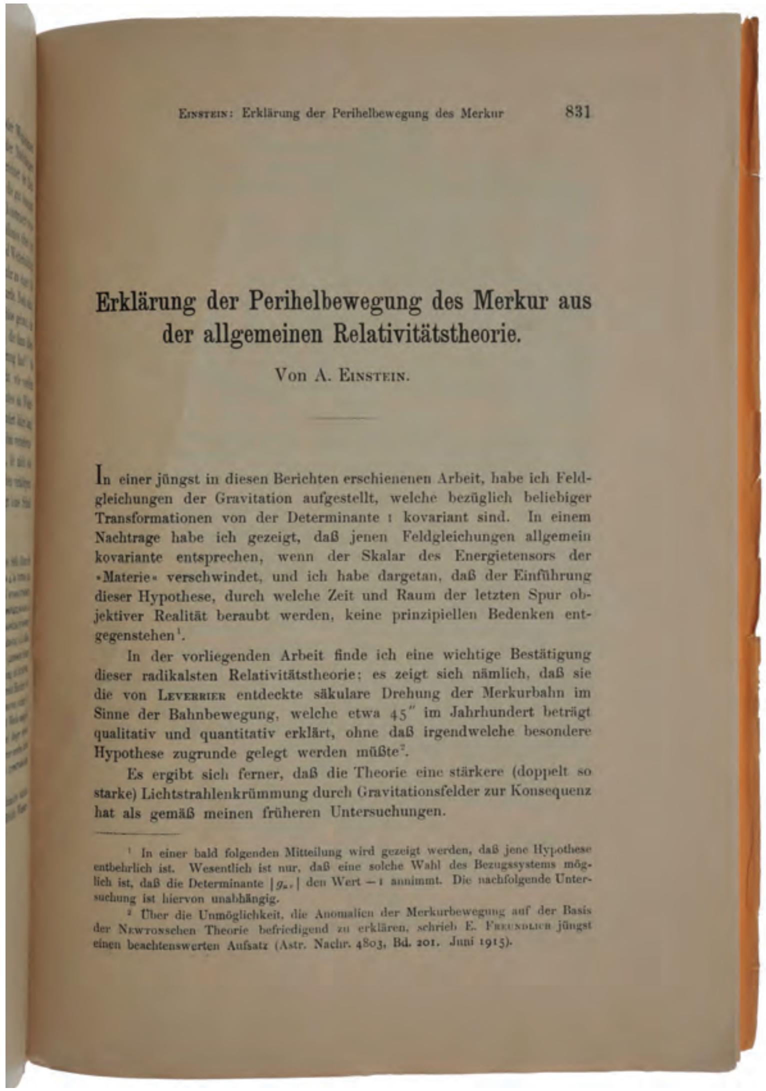
"In the late nineteenth and early twentieth centuries, attempts at a theoretical interpretation of the Mercury anomaly were numerous. Le Verrier's suggestions of an intramercurial planet or planetary ring were reconsidered. Other mechanisms examined were a Mercury moon (again as yet unseen), interplanetary dust, and a possible oblateness of the sun. Each idea had its proponents at one time or another. None was ever generally accepted. All of them had in common that Newton's $1/r^2$ law of gravitation was assumed to be strictly valid. There were also a number of proposals to explain the anomaly in terms of a deviation from this law … These attempts either failed or are uninteresting because they involve adjustable parameters. Whatever was tried, the anomaly remained puzzling. In his later years, Newcomb tended 'to prefer provisionally the hypothesis that the sun's gravitation is not exactly as the inverse square'.

"Against this background, Einstein's joy in being able to give an explanation 'without any special hypothesis' becomes all the more understandable" (Pais, pp. 253-4).

"Let us briefly recapitulate Einstein's progress in understanding the bending of light. 1907. The clerk at the patent office in Bern discovers the equivalence principle, realizes that this principle by itself implies some bending of light, but believes that the effect is too small to ever be observed. 1911. The professor at Prague finds that the effect *can* be detected for starlight grazing the sun during a total eclipse and finds that the amount of bending in that case is 0".87. He does not yet know that space is curved and that, therefore, his answer is incorrect. He is still too close to Newton, who believed that space is flat and who could have himself computed the 0".87 (now called the Newton value) from his law of gravitation and his corpuscular theory of light. 1912. The professor at Zürich discovers that space is curved. Several years pass before he understands that the curvature of space modifies the bending of light. 1915. The member of the Prussian Academy discovers that general relativity implies a bending of light by the sun equal to 1".74, the Einstein value, twice the Newton value. This factor of 2 sets the stage for a confrontation between Newton and Einstein …

"An opportunity to observe an eclipse in Venezuela in 1916 had to be passed up because of the war. Early attempts to seek deflection in photographs taken during past eclipses led nowhere. An American effort to measure the effect during the eclipse of June 1918 never gave conclusive results. It was not until May 1919 that two British expeditions obtained the first useful photographs and not until November 1919 that their results were formally announced.

"English interest in the bending of light developed soon after copies of Einstein's general relativity papers were sent from Holland by de Sitter to Arthur Stanley Eddington at Cambridge (presumably these were the first papers on the theory to reach England). In addition, de Sitter's beautiful essay on the subject, published in June 1916 in the *Observatory*, as well as his three important papers in the *Monthly Notices* further helped to spread the word. So did a subsequent report by Eddington, who in a communication to the Royal Astronomical Society in February 1917 stressed the importance of the deflection of light. In March 1917 the Astronomer Royal, Sir Frank Watson Dyson, drew attention to the excellence of the star configuration on May 29, 1919, (another eclipse date) for measuring the alleged deflection, adding that 'Mr Hinks has kindly undertaken to obtain for the Society information of the stations which may be occupied'. Two expeditions were mounted, one to Sobral in Brazil, led by Andrew Crommelin from the Greenwich Observatory, and one to Principe Island off the coast of Spanish Guinea, led by



EINSTEIN: Erklärung der Perihelbewegung des Merkur 831

Erklärung der Perihelbewegung des Merkur aus der allgemeinen Relativitätstheorie.

Von A. EINSTEIN.

In einer jüngst in diesen Berichten erschienenen Arbeit, habe ich Feldgleichungen der Gravitation aufgestellt, welche bezüglich beliebiger Transformationen von der Determinante 1 kovariant sind. In einem Nachtrage habe ich gezeigt, daß jenen Feldgleichungen allgemein kovariante entsprechen, wenn der Skalar des Energietensors der »Materie« verschwindet, und ich habe dargetan, daß der Einführung dieser Hypothese, durch welche Zeit und Raum der letzten Spur objektiver Realität beraubt werden, keine prinzipiellen Bedenken entgegenstehen[1].

In der vorliegenden Arbeit finde ich eine wichtige Bestätigung dieser radikalsten Relativitätstheorie; es zeigt sich nämlich, daß sie die von LEVERRIER entdeckte säkulare Drehung der Merkurbahn im Sinne der Bahnbewegung, welche etwa 45" im Jahrhundert beträgt qualitativ und quantitativ erklärt, ohne daß irgendwelche besondere Hypothese zugrunde gelegt werden müßte[2].

Es ergibt sich ferner, daß die Theorie eine stärkere (doppelt so starke) Lichtstrahlenkrümmung durch Gravitationsfelder zur Konsequenz hat als gemäß meinen früheren Untersuchungen.

[1] In einer bald folgenden Mitteilung wird gezeigt werden, daß jene Hypothese entbehrlich ist. Wesentlich ist nur, daß eine solche Wahl des Bezugssystems möglich ist, daß die Determinante $|g_{\mu\nu}|$ den Wert -1 annimmt. Die nachfolgende Untersuchung ist hiervon unabhängig.

[2] Über die Unmöglichkeit, die Anomalien der Merkurbewegung auf der Basis der NEWTONschen Theorie befriedigend zu erklären, schrieb E. FREUNDLICH jüngst einen beachtenswerten Aufsatz (Astr. Nachr. 4803, Bd. 201, Juni 1915).

Eddington. Before departing, Eddington wrote, 'The present eclipse expeditions may for the first time demonstrate the weight of light [i.e., the Newton value]; or they may confirm Einstein's weird theory of non- Euclidean space; or they may lead to a result of yet more far-reaching consequences – no deflection'. Under the heading 'Stop Press News,' the June issue of the *Observatory* contains the text of two telegrams, one from Sobral: 'Eclipse splendid. Crommelin,' and one from Principe: 'Through cloud. Hopeful. Eddington'. The expeditions returned. Data analysis began. According to a preliminary report by Eddington to the meeting of the British Association held in Bournemouth on September 9-13, the bending of light lay between 0".87 and double that value. Word reached Lorentz. Lorentz cabled Einstein, whose excitement on receiving this news after seven years of waiting will now be clearer. Then came November 6, 1919, the day on which Einstein was canonized.

"Ever since 1905 Einstein had been *beatus,* having performed two first-class miracles. Now, on November 6, the setting, a joint meeting of the Royal Society and the Royal Astronomical Society, resembled a Congregation of Rites. Dyson acted as postulator, ably assisted by Crommelin and Eddington as advocate-procurators. Dyson, speaking first, concluded his remarks with the statement, 'After a careful study of the plates I am prepared to say that they confirm Einstein's prediction. A very definite result has been obtained, that light is deflected in accordance with Einstein's law of gravitation'" (Pais, pp. 303-5).

Janssen, 'Of pots and holes: Einstein's bumpy road to general relativity,' *Annalen der Physik* 14, Supplement, 2005, pp. 58-85; Pais, *Subtle is the Lord*, 1982; Weil 76.

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sammen mit den allgemeinen Bedingungen, welche wir unserer Lösung auferlegt haben. Die letzte Feldgleichung

$$\sum_\sigma \frac{\partial \Gamma^\sigma_{44}}{\partial x_\sigma} + \sum_{\sigma\tau} \Gamma^\sigma_{4\tau} \Gamma^\tau_{4\sigma} = 0$$

geht mit Rücksicht auf (6 b) bei Vernachlässigung von Größen dritter und höherer Ordnung über in

$$\sum_\sigma \frac{\Gamma^\sigma_{44}}{\partial x_\sigma} = \frac{\alpha^2}{2 r^4}.$$

Hieraus folgern wir mit Rücksicht auf (6 b) und die Symmetrieeigenschaften unserer Lösung

$$\Gamma^\sigma_{44} = -\frac{\alpha}{2} \frac{x_\sigma}{r^3} \left(1 - \frac{\alpha}{r}\right). \tag{6c}$$

§ 2. Die Planetenbewegung.

Die von der allgemeinen Relativitätstheorie gelieferten Bewegungsgleichungen des materiellen Punktes im Schwerefelde lauten

$$\frac{d^2 x_\nu}{ds^2} = \sum_{\sigma\tau} \Gamma^\nu_{\sigma\tau} \frac{dx_\sigma}{ds} \frac{dx_\tau}{ds}. \tag{7}$$

Aus diesen Gleichungen folgern wir zunächst, daß sie die NEWTONschen Bewegungsgleichungen als erste Näherung enthalten. Wenn nämlich die Bewegung des Punktes mit gegen die Lichtgeschwindigkeit kleiner Geschwindigkeit stattfindet, so sind dx_1, dx_2, dx_3 klein gegen dx_4. Folglich bekommen wir eine erste Näherung, indem wir auf der rechten Seite jeweilen nur das Glied $\sigma = \tau = 4$ berücksichtigen. Man erhält dann mit Rücksicht auf (6 b)

$$\left.\begin{aligned} \frac{d^2 x_\nu}{ds^2} &= \Gamma^\nu_{44} = -\frac{\alpha}{2} \frac{x_\nu}{r^3} \quad (\nu = 1, 2, 3) \\ \frac{d^2 x_4}{ds^2} &= 0 \end{aligned}\right\}. \tag{7a}$$

Diese Gleichungen zeigen, daß man für eine erste Näherung $s = x_4$ setzen kann. Dann sind die ersten drei Gleichungen genau die NEWTONschen. Führt man in der Bahnebene Polargleichungen r, ϕ ein, so liefern der Energie- und der Flächensatz bekanntlich die Gleichungen

$$\left.\begin{aligned} \frac{1}{2} u^2 + \Phi &= A \\ r^2 \frac{d\phi}{ds} &= B \end{aligned}\right\}, \tag{8}$$

Sitzungsberichte 1915. 82

A TURNING POINT IN THE HISTORY OF MODERN PHYSICS

[EINSTEIN, LORENTZ, PLANCK, et al.] *La théorie du rayonnement et les quanta: rapports et discussions de la réunion tenue à Bruxelles, du 30 octobre au 3 novembre 1911 sous les auspices de M. E. Solvay; Publiés par MM. Langevin et M. de Broglie.* Paris: Gauthier-Villars, 1912.

$2,500

8vo (258 x 165 mm), pp. vi, 461, [1], text illustrated throughout. Original printed wrappers (spine a little faded). A very fine and completely unrestored copy.

First edition, rare in the original printed wrappers, of the proceedings of the First Solvay Congress, widely considered a turning point in the history of modern physics, which was devoted to the problem of reconciling classical physics with quantum theory, the central problem in physics at that time. "The breakthrough was, in fact, achieved in 1911; the climate of thought had become completely transformed. The quantum concept was no longer the view of outsiders but became a matter of significance recognized by many leading scientists … The Solvay Congress in Brussels played a crucial role in this development. Physicists who until then had not been involved — such as Henri Poincaré — were won over to the quantum concept. Those already convinced were exposed to an even stronger and deeper impression of the significance of the quantum problem. Thus, Max Planck wrote to Willy Wien on December 8, 1911: 'I certainly hope that our scientifically stimulating days in Brussels have also agreed with you ... the impressions which we had the opportunity of gathering there will give us food for thought for a long time to come' … The official report of the congress [offered here] represented a comprehensive handbook on the quantum problem. Due to the fundamental importance of the questions which it treated and in view of the participation of the leading physicists of the day, its publication strongly stimulated the interest of all physicists involved in new developments … After the congress, Louis de Broglie … described the great impact which the congress report had made on him in the following words: 'With youthful vigor, I became enthusiastic about these interesting problems which had been researched, and I promised myself to spare no effort in gaining an understanding of the true nature of these mysterious quanta which Planck had introduced ten years earlier into theoretical physics but whose great significance had not been understood at that time' … In a private discussion with Ernest Rutherford, Niels Bohr obtained, toward the end of 1911, 'a vivid account' of the discussions held at the Solvay Congress; when the congress report appeared several months later, Bohr studied it closely" (Hermann, *The Genesis of Quantum Theory*, pp. 141-143). The conference "set the style for a new type of scientific meeting, in which a select group of the most well informed experts in a given field would meet to discuss the problems at its frontiers, and would seek to define the steps for their solution" (Mehra, *The Solvay Conferences on Physics,* p. xv). During the preparation phase of the congress several participants were asked to write detailed reports that were mimeographed and copies sent in advance to the invited members. These 'rapporteurs' were: Lorentz, Jeans, Warburg, Rubens, Planck, Knudsen, Perrin, Nernst, Kamerlingh Onnes, Sommerfeld, Langevin, and Einstein. The discussions on these reports, often of great historical interest, are also fully included in the present work.

"The most important advances in physics in the 19th century were perhaps the development of Maxwell's electromagnetic theory, which afforded a far-reaching explanation of radiative phenomena, and the statistical formulation and interpretation of thermodynamics which culminated in Boltzmann's relation between the entropy and probability of the state of a complex mechanical system. However, as Rayleigh's analysis of black-body radiation had shown, the physical and mathematical description of the spectral distribution of cavity radiation in thermal equilibrium presented unsuspected difficulties.

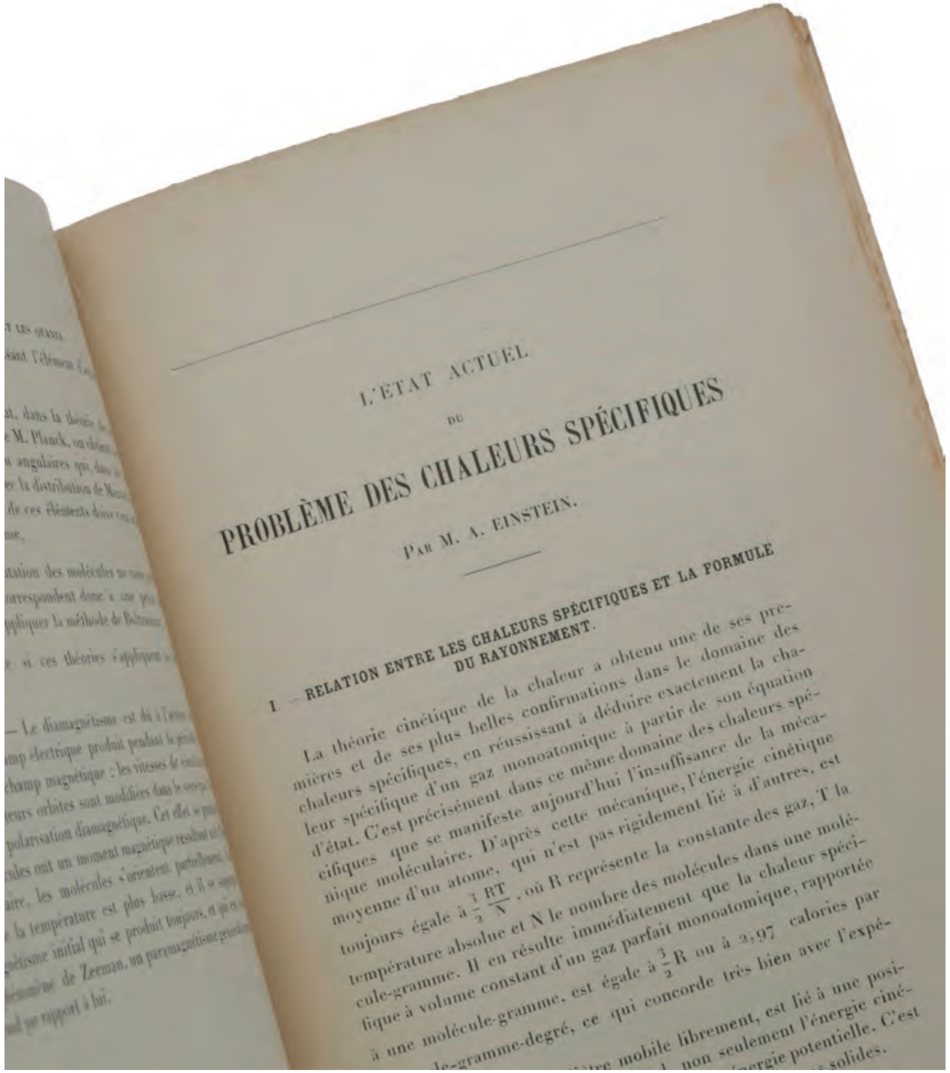
"Planck's discovery of the universal quantum of action in 1900 was a turning point in the development. It revealed a feature of "discreteness" in atomic processes, which was completely foreign to classical physics. Einstein not only emphasized the apparent paradoxes which had arisen in the detailed description of the interaction between matter and radiation, but provided support for Planck's ideas by his investigation of the specific heat of solids at low temperatures. Einstein also introduced the idea of light quanta or photons as carriers of energy and momentum in elementary radiative processes, and successfully used it in his explanation of the photoelectric effect" (Mehra, pp. 13-14).

The great German physical chemist Walther Nernst, then professor at the University of Berlin, took notice of the development of the quantum theory, and particularly of Einstein's theory of specific heat. "In spring 1910 he encountered, at the home of the chemist Robert Goldschmidt in Brussels, the Belgian industrialist Ernest Solvay. Solvay, who had invented a special process to produce sodium carbonate and had founded the firm of Solvay & Company in 1863, was, for a long time, attracted to the study of the structure of matter in the context of a theory of gravitation. In his meeting with Nernst at Goldschmidt's house, Solvay talked about his scientific ideas and wondered whether they could be brought to the attention of the great physicists like Planck, Lorentz, Poincaré and Einstein. Nernst at once saw a great opportunity, namely, to connect Solvay's desire to have his work discussed with the possibility of holding an international conference on the current problems of the kinetic theory of matter and the quantum theory of radiation. Thus he proposed a council, or conference, to be held for that purpose.

Solvay responded favourably and charged Nernst to explore matters further with Planck, Lorentz, Einstein and other prominent scientists.

"Nernst drafted a short memorandum on the plan and purpose of the conference he had in mind and sent it to Max Planck for comments. Planck gave a detailed answer; he wrote: 'To the marginal notes that I have already made, with your permission, on your manuscript allow me to add a few more generalities. Your idea corresponds, fully and completely, to the problem whose solution is envisaged, and I can only associate myself with it with full conviction. However [he continued], I am not able to hide my great concern about its execution. As I have already mentioned in my marginal notes, such a conference will be more successful if you wait until more factual material is available' (Planck to Nernst, 11 June 1910). Planck thought that at that time most scientists, including many of those whom Nernst had suggested should participate in the conference, were not really excited about the quantum problems. 'Among all those mentioned by you,' he said, 'I believe that, other than ourselves, only Einstein, Lorentz, W. Wien and Larmor will be seriously interested in the matter' (*ibid.*). Planck, therefore, suggested to postpone the conference. 'Let one or better two years go by,' he wrote, 'and we shall see how the gap which begins to open in the theory shall develop, and how finally those who still stand at a distance will be forced to join in' (*ibid.*).

"In spite of Planck's cautious warning, Nernst went ahead with the plan. Already on 26 July he wrote a letter to Solvay, in which he enclosed the draft of an 'Invitation to an International Scientific Conference to Elucidate Certain Current Questions of the Kinetic Theory.' This draft opened with the remarks: 'It appears that we find ourselves at present in the midst of an all-encompassing reformulation of the principles on which the erstwhile kinetic theory of matter has been based. On the one hand, this theory leads to a logical formulation – which nobody contests – of a radiation formula whose validity is contradicted by all experiments; on



the other, there follows from the same theory certain results on the specific heat (constancy of the specific heat of a gas with the variation of temperature, the validity of Dulong and Petit's law up to the lowest temperature), which are also completely refuted by many measurements. As Planck and Einstein in particular have shown, these contradictions disappear if one places certain limits (doctrine of energy quanta) on the motion of electrons and atoms in the case of their oscillations around a position of rest. But this interpretation, in turn, is so far removed from the equations of motion of material points employed until now, that its acceptance would incontestably lead to a far-reaching reformulation of our erstwhile fundamental notions.'

"The draft then proposed that perhaps a solution might be found by 'a personal exchange of views on these problems between the researchers who are more or less actively concerned with them'. Nernst suggested, in the draft, certain topics to be discussed at the conference, and he further proposed a list of eighteen participants with Lord Rayleigh as chairman. Solvay approved Nernst's plan in principle; as for the date of the conference, which Nernst had put around Easter 1911, he preferred to shift it to October of the same year. After due preparations the invitations finally went out in June 1911. All the invited scientists accepted, but for Joseph Larmor – who thought he had not had the time to keep up with the recent progress – and Lord Rayleigh – who thought of himself as 'too poor a linguist' to be useful at the conference. At the end of October the following participants arrived in Brussels to take part in the first Solvay Conference (30 October to 3 November 1911): Walther Nernst, Max Planck, Heinrich Rubens, Arnold Sommerfeld, Emil Warburg and Willy Wien from Germany; James Hopwood Jeans and Ernest Rutherford from England; Marcel Brillouin, Marie Curie, Paul Langevin, Jean Perrin and Henri Poincaré from France; Albert Einstein and Friedrich Hasenöhrl from Austria; Heike Kamerlingh Onnes and Hendrik Lorentz from Holland; and Martin Knudsen from Denmark. Lorentz assumed the Chairmanship and Robert Goldschmidt, Maurice de Broglie and Frederick A. Lindemann acted as Scientific Secretaries of the Conference, which was devoted to the problems of 'Radiation Theory and the Quanta'" (Mehra & Rechenberg, *The Historical Development of Quantum Theory*, Vol. 1, pp. 126-9).

"The discussions at the Conference were initiated after the report of H. A. Lorentz. He developed the arguments based on classical ideas leading to the principle of the equipartition of energy between the various degrees of freedom of a physical system, including not only the motion of its constituent material particles but also the normal modes of vibration of the electromagnetic field associated with the electric charge of the particles. These arguments, which followed the lines of Rayleigh's analysis of thermal radiative equilibrium, led to the well-known paradoxical result that no temperature equilibrium was possible since the whole energy of the system would be gradually transferred to electromagnetic vibrations of steadily increasing frequencies …

"After the reports of Warburg and Rubens on the experimental evidence supporting Planck's law of temperature radiation, Planck himself gave an exposition of the arguments which had led him to the discovery of the quantum of action. Planck was deeply concerned with the problems of harmonizing this new feature with the conceptual framework of classical physics. He emphasized that the essential point was not the introductions of a new hypothesis of energy quanta, but rather a reformulation of the very concept of action. Planck expressed the conviction that the principle of least action, which had also been upheld in the theory of relativity, would serve to guide the further development of quantum theory.

"Walther Nernst, in his report on the applications of quantum theory to various problems of physics and chemistry, considered the properties of matter at very low temperatures. Nernst remarked that his theorem regarding the entropy at absolute zero, of which he had made important applications since 1906, now appeared as a special case of a more general law derived from the theory of quanta.

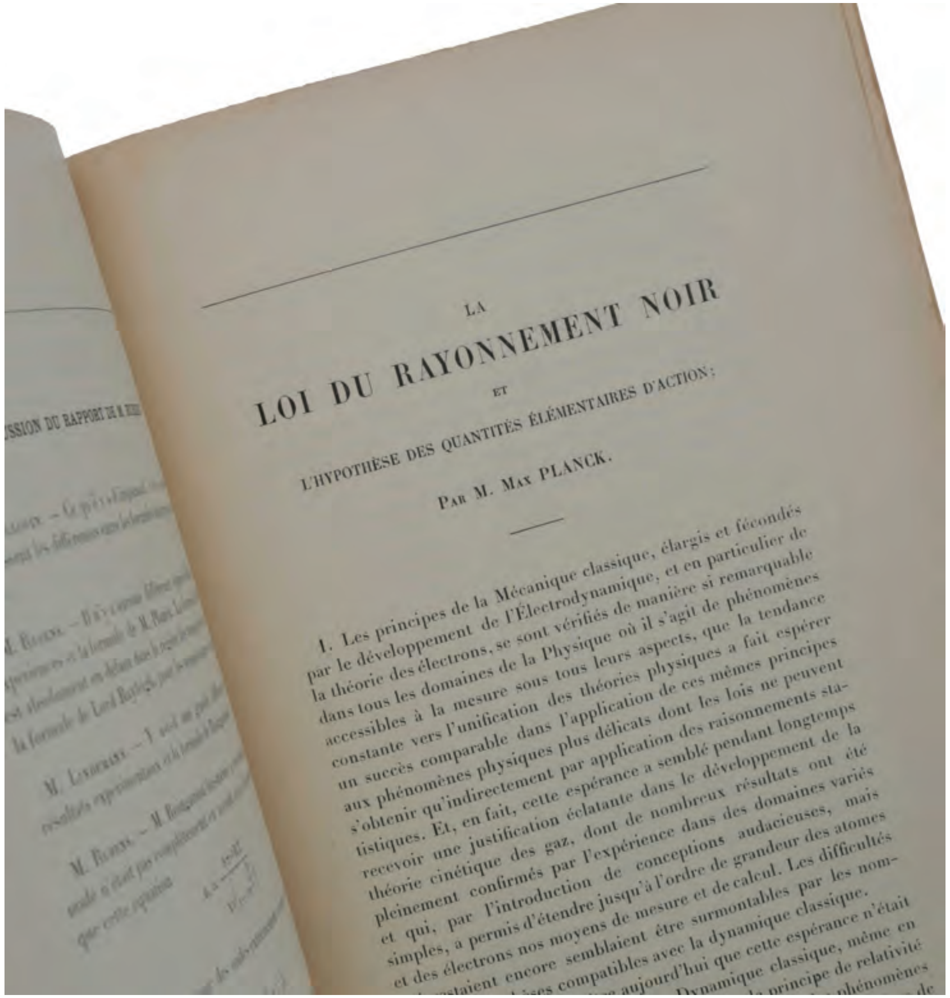
“Kammerlingh Onnes reported on the discovery of superconductivity of certain metals at very low temperatures. This phenomenon presented a great puzzle, and would find its explanation only several decades later …

“Paul Langevin reported on the variation of the magnetic properties of matter with temperature. He made special reference to the idea of the magneton, which had been introduced by Pierre Weiss to explain the remarkable numerical relations between the strength of the elementary magnetic moments of atoms deduced from the analysis of his measurements. Langevin showed that the value of the magneton could be approximately derived on the assumption that the electrons in atoms were rotating with angular momenta corresponding to a Planck quantum of action.

“Arnold Sommerfeld discussed the production of X-rays by high speed electrons as well as problems involving the ionization of atoms in the photoelectric effect and by electronic impact. Sommerfeld considered the existence of Planck’s quantum of action as fundamental for any approach to questions of the constitution of atoms and molecules …

“The last report of the conference was given by Albert Einstein. He summarized many applications of the quantum concept and dealt in particular with the fundamental arguments used in his explanation of the anomalies of specific heats at low temperatures” (Mehra, pp.14-15).

“Einstein’s talk was titled ‘The Present State of the Problem of Specific Heats.’ Specific heat – the quantity of energy required to increase the temperature of



a specific amount of substance by a certain amount – had been a specialty of Einstein's former professor and antagonist at the Zürich Polytechnic, Heinrich Weber. Weber had discovered some anomalies, especially at low temperatures, in the laws that were supposed to govern specific heat. Beginning in late 1906, Einstein had come up with what he called a 'quantized' approach to the problem by surmising that the atoms in each substance could absorb energy only in discrete packets.

"In his 1911 Solvay lecture, Einstein put these issues into the larger context of the so-called quantum problem. Was it possible, he asked, to avoid accepting the physical reality of these atomistic particles of light, which were like bullets aimed at the heart of Maxwell's equations and, indeed, all of classical physics?

"Planck, who had pioneered the concept of the quanta, continued to insist that they came into play only when light was being emitted or absorbed. They were not a real-world feature of light itself, he argued. Einstein, in his talk to the conference, sorrowfully demurred: 'These discontinuities, which we find so distasteful in Planck's theory, seem really to exist in nature' …

"When he was finished, Einstein faced a barrage of challenges from Lorentz, Planck, Poincaré, and others. Some of what Einstein said, Lorentz rose to point out, 'seems in fact to be totally incompatible with Maxwell's equations.' Einstein agreed, perhaps too readily, that 'the quantum hypothesis is provisional' and that it 'does not seem compatible with the experimentally verified conclusions of the wave theory.' Somehow it was necessary, he told his questioners, to accommodate both wave and particle approaches to the understanding of light. 'In addition to Maxwell's electrodynamics, which is essential to us, we must also admit a hypothesis such as that of quanta.'

"It was unclear, even to Einstein, whether Planck was persuaded of the reality of quanta. 'I largely succeeded in convincing Planck that my conception is correct, after he has struggled against it for so many years,' Einstein wrote his friend Heinrich Zangger. But a week later, Einstein gave Zangger another report: 'Planck stuck stubbornly to some undoubtedly wrong preconceptions'" (Isaacson, *Einstein*, pp. 169-170).

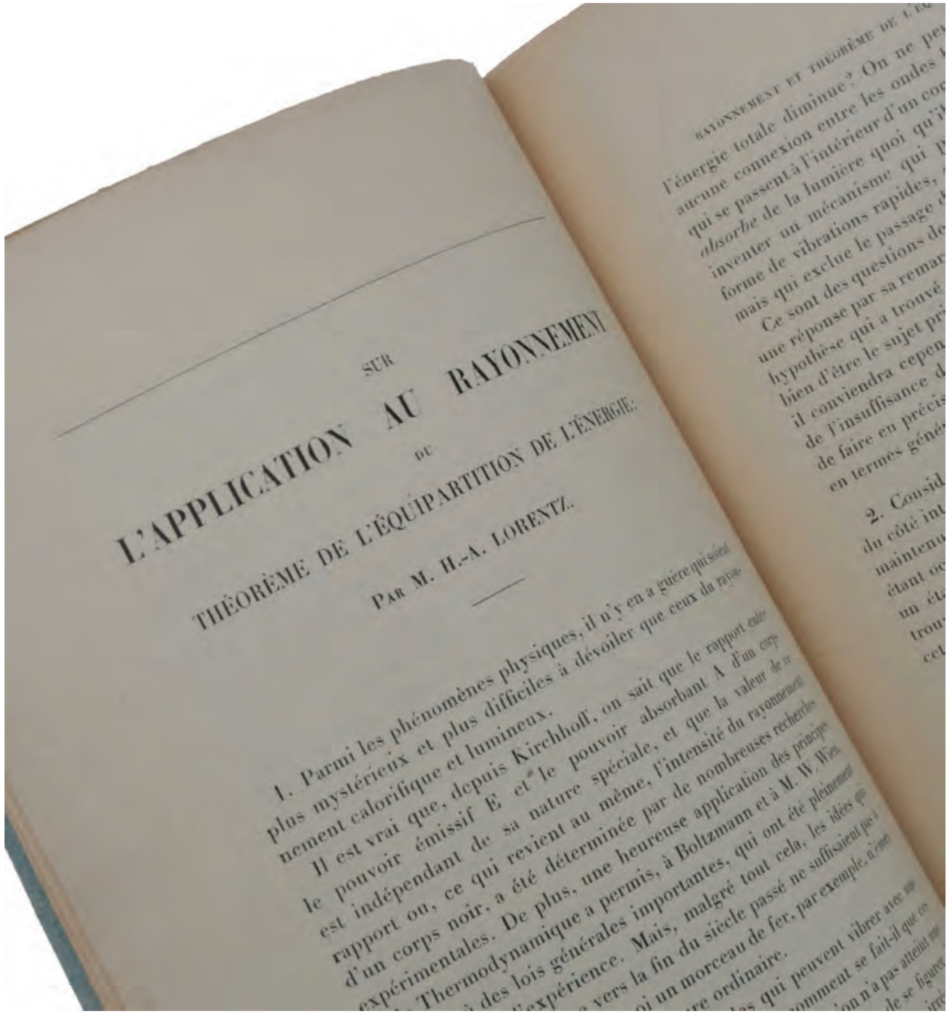
"The Conference helped considerably in establishing quantum theory, previously the occupation of a few scientists, as a field of serious and active research in many places and in many countries. The reports and discussions of the Solvay Conference also provided a great stimulus to those who had been involved in quantum problems for years. After all, it was the first time that a meeting of the leading experts in this field had taken place. They had been given the opportunity of meeting together personally, of presenting their respective arguments in detail, and of debating for several days all the fundamental as well as controversial aspects … However, the full impact of the Conference, also on Einstein, would show up only gradually. Then it would become evident that … the Solvay Conference had in fact achieved its primary goal: to bring the full extent of the quantum problem to the attention of the experts and to persuade them to cooperate more closely for the future growth of quantum theory" (Mehra & Rechenberg, p. 136).

The Solvay Congress had a major impact on Einstein's academic career. Shortly after the meetings, Mme. Curie wrote of 'the clarity of [Einstein's] mind, the vastness of his documentation, and the profundity of his knowledge.' Poincaré wrote that he was 'one of the most original thinkers I have ever met … What one has to admire in him above all is the facility with which he adapts himself to new concepts and knows how to draw from them every possible conclusion.' Lindemann later wrote, 'I well remember my co-secretary, M. de Broglie, saying that of all those present Einstein and Poincaré moved in a class by themselves.'

Einstein's move in 1912 from Prague to the ETH in Zürich was made easier by Mme. Curie's and Poincaré's strong recommendations, which were sent shortly after the conference. The next step, which brought him to Berlin, as a member of the Prussian Academy, took place in 1913, when the four Berliners (Nernst, Planck, Rubens & Warburg) from the Solvay Council signed the pivotal election proposal.

The Solvay conference became well known to the general public for another reason: just as it was getting under way, the romance between the widowed Marie Curie and Paul Langevin became public. Almost at the same moment it was announced that Madame Curie had won the Nobel Prize in chemistry. After the furore, Einstein wrote a gracious letter to her.

This first Solvay Conference was so successful that in the following year Solvay established a foundation, now known as the International Solvay Institutes for Physics and Chemistry, "to encourage the researches which would extend and deepen the knowledge of natural phenomena" (Mehra, p. xv) and to sponsor further conferences. The next two Solvay Conferences met in 1913 and 1921; subsequent conferences have been held every three years except during wartime.



AUTOGRAPH LECTURES ON PARTICLE PHYSICS

FEYNMAN, Richard Phillips. *Autograph manuscript, unsigned, entitled 'Talk at Vancouver New Particles etc.' Vancouver, Canada, 22 November 1975.*

$32,500

Four pages (280 x 216), in black ballpoint pen on four sheets of yellow lined paper, creases where previously folded.

A detailed draft for a talk given to the Canadian Association of Physics Students, in Vancouver, on the contemporary state of subatomic particle physics, its current difficulties and possible future developments. Although a 'popular' talk, it is pitched at a high level, appropriate to postgraduate students in physics. Feynman manuscripts with scientific content are very rare on the market – this is one of a small collection of such manuscripts that was retained by Feynman's family until 2018 when it was consigned to auction. Widely regarded as the most brilliant, influential, and iconoclastic figure in theoretical physics in the post-World War II era, Feynman shared the Nobel Prize in Physics 1965 with Sin-Itiro Tomonaga and Julian Schwinger "for their fundamental work in quantum electrodynamics, with deep-ploughing consequences for the physics of elementary particles." Feynman refers briefly to a talk given to students in Vancouver in *Surely You're Joking Mr. Feynman*! (p. 343): 'In Canada they have a big association of physics students. They have meetings; they give papers, and so on. One time the Vancouver chapter wanted to have me come and talk to them. The girl in charge of it arranged with my secretary to fly all the way to Los Angeles without telling me. She just walked into

my office. She was really cute, a beautiful blonde. (That helped; it's not supposed to, but it did.) And I was impressed that the students in Vancouver had financed the whole thing. They treated me so nicely in Vancouver that now I know the secret of how to really be entertained and give talks: Wait for the students to ask you.' In fact, Feynman gave more than one talk to the Vancouver students, the offered notes probably referring to the second such talk, for in a letter to Mariela Johansen of April 1975, he writes: 'I often remember vividly my most enjoyable trip. Many things bring it to mind – like the dark blue T-shirt in my drawer at home – or the interference picture in my office – or just now when my secretary asked me if I wanted to talk to students at a nearby university (USC 20 miles) or high school. My answer was that I will talk to students anytime they are near enough to home, or are at Vancouver, B.C.' (*The Quotable Feynman* (2015), p. 296). That these notes probably refer to the second Vancouver talk is confirmed by the first sentence on p. 1, in which Feynman writes: 'Difference from last time … last year …' The slides for this talk, which Feynman refers to at one point in these notes, are preserved in the Feynman archives at Caltech.

By 1975, what is now called the 'Standard Model' of particle physics was close to being established. It provides a 'unified' description of three of the four forces through which subatomic particles interact – the electromagnetic, weak and strong forces; the fourth force, gravity, has still not been unified with the other three. In this manuscript, Feynman summarizes the current understanding of the Standard Model in 1975, and discusses several significant 'loose ends'. Some of these were resolved in the years following his talk, while others are still open today.

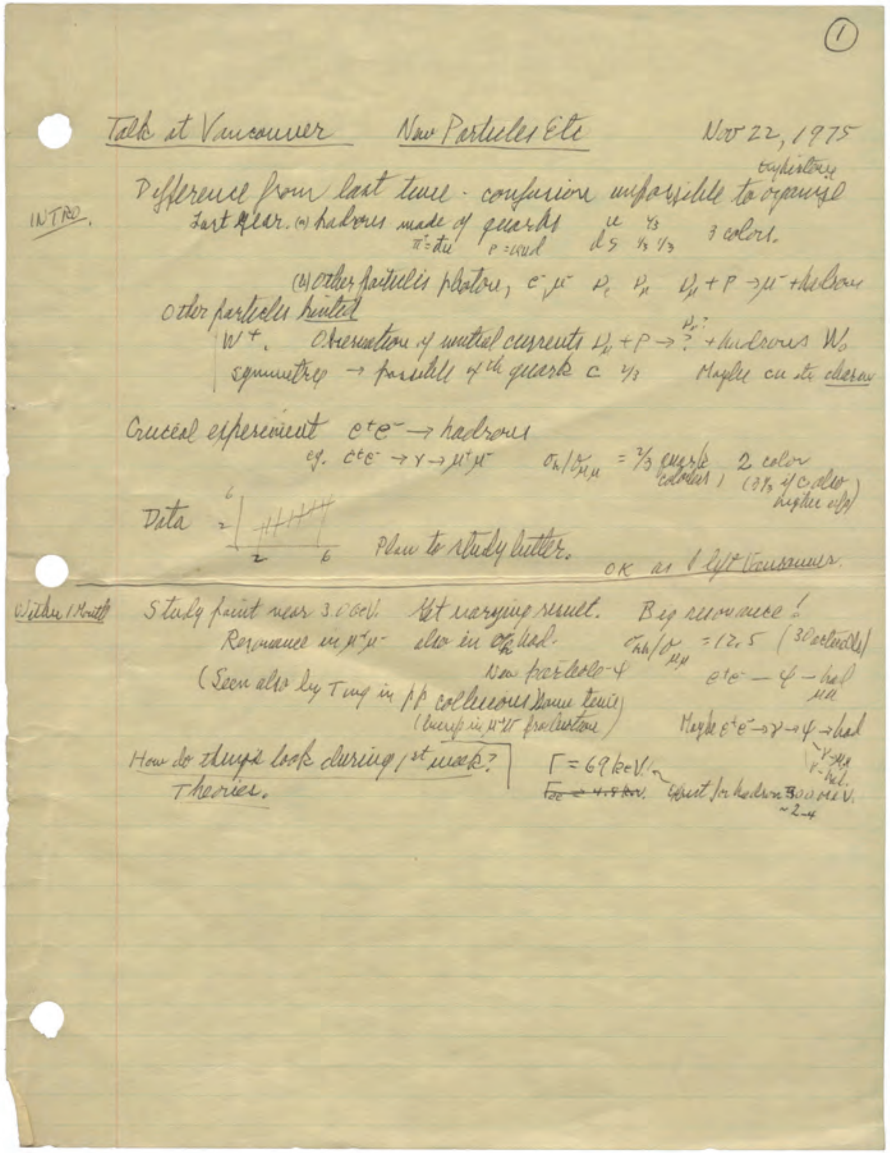
The first two of the three forces to be 'unified' were the weak and electromagnetic. In 1957 Julian Schwinger postulated that three different bosons (particles with whole number spin, that obey Bose-Einstein statistics) must be involved in transmitting the weak force to take account of all the possible different ways the nucleons can interact in the nucleus. Two of these bosons were required to exchange positive and negative charges, now called the W^+ and W^- (weak) bosons; a third neutral boson, the Z^0 (which Feynman calls the W^0) was required for reactions in which no charge was transferred. In 1973, 'neutral weak currents' (i.e., interactions between particles that involve the exchange of W^0 bosons) were observed at CERN, and the electroweak theory became widely accepted. However, the W^+, W^- and W^0 bosons themselves were not observed experimentally until 1983.

The theory of the strong force, called quantum chromodynamics (QCD), acquired its modern form in 1973-74. In 1964, Murray Gell-Mann and George Zweig (a student of Feynman) independently postulated that baryons (protons and neutrons) were composed of triplets of very small, strongly interacting, fundamental particles which Gell-Mann called 'quarks'. It was also predicted that mesons were similarly composed of these same fundamental particles but in the form of quark-antiquark pairs. The proposed quarks had very unusual properties in that their charge had fractional rather than integer values. At the time only three types (also known as flavours) of quarks were known: 'up, 'down' and 'strange' (u, d and s) with electric charges 2/3, -1/3, -1/3, respectively. The proton contains 2 up quarks and 1 down quark giving it a total charge of 1; the neutron contains 2 down quarks and 1 up quark giving it a total charge of 0; mesons could be composed of a variety of quark/antiquark pairs such as uu, dd, ud, du and others. Nobody has actually isolated or seen a single individual quark since they are permanently 'confined' within observable particles like the proton and neutron from which single quarks cannot escape due to the strong inter-quark (nuclear) force, which holds the particle together.

In 1964 Oscar Greenberg pointed out that having two identical quarks in the hadron's triplet of quarks violated Pauli's exclusion principle, a basic rule of quantum physics which does not allow a particle to contain more than one quark in the same quantum state. To overcome this problem he suggested that quarks should have three new degrees of freedom. In 1965 Greenberg's idea was taken up by Moo-Young Han and Yoichiro Nambu who introduced the notion of a quantum 'colour charge' with three possible values, red, green or blue; colours can also be positive or negative. Analogous to the electromagnetic force, like-coloured charges repel each other and different-coloured charges attract, but the three colour charges when combined result in a neutral charge. In 1968 evidence of the existence of quarks was confirmed by a team at Stanford Linear Accelerator Center (SLAC).

Although quarks are 'confined' inside hadrons, they can advertise their presence indirectly by generating jets of particles in high-energy collisions. For example, an electron and positron can annihilate each other creating a quark and antiquark pair. If the collision energy is high enough the quark and antiquark fly apart, degenerating into hadrons such as pions and kaons which are emitted as two 'jets' radiating outwards in the same plane from the collision point.

In 1974, Burton Richter from SLAC announced the results of his experiments with high-energy electron-positron collisions and, at the same meeting, Samuel Ting from Brookhaven National Laboratory announced the results of his own investigations into the interactions of high-energy protons on a beryllium target. By coincidence both experimenters had produced a stream of new particles with a resonance spike in the number of particles formed with an energy of 3.1 GeV giving the particles a mass of three times the mass of a proton. It turned out that they had independently produced the same new particle, now known as the 'J/ψ' particle. Investigations showed that this was a meson consisting of a charm quark and an anticharm quark, the first evidence of the existence of the charm, the


(1)

Talk at Vancouver New Particles Etc Nov 22, 1975

INTRO. Difference from last time · confusion impossible to organize

Last year. (a) hadrons made of quarks $\pi^+ = d\bar{u}$ $p = uud$ u d s 3 colors.

(b) other particles photon, e^- μ^- ν_e ν_μ $\nu_\mu + p \to \mu^- +$ hadrons

other particles hinted

W^+. Observation of neutral currents $\nu_\mu + p \to ? +$ hadrons W_0

symmetry → possible 4th quark c 2/3 Maybe in the [illegible]

Crucial experiment $e^+e^- \to$ hadrons

eg. $e^+e^- \to \gamma \to \mu^+\mu^-$ $\sigma_h/\sigma_{\mu\mu} = 2/3$ quarks 2 color

Data

Plan to study better.

OK as I left Vancouver.

Within 1 Month Study point near 3.0 GeV. Get varying result. Big resonance!

Resonance in $\mu^+\mu^-$ also in σ_h had. $\sigma_{hh}/\sigma_{\mu\mu} = 12.5$

New particle ψ $e^+e^- - \psi -$ had

(Seen also by Ting in pp collisions same time)

Maybe $e^+e^- \to \gamma \to \psi \to$ had

How do things look during 1st week? $\Gamma = 69$ keV

Theories.

fourth quark.

The discovery of the J/ψ (and thus the charm quark) ushered in a series of breakthroughs which are collectively known as the 'November Revolution,' as it triggered additional searches for unknown elementary particles, explorations that would reveal the final shape of the Standard Model.

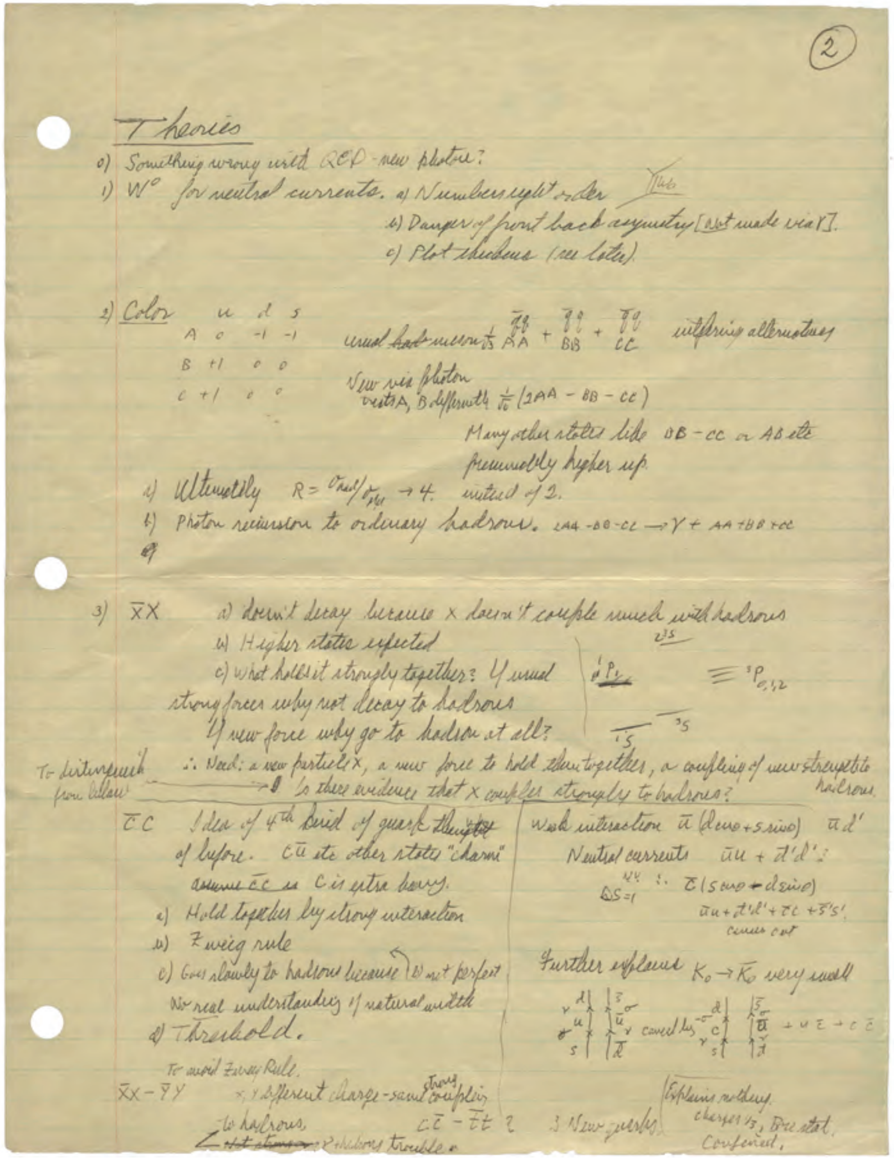
This was the state of knowledge of particle physics when Feynman stood up to give his talk in November 1975.

On page 1 of this manuscript, Feynman begins with a recap of the situation in particle physics when he gave his talk 'last year'. He recalls that hadrons are made of quarks, which come in three 'colours'; other known particles include the photon, electron, muon, pions, … The existence of other particles was hinted at: the W-bosons, and a possible fourth quark. Feynman mentions that the crucial experiment would involve the collision of high-energy electrons and positrons, producing hadrons. In 1976 John Ellis, Graham Ross and Mary Gaillard theorised that very high-energy electron-positron collisions would result in three co-planar jets of hadrons. In 1979 these 'three-jet' events were detected by the TASSO team working on the PETRA particle accelerator at the Deutsches Elektronen-Synchrotron (DESY) in Hamburg. They provided the first direct experimental evidence for the existence of gluons, the carriers of the strong nuclear force. In the last few lines of this page, Feynman refers to the experiments of Richter and Ting which led to the discovery of the fourth type of quark.

On page 2, Feynman discusses the theories of quarks and the colour force in more detail, and in particular the decay of hadrons into other hadrons. He mentions 'Zweig's rule', proposed in the 1960s, which states that when hadrons decay the constituent quarks have to survive (this means that some apparently possible decays of hadrons are actually forbidden).

On page 3, Feynman discusses how the J/ψ -particle decays into hadrons. The J/ψ is composed of a charm quark and a charm antiquark. The Zweig rule means that most modes of decay into hadrons are forbidden. This gives the J/ψ-particle a longer lifetime than would otherwise have been expected, and allows it to decay into photons, which then further decay into hadrons, or into electron-positron or muon-anti-muon pairs.

On page 4, Feynman speculates that there should be a new lepton ('heavy muon'). He was proved correct when, in 1975, Martin Lewis Perl and his team experimenting with high-energy electron-positron collisions at SLAC discovered the 'tau lepton', the most massive of the lepton family, having a mass about 3,490 times the mass of the electron. Feynman points out that there is still no explanation of quark confinement, or of Zweig's rule, and speculates that theories of weak interactions may lead to the existence of yet more new quarks and leptons. In this he was proved partially correct: in 1977 scientists at Fermilab discovered the fifth quark, the bottom quark, and in 1995 they discovered the sixth one, the top. To date, however, no further leptons beyond the electron, muon and tau, and their associated neutrinos and anti-particles, have been discovered.



(2)

Theories

0) Something wrong with QED - new photon?
1) W^0 for neutral currents. a) Numbers right order
b) Danger of front back asymmetry [not made via γ].
c) Plot theories (see later).

2) Color

	u	d	s
A	0	-1	-1
B	+1	0	0
C	+1	0	0

usual ~~had~~ meson $\frac{1}{\sqrt{3}}\left(\frac{\bar{q}q}{AA} + \frac{\bar{q}q}{BB} + \frac{\bar{q}q}{CC}\right)$ interfering alternatives

New via photon
treats A, B differently $\frac{1}{\sqrt{6}}(2AA - BB - CC)$

Many other states like BB - CC or AB etc
presumably higher up.

a) Ultimately $R = \sigma_{had}/\sigma_{\mu\mu} \to 4.$ instead of 2.
b) Photon reversion to ordinary hadrons. $2AA - BB - CC \to \gamma + AA + BB + CC$

3) $\bar{X}X$ a) doesn't decay because X doesn't couple much with hadrons
b) Higher states expected
c) What holds it strongly together? If usual strong forces why not decay to hadrons
If new force why go to hadron at all?

2^3S ; ${}^1P_1$; $\equiv {}^3P_{0,1,2}$; ${}^1S$; ${}^3S$

To distinguish from below: ∴ Need: a new particle X, a new force to hold them together, a coupling of new strength to hadrons.
Is there evidence that X couples strongly to hadrons?

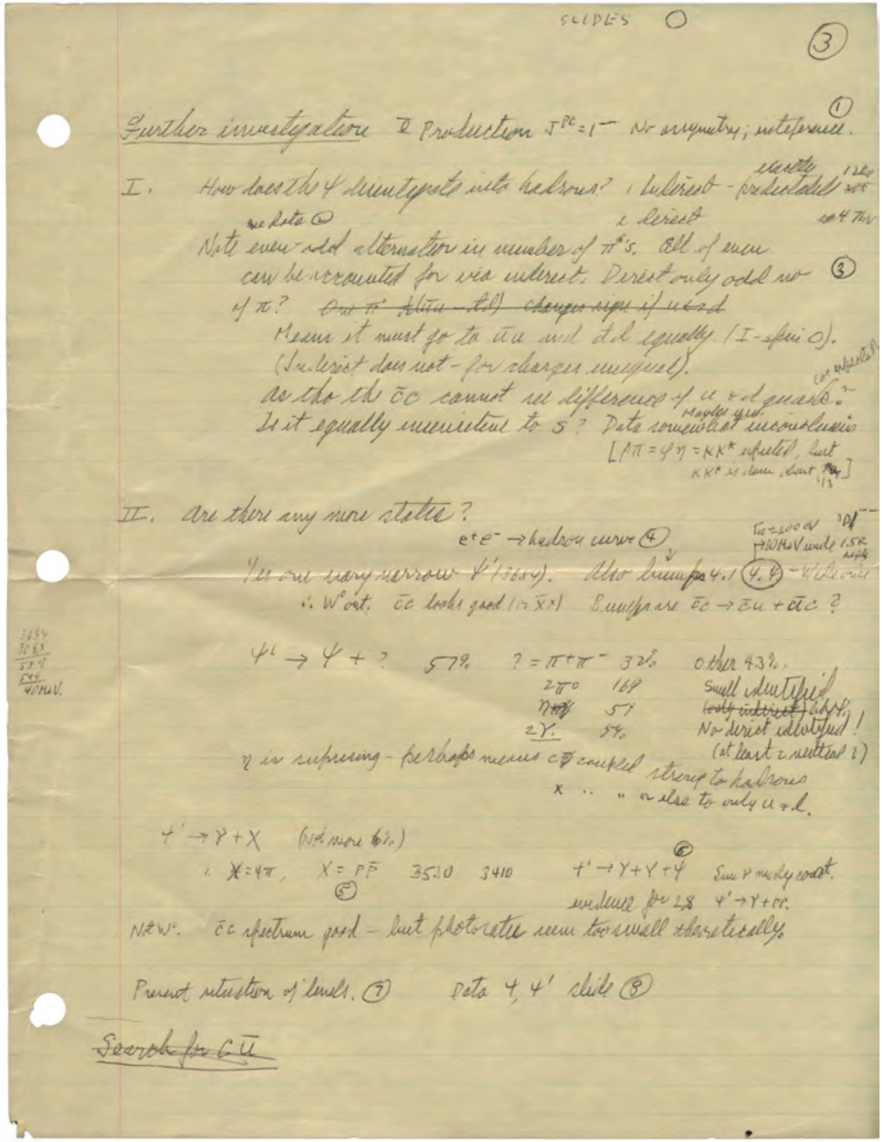
$\bar{c}c$ Idea of 4th kind of quark thought of before. $c\bar{u}$ etc other states "charm"
assume $\bar{c}c$ so c is extra heavy.
a) Hold together by strong interaction
b) Zweig rule
c) Goes slowly to hadrons because b) not perfect
No real understanding of natural width
d) Threshold.

Weak interaction $\bar{u}$ (d cos + s sin) $\bar{u}d'$
Neutral currents $\bar{u}u + \bar{d}'d'$:
$\Delta S = 1$ ∴ $\bar{c}$(s cos - d sin)
$\bar{u}u + \bar{d}'d' + \bar{c}c + \bar{s}'s'$ cancels out

Further explains $K_0 \to \bar{K}_0$ very small
cancelled by ... $+ u\bar{c} \to c\bar{c}$

To avoid Zweig Rule.
$\bar{X}X - \bar{Y}Y$ x, y different charge - same strong coupling to hadrons
$c\bar{c} - \bar{t}t$?
~~Not strong~~ ? hadrons trouble.

3 New quarks. Explains nothing. charges 1/3, Bose stat. Confined.



SLIDES 0

(3)

Further investigation Ψ Production $J^{PC} = 1^-$ No asymmetry; interference. (1)

I. How does the Ψ disintegrate into hadrons? 1 Indirect - predictable exactly
2 direct

see data (2)
Note even-odd alternation in number of π's. All of even can be accounted for via indirect. Direct only odd no of π? ~~One π ... changes sign if used~~ (3)
Means it must go to $\bar{u}u$ and $\bar{d}d$ equally. (I-spin 0).
(Indirect does not - for charges unequal).
As tho the $\bar{c}c$ cannot see difference of u & d quarks? (as expected!)
Is it equally insensitive to s? Data somewhat inconclusive. Maybe yes.
[$\rho\pi = \varphi\eta = KK^*$ expected, but KK^* is down about 1/3]

II. Are there any more states?
$e^+e^- \to$ hadron curve (4)
Yes one very narrow Ψ'(3684). Also bumps 4.1 (4.4) - Wide ones
∴ W^0 out. $\bar{c}c$ looks good ($\bar{X}X$) Bumps are $\bar{c}c \to \bar{c}u + c\bar{u}$?

3684 - 3095 = 589 ; 549 ; 40 MeV

$\Psi' \to \Psi + ?$ 57%. $? = \pi^+\pi^-$ 32% ; $2\pi^0$ 16% ; η 5% ; 2γ 5%. other 43%.
Small identified ~~(only indirect)~~ ... No direct identified! (at least 2 neutral ?)
η is surprising - perhaps means $c\bar{c}$ coupled strong to hadrons
X " " or else to only u & d.

$\Psi' \to \gamma + X$ (Not more 6%)
∴ ~~X~~ = 4π, $X = p\bar{p}$ (5) 3530 3410
$\Psi' \to \gamma + \gamma + \Psi$ (6) Since γ nearly const. evidence for 2S $\Psi' \to \gamma + ...$

Not W^0. $\bar{c}c$ spectrum good - but photorates seem too small theoretically.

Present situation of levels. (7) Data Ψ, Ψ' slide (8)

~~Search for $c\bar{u}$~~

SIGNED OFFPRINT OF FEYNMAN'S THESIS

FEYNMAN, Richard Phillips. *Forces in molecules. Offprint from: Physical Review, Second Series, Vol. 56, No. 4, August 15, 1939.* [Lancaster, PA & New York, NY: American Institute of Physics for the American Physical Society, 1939].

$42,500

Large 8vo (267 x 200 mm), pp. 340-343. Self-wrappers as issued, fine. Custom calf box with gilt spine lettering.

First edition, extremely rare offprint, **inscribed by Feynman**, of Feynman's senior undergraduate thesis at MIT, a fundamental discovery "that has played an important role in theoretical chemistry and condensed matter physics" (*Selected Papers*, p. 1), published when he was just twenty-one. This is a remarkable paper, documenting the first steps in original research of one of the most brilliant minds of the twentieth century. "Feynman was one of the most creative and influential physicists of the twentieth century. A veteran of the Manhattan Project of World War II and a 1965 Nobel laureate in physics, he made lasting contributions across many domains, from electrodynamics and quantum theory to nuclear and particle physics, solid-state physics, and gravitation" (DSB). Feynman showed that "the force on an atom's nucleus is no more or less than the electrical force from the surrounding field of charged electrons – the electrostatic force. Once the distribution of charge has been calculated quantum mechanically, then from that point forward quantum mechanics disappears from the picture. The problem becomes classical; the nuclei can be treated as static points of mass and

R. P. Feynman

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Forces in Molecules

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Formulas have been developed to calculate the forces in a molecular system directly, rather than indirectly through the agency of energy. This permits an independent calculation of the slope of the curves of energy *vs.* position of the nuclei, and may thus increase the accuracy, or decrease the labor involved in the calculation of these curves. The force on a nucleus in an atomic system is shown to be just the classical electrostatic force that would be exerted on this nucleus by other nuclei and by the electrons' charge distribution. Qualitative implications of this are discussed.

MANY of the problems of molecular structure are concerned essentially with forces. The stiffness of valence bonds, the distortions in geometry due to the various repulsions and attractions between atoms, the tendency of valence bonds to occur at certain definite angles with each other, are some examples of the kind of problem in which the idea of force is paramount.

Usually these problems have been considered through the agency of energy, and its changes with changing configuration of the molecule. The reason for this indirect attack through energy, rather than the more qualitatively illuminating one, by considerations of force, is perhaps twofold. First it is probably thought that force is a quantity that is not easily described or calculated by wave mechanics, while energy is, and second, the first molecular problem to be solved is the analysis of band spectra, strictly a problem of energy as such. It is the purpose of this paper to show that forces are almost as easy to calculate as energies are, and that the equations are quite as easy to interpret. In fact, all forces on atomic nuclei in a molecule can be considered as purely classical attractions involving Coulomb's law. The electron cloud distribution is prevented from collapsing by obeying Schrödinger's equation. In these considerations the nuclei are considered as mass points held fixed in position.

A usual method of calculating interatomic forces runs somewhat as follows.

For a given, fixed configuration of the nuclei, the energy of the entire system (electrons and nuclei) is calculated. This is done by the variation method or other perturbation schemes. This entire process is repeated for a new nuclear position, and the new value of energy calculated. Proceeding in this way, a plot of energy *vs.* position is obtained. The force on a nucleus is of course the slope of this curve.

The following method is one designed to obtain the forces at a given configuration, when only the configuration is known. It does not require the calculations at neighboring configurations. That is, it permits a calculation of the slope of the energy curve as well as its value, for any particular configuration. It is to be emphasized that this allows a considerable saving of labor of calculations. To obtain force under the usual scheme the energy needs to be calculated for two or more different and neighboring configurations. Each point requires the calculation of the wave functions for the entire system. In this new method, only one configuration, the one in question, need have its wave functions computed in detail. Thus the labor is considerably reduced. Because it permits one to get an independent value of the slope of the energy curve, the method might increase the accuracy in the calculation of these curves, being especially helpful in locating the normal separation, or position of zero force.

In the following it is to be understood that the nuclei of the atoms in the molecule, or other atomic system, are to be held fixed in position, as point charges, and the force required to be applied to the nuclei to hold them is to be calculated. This will lead to two possible definitions of force in the nonsteady state, for then the energy is not a definite quantity, and the slope of the energy curve shares this indefinite-

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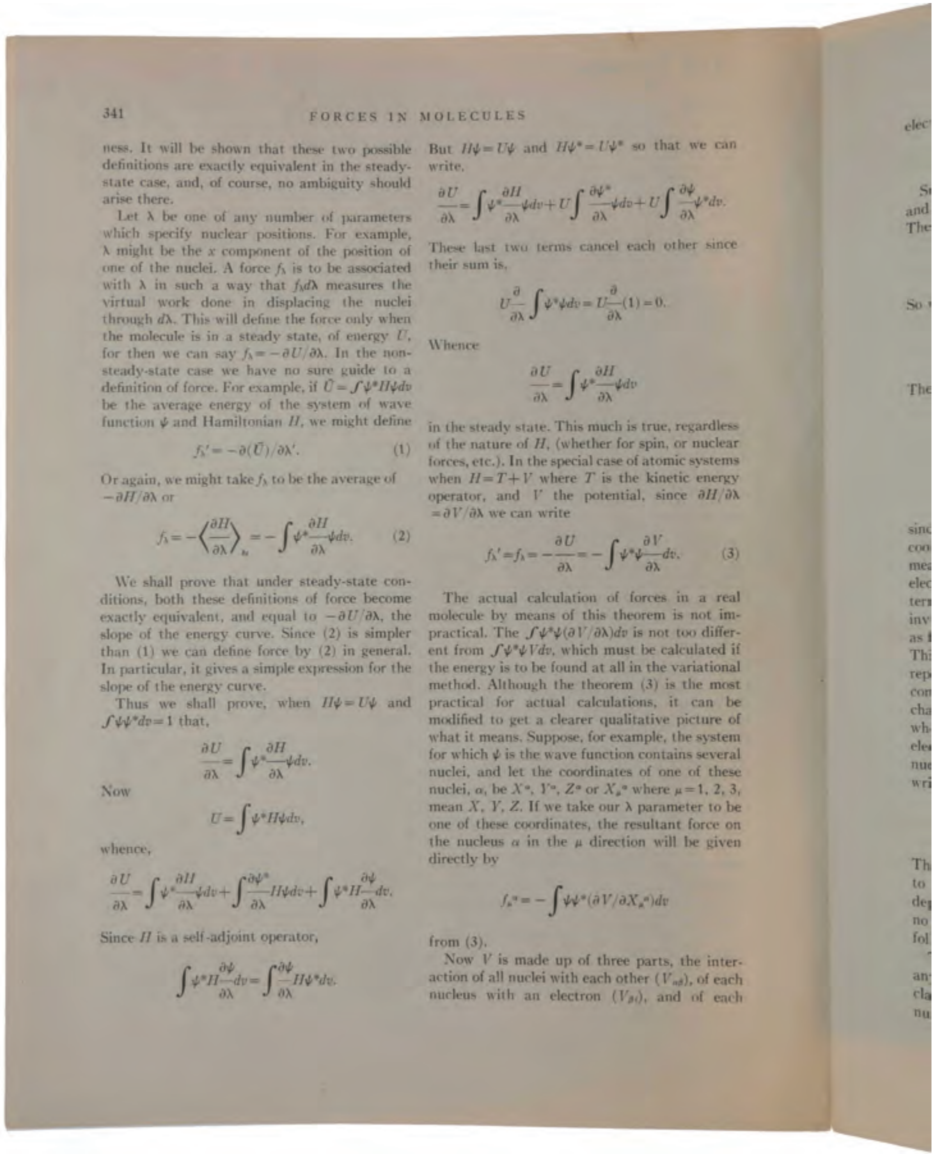
charge. Feynman's approach applies to all chemical bonds. If two nuclei act as though strongly attracted to each other, as the hydrogen nuclei do when they bond to form a water molecule, it is because the nuclei are each drawn toward the electrical charge concentrated quantum mechanically between them" (Gleick, *Genius*: *The Life and Science of Richard Feynman*). His discovery, now known as Feynman's theorem or the Feynman-Hellmann theorem, has endured as an efficient approach to the calculation of forces in molecules. "The importance of the forces on the atomic nuclei for molecular geometry, the theory of chemical binding, and for crystal structure is evident" (*Selected Papers*, p. 1). ABPC/RBH lists no copy of any offprint of any of Feynman's papers in *Physical Review* (where he published almost all of his most important work). Not on OCLC.

Provenance: Signed 'R. P. Feynman' in pencil in top margin of first page. This offprint was signed by Feynman and given by him to Robert Kinsel Smith (1920-99), a classmate and personal friend of Feynman's at Princeton University, where both Feynman and Kinsel Smith studied for their PhDs (a letter from Kinsel Smith's son testifying to this provenance accompanies the offprint).

Born in Far Rockaway in the Queens section of New York City, Feynman (1918-88) entered the Massachusetts Institute of Technology (MIT) in 1935 to begin his undergraduate studies. Although he originally majored in mathematics, he later switched to electrical engineering, as he considered mathematics to be too abstract. Noticing that he 'had gone too far,' he then switched to physics, which he claimed was 'somewhere in between.' To complete their bachelor's degree, all physics majors at MIT were then (as now) required to write a 'senior thesis'. "Thirteen physics majors completed senior theses in 1939. The world of accumulated knowledge was still small enough that MIT could expect a thesis to represent original and possibly publishable work. The thesis should begin the scientist's normal career and meanwhile supply missing blocks in the wall of organized knowledge, by analyzing such minutiae as the spectra of singly ionized gadolinium or hydrated manganese chloride crystals … Seniors could devise new laboratory instruments or investigate crystals that produced electrical currents when squeezed. Feynman's thesis began as a circumscribed problem like these. It ended as a fundamental discovery about the forces acting within the molecules of any substance" (Gleick).

Feynman recounted his work on the thesis in an interview with Charles Weiner in March, 1966. "I went to Slater [the renowned solid-state theorist John Clarke Slater (1900-76)], and he gave me a problem, which was … why does quartz have such a small coefficient of expansion? He thought that maybe the possibility was that the quartz crystal has moveable — see it's silicone dioxide, SiO_2 so I think there are oxygen's clinging to silicones, and in the motion the oxygen can swing back and forth, and it's a bent angle, turning back and forth, like the bores on the governor of an old steam engine, and when it turns — when this is oscillating, it's the same idea — it pulls the heads of the steam engine together, the ends, because the bore goes out by centrifugal force. And so the bent bottom will be shortened — I mean, it will be pulled together by the motion — and this will compensate the ordinary effects which tend to make something expand, so that the expansion will be much less than usual. Can I work out any details or estimates or something to show that in fact that's the reason that quartz doesn't expand? All right, that was the problem. I was very interested in it. The first thing I did was, I looked up the forms, crystobalite A, crystobalite B, crystal forms, and so on, to get the idea of the bonds and the angles and so on. I got in the crystal business. Then I realized I'd have to figure out how a change in forces will change the dimensions of the crystal. So then I got involved … with the connection between the forces between the atoms, and the forces — all together. For example, if a crystal is compressed, what is the compressent strength? Supposing I assume certain spring constants between all the atoms and I want to know what the elastic constants of the whole crystal are. I realized that what I had to do there was an infinite bridge truss problem, like the guys in applied engineering with bridges with a lot of members. I had an infinite number of members. But, because of the periodicity, I had an advantage that I could work out. Then I gradually developed the theory of the connection between the elastic bonds … So I worked that out, and then discovered by fooling around that I could get it for a principle of energy minimum … But anyway, in the meantime I'd found this theorem about the forces, but the force on the nucleus is nothing but the electrostatic attraction of all the electrons, the distribution of the electrons being determined by the Schrödinger equation. Slater found this interesting and unusual, and hadn't known that. He challenged me, and said, 'How do you explain van der Waals forces that way?' So I went back and I proved that the van der Waals force could also be understood that way. Then he said it was worthwhile, I ought to publish it. So I wrote it up. And he said, 'Let Conyers Herring look at it' [Conyers Herring was then a postdoctoral fellow under Slater]. He was good at these things. Herring looked at it, and there was a long session. He said, 'Take this out.' I had the proof that the integral of F times HG, where H is the Hamiltonian, was the same as HF star times G, integrated. And I proved it with the Schrödinger equation … Herring says to me, 'Take that out, and write instead such and such equals such and such because H is Hermitian self-adjoining [i.e., self-adjoint].' I said, 'What does that mean?' He said, 'That's what it means. It means that equation is true.' I'm just saying this to show the level of what I knew. See, I proved everything by hand — I didn't go into the general. So he took that out, and so on, and then I proved this theorem, and it was in this paper" (*AIP Oral History Interviews, Richard Feynman – Session II*, https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5020-2).

Mehra gives the following account of the theorem Feynman is referring to. "The primary motivation for the thesis appears to stem from the following intuitively appealing theorem, which Feynman enunciated and proved:



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ness. It will be shown that these two possible definitions are exactly equivalent in the steady-state case, and, of course, no ambiguity should arise there.

Let λ be one of any number of parameters which specify nuclear positions. For example, λ might be the x component of the position of one of the nuclei. A force f_λ is to be associated with λ in such a way that $f_\lambda d\lambda$ measures the virtual work done in displacing the nuclei through $d\lambda$. This will define the force only when the molecule is in a steady state, of energy U, for then we can say $f_\lambda = -\partial U/\partial\lambda$. In the non-steady-state case we have no sure guide to a definition of force. For example, if $\bar{U} = \int \psi^* H\psi dv$ be the average energy of the system of wave function ψ and Hamiltonian H, we might define

$$f_\lambda' = -\partial(\bar{U})/\partial\lambda'. \quad (1)$$

Or again, we might take f_λ to be the average of $-\partial H/\partial\lambda$ or

$$f_\lambda = -\left\langle \frac{\partial H}{\partial\lambda} \right\rangle_{av} = -\int \psi^* \frac{\partial H}{\partial\lambda} \psi dv. \quad (2)$$

We shall prove that under steady-state conditions, both these definitions of force become exactly equivalent, and equal to $-\partial U/\partial\lambda$, the slope of the energy curve. Since (2) is simpler than (1) we can define force by (2) in general. In particular, it gives a simple expression for the slope of the energy curve.

Thus we shall prove, when $H\psi = U\psi$ and $\int \psi\psi^* dv = 1$ that,

$$\frac{\partial U}{\partial\lambda} = \int \psi^* \frac{\partial H}{\partial\lambda} \psi dv.$$

Now

$$U = \int \psi^* H\psi dv,$$

whence,

$$\frac{\partial U}{\partial\lambda} = \int \psi^* \frac{\partial H}{\partial\lambda} \psi dv + \int \frac{\partial\psi^*}{\partial\lambda} H\psi dv + \int \psi^* H \frac{\partial\psi}{\partial\lambda} dv.$$

Since H is a self-adjoint operator,

$$\int \psi^* H \frac{\partial\psi}{\partial\lambda} dv = \int \frac{\partial\psi}{\partial\lambda} H\psi^* dv.$$

But $H\psi = U\psi$ and $H\psi^* = U\psi^*$ so that we can write,

$$\frac{\partial U}{\partial\lambda} = \int \psi^* \frac{\partial H}{\partial\lambda} \psi dv + U\int \frac{\partial\psi^*}{\partial\lambda} \psi dv + U\int \frac{\partial\psi}{\partial\lambda} \psi^* dv.$$

These last two terms cancel each other since their sum is,

$$U\frac{\partial}{\partial\lambda}\int \psi^*\psi dv = U\frac{\partial}{\partial\lambda}(1) = 0.$$

Whence

$$\frac{\partial U}{\partial\lambda} = \int \psi^* \frac{\partial H}{\partial\lambda} \psi dv$$

in the steady state. This much is true, regardless of the nature of H, (whether for spin, or nuclear forces, etc.). In the special case of atomic systems when $H = T + V$ where T is the kinetic energy operator, and V the potential, since $\partial H/\partial\lambda = \partial V/\partial\lambda$ we can write

$$f_\lambda' = f_\lambda = -\frac{\partial U}{\partial\lambda} = -\int \psi^*\psi \frac{\partial V}{\partial\lambda} dv. \quad (3)$$

The actual calculation of forces in a real molecule by means of this theorem is not impractical. The $\int \psi^*\psi(\partial V/\partial\lambda)dv$ is not too different from $\int \psi^*\psi V dv$, which must be calculated if the energy is to be found at all in the variational method. Although the theorem (3) is the most practical for actual calculations, it can be modified to get a clearer qualitative picture of what it means. Suppose, for example, the system for which ψ is the wave function contains several nuclei, and let the coordinates of one of these nuclei, α, be X^α, Y^α, Z^α or X_μ^α where $\mu = 1, 2, 3$, mean X, Y, Z. If we take our λ parameter to be one of these coordinates, the resultant force on the nucleus α in the μ direction will be given directly by

$$f_\mu^\alpha = -\int \psi\psi^*(\partial V/\partial X_\mu^\alpha)dv$$

from (3).

Now V is made up of three parts, the interaction of all nuclei with each other ($V_{\alpha\beta}$), of each nucleus with an electron ($V_{\beta i}$), and of each

'The force on any nucleus (considered fixed) in any system of nuclei and electrons is just the classical electrostatic attraction exerted on the nucleus in question by other nuclei and by the electron charge distribution' (*) …

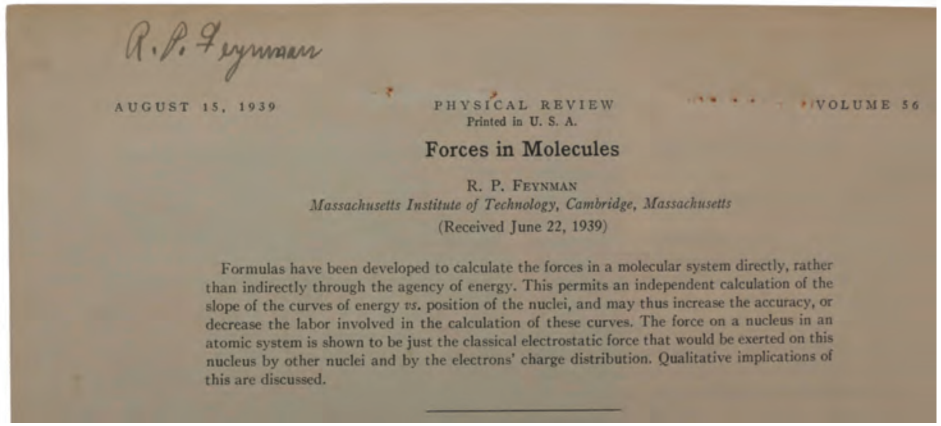
"Feynman's theorem (*) opened up a new and more revealing picture of the physical mechanism of the van der Waals force. The same may be said of the physical mechanism of the covalent or homopolar chemical bond, in which connection Feynman's theorem is held in considerable esteem among the physical chemists. In order to understand why this should be so, it is only necessary to observe that the older textbooks treating the bond tended to display the bonding effect as a somewhat mysterious quantum mechanical concomitant of the exchange antisymmetry of the overall wave function and the pairing of electron spins, whereas Feynman focused attention solely on the charge distribution engendered by the spatial factor of the wave function and the chemically desirable Coulomb forces exerted by this charge distribution.

"Feynman's theorem (*) is often referred to in the literature as the 'Hellman-Feynman theorem.' This terminology may be inexact, in so far as Hellman [a German physical chemist] did not envisage the Feynman force theorem (*)" (Mehra, *The Beat of a Different Drum*, pp. 72-78).

The thesis carried the title 'Forces and stresses in molecules.' "Slater made him rewrite the first version. He complained that Feynman wrote the way he talked, hardly an acceptable style for a scientific paper. Then he advised him to submit a shortened version for publication. The *Physical Review* accepted it, with the title shortened as well, to 'Forces in Molecules'" (Gleick).

"Many years later, in summer 1949, Feynman went to give lectures on quantum electrodynamics at the University of Michigan summer school at Ann Arbor. There, 'Ted Berlin, who was in physical chemistry, said, 'Have you heard all the debates about your Feynman-Hellman theorem?' I said, 'What theorem?' He said, 'It's called the Feynman-Hellman theorem, the force law between molecules.' Everyone was arguing it across the ocean. The Americans were saying I was right and the Germans were saying that I had made two mistakes: I left out the kinetic energy and something else. But anyway, they thought that things cancelled out and I was just lucky. But my proof was really sound, and it wasn't just a matter of luck. There was no mistake'" (Mehra, p. 78).

In the published paper 'Forces in Molecules,' Feynman cited no references, but he expressed gratitude to Slater and Conyers Herring. The paper has been cited almost 2000 times.


R. P. Feynman

AUGUST 15, 1939 PHYSICAL REVIEW VOLUME 56
Printed in U. S. A.

Forces in Molecules

R. P. FEYNMAN
Massachusetts Institute of Technology, Cambridge, Massachusetts
(Received June 22, 1939)

Formulas have been developed to calculate the forces in a molecular system directly, rather than indirectly through the agency of energy. This permits an independent calculation of the slope of the curves of energy *vs.* position of the nuclei, and may thus increase the accuracy, or decrease the labor involved in the calculation of these curves. The force on a nucleus in an atomic system is shown to be just the classical electrostatic force that would be exerted on this nucleus by other nuclei and by the electrons' charge distribution. Qualitative implications of this are discussed.

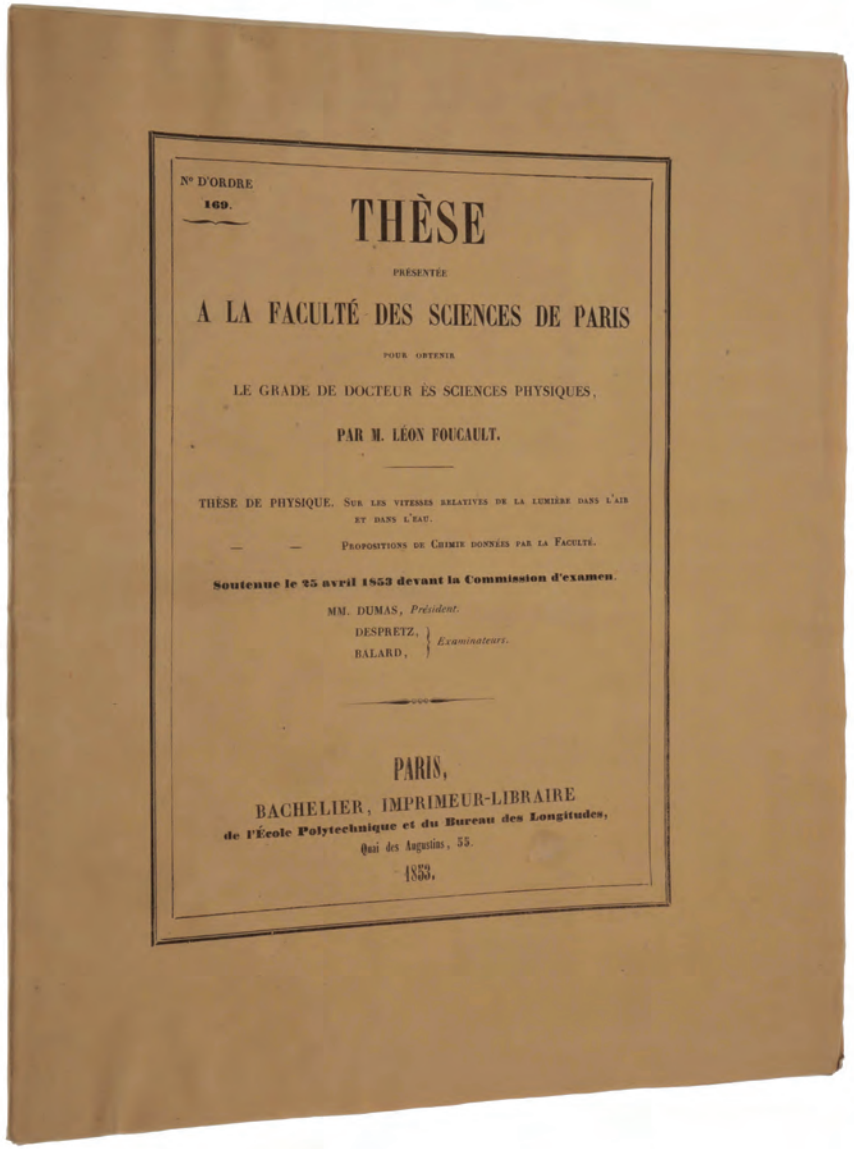
FOUCAULT'S THESIS - PROVING THE WAVE THEORY OF LIGHT

FOUCAULT, Jean Bernard Léon. *Thèse présentée à la faculté des sciences de Paris... Sur les vitesses rélatives de la lumière dans l'air et dans l'eau.* Paris: Bachelier, 1853.

$28,500

4to (282 x 230 mm), pp. [iv], 35, [1], with one large folding engraved plate. Original printed wrappers, unopened. Marginal corrections to text on pages 3 and 5 (in the author's hand?). Very rare in such fine condition.

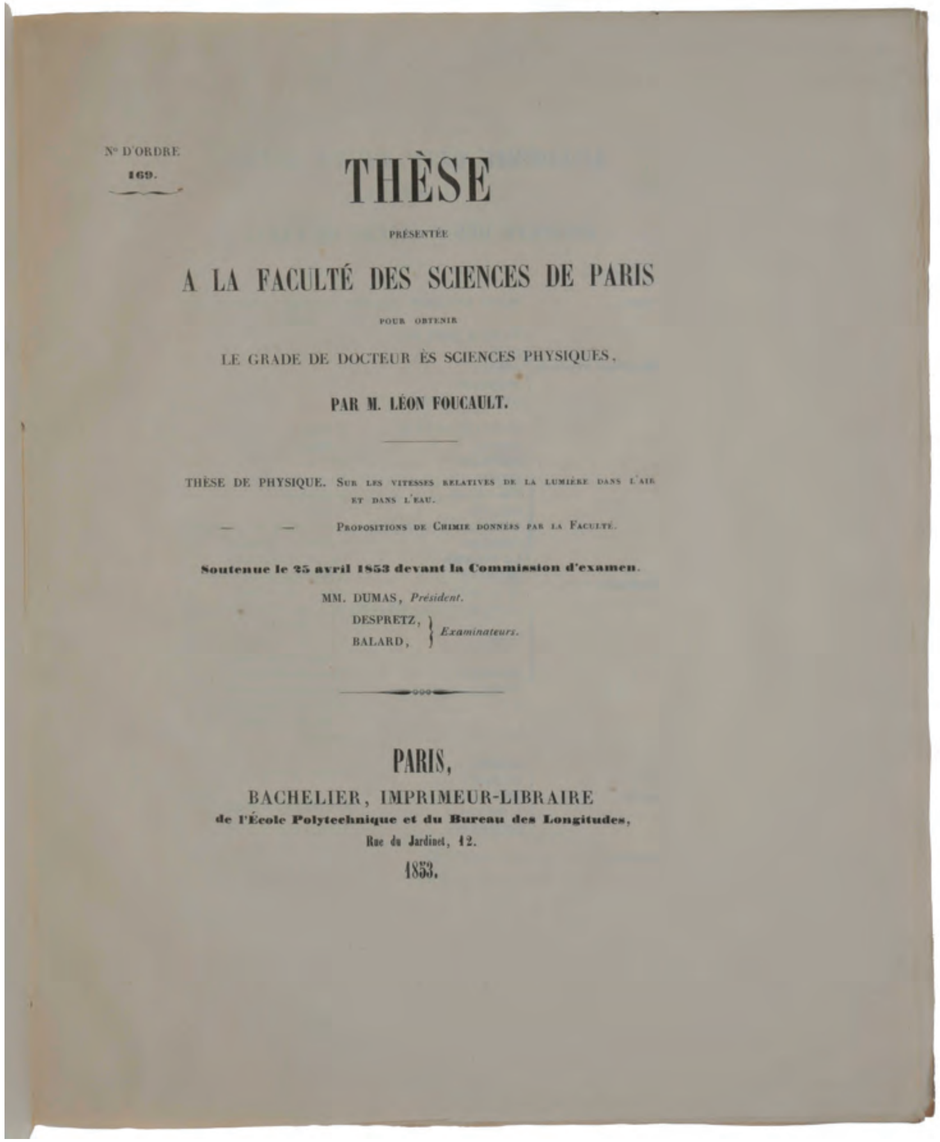
First edition, rare, and an exceptionally fine copy, of Foucault's doctoral thesis on the speed of light, in which he provides a convincing proof for the wave theory of light. In the 1840s Foucault undertook a series of optical experiments using an apparatus of rotating mirrors to determine the velocity of light. Originally developed by Charles Wheatstone to measure the velocity of electricity, the rotating mirror apparatus had been proposed as an instrument for the measurement of light in 1838 by Dominique-François Arago who failed in his own attempts to carry out the experiment. Foucault's initial work was carried out in conjunction with the physicist Armand Hippolyte Louis Fizeau (1819-1896); but a personal dispute broke up their partnership in 1847 and the two collaborators became rivals, working separately on the same problem using the same technique. Both reached the same conclusion, but while Fizeau was the first to obtain, in 1849, a precision measurement of the velocity of light, Foucault pre-empted him in announcing, on 30 April 1850, that light travels faster in air than in water, a decisive argument



in favour of the wave theory of light, which by the mid-nineteenth century had become generally accepted. In his thesis Foucault gives a detailed account of his experiment, illustrating his apparatus; it was not until 1862 that he was able to determine a numerical value for the speed of light, of about 298,000 kilometers per second, a figure significantly smaller, and more accurate, than Fizeau's. Foucault is today best known for the pendulum experiments demonstrating the rotation of the earth which he performed in 1851. Perhaps he considered these experiments to be unsuitable as a thesis topic as the result (the rotation of the earth) was well known to everyone, whereas the results of his air-and-water experiments, though expected by most scientists, were new. ABPC/RBH list five copies in the last 40 years: Christie's 2008, $17,395; Christie's, Paris 2004, €9000; Christie's 2004, $8,812; Christie's 1999, $10,350; Christie's 1998, $7,475.

"The early-to-mid 1800s were a period of intense debate on the particle-versus-wave nature of light. Although the observation of the Arago spot in 1819 may seem to have settled the matter definitively in favor of Fresnel's wave theory of light, various concerns continued to appear to be addressed more satisfactorily by Newton's corpuscular theory …

"In 1834, Charles Wheatstone developed a method of using a rapidly rotating mirror to study transient phenomena, and applied this method to measure the velocity of electricity in a wire and the duration of an electric spark ['An Account of Some Experiments to Measure the Velocity of Electricity and the Duration of Electric Light,' Philosophical Transactions of the Royal Society of London, vol. 124, pp. 583–591]. He communicated to François Arago the idea that his method could be adapted to a study of the speed of light. Arago expanded upon Wheatstone's concept in an 1838 publication ['Sur un système d'expériences à l'aide duquel la théorie de l'émission et celle des ondes seront soumises à des épreuves décisives,' *Comptes rendus hebdomadaires des séances de l'Académie des*


Nº D'ORDRE
169.

THÈSE

PRÉSENTÉE

A LA FACULTÉ DES SCIENCES DE PARIS

POUR OBTENIR

LE GRADE DE DOCTEUR ÈS SCIENCES PHYSIQUES,

PAR M. LÉON FOUCAULT.

THÈSE DE PHYSIQUE. Sur les vitesses relatives de la lumière dans l'air et dans l'eau.

— — Propositions de Chimie données par la Faculté.

Soutenue le 25 avril 1853 devant la Commission d'examen.

MM. DUMAS, *Président.*
DESPRETZ, }
BALARD, } *Examinateurs.*

PARIS,

BACHELIER, IMPRIMEUR-LIBRAIRE
de l'École Polytechnique et du Bureau des Longitudes,
Rue du Jardinet, 12.

1853.

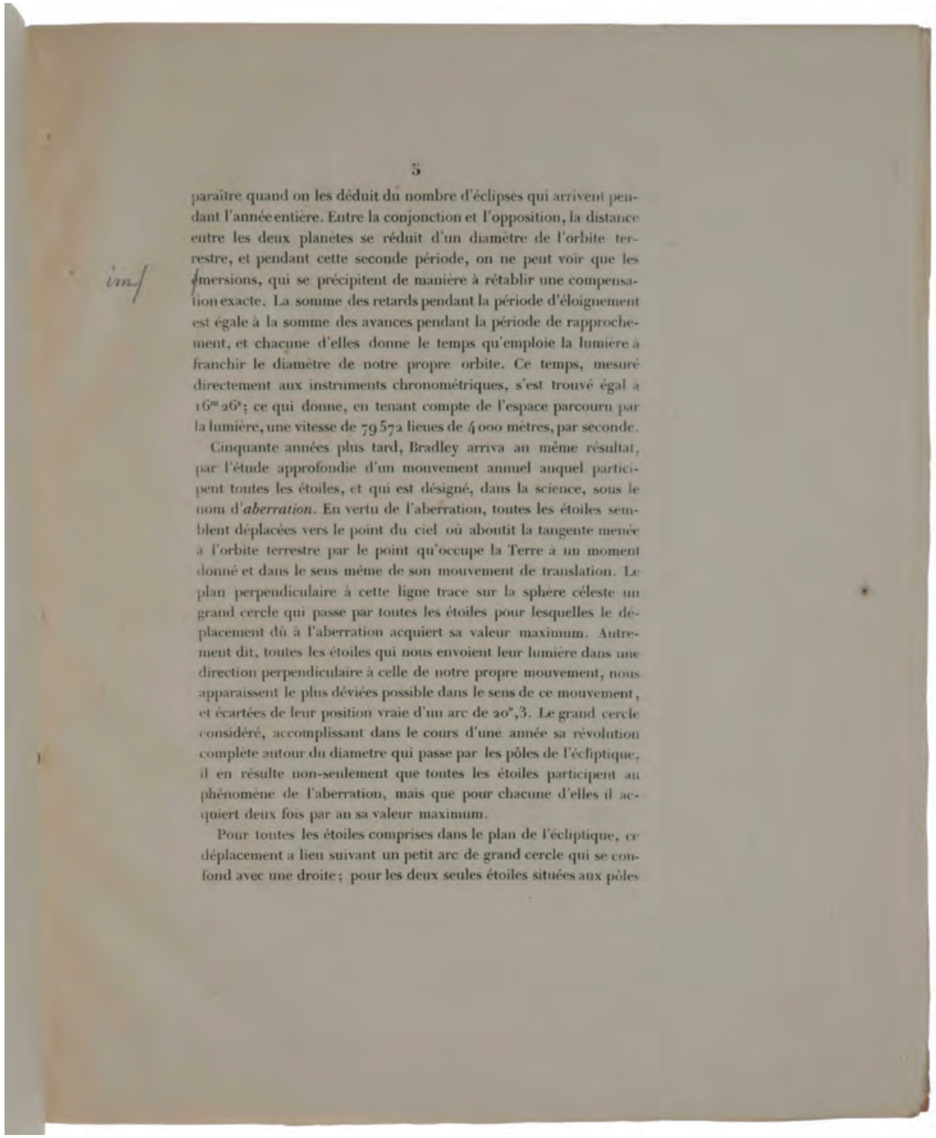
sciences, vol. 7, pp. 954-960], emphasizing the possibility that a test of the relative speed of light in air versus water could be used to distinguish between the particle and wave theories of light” (Wikipedia).

“A comparison of this velocity in air and in water would be a clear experimental test between the wave and particle theories of light, since the former required light to travel faster in air; the latter, in water” (DSB).

“In 1845, Arago suggested to Fizeau and Foucault that they attempt to measure the speed of light. Sometime in 1849, however, it appears that the two had a falling out, and they parted ways pursuing separate means of performing this experiment. In 1848-49, Fizeau used, not a rotating mirror, but a toothed wheel apparatus to perform an absolute measurement of the speed of light in air …

“In 1850 and in 1862, Léon Foucault made improved determinations of the speed of light substituting a rotating mirror for Fizeau’s toothed wheel. The apparatus involves light from a slit S reflecting off a rotating mirror R, forming an image of the slit on the distant stationary mirror M, which is then reflected back to reform an image of the original slit. If mirror R is stationary, then the slit image will reform at S regardless of the mirror’s tilt. The situation is different, however, if R is in rapid rotation. As the rotating mirror R will have moved slightly in the time it takes for the light to bounce from R to M and back, the light will be deflected away from the original source by a small angle.

“Guided by similar motivations as his former partner, Foucault in 1850 was more interested in settling the particle-versus-wave debate than in determining an accurate absolute value for the speed of light. Foucault measured the differential speed of light through air versus water by inserting a tube filled with water between the rotating mirror and the distant mirror. His experimental results, announced



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paraître quand on les déduit du nombre d'éclipses qui arrivent pendant l'année entière. Entre la conjonction et l'opposition, la distance entre les deux planètes se réduit d'un diamètre de l'orbite terrestre, et pendant cette seconde période, on ne peut voir que les émersions, qui se précipitent de manière à rétablir une compensation exacte. La somme des retards pendant la période d'éloignement est égale à la somme des avances pendant la période de rapprochement, et chacune d'elles donne le temps qu'emploie la lumière à franchir le diamètre de notre propre orbite. Ce temps, mesuré directement aux instruments chronométriques, s'est trouvé égal à $16^m 26^s$; ce qui donne, en tenant compte de l'espace parcouru par la lumière, une vitesse de 79 572 lieues de 4 000 mètres, par seconde.

im/

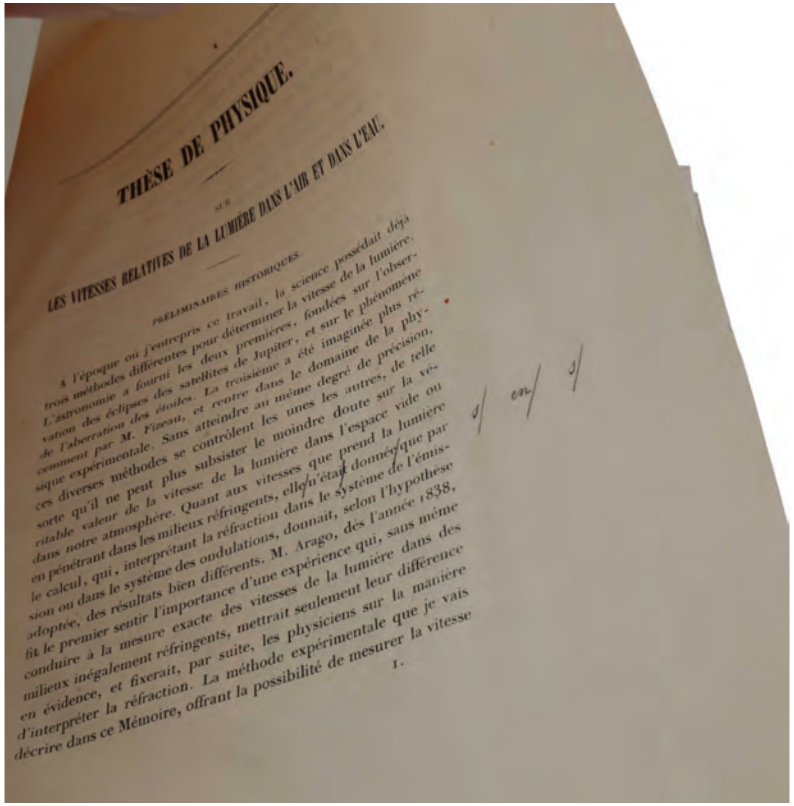
Cinquante années plus tard, Bradley arriva au même résultat, par l'étude approfondie d'un mouvement annuel auquel participent toutes les étoiles, et qui est désigné, dans la science, sous le nom d'*aberration*. En vertu de l'aberration, toutes les étoiles semblent déplacées vers le point du ciel où aboutit la tangente menée à l'orbite terrestre par le point qu'occupe la Terre à un moment donné et dans le sens même de son mouvement de translation. Le plan perpendiculaire à cette ligne trace sur la sphère céleste un grand cercle qui passe par toutes les étoiles pour lesquelles le déplacement dû à l'aberration acquiert sa valeur maximum. Autrement dit, toutes les étoiles qui nous envoient leur lumière dans une direction perpendiculaire à celle de notre propre mouvement, nous apparaissent le plus déviées possible dans le sens de ce mouvement, et écartées de leur position vraie d'un arc de 20",3. Le grand cercle considéré, accomplissant dans le cours d'une année sa révolution complète autour du diametre qui passe par les pôles de l'écliptique, il en résulte non-seulement que toutes les étoiles participent au phénomène de l'aberration, mais que pour chacune d'elles il acquiert deux fois par an sa valeur maximum.

Pour toutes les étoiles comprises dans le plan de l'écliptique, ce déplacement a lieu suivant un petit arc de grand cercle qui se confond avec une droite; pour les deux seules étoiles situées aux pôles shortly before Fizeau announced his results on the same topic, were viewed as 'driving the last nail in the coffin' of Newton's corpuscle theory of light when it showed that light travels more slowly through water than through air. Newton had explained refraction as a pull of the medium upon the light, implying an increased speed of light in the medium. The corpuscular theory of light went into abeyance, completely overshadowed by wave theory. This state of affairs lasted until 1905, when Einstein presented heuristic arguments that under various circumstances, such as when considering the photoelectric effect, light exhibits behaviors indicative of a particle nature.

"In contrast to his 1850 measurement, Foucault's 1862 measurement was aimed at obtaining an accurate absolute value for the speed of light, since his concern was to deduce an improved value for the astronomical unit. At the time, Foucault was working at the Paris Observatory under Urbain le Verrier. It was le Verrier's belief, based on extensive celestial mechanics calculations, that the consensus value for the speed of light was perhaps 4% too high. Technical limitations prevented Foucault from separating mirrors R and M by more than about 20 meters. Despite this limited path length, Foucault was able to measure the displacement of the slit image (less than 1 mm) with considerable accuracy. In addition, unlike the case with Fizeau's experiment (which required gauging the rotation rate of an adjustable-speed toothed wheel), he could spin the mirror at a constant, chronometrically determined speed. Foucault's measurement confirmed le Verrier's estimate. His 1862 figure for the speed of light (298000 km/s) was within 0.6% of the modern value" (Wikipedia).

Jean Bernard Léon Foucault (1819-68) "worked in a laboratory set up in his home until, following the award of the Cross of the Legion of Honor in 1851 (for his pendulum experiment) and the *docteur ès sciences physiques* in 1853 (for his thesis comparing the velocity of light in air and water), he was given a place as physicist at the Paris observatory by Napoleon III. Further honors followed: the Copley Medal of the Royal Society in 1855, officer of the Legion of Honor and member of the Bureau des Longitudes in 1862, and foreign member of the Royal Society (1864) and the academies of Berlin and St. Petersburg. Finally, after having failed to be elected in 1857, Foucault was chosen in 1865, following the death of Clapeyron, a member of the Académie des Sciences" (DSB).

En français dans le texte 270; Norman 820.



THÈSE DE PHYSIQUE.

SUR

LES VITESSES RELATIVES DE LA LUMIÈRE DANS L'AIR ET DANS L'EAU.

PRÉLIMINAIRES HISTORIQUES.

A l'époque où j'entrepris ce travail, la science possédait déjà trois méthodes différentes pour déterminer la vitesse de la lumière. L'astronomie a fourni les deux premières, fondées sur l'observation des éclipses des satellites de Jupiter, et sur le phénomène de l'aberration des étoiles. La troisième a été imaginée plus récemment par M. Fizeau, et rentre dans le domaine de la physique expérimentale. Sans atteindre au même degré de précision, ces diverses méthodes se contrôlent les unes les autres, de telle sorte qu'il ne peut plus subsister le moindre doute sur la véritable valeur de la vitesse de la lumière dans l'espace vide ou dans notre atmosphère. Quant aux vitesses que prend la lumière en pénétrant dans les milieux réfringents, elle n'était donnée que par le calcul, qui, interprétant la réfraction dans le système de l'émission ou dans le système des ondulations, donnait, selon l'hypothèse adoptée, des résultats bien différents. M. Arago, dès l'année 1838, fit le premier sentir l'importance d'une expérience qui, sans même conduire à la mesure exacte des vitesses de la lumière dans des milieux inégalement réfringents, mettrait seulement leur différence en évidence, et fixerait, par suite, les physiciens sur la manière d'interpréter la réfraction. La méthode expérimentale que je vais décrire dans ce Mémoire, offrant la possibilité de mesurer la vitesse

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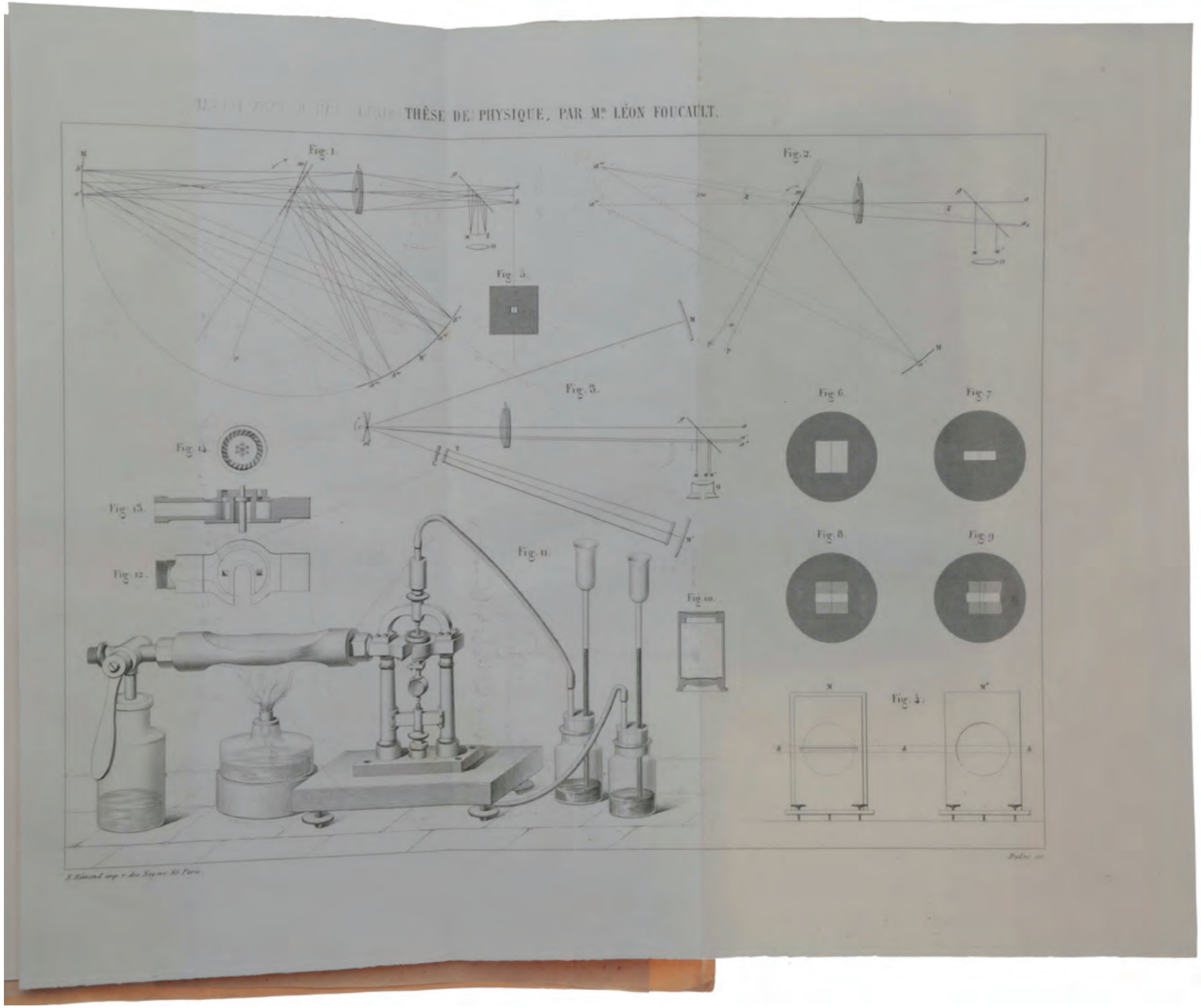

THÈSE DE PHYSIQUE, PAR Mr LÉON FOUCAULT.
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13.
Fig. 14.

PMM 240 - ONE OF TWELVE COPIES

GALVANI, Luigi. *De viribus electricitatis in motu musculari commentarius.* Bologna: Press of the Academy of Sciences, 1791.

$195,000

4to (262 x 206 mm), pp. [1-2], 3-58, with four folding engraved plates. Contemporary marbled wrappers (wrappers rubbed and chipped at extremities, rear wrapper with closed tear, internally a few spots in the margins and some light browning and soiling).

First edition, incredibly rare offprint, **one of 12 copies issued**, of this epoch-making work, one of the most important in the history of electricity. Galvani's paper was published in one of the 'Opuscula' of the journal *De Bononiensi scientiarum et artium instituto atque academia* (vol. 7, pp. 363-418); this offprint probably precedes the journal printing (see below). It is accompanied by four engraved plates, by his friend Jacopo Zambelli, which graphically illustrate Galvani's dissections and electrical apparatus. Along with Harvey's plate of the veins of the arm, they are the most famous of all illustrations in the history of biology. Evans summarized Galvani's paper succinctly as "the description of the production of 'current electricity' by contact between two different metals and the legs of a frog (the latter acting as a galvanometer), which inaugurated the modern epoch in electricity." "Although the existence of electricity had been recognized for many centuries, it was not until the eighteenth century, with the invention of man-made apparatus for its creation, that the study of electricity became a science. That electricity was involved somehow in the function of living tissue had been noted in studies of the activity of electric fishes and eels, but it was Luigi Galvani,

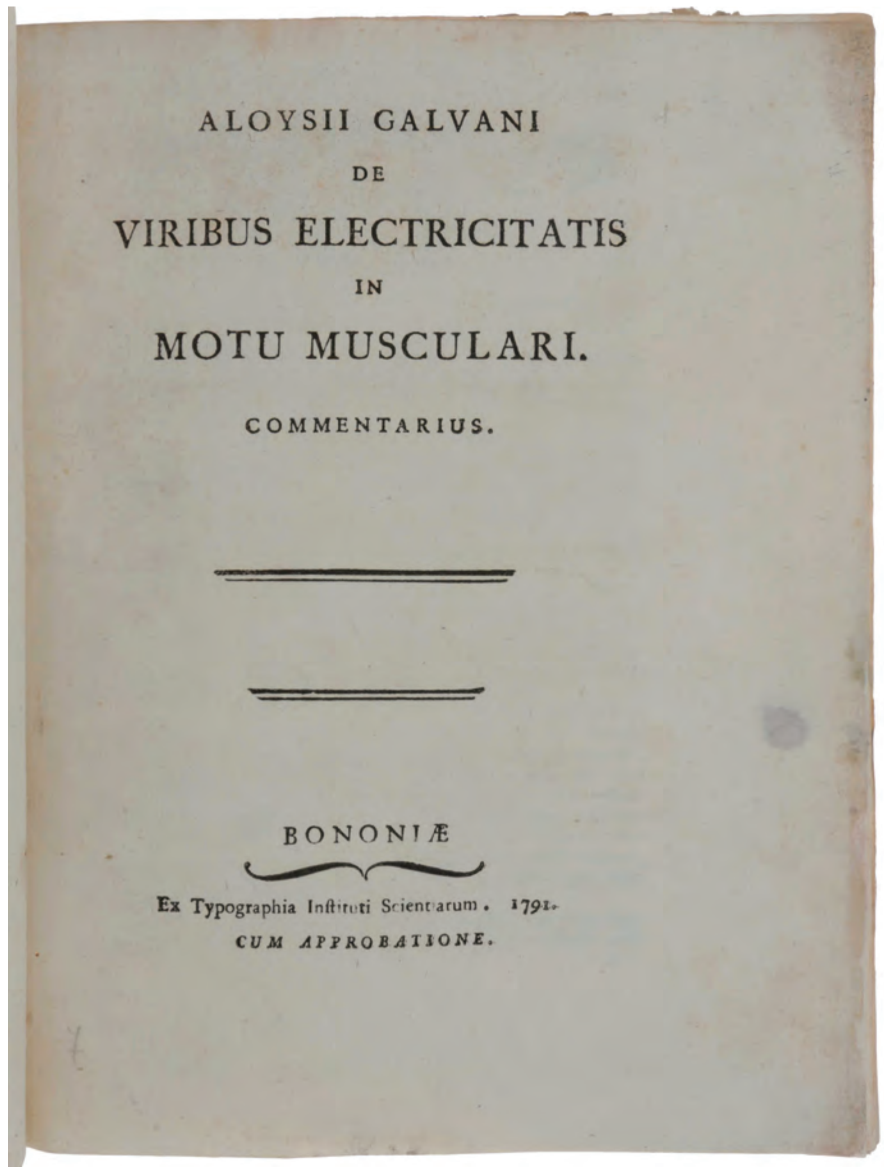
professor of anatomy in Bologna, who first carried out systematic experiments demonstrating that muscular contraction results from an electric current" (Grolier, *Medicine*, p. 183). Alessandro Volta (1745-1827) repeated Galvani's experiments, correctly interpreted the results as being due to contact electricity, and was thus led directly to the invention of the 'Voltaic pile', the first source of a continuous electric current and precursor of the modern electric battery. There are two different issues of this offprint. Both have separate pagination and register, and in both an ornamental border is added above the beginning of the article. The first issue is printed from the standing type of the journal, and has a half-title but no title page; the second issue, offered here, has a title but no half-title. "In this second issue [offered here] gathering A (eight pages) has been reset [and is] distinguished from the first issue by the catch syllables 'si', 'His,' and 'mi-' on pp. 3, 5 and 7 respectively" (hagstromerlibrary.ki.se/books/18685). It was a copy of this second issue that Galvani sent to Volta, and which led to Volta's great discoveries (this copy is now at the Huntington). OCLC lists copies (verified) at the Bodleian, University of British Columbia, Harvard, Huntington, University of Oklahoma, and the Muséum national d'Histoire naturelle, Paris; there are also copies at the University of Bologna and the Hagströmer Library (Stockholm).ABPC/RBH records no copy of the first issue, and only Honeyman's copy of the second (Sotheby's, 5 November 1979, lot 1428, £15,400 = $35,831 – for comparison, Honeyman's two copies of the 1687 *Principia* made £7700 and £8800). Only two copies of the first issue are known, the Fulton copy at Yale and one from the library of the Italian scholar Giaconto Amati offered by Quaritch in 2005 (Cat. 1332, no. 37) (OCLC also lists a copy at Columbia, which may be the Amati copy). A book edition of Galvani's paper was published several months later at Modena (dated 1792), with notes and commentary by Giovanni Aldini, Galvani's nephew and principal apologist, and with Don Bassano Carminati's report of Volta's repetition of Galvani's experiments; in the book edition, the plates were re-engraved and printed on three sheets instead of four.

"By the end of the eighteenth century the connexion between nervous action and electricity had been the subject of investigation for some time. Newton, when discussing the properties of aether, had made suggestions that an electric spirit might convey sensations to the brain along the nerves and produce muscular reactions. Haller also made experiments trying to prove a connexion between electrical action and reflexes of the muscles. It was left to Luigi Galvani, professor of anatomy at Bologna, in 'On the Effects of Electricity on Muscular Motion', to provide, as he thought, dramatic experiments on what he called 'animal electricity' and afterwards 'galvanism'. Galvani observed in his laboratory that when a nerve in a frog's leg was touched with a scalpel, violent contractions of the muscles occurred simultaneous with the sparks discharged from a nearby electrical machine. He further discovered that when one metal was placed in contact with a frog's nerve, another with a muscle, and the metals touched, contraction of the muscle took place, without needing a spark from an electrical machine. As a physiologist, Galvani thought that this action was due to the presence of electricity in the animal itself, as in the 'electric eel' and that the metal wires simply served as conductors. He did not realize that he had not discovered just a new physiological source of electricity, but a new source of continuous electric flow in chemical action. It was Alessandro Volta, a physicist, who proved that animals were inessential to 'galvanic' electricity, and who constructed the first battery to cause a current to flow by chemical action" (*Printing and the Mind of Man*).

"No one who lives in the present age of electrical power needs to be reminded of the practical importance for technology of the availability of an electric current. Before the time of Galvani, electricity was available only at high potential and in short surges of charge, as in spark discharges. But once the electrical battery had been invented, the potential could be chosen at will within large ranges, and continuous currents at constant amperage were available. The development of the battery, with the allied physical principles, marked the beginning of a wholly new ear in physics. And in chemistry, the battery made possible the decomposition of many compounds and the isolation of new chemical elements, and it led to an understanding of the bonding forces that hold together the constituent parts of molecules. These great revolutions in physics, chemistry and engineering derived from the publication of Galvani's book – although they were the product of an investigation that had as its aim to disprove Galvani's fundamental postulate of animal electricity" (Cohen, p. 29).

"During the 1770's [Galvani's] research interests shifted to a considerable extent from largely anatomical to more strictly physiological studies, specifically on nerves and muscles. In 1772 Galvani read a paper on Hallerian irritability to the Istituto delle Scienze, and in 1773 he discussed the muscle movement of frogs before the same body. In 1774 he read a paper on the effect of opiates on frog nerves. These researches fused in his mind with slightly earlier eighteenth-century studies, several of them by Italians, on the electrical stimulation of nerves and muscles. Picking up where Beccaria, Leopoldo Caldani, Felice Fontana, and Tommaso Laghi had recently left off, Galvani began in late 1780 an extensive and meticulous series of investigations into the irritable responses elicited by static electricity in properly prepared frogs" (DSB).

"The initial observation, described by Galvani in his *De viribus electricitatis*, occurred when a dissected frog lay on a table on which there was an electrical machine. Violent contractions of the muscles of the frog's limb occurred when an assistant touched the inner crural nerve with the point of a scalpel; and it was observed that the contractions occurred simultaneously with the discharge of a spark from the electrical machine, and only if the scalpel were grounded by the experimenter touching with a finger the iron nails that fastened the blade to the handle – if the experimenter held the bone without touching the nails or blade,



so that the blade was insulated, no contractions occurred. What puzzled Galvani was the fact that these contractions occurred even if the frog was completely insulated from the machine and at some distance from it. He did not know that the insulated frog was most likely charged by induction, even though insulated from the machine, and that if the nerve were grounded when the machine was discharged, then the dissected frog would be discharged through the scalpel and experimenter, and that the sudden change of potential at the point where the scalpel was in contact with the nerve would produce a muscle contraction in no way dissimilar to the contractions excited electrically in experiments on living and dead animals for at least thirty years past …

"Galvani was stimulated by his observation and bewildered. But, like any good experimenter, he studied the puzzling phenomenon by varying the parameters. Thus, he changed over from using the charge produced by an electrical machine to the charges naturally produced in thunder-clouds. He found that his frog preparations, hanging by copper hooks from an iron railing, contracted not only during thunder-storms but in calm weather too. Impatient at the long wait between contractions in fair weather, he tells us that he began to scrape and press the copper hook (which was fastened to the backbone of the frog) against the iron railing and discovered that contractions were frequently produced, apparently in independence of variations in the weather. Similar results were produced indoors when the frog was placed on an iron plate and the brass hook was placed against the plate. This last experiment was varied in different ways, being performed in different places and at different times of the day, and using different metals. The major effects noticed were a variation in the intensity of the contractions with different metals and a complete absence of of contractions when non-conductors such as glass, gum, resin, stone or dry wood were employed. These results led him to believe that an electric fluid must be in the animal itself and he likened the whole process of a fine nervous fluid flowing from the nerves into the muscles to the passage of electricity in the discharge of a Leyden jar.

"The remainder of the book is an exploration of the action of this postulated animal electricity, or animal nervous electric fluid, in producing muscular contractions in cold-blooded and warm-blooded animals. The complete theory, developed at length in the book, has been summarised by [Emil] du Bois-Reymond as follows:

"1. Animals have an electricity peculiar to themselves, which is called Animal Electricity.

"2. The organs to which this animal electricity has the greatest affinity, and in which it is distributed, are the nerves, and the most important organ of its secretion is the brain.

"3. The inner substance of the nerve is specialized for conducting electricity, while the outer oily layer prevents its dispersal, and permits its accumulation.

"4. The receivers of animal electricity are the muscles, and they are like a Leyden jar, negative on the outside and positive on the inside.

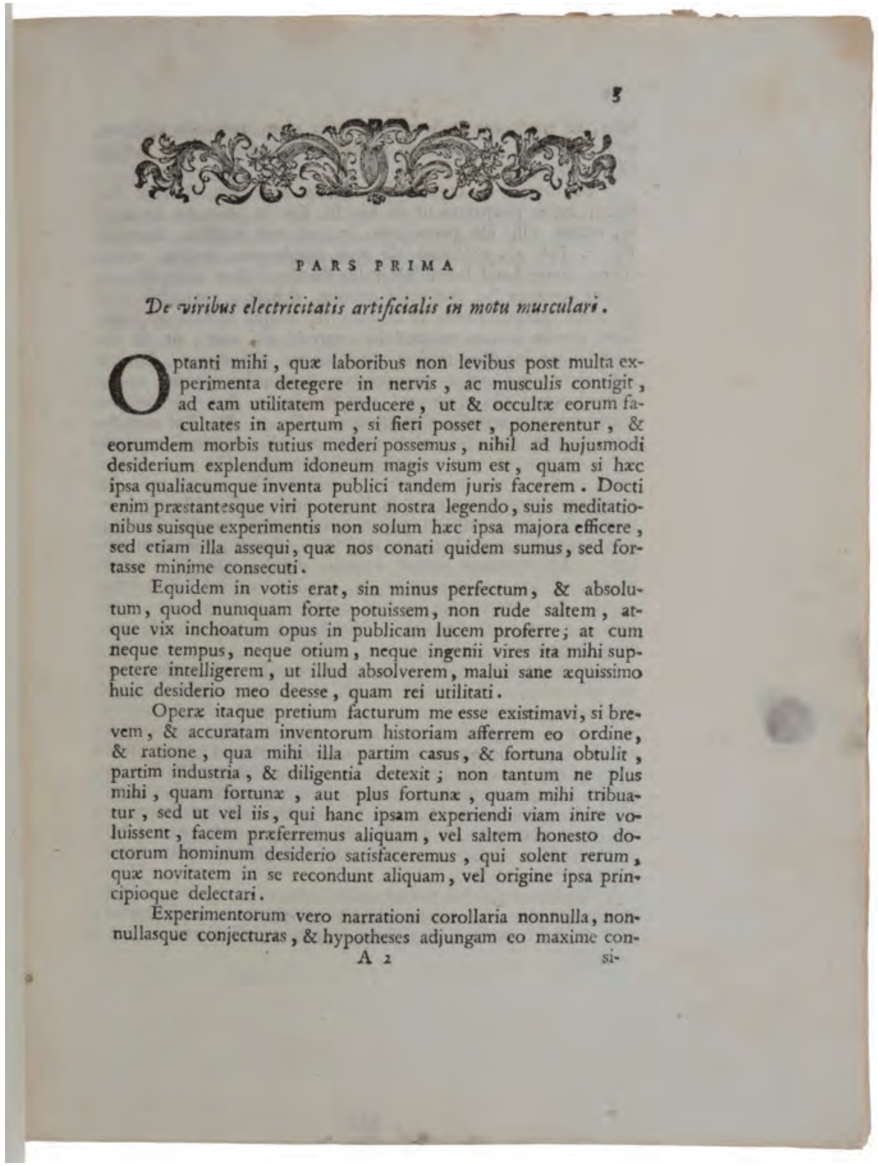
"5. The mechanism of motion consists in the discharge of the muscular fluid from the inside of the muscle via the nerve to the outside, and this discharge of the muscular Leyden jar furnishes an electrical stimulus to the irritable muscle fibres, which therefore contract."

"Such were the bases of Galvanism" (Cohen, pp. 27-8).

"Galvani's celebrated treatise was issued in 1791, but just when it appeared is not clear. The imprimatur at the end of the volume is dated 27 March 1791 [this refers to the journal appearance]; but nearly a year elapsed from that date before the first repercussions aroused by the paper began to be heard. Galvani was supplied with a few offprints, one of which was forwarded to Volta and bears the inscription "Ex dono auctoris." One would like to know the general tenor of Volta's acknowledgement to the author, but in lieu of that one must be satisfied with the comments of Volta's colleague, the professor of medicine at Pavia, Don Bassano Carminati, who under the date of 3 April 1792 acknowledged the receipt presumably of another copy which a mutual friend, the Abbé Felice Fontana, had brought him.

"In this acknowledgement Carminati tells of the interest aroused by the paper among his colleagues, particularly on the part of Spallanzani and of Volta who had ventured to suggest other interpretations than animal electricity to explain the convulsive movements. To this Galvani replied at length on 8 May, citing further observations – one on a human arm and leg amputated that very day – which appeared to favour his original interpretation of phenomena, "that served to bring somewhat nearer solution the most important problem in physiology – what is the physical cause of voluntary movement.

"Volta's first public reaction to Galvani's experiments was given in an address on 5 May 1792 at the Aula of the University of Pavia on the occasion of a "promotion" ceremony. Cameron Walker has pointed out that Volta did not take vigorous exception to Galvani's work at this time but merely to his conclusions concerning animal electricity. Volta was prepared to suppose that the electricity might possibly come from an inherent animal electricity but he added, "I want more convincing proofs to overcome my lack of faith with regard to animal electricity." Cameron Walker adds that this lack of faith did not refer to Galvani's experiments as such but only to the inference that Galvani had thus proved the existence of animal electricity. Various other passages in Volta's first memoir indicate that he



5

PARS PRIMA

De viribus electricitatis artificialis in motu musculari.

Optanti mihi, quæ laboribus non levibus post multa experimenta detegere in nervis, ac musculis contigit, ad eam utilitatem perducere, ut & occultæ eorum facultates in apertum, si fieri posset, ponerentur, & eorumdem morbis tutius mederi possemus, nihil ad hujusmodi desiderium explendum idoneum magis visum est, quam si hæc ipsa qualiacumque inventa publici tandem juris facerem. Docti enim præstantesque viri poterunt nostra legendo, suis meditationibus suisque experimentis non solum hæc ipsa majora efficere, sed etiam illa assequi, quæ nos conati quidem sumus, sed fortasse minime consecuti.

Equidem in votis erat, sin minus perfectum, & absolutum, quod numquam forte potuissem, non rude saltem, atque vix inchoatum opus in publicam lucem proferre; at cum neque tempus, neque otium, neque ingenii vires ita mihi suppetere intelligerem, ut illud absolverem, malui sane æquissimo huic desiderio meo deesse, quam rei utilitati.

Operæ itaque pretium facturum me esse existimavi, si brevem, & accuratam inventorum historiam afferrem eo ordine, & ratione, qua mihi illa partim casus, & fortuna obtulit, partim industria, & diligentia detexit; non tantum ne plus mihi, quam fortunæ, aut plus fortunæ, quam mihi tribuatur, sed ut vel iis, qui hanc ipsam experiendi viam inire voluissent, facem præferremus aliquam, vel saltem honesto doctorum hominum desiderio satisfaceremus, qui solent rerum, quæ novitatem in se recondunt aliquam, vel origine ipsa principioque delectari.

Experimentorum vero narrationi corollaria nonnulla, nonnullasque conjecturas, & hypotheses adjungam eo maxime con-

A 2 si-was most cordial to Galvani and full of admiration for his work. It is also clear from internal evidence that on 5 May 1791 he had done little more than digest the contents of Galvani's treatise and had not yet attempted to repeat his experiments, for he says: "The treatise which appeared a few months ago concerning the action of electricity on the movement of muscles, written by Signor A. Galvani, member of the Institute of Bologna and Professor of the University of that place, who has already distinguished himself by other anatomical and physiological discoveries, contains one of those great and brilliant discoveries which deserves to mark a new era in the annals of physics and medicine."

"To be more specific, Volta believed that the convulsive movements described by Galvani were due to electric currents generated by friction – in short, "frictional" electricity rather than "animal" electricity …

"A rejoinder to Volta's discourse soon came from the youthful Aldini, who must assuredly have received his uncle's permission to reply; for he issued from a Mantua press under an imprimatur dated 28 July 1792 a reprint of *De viribus electricitatis* [the offered work] together with the Galvani-Carminati correspondence, to which is prefixed a 26-page commentary by himself under the title *De animalis electricae theoriae ortu atque incrementis*.

"From that time on, with but one exception, it is no longer from Galvani that the public hears in defence of animal electricity, but chiefly from the irrepressible Aldini, to whom, rather than to Galvani, Volta addresses some of his published correspondence. In 1793 and again in 1794 Aldini read before the Institute of Bologna a dissertation on the subject, the two having been published together in the latter year. These papers, translated into English, were reprinted in 1803 at which time Aldini subjoined three pages of "Conclusions" which do not happen to appear in the originals" (Cohen, pp. 159-160).

"The large number of publications devoted by Volta to Galvanism are an indication of the fascination of the subject. Two letters to Tiberius Cavallo in England, written in the fall of 1792, read at a meeting of the Royal Society in 1793, and published in the *Philosophical Transactions*, presented an account of Galvani's discoveries together with Volta's own experiments and observations. Although Volta indicates in this letter a belief in animal electricity … Volta also indicated that the stimulus in Galvani's experiments was the juncture of two different metals by a moist body. Without completely discarding the possibility of an animal electricity, Volta made it plain that "metals used in the experiments, being applied to the moist bodies of animals, can by themselves, and by their proper virtue, excite and dislodge the electric fluid from its state of rest, so that the organs of the animal act only passively." By the end of 1793 Volta had altogether rejected the existence of animal electricity and had begun to work out his own theory … Finally, one year after Galvani's death, Volta hit upon a device for making the feeble Galvanic effect stronger. Volta showed that if a number of pairs of discs, one of copper and the other of zinc, were placed in a line, each pair separated from the next by a moistened cardboard disc, a greatly increased effect would be produced. This "pile," as he called it, could even produce a shock if an experimenter simultaneously touched the copper disc at one end of the pile with one hand and the zinc disc at the other end of the pile with the other hand. This effect could be produced again and again, so that the instrument was like a Leyden jar with powers of restoring its charge after each discharge, with "an inexhaustible charge, a perpetual action or impulsion on the electric fluid" … So were the battery and the continuous current discovered" (Cohen, pp. 30-33).

"In retrospect, Galvani and Volta are both seen to have been partly right and partly wrong. Galvani was correct in attributing muscular contractions to an electrical stimulus but wrong in identifying it as an "animal electricity." Volta


Tom. VII.
Tav. 3.
Fig. 15
Fig. 16
Fig. 11
Fig. 12
Fig. 10
Fig. 14
Fig. 9
Fig. 13

correctly denied the existence of an “animal electricity” but was wrong in implying that every electrophysiological effect requires two different metals as sources of current. Galvani, shrinking from the controversy over his discovery, continued his work as teacher, obstetrician, and surgeon, treating both wealthy and needy without regard to fee. In 1794 he offered a defense of his position in an anonymous book, *Dell’uso e dell’attività dell’arco conduttore nella contrazione dei muscoli* (“On the Use and Activity of the Conductive Arch in the Contraction of Muscles”), the supplement of which described muscular contraction without the need of any metal. He caused a muscle to contract by touching the exposed muscle of one frog with a nerve of another and thus established for the first time that bioelectric forces exist within living tissue” (Britannica).

The journal volume containing Galvani’s paper has the same imprint (and date) as the present offprint, but “just when it appeared is not clear” (Cohen, p. 159). This question has now been answered by Walter Bernardi. “Galvani published his discovery in a 53-page [sic] Latin paper, which was included in the seventh volume of the *Commentarii* of the Bologna Academy of Sciences. When was Galvani’s *Commentarius* printed? The publishing date of his masterpiece, which is also the date of the birth of electrodynamics and electrophysiology, has always been a mystery to historians. Now, the examination of Sebastiano Canterzani’s manuscript correspondence in the University Library of Bologna allows us to solve the problem. [Canterzani (1734-1818) was secretary of the Bologna Academy of Sciences from 1766 until 1796.] The seventh volume of the *Commentarii* was dated 1791 and the imprimatur had been awarded on March 27, but it was published at the beginning of 1792, perhaps on January 2 or 3” (Bernardi, p. 102). “The delay in publication was a typical feature of the *Commentarii*, which was the official publication of the Bologna Institute of Sciences that collected the research work of its members. For this reason and also for its relatively small circulation, the *Commentarii* was not a very effective means of scientific communication. Galvani

thus decided to print a few copies of *De viribus* separately, probably in 1791, and he sent these to various scientists; one of them reached Volta in Paris, in March 1792" (Piccolini & Bresadola, pp. 143-144). The long delay in printing this volume of the *Commentarii* explains why, despite the profound interest eventually excited by Galvani's discoveries, there was little reaction until well into 1792, and no definitely dated reaction earlier than 3 April 1792, when Carminati received his copy of the offprint. As Fulton & Cushing have noted (p. 258), it was Carminati's letter of acknowledgement to Galvani that "first drew the attention of the learned world to Galvani's studies, and thus inaugurated the controversy with Volta which immediately followed."

Dibner, *Heralds of Science* 59; Evans 34; Fulton and Stanton, *Galvan*i 4; Garrison-Morton 593; Grolier, *One Hundred Books Famous in Medicine* 50; Horblit, *One Hundred Books Famous in Science* 37a; Norman 869; Osler 1243; *Printing and the Mind of Man* 240; Waller 11346; Wellcome III, p. 86; Wheeler Gift 575. Bernardi, 'The Controversy on Animal Electricity in Eighteenth-Century Italy: Galvani, Volta and Others,' in *Nova Voltiana. Studies on Volta and His Times*, edited by F. Bevilacqua and L. Fragonese, Vol. 1, pp. 101-114; De Andrade Martins, 'Romagnosi and Volta's Pile: Early Difficulties in the Interpretation of Voltaic Electricity,' in *ibid.*, Vol. 3, pp. 81-102. Fulton & Cushing, 'A Bibliographical Study of the Galvani and the Aldini Writings on Animal Electricity,' *Annals of Science*, Vol. 1 (1936), pp. 239-68 with 9 plates; Galvani, *Commentary of the Effects of Electricity on Muscular Motion.* Translated into English by Margaret Glover Foley, with Notes and a Critical Introduction by I. Bernard Cohen. Together with a Facsimile of Galvani's *De Viribus* . . . and a Bibliography of the Editions and Translations of Galvani's Book prepared by John Farquhar Fulton and Madeline E. Stanton (1953). Piccolino & Bresadola, *Shocking Frogs*: *Galvani, Volta, and the Electric Origins of Neuroscience*, Oxford, 2013.

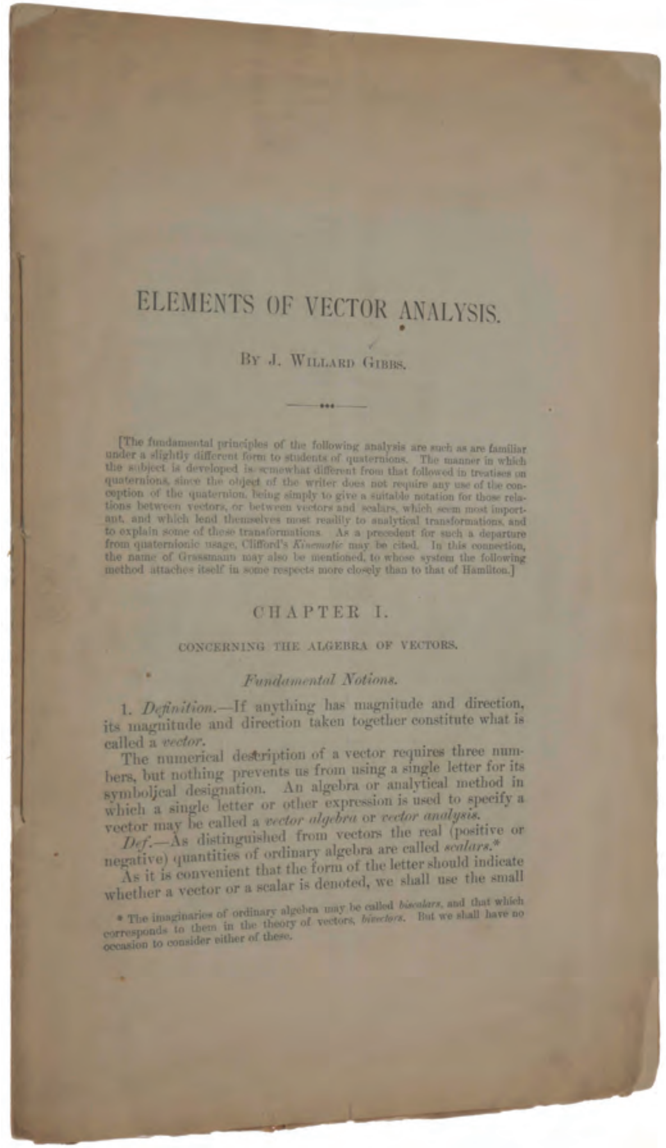
DIBNER 177 - THE BEGINNINGS OF VECTOR ANALYSIS

GIBBS, Josiah Willard. *Elements of Vector Analysis. [Offered with:] Autograph letter from Gibbs to John Monroe Van Vleck.* New Haven: Tuttle, Morehouse & Taylor, 1881.

$19,500

8vo (245 x153 mm), pp 36. Stitched as issued (with original stitching), upper inner corner of final leaf torn away (no text loss), and tear to second final leaf (again not effecting the text).

First edition, first issue (see below), of this extremely rare pamphlet which "marks the beginning of modern vector analysis" (Crowe, p. 150). "Nearly all branches of classical physics and many areas of modern physics are now presented in the language of vectors, and the benefits derived thereby are many. Vector analysis has likewise proved a valuable aid for many problems in engineering, astronomy and geometry" (*ibid*. p. v). The genesis of the present work was described in Gibbs's own words in an 1888 letter to Victor Schlegel: "My first acquaintance with quaternions was in reading Maxwell's E & M [i.e. *Treatise on Electricity and Magnetism*, 1873] where quaternion notations are considerably used. I became convinced that to master those subjects, it was necessary for me to commence by mastering those methods. At the same time I saw, that although the methods were called quaternionic the idea of the quaternion was quite foreign to the subject. In regard to the product of vectors, I saw that there were two important functions (or products) called the vector part & the scalar part of the product, but that the



union of the two to form what was called the (whole) product did not advance the theory as an instrument of geom[etric] investigation. Again with respect to the operator ∇ as applied to a vector I saw that the vector part & the scalar part of the result represented important operations, but their union (generally to be separated afterwards) did not seem a valuable idea … I therefore began to work out *ab initio*, the algebra of the two kinds of multiplication, the three differential operations ∇ applied to a scalar, & the two operations to a vector … This I ultimately printed but never published, although I distributed a good many copies among such persons as I though might possibly take an interest in it" (*ibid*. pp. 152-3). This is the first issue; a second issue, with two additional chapters, was published in 1884. Both issues of Gibbs's pamphlet are extremely rare in commerce. We have been unable to locate any copy of this first issue in auction records, and only two of the second issue: the Horblit copy (Christie's, 16 February 1994) and the Richard Green copy (Christie's, 17 June 2008).

"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" (Dyson). "During the 1880's Gibbs seems to have concentrated on optics and particularly on Maxwell's electromagnetic theory of light … Gibbs's reading Maxwell's *Treatise on Electricity and Magnetism* led him to a study of quaternions, since Maxwell had used the quaternion notation to a limited extent in that work. Gibbs decided, however, that quaternions did not really provide the mathematical language appropriate for theoretical physics, and he worked out a simpler and more straightforward vector analysis" (DSB). From Schlegel's letter, we learn that "Gibbs commenced his search for a vector analysis 'with some knowledge of Hamilton's methods' and ended up with methods that were 'nearly those of Hamilton' … Gibbs also stated that he was not 'conscious that Grassmann exerted any particular influence on my V-A.' This is to be expected since Gibbs had begun searching for a new vector system 'long before my acquaintance with Grassmann.' When (1877 or later) Gibbs finally began to read Grassmann, he found a kindred spirit. Although Gibbs admitted he had never been able to read through either of Grassmann's books, he did recognize Grassmann's priority and warmly praised his ideas on numerous occasions" (Crowe, pp. 153-4).

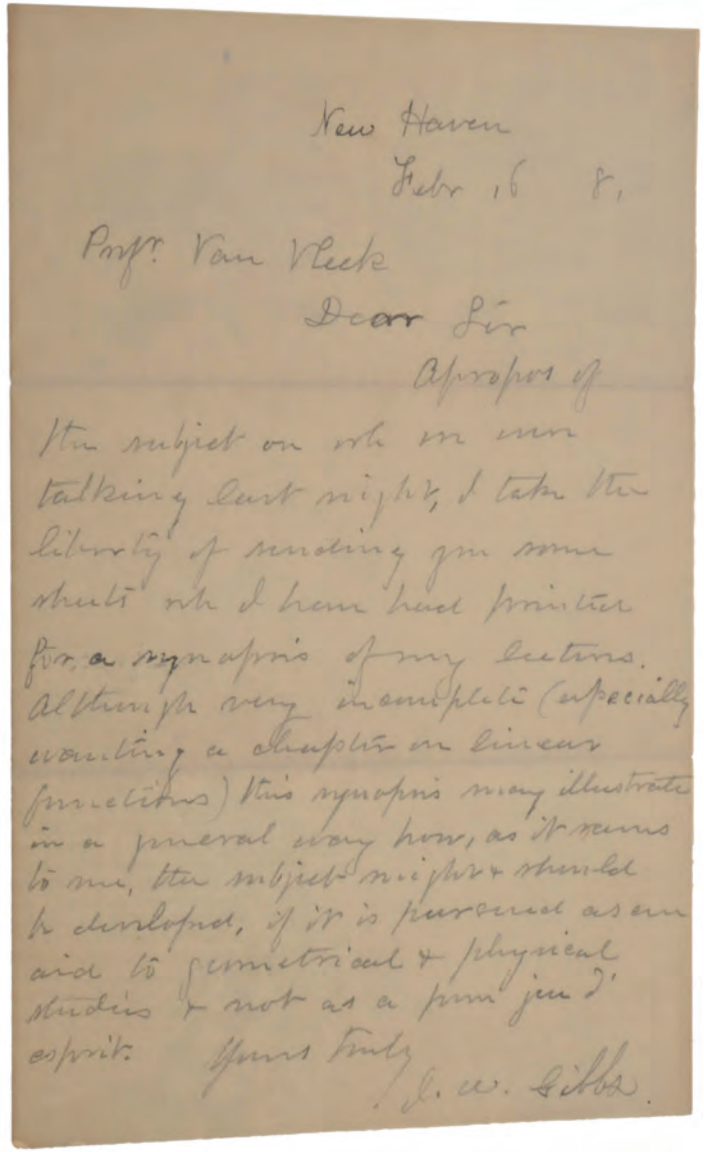
"In 1879 Gibbs gave a course in vector analysis with applications to electricity and magnetism, and in 1881 he arranged for the private printing of the first half of his *Elements of Vector Analysis*; the second half appeared in 1884. In an effort to make his system known, Gibbs sent out copies of this work to more than 130 scientists and mathematicians. Many of the leading scientists of the day received copies, for example, Michelson, Newcomb, J. J. Thomson, Rayleigh, FitzGerald, Stokes, Kelvin, Cayley, Tait, Sylvester, G. H. Darwin, Heaviside, Helmholtz, Clausius, Kirchhoff, Lorentz, Weber, Felix Klein, and Schlegel. Though the work was not given the advertisement that a regular publication would have had, such a selective distribution must have aided in making it known.

"Some idea of the form of Gibbs' *Elements of Vector Analysis* may be obtained from Gibbs' introductory paragraph: 'The fundamental principles of the following analysis are such as are familiar under a slightly different form to students of quaternions. The manner in which the subject is developed is somewhat different from that followed in treatises on quaternions, since the object of the writer does not require any use of the conception of the quaternion, being simply to give a suitable notation for those relations between vectors, or between vectors and scalars, which seem most important, and which lend themselves most readily to analytical transformations, and to explain some of these transformations. As a precedent for such a departure from quaternionic usage, Clifford's *Kinematic* may be cited. In this connection, the name of Grassmann may also be mentioned, to whose system the following method attaches itself in some respects more closely than to that of Hamilton.'

"Although Gibbs mentioned only Clifford and Grassmann in the introductory paragraph, the previously cited letter makes it clear that his chief debt was not to either Clifford or Grassmann but to the quaternionists. In the discussion of Gibbs' book this point will be illustrated; specifically it will be suggested that Gibbs was strongly influenced by the content and form of presentation found in the second edition of Tait's *Treatise on Quaternions*.

"Chapter I, 'Concerning the Algebra of Vectors,' began with such definitions as 'vector', 'scalar,' and 'vector analysis'. In much of the symbolism introduced, Gibbs followed the quaternion traditions: for example, Gibbs represented vectors by Greek eletters, their components by means of *i*, *j*, and *k* … In dealing with vector products Gibbs introduced the 'direct product,' written $\alpha.\beta$, and the 'skew product,' written $\alpha \times \beta$. These are the now-current scalar (dot) and vector (cross) products … Chapter I concluded with a treatment of methods for solving vectorial equations, and chapter II was entitled 'Concerning the Differential and Integral Calculus of Vectors.' Herein Gibbs introduced the operator ∇, proved the related transformation theorems, and gave an extended treatment of the mathematics of potential theory". The part of Gibbs' booklet printed in 1881 terminated near the end of chapter II; the remainder was printed in 1884.

The present first issue terminated at the end of Chapter II. It was issued without a separate title page. In 1884 a second issue appeared, with a variant title "Elements of vector analysis, arranged for the use of students in physics". To the second issue was added a further 47 pages, comprising Chapters III and IV, which dealt



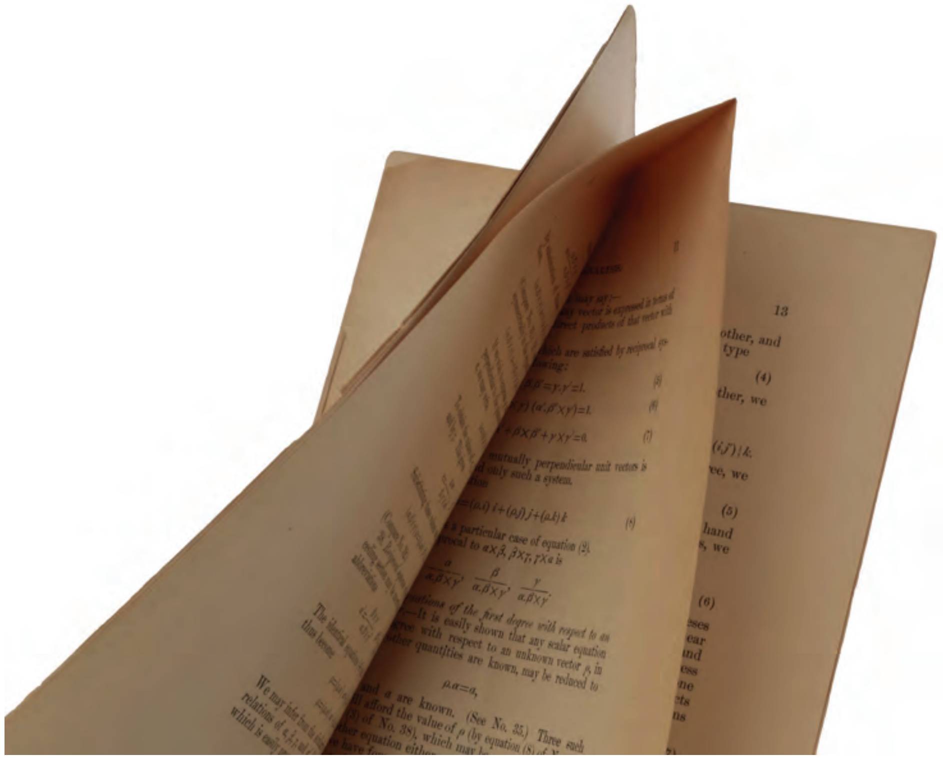
New Haven
Febr. 16 8.

Profr. Van Vleck
Dear Sir
Apropos of the subject on wh we were talking last night, I take the liberty of sending you some sheets wh I have had printed for a synopsis of my lectures. Although very incomplete (especially wanting a chapter on linear functions) this synopsis may illustrate in a general way how, as it seems to me, the subject might & should be developed, if it is pursued as an aid to geometrical & physical studies & not as a pure jeu d'esprit.
Yours truly
J. W. Gibbs.

with linear vector functions, or "dyads" in Gibbs's terminology. (See the Harvard library catalogue for comments on the differences between the two issues.) Gibbs's version of the vector analysis was not formally published until 1901, when one of his students, Edwin B. Wilson, prepared a textbook based on Gibbs's lectures.

"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).



The letter to John Monroe Van Vleck, which accompanied a copy of the pamphlet, describes the additions made to the second issue as follows:

"Apropos of the subject on wh[ich] we were talking last night, I take the liberty of sending you some sheets which I have had printed for a synopsis of my lectures. Although very incomplete (especially wanting a chapter on linear functions) this synopsis may illuminate in a general way how, as it seems to me, this subject might [and] should be developed, if it is pursued as an aid to geometrical [and] physical studies [and] not as a pure jeu d'esprit."

J. M. Van Vleck was professor of mathematics and astronomy at Wesleyan University in Connecticut from 1853 to 1904 (Gibbs spent most of his academic career at nearby New Haven). He was the grandfather of John Hasbrouck Van Vleck (1899-1980), the most important American theoretical physicist between Gibbs and the postwar generation, and winner of the Nobel Prize for Physics in 1977.

Dibner 117; DSB V: 391; Crowe, *A History of Vector Analysis*, 1967.

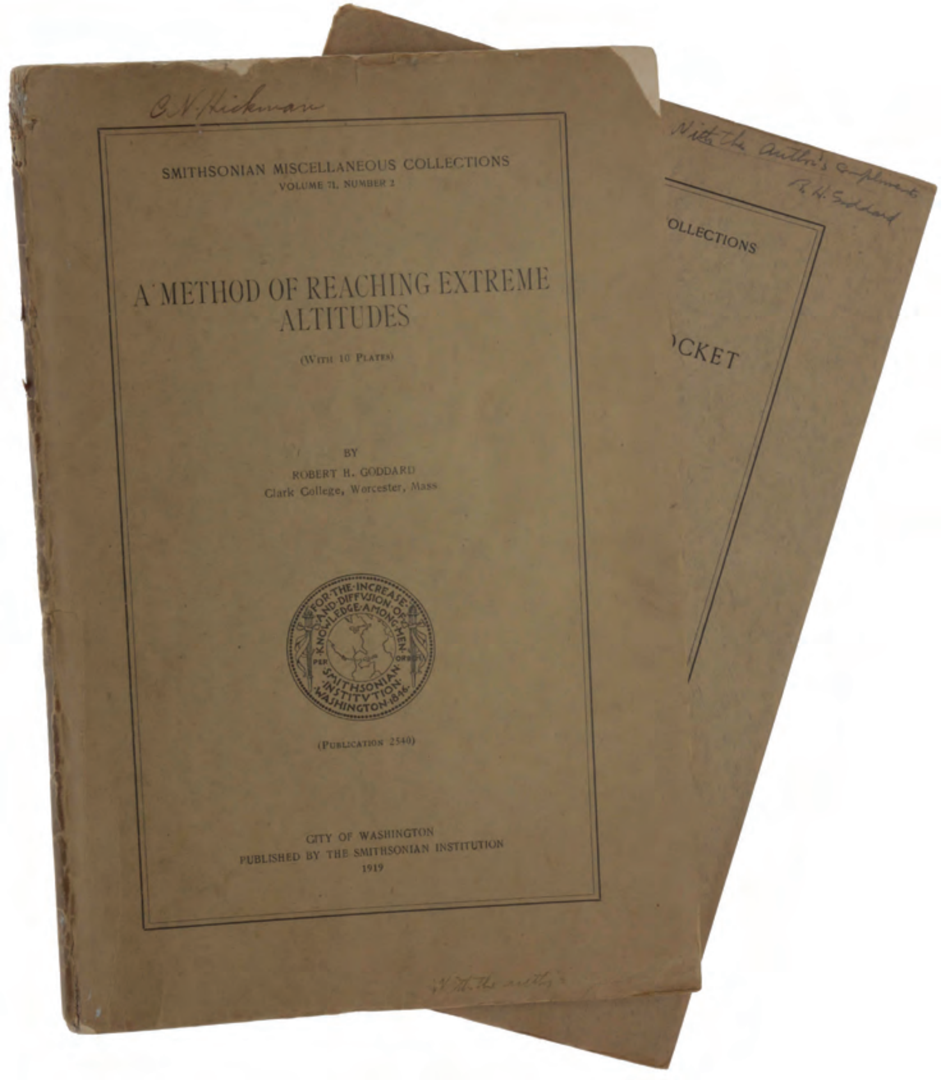
FOUNDATION OF THE SPACE AGE - PRESENTATION COPIES

GODDARD, Robert Hutchins. *A Method of Reaching Extreme Altitudes [with:] Liquid Propellant Rocket Development.* Washington DC: Smithsonian Institution, 1919 & 1936.

$39,500

8vo (245 x 165 mm), pp. [4], 69, with 25 photographic images on 10 plates. Original brown printed wrappers (spine rubbed, slightly chipped at extremities). [With:] 8vo, pp. [2], 10, with 11 pages of photographic plates. Original brown printed wrappers (very slight wear to extremities, light vertical crease), uncut, very good copies.

First edition, presentation copies in the original printed wrappers, **inscribed by Goddard**, and with a highly important association, of the most influential early works on rocketry, which provided the foundation for the modern space age. “Having explored the mathematical practicality of rocketry since 1906 and the experimental workability of reaction engines in laboratory vacuum tests since 1912, Goddard began to accumulate ideas for probing beyond the earth’s stratosphere. His first two patents in 1914, for a liquid-fuel gun rocket and a multistage step rocket, led to some modest recognition and financial support from the Smithsonian Institution … The publication in 1919 of his seminal paper ‘A Method of Reaching Extreme Altitudes’ gave Goddard distorted publicity because he had suggested that jet propulsion could be used to attain escape velocity and that this theory could be proved by crashing a flash-powder missile on the moon. Sensitive to criticism of his moon-rocket idea, he worked quietly



and steadily toward the perfection of his rocket technology and techniques … Among Goddard's successful innovations were fuel-injections systems, regenerative cooling of combustion chambers, gyroscopic stabilization and control, instrumented payloads and recovery systems, guidance vanes in the exhaust plume, gimbaled and clustered engines, and aluminium fuel and oxidizer pumps" (DSB). The 1919 paper described work on rockets that were fed with a continuous stream of solid charges, but this method eventually proved unfeasible, and in 1922 Goddard went back to an earlier idea of his, proposed independently by Oberth in Germany and also noted by Tsiolkovsky in Russia: a liquid-fuel rocket. Goddard successfully launched the first such rocket on March 16, 1926, ushering in an era of space flight and innovation. The results of Goddard's research into liquid-fuel rockets are presented in the 1936 paper offered here. "Years after his death, at the dawn of the Space Age, Goddard came to be recognized as one of the founding fathers of modern rocketry, along with Robert Esnault-Pelterie, Konstantin Tsiolkovsky, and Hermann Oberth. He not only recognized the potential of rockets for atmospheric research, ballistic missiles and space travel but was the first to scientifically study, design and construct the rockets needed to implement those ideas. NASA's Goddard Space Flight Center was named in Goddard's honor in 1959" (Wikipedia). We have located only one presentation copy of the first work in auction records (Christie's, 13 December 2006, lot 145, £4200), a copy rebound in modern wrappers and with later institutional stamps; and one of the second (RR Auction, 2014, $4773).

Provenance: "With the author's compliments" written in ink in Goddard's hand at lower right corner of front wrapper of first work, "With the author's compliments // R. H. Goddard" written in ink in Goddard's hand at upper right corner of front wrapper of second work, presented to; Clarence Hickman (signed by him in ink at top of front wrapper and on first page of text of first work).

Goddard (1882-1945) became interested in space when he read H. G. Wells' science fiction classic *The War of the Worlds* at 16 years old. He received his B.S. degree in physics from Worcester Polytechnic in 1908, and after serving there for a year as an instructor, he began his graduate studies at Clark University in the fall of 1909. Goddard received his M.A. degree in physics from Clark in 1910, and then stayed on to complete his Ph.D. in physics in 1911. After another year at Clark as an honorary fellow in physics, in 1912 he accepted a research fellowship at Princeton University.

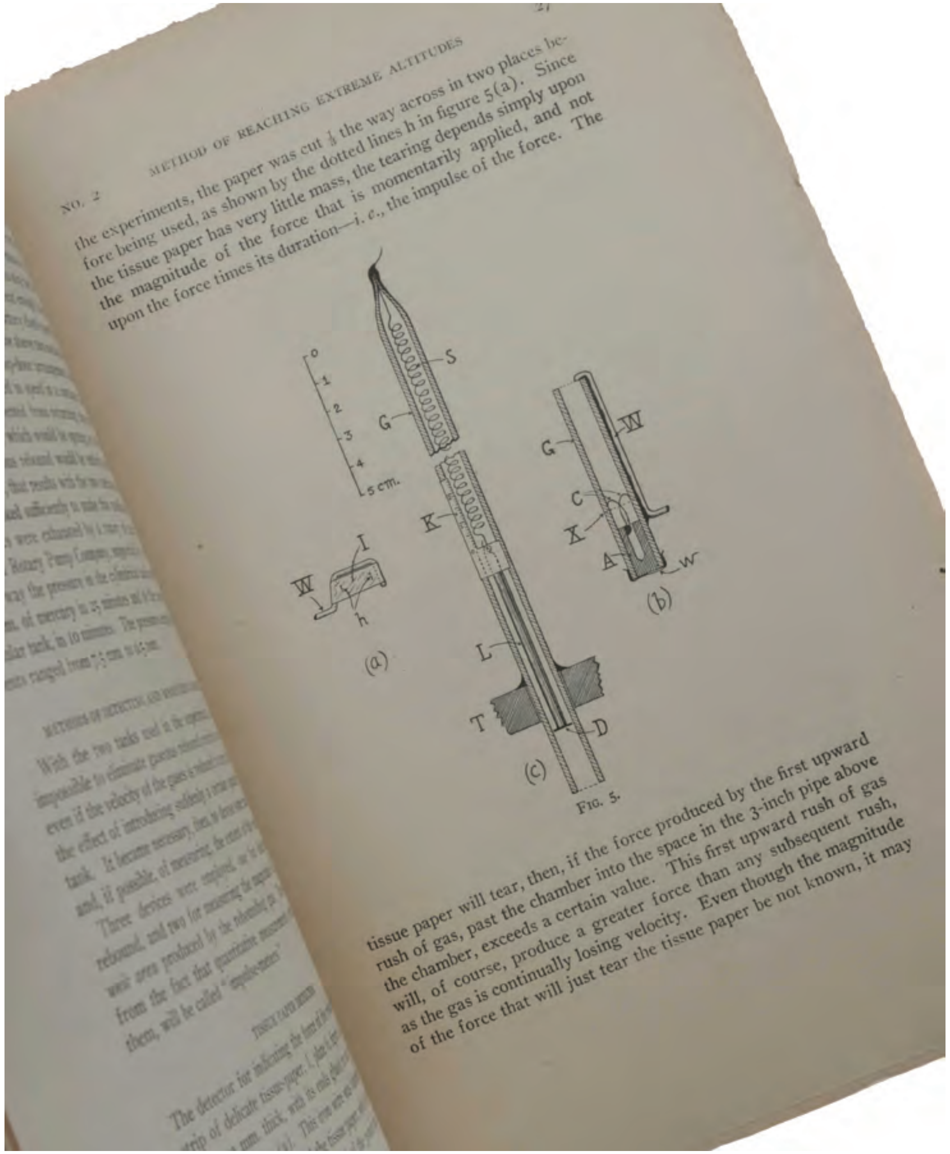
By this time he had in his spare time developed the mathematics which allowed him to calculate the position and velocity of a rocket in vertical flight, given the weight of the rocket and weight of the propellant and the velocity (with respect to the rocket frame) of the exhaust gases. In effect he had independently developed the Tsiolkovsky rocket equation published a decade earlier in Russia. In early 1913, Goddard became seriously ill with tuberculosis and had to leave his position at Princeton. He then returned to Worcester, where he began a prolonged process of recovery at home. It was during this period of recuperation that Goddard began to produce some of his most important work. By the fall of 1914, Goddard's health had improved, and he accepted a part-time position as an instructor and research fellow at Clark University. His position at Clark allowed him to further his rocketry research, but by 1916 the cost of Goddard's rocket research had become too great for his modest teaching salary to bear. He began to solicit potential sponsors for financial assistance, beginning with the Smithsonian Institution. The Smithsonian was interested and asked Goddard to elaborate upon his initial inquiry. Goddard responded with a detailed manuscript he had already prepared, entitled *A Method of Reaching Extreme Altitudes*. Two years later, Goddard arranged for the Smithsonian to publish this manuscript, updated with footnotes.

"In late 1919, the Smithsonian published Goddard's groundbreaking work, *A Method of Reaching Extreme Altitudes*. The report describes Goddard's mathematical theories of rocket flight, his experiments with solid-fuel rockets, and the possibilities he saw of exploring Earth's atmosphere and beyond. Along with Konstantin Tsiolkovsky's earlier work, *The Exploration of Cosmic Space by Means of Reaction Devices*, which was not widely disseminated outside Russia, Goddard's report is regarded as one of the pioneering works of the science of rocketry, and 1750 copies were distributed worldwide. Goddard also sent a copy to individuals who requested one, until his personal supply was exhausted. Smithsonian aerospace historian Frank Winter said that this paper was 'one of the key catalysts behind the international rocket movement of the 1920s and 30s.'

"Goddard described extensive experiments with solid-fuel rocket engines burning high-grade nitrocellulose smokeless powder. A critical breakthrough was the use of the steam turbine nozzle invented by the Swedish inventor Gustaf de Laval. The de Laval nozzle allows the most efficient conversion of the energy of hot gases into forward motion. By means of this nozzle, Goddard increased the efficiency of his rocket engines from two percent to 64 percent and obtained supersonic exhaust velocities of over Mach 7.

"Though most of this work dealt with the theoretical and experimental relations between propellant, rocket mass, thrust, and velocity, a final section, entitled 'Calculation of minimum mass required to raise one pound to an 'infinite' altitude,' discussed the possible uses of rockets, not only to reach the upper atmosphere but to escape from Earth's gravitation altogether. He determined, using an approximate method to solve his differential equations, that a rocket with an effective exhaust velocity of 7000 feet per second and an initial weight of 602 pounds would be able to send a one-pound payload to an infinite height" (Wikipedia).

"Toward the end of [*A Method of Reaching Extreme Altitudes*], Goddard outlined



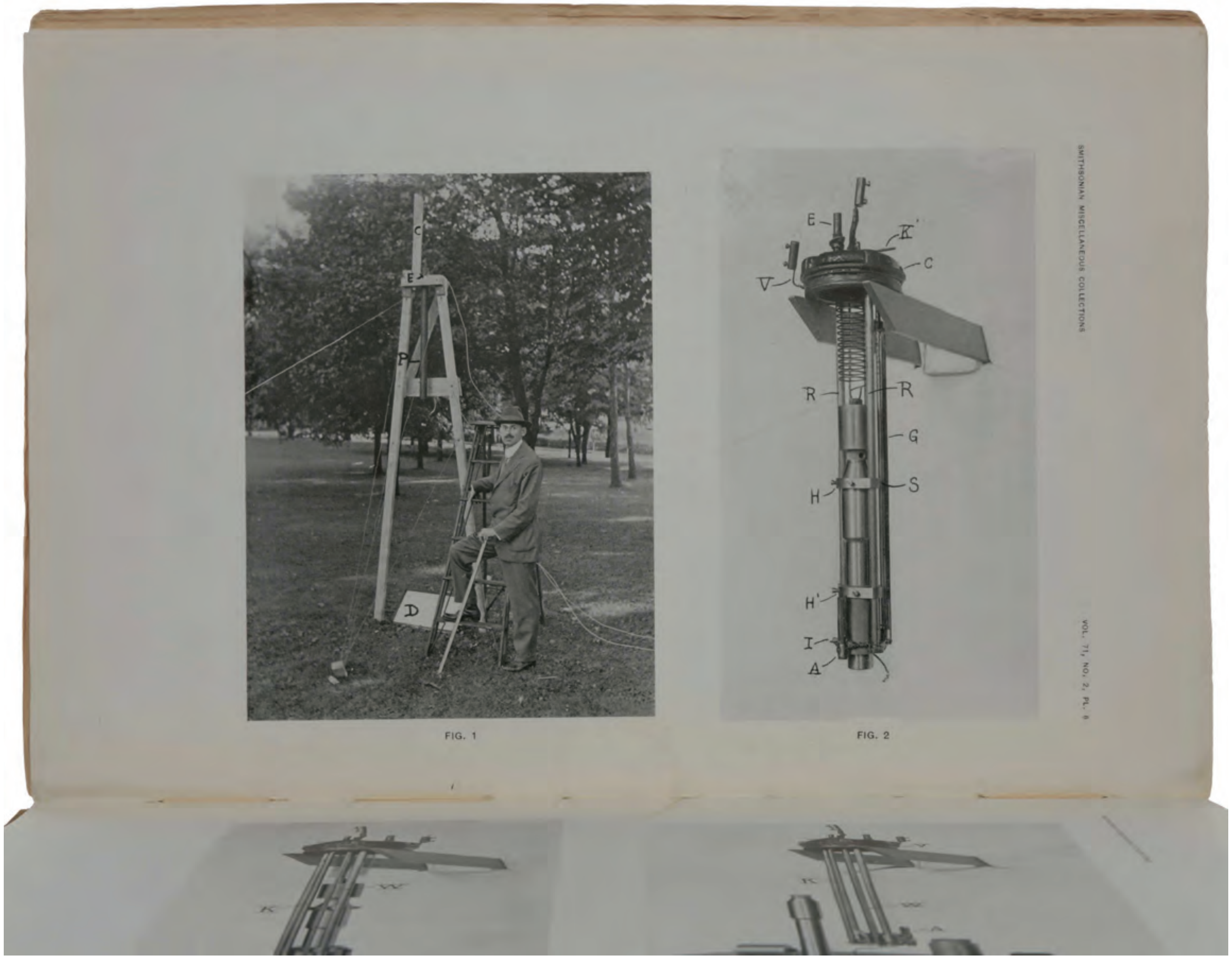
the possibility of a rocket reaching the moon and exploding a load of flash powder there to mark its arrival. The bulk of his scientific report to the Smithsonian was a dry explanation of how he used the $5,000 grant in his research. The press picked up Goddard's scientific proposal about a rocket flight to the moon, however, and created a journalistic controversy concerning the feasibility of such a thing. The resulting ridicule created in Goddard firm convictions about the nature of the press corps, which he held for the rest of his life.

"Goddard's greatest engineering contributions were made during his work in the 1920s and 1930s. He received a total of $10,000 from the Smithsonian by 1927, and through the personal efforts of Charles A. Lindbergh, he subsequently received financial support from the Daniel and Florence Guggenheim Foundation. Progress on all of his work, titled 'Liquid Propellant Rocket Development,' was published by the Smithsonian in 1936.

"Goddard's work largely anticipated in technical detail the later German V-2 missiles, including gyroscopic control, steering by means of vanes in the jet stream of the rocket motor, gimbal-steering, power-driven fuel pumps and other devices. His rocket flight in 1929 carried the first scientific payload, a barometer, and a camera. Goddard developed and demonstrated the basic idea of the 'bazooka' two days before the Armistice in 1918 at the Aberdeen Proving Ground in Maryland. His launching platform was a music rack. In World War II, Goddard again offered his services and was assigned by the U.S. Navy to the development of practical jet assisted take-off and liquid propellant rocket motors capable of variable thrust. In both areas, he was successful...

"Goddard was the first scientist who not only realized the potentialities of missiles and space flight but also contributed directly in bringing them to practical realization. Goddard had a rare talent in both creative science and practical engineering. The dedicated labors of this modest man went largely unrecognized in the United States until the dawn of the Space Age. High honors and wide acclaim, belated but richly deserved, now come to the name of Robert H. Goddard" (https://www.nasa.gov/centers/goddard/about/history/dr_goddard.html).

Both pamphlets were presented by Goddard to Clarence N. Hickman (1889-1981), an early associate and long-time friend of Goddard. They met shortly after Hickman began graduate studies at Clark University in Worcester, Massachusetts in 1917. Goddard, who was then head of the physics department at Clark, had heard about Hickman's reputation for mechanical ability and asked him about a problem he was having with multiple-charge rockets. Hickman solved the problem and Goddard suggested that Hickman continue working on rocket development with him once Hickman completed his degree. After Goddard moved from Worcester to Pasadena's Mount Wilson Observatory in 1918, Hickman went with him and the two continued their work together. It was there that they successfully developed the 'Rocket-Powered Recoilless Weapon', later known as the bazooka. Although further work on the bazooka was suspended after the war ended, the two men continued to work together at Clark's Industrial Research Laboratory during the early 1920s. Hickman joined Bell Laboratories in 1930, and there developed magnetic recording on metal tape. In 1940, Hickman headed Section H of the National Defense Research Committee, an organization created "to coordinate, supervise, and conduct scientific research on the problems underlying the development, production, and use of mechanisms and devices of warfare", and appointed Goddard as a consultant. But Goddard's work was rejected by the US authorities, a decision they came to regret as it eventually fell into the hands of Werner von Braun, who used it to develop the V-1 and V-2 rockets which the Germans used to devastating effect in the latter part of the War. After the War ended, Hickman returned to Bell Labs, retiring in 1950. Parkinson p. 489.


SMITHSONIAN MISCELLANEOUS COLLECTIONS
VOL. 71, NO. 2, PL.
FIG. 1
FIG. 2

'THE GREATEST PAPER IN MATHEMATICAL ECONOMICS THAT WAS EVER WRITTEN'

GÖDEL, Kurt, VON NEUMANN, John (& others). *Thirteen papers by Gödel on the logical foundations of mathematics, together with von Neumann's historic paper on general economic equilibrium, all first editions, in Ergebnisse eines mathematischen Kolloquiums, unter Mitwirkung von Kurt Gödel und Georg Nöbeling. Herausgegeben von Karl Menger. Heft 1-8.* Leipzig & Berlin: B.G. Teubner & F. Deuticke, 1931-1937.

$48,500

Eight separate issues, 8vo (232 x 152 mm), pp. 31, [1, blank]; 38, [2, blank]; 26; 45, [1, blank]; 42; 47, [1, blank]; 61, [1, blank]; 84. Original printed wrappers. A little rusting to staples of the first issue, otherwise a very fine set.

First editions, and a fine complete set in the original printed wrappers, of all eight issues of these proceedings to which Gödel contributed thirteen important papers and remarks on the foundations of logic and mathematics. The last three issues are particularly rare, and are important for containing several seminal papers on mathematical economics, notably von Neumann's "A model of general economic equilibrium" in Heft 8, which "E. Roy Weintraub, current President of the History of Economics Society, described as 'the greatest paper in mathematical economics that was ever written'" (Cabral, p. 126). "In stark contrast to the short eight years of its existence, the colloquium that met in Vienna from 1928 to 1936 had a long lasting influence on economic theory" (Debreu – winner of the

1983 Nobel Prize in Economics). The most important of the Gödel papers are perhaps 'Über Vollständigkeit und Widerspruchsfreiheit' ('On completeness and consistency') in Heft 3 and 'Zur intuitionistischen Arithmetik und Zahlentheorie' ('On intuitionist arithmetic and number theory') in Heft 4. Based on the lecture at the Colloquium required for his *Habilitation*, in the first paper Gödel presented a different approach to his epochal incompleteness theorem, published just a few months earlier in *Monatshefte für Mathematik*: instead of Russell's theory of types, in the present version he used Peano's axioms for the natural numbers; this soon became the standard approach. In the second paper, Gödel proved that intuitionist mathematics is no more certain, or more consistent, than ordinary mathematics. "By invitation, in October 1929 Gödel began attending Menger's mathematics colloquium, which was modelled on the Vienna Circle. There in May 1930 he presented his dissertation results, which he had discussed with Alfred Tarski three months earlier, during the latter's visit to Vienna. From 1932 to 1936 he published numerous short articles in the proceedings of that colloquium (including his only collaborative work) and was co-editor of seven of its volumes. Gödel attended the colloquium quite regularly and participated actively in many discussions, confining his comments to brief remarks that were always stated with the greatest precision" (*DSB* XVII: 350). Von Neumann also attended the colloquium in the early years. Although subsets of this collection occasionally appear on the market, complete sets of all eight issues are very rare.

Working under Hans Hahn, Karl Menger (1902-85) received his PhD from the University of Vienna in 1924 and accepted a professorship there three years later. "During the academic year 1928/29, several students asked Menger to direct a Mathematical Colloquium, somewhat analogous to the philosophically motivated Vienna Circle ... This Colloquium, which met on alternate Tuesdays during semester time, had a flexible agenda including lectures by members or invited guests, reports on recent publications and discussion of unsolved problems. Menger kept a record of these meetings, which he published, regularly in November of the following year, under the title 'Ergebnisse eines mathematischen Kolloquiums' …

"Gödel had entered the university in 1924, and Menger first met him as the youngest and most silent member of the Vienna Circle. In 1928, Gödel started working on Hilbert's program for the foundation of mathematics, and in 1929 he succeeded in solving the first of four problems of Hilbert, proving in his PhD thesis (under Hans Hahn) that first order logic is complete: Any valid formula could be derived from the axioms.

"At that time Menger, who was greatly impressed by the Warsaw mathematicians, had invited Alfred Tarski to deliver three lectures at the Colloquium. Gödel, who had asked Menger to arrange a meeting with Tarski, soon took a hand in running the Colloquium and editing its Ergebnisse.

"Menger was visiting the USA [in 1931] when Gödel discovered the incompleteness theorem and used it to refute the remaining three of Hilbert's conjectures. He learned by letter that Gödel had lectured in the Colloquium 'On Completeness and Consistency'. This was the lecture required for Gödel's habilitation. The paper required for the same procedure, 'On the undecidability of certain propositions in the Principia Mathematica,' had been published in Hahn's 'Monatshefte'. In his Colloquium lecture, Gödel presented a simpler approach. Instead of Russell's theory of types, he used Peano's axioms for the natural numbers. This soon became the standard approach …

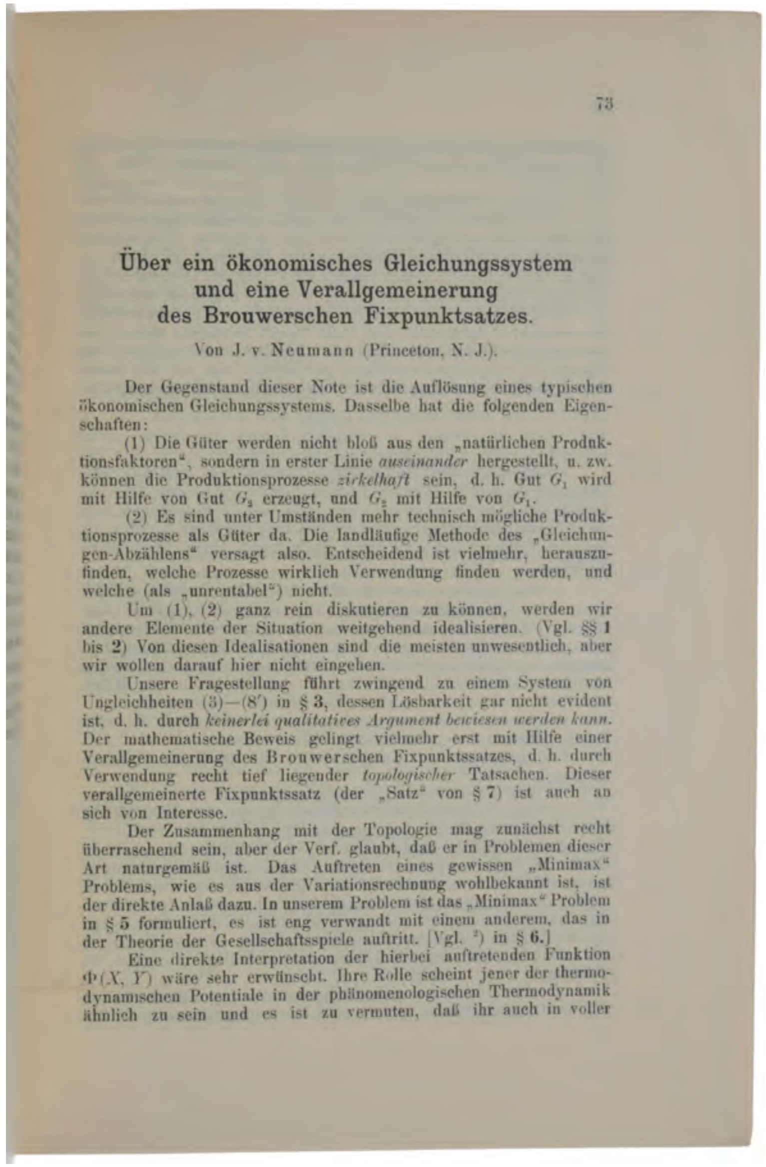
"Menger was particularly fond of Gödel's results on intuitionism. These vindicated his own tolerance principle. Specifically, Gödel proved that intuitionist mathematics is no more certain, or more consistent, than ordinary mathematics

(‘Zur intuitionistischen Arithmetik und Zahlentheorie’, Heft 4) … Menger brought Oswald Veblen to the Colloquium when Gödel lectured on this result. Veblen, who had been primed by John von Neumann, was tremendously impressed by the talk and invited Gödel to the Institute for Advanced Study during its first full year of operation: A signal honour that proved a blessing in Gödel’s later years” (Karl Sigmund in *Selecta Mathematica*, pp. 14).

The Gödel papers contained in these five volumes are as follows, with summaries based on the Annotated Bibliography of Gödel by John Dawson:

(1) ‘Ein Spezialfall des Entscheidungsproblems der theoretischen Logik,’ Heft 2, pp. 27-28. This undated contribution was not presented to a regular meeting of the colloquium, but appeared among the *Gesammelte Mitteilungen* for 1929/30. In the context of the first-order predicate calculus without equality, Gödel describes an effective procedure for deciding whether or not a certain formula is satisfiable; the procedure is related to the method used in [his dissertation *Die Vollstandigkeit der Axiome des logischen Funktionenkalküls*] to establish the completeness theorem.

(2) ‘Über Vollständigkeit und Widerspruchsfreiheit,’ Heft 3, pp. 12-13. Closely related to [Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I, 1931], this paper notes extensions of the incompleteness theorems to a wider class of formal systems. The system considered in his 1931 paper is based on *Principia Mathematica* and allows variables of all finite types. Here Gödel observes that any finitely-axiomatizable, omega-consistent formal system S with just substitution and implication (modus ponens) as rules of inference will possess undecidable propositions whenever S extends the theory Z of first-order Peano arithmetic plus the schema of definition by recursion; and indeed, that the same is true of infinite axiomatizations so long as the class of Gödel numbers of axioms, together with the relation



73

Über ein ökonomisches Gleichungssystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes.

Von J. v. Neumann (Princeton, N. J.).

Der Gegenstand dieser Note ist die Auflösung eines typischen ökonomischen Gleichungssystems. Dasselbe hat die folgenden Eigenschaften:

(1) Die Güter werden nicht bloß aus den „natürlichen Produktionsfaktoren“, sondern in erster Linie *auseinander* hergestellt, u. zw. können die Produktionsprozesse *zirkelhaft* sein, d. h. Gut G_1 wird mit Hilfe von Gut G_2 erzeugt, und G_2 mit Hilfe von G_1.

(2) Es sind unter Umständen mehr technisch mögliche Produktionsprozesse als Güter da. Die landläufige Methode des „Gleichungen-Abzählens“ versagt also. Entscheidend ist vielmehr, herauszufinden, welche Prozesse wirklich Verwendung finden werden, und welche (als „unrentabel“) nicht.

Um (1), (2) ganz rein diskutieren zu können, werden wir andere Elemente der Situation weitgehend idealisieren. (Vgl. §§ **1** bis **2**) Von diesen Idealisationen sind die meisten unwesentlich, aber wir wollen darauf hier nicht eingehen.

Unsere Fragestellung führt zwingend zu einem System von Ungleichheiten (3)—(8′) in § **3**, dessen Lösbarkeit gar nicht evident ist, d. h. durch *keinerlei qualitatives Argument bewiesen werden kann*. Der mathematische Beweis gelingt vielmehr erst mit Hilfe einer Verallgemeinerung des Brouwerschen Fixpunktssatzes, d. h. durch Verwendung recht tief liegender *topologischer* Tatsachen. Dieser verallgemeinerte Fixpunktssatz (der „Satz“ von § **7**) ist auch an sich von Interesse.

Der Zusammenhang mit der Topologie mag zunächst recht überraschend sein, aber der Verf. glaubt, daß er in Problemen dieser Art naturgemäß ist. Das Auftreten eines gewissen „Minimax“ Problems, wie es aus der Variationsrechnung wohlbekannt ist, ist der direkte Anlaß dazu. In unserem Problem ist das „Minimax“ Problem in § **5** formuliert, es ist eng verwandt mit einem anderem, das in der Theorie der Gesellschaftsspiele auftritt. [Vgl. [2]) in § **6**.]

Eine direkte Interpretation der hierbei auftretenden Funktion $\Phi(X, Y)$ wäre sehr erwünscht. Ihre Rolle scheint jener der thermodynamischen Potentiale in der phänomenologischen Thermodynamik ähnlich zu sein und es ist zu vermuten, daß ihr auch in voller of immediate consequence under the rules of inference, is definable and decidable in Z.

(3) 'Eine Eigenschaft der Realisierungen des Aussagenkalküls,' Heft 3, pp. 20-21. In answer to a question of Menger, Gödel shows that given an arbitrary realization of the axioms of the propositional calculus in a structure with operations interpreting the connectives ~ and , the elements of the structure can always be partitioned into two disjoint classes behaving exactly like the classes of true and false propositions.

(4) Untitled remark following W. T. Parry Ein Axiomensystem für eine neue Art von Implikation (analytische Implikation), Heft 4, p. 6. During the 33rd session of the colloquium, November 7, 1931, the American visitor Parry introduced an axiom system for "analytic implication," a concept of logical consequence entailing the unprovability of A -> B whenever B contains a propositional variable not occurring in A. Following Parry's demonstration (via multi-valued truth tables) of this characteristic property, Gödel suggested that a completeness proof be sought for Parry's axioms, while noting that the question whether Heyting's propositional calculus could be realized using only finitely many truth values was then open. On p. 4 of this same issue, an article by Alt ("Zur Theorie der Krümmung") mentions an unpublished suggestion by Gödel.

(5) 'Über Unabhängigkeitsbeweise im Aussagenkalkül,' Heft 4, pp. 9-10. To Hahn's question, "Can every independence proof for statements of the propositional calculus be carried out by means of finite multi-valued truth tables?" Gödel provides a negative answer.

(6) 'Über die metrische Einbettbarkeit der Quadrupel des R^3 in Kugelflächen,' Heft 4, pp. 16-17.

(7) 'Über die Waldsche Axiomatik des Zwischenbegriffes,' Heft 4, pp. 17-18. Gödel's contributions to geometry have been overlooked by bibliographers. Both (6) and (7) formed part of the 42nd colloquium, held February 18, 1932. In the former, Gödel answers a question raised by Laura Klanfer at the 37th colloquium, December 2, 1931: he shows that whenever a quadruple of points in a metric space is isometric to four noncoplanar points of R^3, the quadruple is isometric, under the geodesic metric, to four points on the surface of a sphere. (The corresponding result for the usual metric on R^3 is trivial.) In the second paper Gödel reformulates Wald's axiomatization of the betweenness concept as a theorem about triples of real numbers, assigning the triple of distances (*ab, bc, ac*) to a triple (*a, b, c*) in a given metric space. The theorem states that *b* lies between *a* and *c* in the sense of Menger if and only if (*ab, bc, ac*) lies in that part of the plane $x + y = z$ for which each of the four quantities x, y, z, and $(x + y - z)(x - y + z)(-x + y + z)$ is nonnegative.

(8) 'Zur Axiomatik der elementargeometrischen Verknüpfungsrelationen,' Heft 4, p. 34. Only two brief comments were published from the discussion with the above title held as the 51st colloquium, May 25, 1932. In translation, Gödel's remark reads in full: "[Some]one should investigate the system of all those statements about fields that in normal form contain no existential prefixes. The concepts of point and line, which are definable by application of existential prefixes (e.g., a point is an element for which there exists no nonempty element that is a proper part of it), are undefinable in this more restricted system."

(9) 'Zur intuitionistischen Arithmetik und Zahlentheorie,' Heft 4, pp. 34-38. In this short but important paper, Gödel shows that although the intuitionistic propositional calculus is customarily regarded as a subsystem of the classical, by a different translation the reverse is true, not only for the propositional calculus but for arithmetic and number theory as well. (Independently and slightly later the same result was discovered by Gentzen and Bernays. Specifically, with each formula A of Herbrand's system of arithmetic Gödel associates a translation A' in an extension of Heyting's arithmetic, such that A' is intuitionistically provable whenever A is classically provable. Since it

provides an intuitionistic consistency proof for classical arithmetic, Gödel's translation gives classical mathematicians grounds for maintaining that insofar as arithmetic is concerned, intuitionistic qualms amount to "much ado about nothing"; for intuitionists, however, the issue is not so much consistency as it is matters of proper interpretation and methodology.

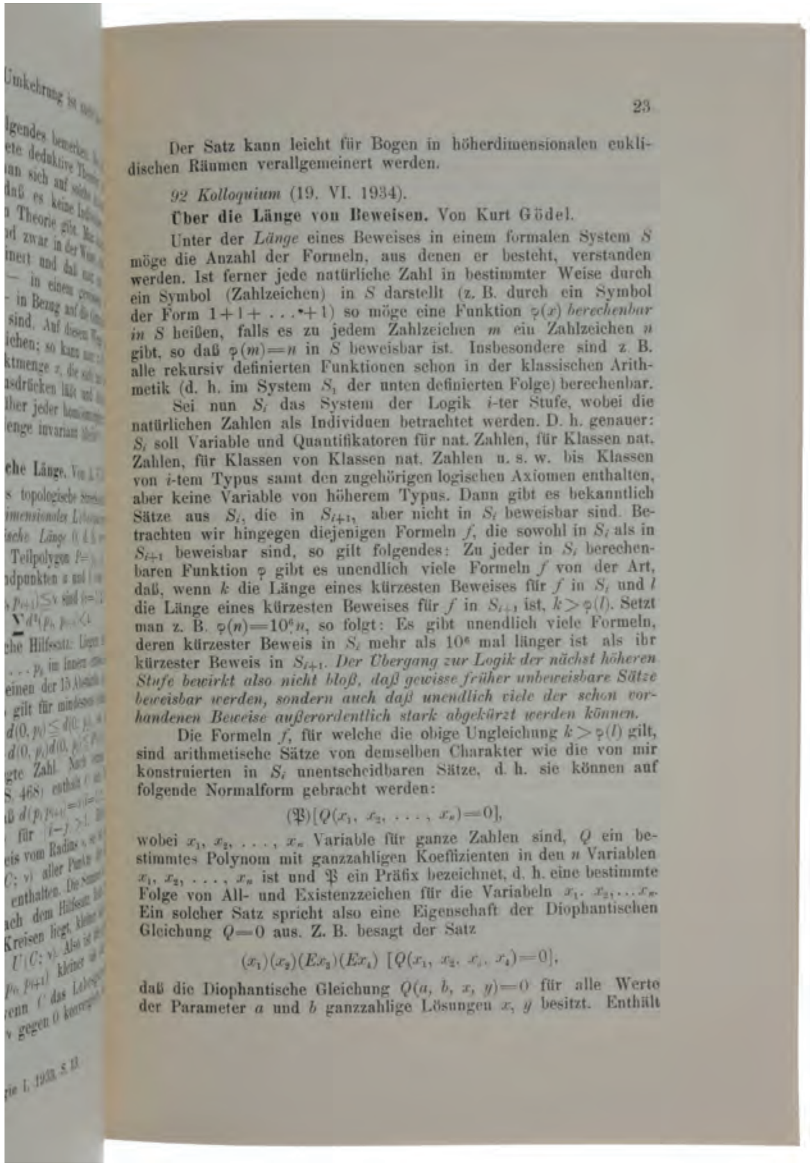
(10) 'Eine Interpretation des intuitionistischen Aussagenkalküls,' Heft 4, pp. 39-40. Gödel shows that Heyting's propositional calculus can be given a natural classical interpretation.

(11) Reprint of 'Zum intuitionistischen Aussagenkalkül' [*Anzeiger der Akademie der Wissenschaften in Wien*, vol. 69, 1932, pp. 65-66], Heft 4, p. 40. Though considerably more accessible than the original printing, this reprint has not been cited in earlier bibliographies. The text is identical to the original except for the addition of an opening clause attributing the question to Hahn.

(12) 'Bemerkung über projektive Abbildungen,' Heft 5, p. 1. This brief note, part of the 53rd colloquium, November 10, 1932, is devoted to proving that every one-to-one mapping of the real projective plane into itself that carries straight lines into straight lines is a collineation.

(13) 'Diskussion über koordinatenlose Differentialgeometrie' (with K. Menger and A. Wald), Heft 5, pp. 25-26. Gödel's only joint paper, previously uncited. A single, mildly technical result is established, whose aim is to show that so-called "volume determinants" are appropriate for giving a coordinate-free characterization of Gaussian surfaces. The paper is intended as a contribution to Menger's program for making precise, in a coordinate-free way, the assertion that Riemannian spaces behave locally like Euclidean spaces.

Three further parts of the *Ergebnisse* were published (1934-37). They are rarely found, and were probably published in much smaller numbers than the first five parts owing to the political turmoil in Vienna which began with the failed Nazi coup in July 1934. They are important for the seminal papers on mathematical



23

Der Satz kann leicht für Bogen in höherdimensionalen euklidischen Räumen verallgemeinert werden.

92 Kolloquium (19. VI. 1934).

Über die Länge von Beweisen. Von Kurt Gödel.

Unter der *Länge* eines Beweises in einem formalen System S möge die Anzahl der Formeln, aus denen er besteht, verstanden werden. Ist ferner jede natürliche Zahl in bestimmter Weise durch ein Symbol (Zahlzeichen) in S darstellt (z. B. durch ein Symbol der Form $1+1+\ldots+1$) so möge eine Funktion $\varphi(x)$ *berechenbar in* S heißen, falls es zu jedem Zahlzeichen m ein Zahlzeichen n gibt, so daß $\varphi(m)=n$ in S beweisbar ist. Insbesondere sind z. B. alle rekursiv definierten Funktionen schon in der klassischen Arithmetik (d. h. im System S_1 der unten definierten Folge) berechenbar.

Sei nun S_i das System der Logik i-ter Stufe, wobei die natürlichen Zahlen als Individuen betrachtet werden. D. h. genauer: S_i soll Variable und Quantifikatoren für nat. Zahlen, für Klassen nat. Zahlen, für Klassen von Klassen nat. Zahlen u. s. w. bis Klassen von i-tem Typus samt den zugehörigen logischen Axiomen enthalten, aber keine Variable von höherem Typus. Dann gibt es bekanntlich Sätze aus S_i, die in S_{i+1}, aber nicht in S_i beweisbar sind. Betrachten wir hingegen diejenigen Formeln f, die sowohl in S_i als in S_{i+1} beweisbar sind, so gilt folgendes: Zu jeder in S_i berechenbaren Funktion φ gibt es unendlich viele Formeln f von der Art, daß, wenn k die Länge eines kürzesten Beweises für f in S_i und l die Länge eines kürzesten Beweises für f in S_{i+1} ist, $k>\varphi(l)$. Setzt man z. B. $\varphi(n)=10^6 n$, so folgt: Es gibt unendlich viele Formeln, deren kürzester Beweis in S_i mehr als 10^6 mal länger ist als ihr kürzester Beweis in S_{i+1}. *Der Übergang zur Logik der nächst höheren Stufe bewirkt also nicht bloß, daß gewisse früher unbeweisbare Sätze beweisbar werden, sondern auch daß unendlich viele der schon vorhandenen Beweise außerordentlich stark abgekürzt werden können.*

Die Formeln f, für welche die obige Ungleichung $k>\varphi(l)$ gilt, sind arithmetische Sätze von demselben Charakter wie die von mir konstruierten in S_i unentscheidbaren Sätze, d. h. sie können auf folgende Normalform gebracht werden:

$$(\mathfrak{P})[Q(x_1, x_2, \ldots, x_n)=0],$$

wobei $x_1, x_2, \ldots, x_n$ Variable für ganze Zahlen sind, Q ein bestimmtes Polynom mit ganzzahligen Koeffizienten in den n Variablen $x_1, x_2, \ldots, x_n$ ist und $\mathfrak{P}$ ein Präfix bezeichnet, d. h. eine bestimmte Folge von All- und Existenzzeichen für die Variabeln $x_1, x_2, \ldots x_n$. Ein solcher Satz spricht also eine Eigenschaft der Diophantischen Gleichung $Q=0$ aus. Z. B. besagt der Satz

$$(x_1)(x_2)(Ex_3)(Ex_4)\ [Q(x_1, x_2, x_3, x_4)=0],$$

daß die Diophantische Gleichung $Q(a, b, x, y)=0$ für alle Werte der Parameter a und b ganzzahlige Lösungen x, y besitzt. Enthält economics they contain, by Schlesinger, Wald (two papers), and von Neumann.

"John von Neumann's famous 1937 paper, the last article in the final number of the *Ergebnisse* (pp. 73-83), is "Über ein ökonomisches Gleichungssystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes" ("On a System of Economic Equilibrium and the Generalization of Brouwer's Fixed Point Theorem"). "Although von Neumann has only three publications that can directly be called contributions to economics [the other two being his paper and book with Morgenstern on game theory], he had enormous influence on the subject … [Von Neumann's] expanding economy model consisted of two parts: first the input-output equilibrium model that permits expansion; and second the fixed point theorem. The linear input-output model is a precursor of the Leontief model of linear programming as developed by Kantorovich and Dantzig, and of Koopman's activity analysis. This paper, together with A. Wald (1935) [also in this volume] raised the level of mathematical sophistication used in economics enormously. Many current younger economists are high-powered applied mathematicians, in part, because of the stimulus of von Neumann's work … His influence will persist for decades and even centuries in economics" (*New Palgrave*, p. 644).

The great importance of von Neumann's paper has been emphasized by Richard Goodwin. "The greatest single intellectual mistake in my career occurred when Schumpeter came to me in 1938 or '39 and asked me to report on a very important new publication: the von Neumann paper given at the Menger seminar...I rashly judged it to be totally unrealistic...[and] reported back to Schumpeter that it was no more than a piece of mathematical ingenuity, failing to see that it contained two aspects close to Schumpeter's heart - a rigorous solution to Walras's central problem and a demonstration that the rate of profit arose from growth not a quantity of capital...I found no reference [in Schumpeter's *History*] to what now appears to me to be one of the great seminal works of this century, the omission being possibly the result of my own blindness" (Goodwin, "Personal Perspective on Mathematical Economics", *Banca Nazionale del Lavoro Quarterly Review*, 152 (1985), pp. 3–13).

The three remaining economics papers in the *Ergebnisse*, by Schlesinger and Wald, are also highly significant.

Karl SCHLESINGER, 'Über die Produktionsgleichungen der ökonomischen Wertlehre', Heft 6, pp. 10-11
Abraham WALD, 'Über die eindeutige positive Lösbarkeit der neuen Produktionsgleichungen', Heft 6, pp. 12-18; and *ibid*., 2. Mitteilung, Heft 7, pp. 1-6.

"The starting signal for the development that would bring a profound transformation to mathematical economics was given by Schlesinger. In the first paper on economics of the Colloquium presented on March 19, 1934 … he raised a question aimed at the center of Walrasian theory… Schlesinger suggested a modification of Leon Walras's (and Gustav Cassel's) equations that soon turned out to be essential … Immediately after that correct formulation was given, Wald proved the existence of a GE [general equilibrium] (also on March 19, 1934), and, at the next session of the Colloquium, on November 6, 1934, established existence under much weaker conditions. His two papers, providing the first proofs of existence for a GE, marked an important moment in the history of mathematical economics" (Debreu, pp. 1-2).

The Vienna Colloquium ended when Menger moved to the US in 1937 to take up a position at the University of Notre Dame. There he reinstated the Colloquium; its proceedings were published as *Reports of a Mathematical Colloquium* (Notre Dame: University Press, 1939-48).

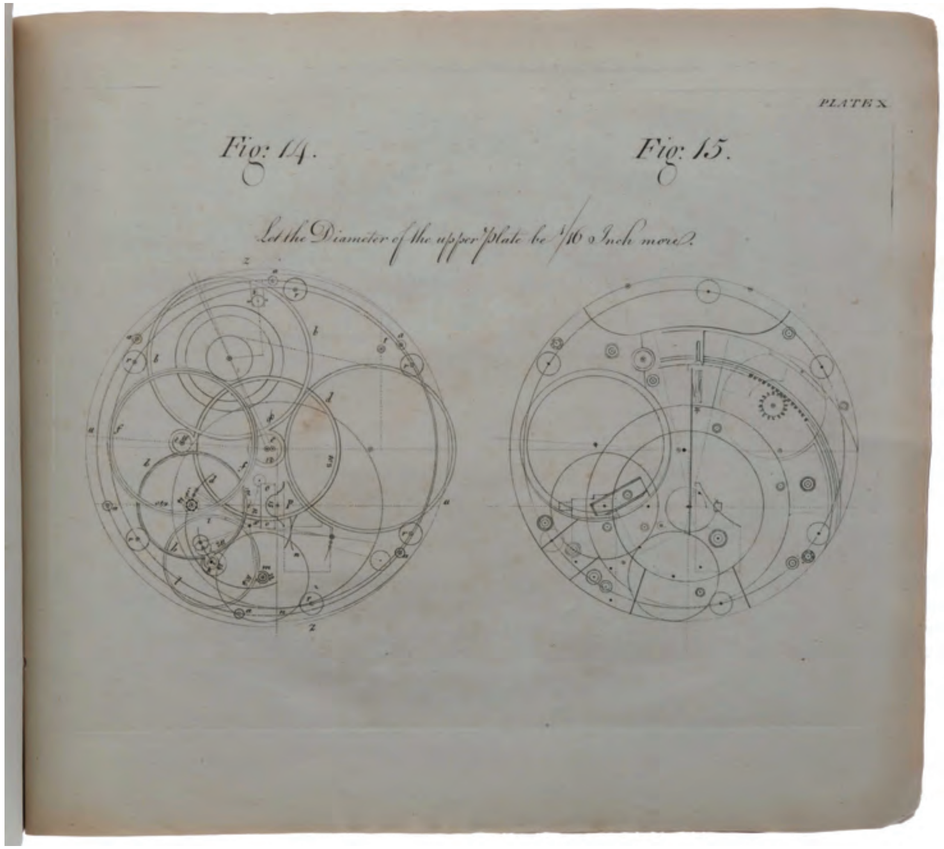
Cabral, 'John von Neumann's contribution to economic science,' *International Social Science Review* 78 (2003), pp. 126-137. Dawson, The Published Work of Kurt Gödel: An Annotated Bibliography, *Notre Dame Journal of Formal Logic* 24 (1983), 255-84; Debreu, 'Foreword: Economics in a Mathematics Colloquium,' in: *Karl Menger*, Dierker & Sigmund, eds., 1998. Menger, *Selecta Mathematica*, Springer, 2002. Punzo, 'Von Neumann and Karl Menger's Mathematical Colloquium,' pp. 29–68 in: *John von Neumann and Modern Economics*, Dore, Chakravarty & Goodwin, eds., 1989.

THE VERY RARE INDIA PAPER ISSUE

HARRISON, John, [and Nevil MASKELYNE]. *The Principles of Mr. Harrison's Time-Keeper, with plates of the same. Published by Order of the Commissioners of Longitude.* London: W. Richardson and S. Clarke for John Nourse and Mess. Mount and Page, 1767.

$135,000

Oblong 4to (282 x 225 mm), pp. xvii, [1, blank] (v-viii: Preface; ix-xvii: 'Notes taken at the discovery of Mr. Harrison's time-keeper', both by Maskelyne), 19-31 (19-21: general account of H4; 22-31: description of the plates, both by Harrison), with 10 engraved plates printed on India paper, one double-page folding. Contemporary marbled wrappers, uncut. An excellent copy, preserved in a morocco-backed box.



First edition, one of the very few copies with the plates printed on India paper, of the "description of the famous solution to the centuries-old world-wide problem of finding the longitude" (Grolier/Horblit). "Harrison's chronometer not only supplied navigators with a perfect instrument for observing the true geographical position at any moment during their voyage, but also laid the foundation for the compilation of exact charts of the deep seas and the coastal waters of the world … There has possibly been no advance of comparable importance in aids to navigation until the introduction of radar" (PMM 208). In 1714 the Board of Longitude offered a reward of £20,000, a colossal sum at the time, to anyone who could find a reliable and accurate method for determining longitude at sea. In 1730 the clockmaker John Harrison (1693-1776) completed a manuscript describing some of his chronometrical inventions, including a chronometer "accurate enough to measure time at a steady rate over long periods, thus permitting the measurement of longitude by comparison of local solar time with an established standard time" (Norman). On the strength of his descriptions, Harrison obtained a loan from George Graham, a leading maker of clocks and watches, for the construction of his timekeeper. After numerous attempts, most of which either Harrison himself or his son William tested on ocean voyages, Harrison succeeded in constructing a chronometer 'H4' that was both accurate and convenient in size. Following successful tests on voyages to the West Indies in 1761 and 1764, Harrison felt that he had a right to the prize, but the Board of Longitude hedged, insisting on a demonstration and full written description of his invention. The demonstration took place on 22 August 1765, in the presence of the astronomer-royal Nevil Maskelyne and a six-member committee of experts appointed by the Board. The results were written up and published in this pamphlet by Maskelyne, along with Harrison's own description of his timekeeper. Officially the Board intended *Principles* to enable other clockmakers to construct H4, but it "was both incomplete of enough information to allow the duplication of the watch … and contain[ed] some accidentally-on-purpose errors. This was likely done as much to help maintain the hard-won knowhow of its inventor, as well as to protect any military advantage, given the importance of the H4 to maritime navigation" (Lake). Maskelyne's Preface explains the reason for this special issue of *Principles*: "for the sake of the curious, and particularly artists who may be desirous to construct other watches after the model of Mr. Harrison's, I have caused a few impressions of the plates to be taken off upon India paper; which, if it be made only a little damp, by being put for a few minutes between two wet sheets of paper, will receive the impression from the plates perfect, and will not shrink at all in the drying" (p. vii). Since this issue was also printed on oblong sheets, rather than the regular 4to, the plates remain unfolded, save the considerably larger 7th plate. Only one copy of this issue has sold at auction since the Frank S. Streeter copy (Christie's, 16 April, 2007, lot 254, $228,000), and according to Christie's no other copy had sold for 30 years previously.

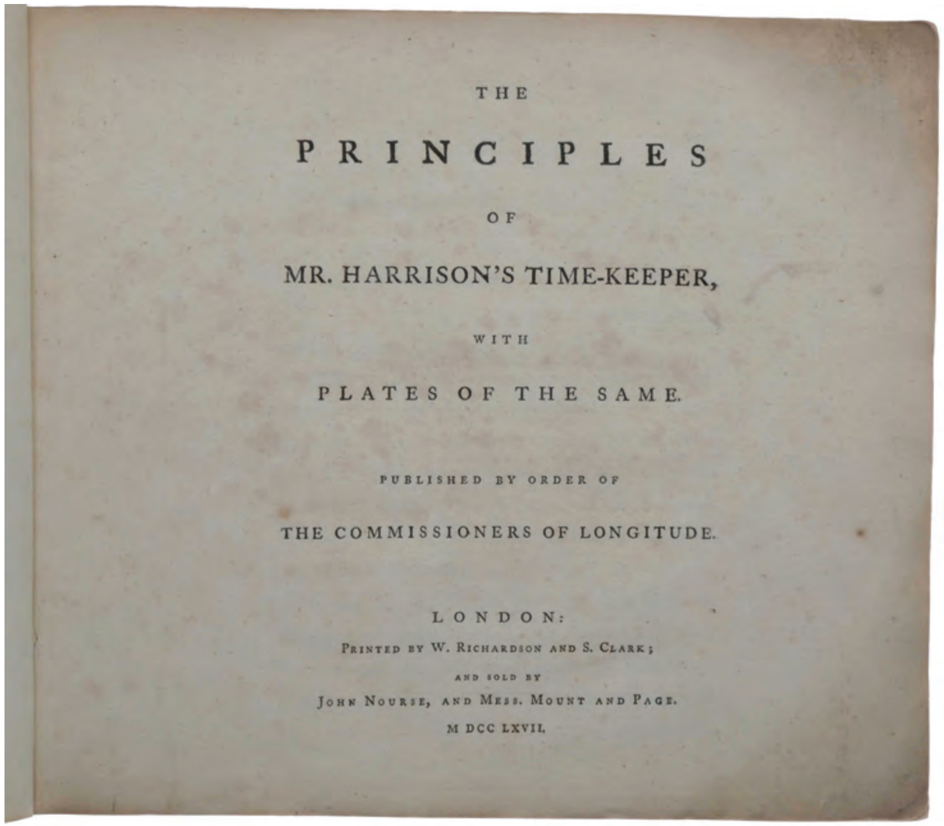
"In the early 1700s, European monarchies aspired to power by building world-spanning networks of colonies and commercial ventures. As a result, the merchant fleets and navies that connected and protected these assets were critically important. Eighteenth-century sailors led dangerous lives, not least because they seldom knew their exact location on the open ocean. Although navigators readily determined latitude, or north-south position, by estimating the height of certain stars at their zenith, they could not determine longitude. This failure caused shipwrecks that killed thousands of mariners and lost cargoes worth fortunes. Several countries offered immense financial rewards for a solution to the problem; Britain promised £20,000 (several million dollars in today's currency) for a way to establish longitude to within half a degree (30 nautical miles at the equator) after a journey from England to the West Indies. To judge proposed solutions, the crown established a Board of Longitude, made up of the Astronomer Royal, various admirals and mathematics professors, the Speaker of the House of Commons and 10 members of Parliament.

"In effect, determining longitude depended on knowing the difference between local time and the time in Greenwich, site of the Royal Observatory. In principle, if a ship had a clock keeping Greenwich time, the navigator could measure the angle of the Sun to note local noon and compare it to the clock. If the clock read 2 p.m., his longitude was two hours, or 30 degrees, west of Greenwich. The problem lay in finding a clock reliable enough to keep time during the long voyages of that era. The best pendulum clocks of the day were accurate enough, but were useless on a heaving ship at sea. Alternately, a less reliable clock might be used if some means could be devised to correct it frequently. In practice this meant an astronomical method, the best of which became known as the method of lunar distances, in reference to the fact that the Moon's orbit causes it to continually change position in the sky. For example, a new moon, which appears close to the Sun, will have moved 180 degrees by the time it becomes a full moon two weeks later. The idea was for astronomers to provide tables of this angle between Moon and Sun (or Moon and selected stars in the night sky) as a function of Greenwich time. A measurement of this angle every few days would provide a correction to the mechanical clock. This scheme had two drawbacks: The first was that, at least initially, astronomers could not accurately predict the Moon's motion; the second was that the mathematical calculations required of the mariner were very complex—they took hours, and errors were common.

"The chronometer and the method of lunar distances each had their proponents, and the solution of the longitude problem came down to a bitter battle between two Englishmen: John Harrison, a self-educated machinist who set out to make an accurate clock, and Nevil Maskelyne, astronomer and scion of the Church of England, who brought the method of lunar distances to fruition.

"John Harrison was born in Yorkshire and followed his father's trade as a carpenter. This led to an interest in wooden (pendulum) clocks, which he became adept at making. His isolation from other clockmakers during this formative time was fortuitous—when most clockmakers learned that the prize required their clocks to maintain a rate constant to within three seconds per day for six weeks at sea, they regarded the task as impossible. Harrison saw it as a challenge, and developed the technical advances that justified his confidence, including the so-called gridiron pendulum (which used metals with different expansion coefficients to overcome temperature sensitivity), a nearly frictionless gear system and a ratcheted spring that kept the clock running while being rewound.

"By 1728 the longitude prize had been open for 14 years without a serious contender. Young Harrison journeyed to London to learn about the award and


THE

PRINCIPLES

OF

MR. HARRISON'S TIME-KEEPER,

WITH

PLATES OF THE SAME.

PUBLISHED BY ORDER OF

THE COMMISSIONERS OF LONGITUDE.

LONDON:

PRINTED BY W. RICHARDSON AND S. CLARK;

AND SOLD BY

JOHN NOURSE, AND MESS. MOUNT AND PAGE.

M DCC LXVII.

met Edmond Halley (1656-1742), the Astronomer Royal. Halley treated him cordially and suggested that Harrison consult George Graham (1673-1751), one of London's leading clockmakers. Harrison and Graham took an immediate liking to each other—they talked for 10 hours that day, leaving Graham so impressed that he offered to lend Harrison money (without security or interest) to develop the young inventor's ideas.

"Seven years passed before Harrison offered a solution to the problem: the clock later known as H1. It was cumbersome and heavy, with jutting arms and counterweights, but a small committee of the Royal Society, including Halley and Graham, declared it a masterpiece of ingenuity and urged submission to the Board of Longitude. This body, ever skeptical after years of spurious claims, proposed a short sea trial before attempting the expensive, official crossing of the Atlantic. So Harrison and H1 embarked on a run to Lisbon and back. The numerical result of the clock's performance has not survived, but the captain was very impressed. The ship's navigator admitted that on the return voyage, just before the English coast was sighted, his own calculations put them more than 90 miles offshore, while Harrison maintained that no, they were just about there.

"Although the voyage didn't immediately lead to a trans-Atlantic test, it was productive for Harrison, who identified several potential improvements to H1. With these in mind, he withdrew his initial submission and set about building a second clock (H2). The Board of Longitude, impressed by H1, awarded Harrison £500 toward development expenses. He completed H2 three years later and spent two more years testing it on land. By 1741, testing was complete, but England was at war with France and her allies. No one was prepared to risk a sea trial of H2 and possible capture by the French. As it turned out, H2 was never tested at sea.

"While waiting for the war to end, Harrison built a third clock, with the Board of Longitude contributing another £500 to meet his costs. But this time, Harrison's expertise failed him. H3 never worked to Harrison's satisfaction, and he abandoned it after five years. Taking a different tack, he proposed in 1746 to build two more clocks. Of these, H4 would be his masterpiece. It took another 13 years to complete, but was quite different from its predecessors. Instead of being all angles and arms, it appeared as a beautifully encased pocket watch, albeit five inches in diameter. After two years of testing the Board accepted it for the full trans-Atlantic test.

"Harrison, now 68, left H4 in the care of his son William for the long voyage in 1761. The trial ended upon reaching Jamaica, where, amazingly, H4 reported the longitude with less than two miles of error. Back in England, the overjoyed Harrisons awaited their award of £20,000. Then everything started to go wrong. The Board of Longitude refused to believe the test results were not just a stroke of luck and demanded a second trial. The Harrisons protested vehemently. They had met the terms of the prize and now they wanted it. Uproar ensued, and to quell it, Parliament offered William Harrison £5,000 for results so far, in return for full disclosure of the construction details of H4. William refused this partial award and decided to attempt a second trial in 1764, this time to Barbados.

"The Reverend Nevil Maskelyne now enters the story. He had no particular influence prior to this stage of Harrison's odyssey, but the Board of Longitude made a decision that quickly embroiled him in the controversy. Although Maskelyne was not then a member of the Board of Longitude, he was a Fellow of Trinity College, Cambridge, and a Fellow of the Royal Society. For some years he had maintained a strong interest in, and advocacy of, the method of lunar distances for determining longitude. Furthermore, as Tobias Mayer (1723-62) in Germany had recently resolved the problem of predicting the motion of the Moon, the method was ripe for testing. Thus, when the Board of Longitude asked

Maskelyne to join the voyage to Barbados, primarily to establish the longitude of the capital, Bridgetown, by observation of Jupiter's satellites, he was also to test the method of lunar distances and its accuracy compared to Harrison's H4 clock.

"The results were announced at a memorable meeting of the Board of Longitude in early 1765. H4 had done it again, producing the Bridgetown longitude with less than ten miles of error after a journey of more than 5,000 miles. The rival method of lunar distances fared slightly worse, yielding the result to "better than 30 miles." By way of explanation, four of the ship's officers at the meeting stated that their calculations were a product of Maskelyne's instructions, and, by implication, subject to their own inexperience. In any case, since the lunar distance method depended on tables that only Maskelyne could calculate, the method was not yet ready to claim the prize.

"The Board of Longitude accepted the result of the Barbados trial of H4, but they remained unconvinced that the instrument was not just a fluke—a one-off that might never be replicated. Parliament passed a new Act that yielded £10,000 to Harrison but withheld the remaining half of the prize until he met a series of conditions. According to Parliament's terms, Harrison had to reveal in writing exactly how the watch had been made, including full drawings of each part and explanation of every detail to a select team of watchmakers. Then he needed to wait for these persons to manufacture similarly accurate watches themselves. Moreover, Harrison was compelled to relinquish H4 to the Astronomer Royal for long-term checking of its accuracy.

"Here was the rub: Two Astronomers Royal had died in rapid succession, and no sooner had Maskelyne returned from Barbados than the King [George III, 1738-1820] appointed him to the position; the appointment automatically put him on the Board of Longitude. As a result, Harrison's nemesis not only became

an influential voice on the Board of Longitude but also took charge of checking his best clock. Not surprisingly, the resulting report on H4 was entirely negative. It seems Maskelyne prevaricated over the meaning of "accuracy" to condemn Harrison's creation. In this era, all clocks gained or lost time at some rate, but so long as that rate was constant and known, one could derive an accurate time. Maskelyne refused to allow these corrections.

"While waiting for the copies of H4 to be completed, Harrison spent three precious years building H5—now approaching 80, he was becoming increasingly desperate. As the Board of Longitude debated sending his watch to the arctic and elsewhere for lengthy tests, Harrison appealed to King George III for help. The King had H5 installed at his own personal observatory at Kew, and he himself supervised its daily winding and checking. Under his care, H5 performed so admirably that "Farmer George" was outraged by what had gone before. "By God, Harrison," he roared, "I'll see you righted!" and threatened to appear in person before Parliament (under a lesser title, of course).

"So the affair finally wound down. The Board of Longitude, under the continuing influence of Nevil Maskelyne, still managed to find reasons to withhold some of Harrison's prize money. Nevertheless, including interim advances, he received a total of £23,065 from the Board over four decades. A copy of H4 made by another watchmaker accompanied Captain James Cook (1728-79) on his second voyage to the Pacific, where it was hailed as having "an amazing degree of accuracy," although Cook also used the lunar distances method with good results. In a triumph of his own, Maskelyne had initiated the annual publication of *The Nautical Almanac*, which included the necessary tables for applying that method. Cook referred to it as "our faithful guide through all vicissitudes of climates." And since sufficiently accurate clocks (later called marine chronometers) remained very expensive for a long while, the method of lunar distances remained in use for more than a hundred years.

"Harrison died in 1776, unreconciled with Maskelyne to the end. One cannot blame him, but because it turned out that posterity owed much to both of them, it would have been pleasing had their relationship ended otherwise" (J. Donald Fernie, 'The Harrison-Maskelyne Affair,' *American Scientist* 91 (2003), 403-5).

Baillie, p. 272; Crone 564; Grolier/Horblit 42b; Norman 995 (all the regular 4to issue). Lake, 'In-Depth: The Microscopic Magic of the H4, Harrison's First Sea Watch' (https://watchesbysjx.com/2019/09/john-harrison-marine-chronometer-h4-diamond-pallets.html). See Lake for a detailed account of the construction of H4.

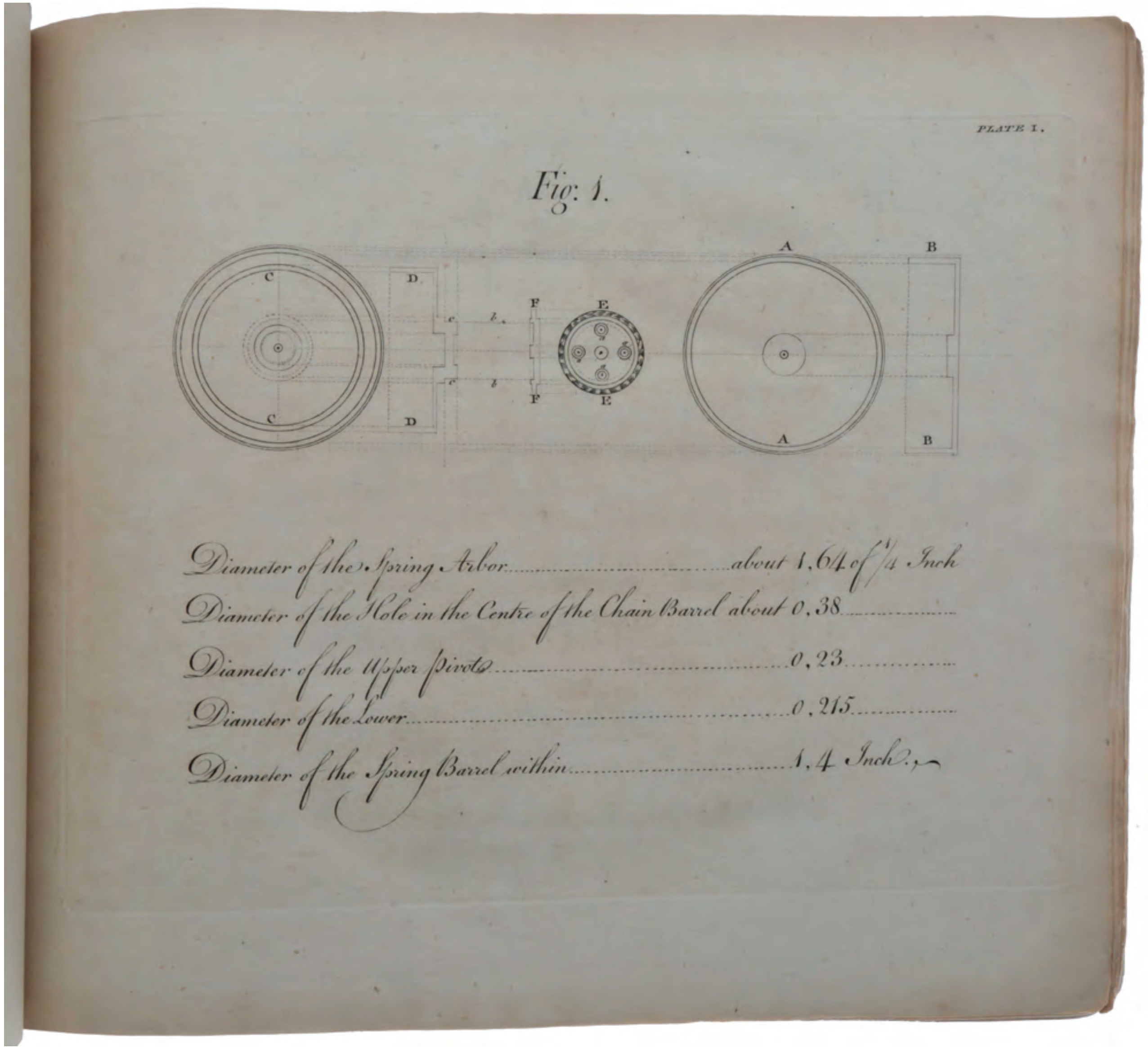


PLATE I.
Fig: 1.
A
B
C
D
F
E
c
b
Diameter of the Spring Arbor.........about 1,64 of 1/4 Inch
Diameter of the Hole in the Centre of the Chain Barrel about 0,38
Diameter of the Upper pivots.........0,23
Diameter of the Lower.........0,215
Diameter of the Spring Barrel within.........1,4 Inch.

INVENTION OF THE OPHTHALMOSCOPE

HELMHOLTZ, Hermann von. *Beschreibung eines Augen-Spiegels zur Untersuchung der Netzhaut im lebenden Auge.* Berlin: A. Förstner, 1851.

$14,500

8vo (224 x 137 mm), pp. [1-3], 4-43, [5], engraved plate by Afinger after Helmholtz. Original yellow printed wrappers, uncut and unopened.

First edition, an exceptionally fine copy in original printed wrappers, of this famous work which describes Helmholtz's announcement of his invention of the ophthalmoscope, one of the most important clinical tools in medicine, which greatly improved the ability of ophthalmologists to diagnose eye disease and revolutionized visual science. The invention of the ophthalmoscope by Helmholtz has been called "the greatest event in the history of ophthalmology, which advanced it toward the goal of independence as a specialty" (Gorin). This invention was a by-product of Helmholtz's attempt to demonstrate to his physiology students that when the human eye is made to glow with reflected light, the light emitted from the pupil follows the same course it took in entering. Realizing that if the light could be brought to a focus the details of the retina would be made visible, he invented a device to accomplish this objective. "With this instrument it was possible for the first time to examine the interior of the living eye. Although crude attempts had been made earlier to see into the eye, it was Helmholtz's invention of a workable instrument in 1850 and the publication of his monograph in 1851 that laid the

basis of scientific ophthalmology. Helmholtz's invention of the ophthalmoscope arose from an attempt to demonstrate for his class in Königsberg the nature of the glow of reflected light sometimes seen in the eyes of animals such as the cat. When the great ophthalmologist A. von Graefe first saw the fundus of the living eye, with its disc and blood-vessels, his face flushed with excitement, and he cried 'Helmholtz has unfolded to us a new world!' (Hagerstrom Library). The *Augen-Spiegel* was printed in a very small edition: there are no copies in the Becker, Osler or Cushing Collections. Rare on the market in such fine condition.

"Helmholtz's invention had its roots in earlier attempts to see the back of the eye, though these were insufficient to permit proper inspection of the human *fundus*. In 1703, Jean Méry (1645–1722), who worked at the Hôtel Dieu, found that the luminosity of the cat's eye could be seen when the animal was held under water, showing that it was essentially an optical phenomenon. Philippe de la Hire, 6 years later, thought it was owing to abolition of corneal refraction under water that the incident light rays emerged divergent and were thus seen by the observer's eye. In the fourth essay in his *Oeuvres* (2 volumes, Leiden, 1717), Edmé Mariotte (1620–1684), who was both physicist and priest, observed that a dog's eye is luminous because its choroid is white; and the darker choroid in man and animals allowed no clear image. Richter provoked further interest when it was found that luminosity could still be present in a blind eye, and in 1792 Georg Joseph Beer had observed the luminosity of the *fundus in aniridia*. However, spontaneous luminosity in man remained unexplained.

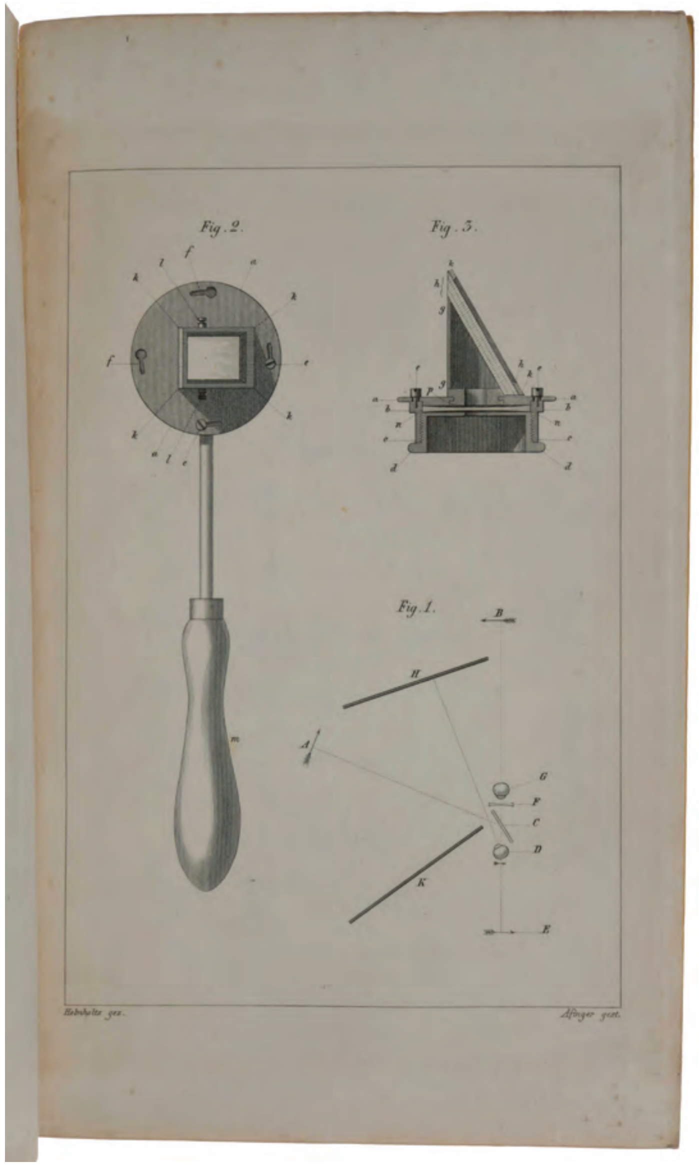
"Bénédict Prévost, Professor of Philosophy at Montaubon in France (1755–1819), repeated Mariotte's experiments, examining the eyes of a cat in the dark, and explained that the retina was invisible: 'It is not the light which proceeds from the eye to an object that enables the eye to perceive that object, but the light which arrives in the eye from it.' This was an important discovery that dispelled the accepted notions that light came from within the eye to permit animals to see in the dark.

"In 1821, the Swedish naturalist Karl Asmund Rudolphi (1771–1832) shone a light into a decapitated cat's eyes and showed that the reflecting eye emitted light along the same line as the direction of the in-going rays. Twenty-seven years before Helmholtz's work, in 1823, Jan Evangelista Purkinje (1787–1869), Professor of Physiology at Breslau, had observed that under certain illumination human eyes could be made luminous: in 1825 Purkinje started to use lenses to examine the back of the eye. His crucial work, published in Latin, was unrecognized for many years: 'I examined the eye of a dog by using the spectacle lens of a myope and placing a candle behind the dog's back … I found the light as the source, which is reflected from the concavity of the spectacle lens into the interior of the eye. From there it is again reflected. I immediately repeated the experiment on a human eye and found the same phenomenon'.

"The pupil too appeared black. The 'beautiful orange glow was reflected when light was thrown into it'. Unnoticed, this was rediscovered independently by William Cumming in England, who, in 1846, wrote 'On a luminous appearance of the human eye and its application to the detection of disease of the retina and posterior part of the eye.' He explained that the axis of illumination and observation had to be coincident to view the fundus. A year later, the Berlin physiologist Ernst Wilhelm Ritter von Brücke (1819–1892) made the same observation: 'A short time ago in the evening as I was standing between the chandelier and the door in the auditorium of this university, I saw a young man whose pupils were illuminated with a bright red light as he turned to close the door through which he had just passed … If one wishes to see this reflex in human eyes … Take the usual oil lamp with its cylindrical wick and the glass chimney, … and regulate the wick in such fashion that it burns with a short, intense flame. Then set the lamp close to you, but place the subject 8 to 10 feet away, … If [the subject] then looks with widely opened lids towards the darkness adjacent to the lamp, or if he slowly moves his eyes to and from, then the pupils will be illuminated with a reddish light, while the iris, in contrast, will appear slightly greenish.' Helmholtz was later generously to say: 'Brucke himself was but a hair's breadth away from the invention of the ophthalmoscope. He had only failed to ask himself what optical image was formed by the rays reflected from the luminous eye. Had it occurred to him, he was the man to answer it just as quickly as I did and to invent the ophthalmoscope.'

"Adolf Kussmaul (1822–1902) attempted to see the *fundus* in 1845. He applied a plano-concave lens of the same power as the cornea to the eye, 'in an effort to see the human optic nerve.' Although Kussmaul failed to view the living fundus, he did show in vitro that removal of the cornea and lens rendered the fundus visible, but lack of illumination thwarted his efforts.

"There is also a British claim for precedence. Three years before Helmholtz announced his discovery, Charles Babbage FRS (1791–1871) invented an instrument consisting of a piece of plain mirror, with the silvering scraped off at 2 or 3 small spots in the centre, held within a tube at an angle so that rays of light falling on it through an opening in the side were reflected into the patient's eye. The observer looked through the clear spots from the other end of the mirror. As Helmholtz noted, Babbage's instrument would have worked if a concave lens of 4 or 5 dioptres had been inserted to correct the convergent rays. Babbage used a plain mirror with a central opening, the first for looking into the eye through an aperture. When he showed it to the ophthalmologist Thomas Wharton Jones, the retina was not adequately seen. Babbage, however, did not pursue his invention. In 1854, Wharton Jones, in his 'Report on the ophthalmoscope', said: 'It is but justice that I should here state, however, that 7 years ago [i.e. 1847] Mr. Babbage showed me the model of an instrument that he had contrived for the purpose of looking into the interior of the eye.' However, Hirschberg has disputed his



claim. The major and lasting achievement came when Helmholtz announced the invention of an 'eye-mirror' in December 1850.

"Helmholtz was born on 31 August 1821 at Potsdam. His father, Ferdinand, was a teacher of philology and philosophy, while his mother was a Hanoverian, a lineal descendant of the great Quaker William Penn. He graduated in Medicine from the military medical school in Berlin in 1843, greatly influenced by Johannes Mueller, who had devised the Law of Specific Nerve Energies. Mueller had also trained Henle, Schwann and Du Bois-Reymond. Helmholtz recalled: 'I recall my student days and the impression made upon us by a man like Johannes Mueller, the physiologist. When one feels himself in contact with a man of the first order, the entire scale of his intellectual conception is modified for life; contact with such a man is perhaps the most interesting thing life has to offer'.

"Helmholtz's graduation thesis described the anatomic connection between neurones and nerves. He wrote in 1847 what has been described as one of the great scientific papers of the 19th century: *Über die Erhaltung der Kraft* [On the conservation of energy]. In this he showed mathematically that all forms of energy can be transformed from one form to another, but energy cannot be created or destroyed. He served as physician to the army for several years, before following an academic career in the physical sciences.

"In his researches Helmholtz noticed that the pupil normally appeared black, but under certain conditions seemed red and emitted light. This convinced him that the emitted light was no more than reflected light. However, advancing from his predecessors, he analyzed how the emitted rays formed optical images. He tried to obtain an optical image of the fundus by devising an instrument that would allow his own eye to be placed directly in line with the light rays entering and leaving the eye. He used 3 microscopic cover glasses as a mirror to reflect light, but they had to be transparent to allow him to see the retina; this he achieved and thus saw the retina in detail.

"The announcement was made in a paper presented by his friend Du Bois-Reymond to the Berlin Physical Society on 6 December 1850. However, the text was lost, and on 17 December 1850, Helmholtz, with ill-concealed excitement, wrote to his father: 'I have made a discovery during my lectures on the Physiology of the Sense-organs, which may be of the utmost importance in ophthalmology … It is, namely, a combination of glasses, by means of which it is possible to see the dark background of the eye, through the pupil, without employing any dazzling light, and to obtain a view of all the elements of the retina at once, more exactly than one can see the external parts of the eye without magnification, because the transparent media of the eye act like a lens with magnifying power of twenty. The blood vessels are displayed in the neatest way, with the branching arteries and veins, the entrance of the optic nerve into the eye, etc. … My discovery makes the minute investigation of the internal structures of the eye a possibility. I have announced this very precious egg of Columbus to the Physical Society at Berlin, as my property, and am now having an improved and more convenient instrument constructed to replace my pasteboard affair. I shall examine as many patients as possible with the chief oculist here, and then publish the matter'.

"Helmholtz's first public announcement of the ophthalmoscope was on 11 November 1851 at the Society for Scientific Medicine of Königsberg. His 43-page monograph was also published in 1851 [as the offered work]. Helmholtz called his new instrument 'Augenspiegel' (eye mirror). It comprised a lens and a mirror for reflecting light. The instrument was used for examining the retina and adnexa of the eye. He recognized 3 essential components: a light source, a mirror to direct light toward the eye and a device to focus the image on the retina. This instrument was known in England as an 'eye speculum', but Maressal de Marsilly of Calais in

1852 called it the 'ophthalmoscope' …

"Von Graefe facilitated the introduction of the ophthalmoscope to clinical ophthalmology. Some held it might harm the eye. Anagnostakis, in 1854, popularized the instrument in France, whilst in England, it was used and developed by W. Spencer Watson and William Bowman, though many remained sceptical and at first it was utilized infrequently in many countries …

"Fundal changes in disease were soon recognized by Albrecht von Graefe (1828–1872), undoubtedly the greatest German ophthalmologist of the 19th century, who founded the *Archiv für Ophthalmologie* and the eye hospital in Berlin. In 1855, von Graefe described albuminuric retinitis. By 1860 he had also shown fundal changes in glaucoma and in retinal arterial embolism; he introduced iridectomy and cataract extraction and described papillo-oedema and its neurological significance. Coccius in 1853 illustrated both retinal detachment and retinitis pigmentosa. Liebriech recognized central retinal venous thrombosis in 1855. However, practising physicians were still slow to appreciate the benefit of ophthalmoscopy. Clifford Allbutt wrote in 1871: 'The number of physicians who are working with the ophthalmoscope today in England may, I believe, be counted on the fingers of one hand.' Allbutt did much to change this with his book *On the Use of the Ophthalmoscope in Diseases of the Nervous System and the Kidneys*, published in 1871, as did *A Manual and Atlas of Medical Ophthalmoscopy* compiled by his friend Sir William Gowers" (Pearce).

Albert 1032; Garrison-Morton 5866; Gorin, *History of Opthalmology*, 1982 (pp. 125-6); Grolier/Medicine 65; Heirs of Hippocrates 1886; Lilly, Notable Medical Books, 205; Norman 1041; Waller 4294. Pearce, 'The Ophthalmoscope: Helmholtz's Augenspiegel,' *European Neurology* 61 (2009), pp. 244-249.

PMM 323 - THE FIRST LAW OF THERMODYNAMICS

HELMHOLTZ, Hermann. *Über die Erhaltung der Kraft, eine physikalische Abhandlung, vorgetragen in der Sitzung der physikalischen Gesellschaft zu Berlin am 23sten Juli 1847.* Berlin: Georg Reimer, 1847.

$45,000

8vo (220 x 135 mm), pp. [iv], 72. Later 19th century plain blue wrappers with original front printed wrapper cut down and mounted on upper cover (wrappers a bit soiled). Three pages of early manuscript notes in an unidentified hand bound in at end discussing the concept of 'Kraft' in various areas of physics and referring to Humboldt's Cosmos.

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 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

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.

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 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.'

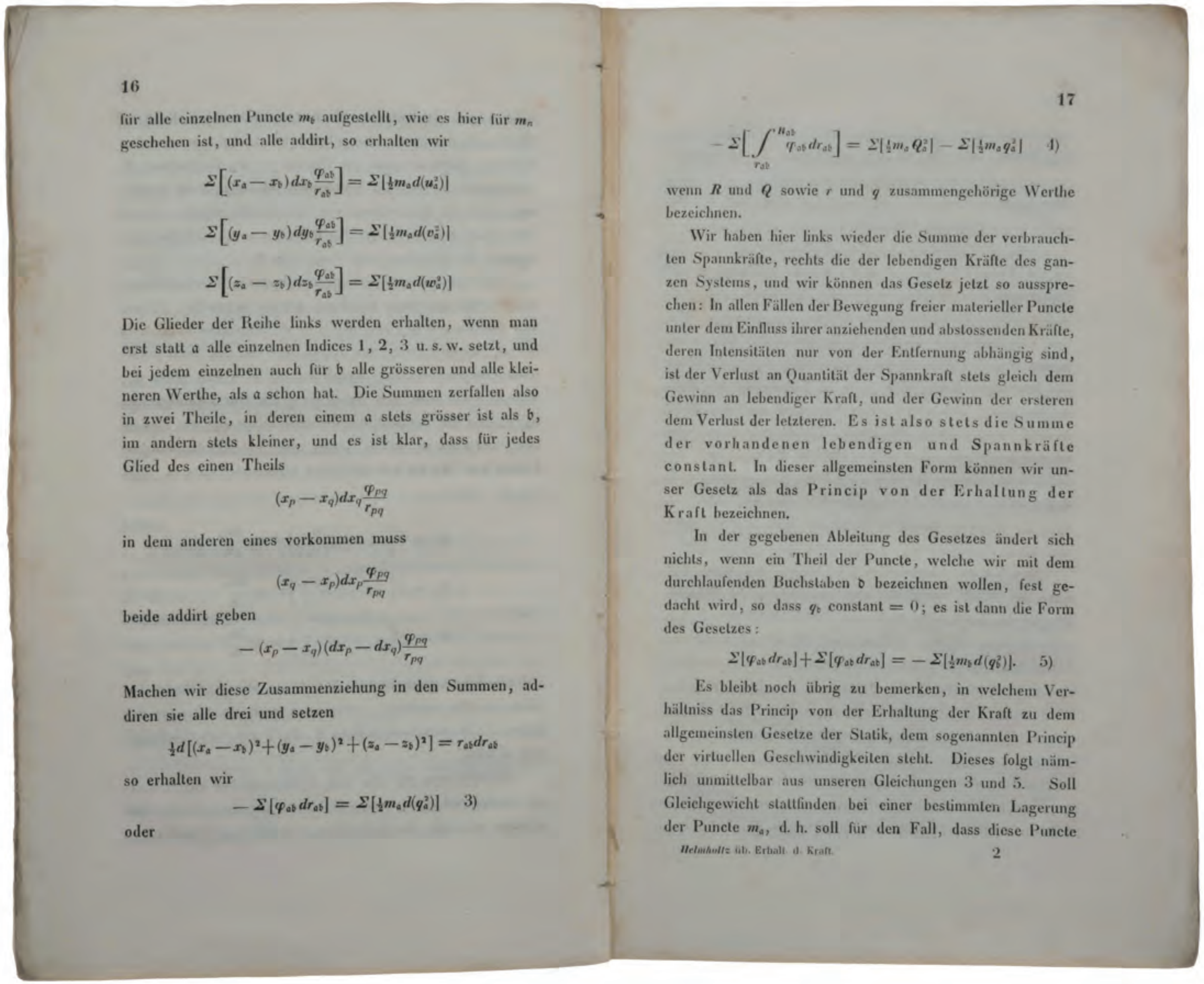
"Throughout his paper on the conservation of force, Helmholtz referred not only to theoretically founded laws but also to a good deal of experimental work on the establishment of laws by Riess, Poggendorff, Weber, and others. He pointed to his predictions as waiting to be tested. His purpose was not 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).



16

für alle einzelnen Puncte m_b aufgestellt, wie es hier für m_a geschehen ist, und alle addirt, so erhalten wir

$$\Sigma\left[(x_a - x_b)\,dx_b \frac{\varphi_{ab}}{r_{ab}}\right] = \Sigma[\tfrac{1}{2}m_a d(u_a^2)]$$

$$\Sigma\left[(y_a - y_b)\,dy_b \frac{\varphi_{ab}}{r_{ab}}\right] = \Sigma[\tfrac{1}{2}m_a d(v_a^2)]$$

$$\Sigma\left[(z_a - z_b)\,dz_b \frac{\varphi_{ab}}{r_{ab}}\right] = \Sigma[\tfrac{1}{2}m_a d(w_a^2)]$$

Die Glieder der Reihe links werden erhalten, wenn man erst statt a alle einzelnen Indices 1, 2, 3 u. s. w. setzt, und bei jedem einzelnen auch für b alle grösseren und alle kleineren Werthe, als a schon hat. Die Summen zerfallen also in zwei Theile, in deren einem a stets grösser ist als b, im andern stets kleiner, und es ist klar, dass für jedes Glied des einen Theils

$$(x_p - x_q)dx_q \frac{\varphi_{pq}}{r_{pq}}$$

in dem anderen eines vorkommen muss

$$(x_q - x_p)dx_p \frac{\varphi_{pq}}{r_{pq}}$$

beide addirt geben

$$-(x_p - x_q)(dx_p - dx_q)\frac{\varphi_{pq}}{r_{pq}}$$

Machen wir diese Zusammenziehung in den Summen, addiren sie alle drei und setzen

$$\tfrac{1}{2}d[(x_a - x_b)^2 + (y_a - y_b)^2 + (z_a - z_b)^2] = r_{ab}dr_{ab}$$

so erhalten wir

$$-\Sigma[\varphi_{ab}\,dr_{ab}] = \Sigma[\tfrac{1}{2}m_a d(q_a^2)] \qquad 3)$$

oder

17

$$-\Sigma\left[\int_{r_{ab}}^{R_{ab}} \varphi_{ab}\,dr_{ab}\right] = \Sigma[\tfrac{1}{2}m_a Q_a^2] - \Sigma[\tfrac{1}{2}m_a q_a^2] \qquad 4)$$

wenn R und Q sowie r und q zusammengehörige Werthe bezeichnen.

Wir haben hier links wieder die Summe der verbrauchten Spannkräfte, rechts die der lebendigen Kräfte des ganzen Systems, und wir können das Gesetz jetzt so aussprechen: In allen Fällen der Bewegung freier materieller Puncte unter dem Einfluss ihrer anziehenden und abstossenden Kräfte, deren Intensitäten nur von der Entfernung abhängig sind, ist der Verlust an Quantität der Spannkraft stets gleich dem Gewinn an lebendiger Kraft, und der Gewinn der ersteren dem Verlust der letzteren. *Es ist also stets die Summe der vorhandenen lebendigen und Spannkräfte constant.* In dieser allgemeinsten Form können wir unser Gesetz als das *Princip von der Erhaltung der Kraft* bezeichnen.

In der gegebenen Ableitung des Gesetzes ändert sich nichts, wenn ein Theil der Puncte, welche wir mit dem durchlaufenden Buchstaben b bezeichnen wollen, fest gedacht wird, so dass q_b constant $= 0$; es ist dann die Form des Gesetzes:

$$\Sigma[\varphi_{ab}\,dr_{ab}] + \Sigma[\varphi_{ab}\,dr_{ab}] = -\Sigma[\tfrac{1}{2}m_b d(q_b^2)]. \qquad 5)$$

Es bleibt noch übrig zu bemerken, in welchem Verhältniss das Princip von der Erhaltung der Kraft zu dem allgemeinsten Gesetze der Statik, dem sogenannten Princip der virtuellen Geschwindigkeiten steht. Dieses folgt nämlich unmittelbar aus unseren Gleichungen 3 und 5. Soll Gleichgewicht stattfinden bei einer bestimmten Lagerung der Puncte m_a, d. h. soll für den Fall, dass diese Puncte

Helmholtz üb. Erhalt. d. Kraft. 2

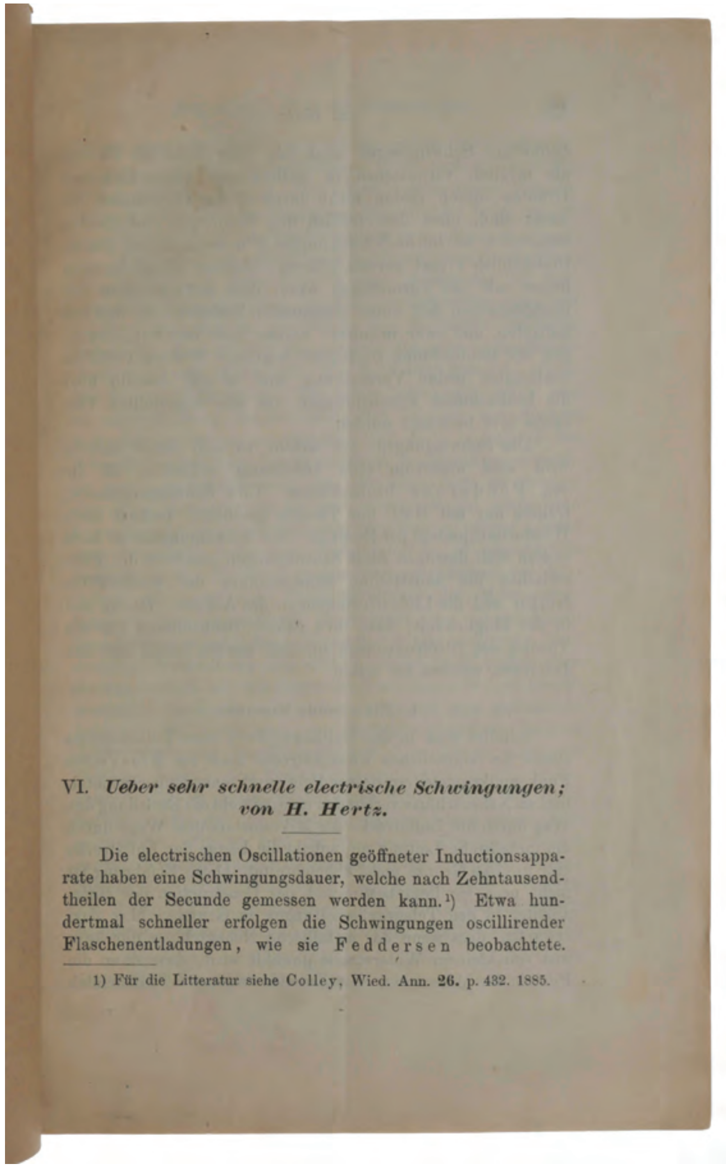
PMM 377 - HERTZIAN WAVES

HERTZ, Heinrich Rudolf. *Ueber sehr schnelle electrische schwingungen. Offprint from Annalen der Physik, Bd. 31 (1887). With six other offprints documenting Hertz's seminal work which demonstrated the existence of electromagnetic waves, which thereby provided the experimental proof of Maxwell's theory and formed the foundation for wireless communication.* Leipzig; Berlin: Johann Ambrosius Barth; George Reimer for the Königlich Akademie der Wissenschaften, 1887-1892.

$38,500

8vo (216 x 140 mm) & 4to (258 x 179 mm), original printed wrappers, front wrapper on no. 1 renewed, minor chipping and dust-soiling, several items creased and some with remains of stamps and postmarks. Presentation inscription in English on front wrapper of no. 7. See below for individual descriptions.

First edition, extremely rare offprint, of the first of Hertz's papers on electromagnetic waves, accompanied by offprints of six further papers on the same subject, including 'Ueber elektrodynamische Wellen im Luftraume und deren Reflexion', in which Hertz first demonstrated the existence of electromagnetic waves propagating in air ('Hertzian waves'). "In his *Treatise on Electricity and Magnetism* (1873) [Maxwell] gave no theory of oscillatory circuits or of the connection between currents and electromagnetic waves. The possibility of producing electromagnetic waves in air was inherent in his theory, but it was by no means obvious and was nowhere spelled out. Hertz's proof of such waves was in part owing to his theoretical penetration into Maxwell's thought" (DSB). "Experimental proof by Hertz of the Faraday-Maxwell hypothesis that electrical waves can be projected through space was begun in 1887, eight years after


VI. *Ueber sehr schnelle electrische Schwingungen; von H. Hertz.*

Die electrischen Oscillationen geöffneter Inductionsapparate haben eine Schwingungsdauer, welche nach Zehntausendtheilen der Secunde gemessen werden kann.[1]) Etwa hundertmal schneller erfolgen die Schwingungen oscillirender Flaschenentladungen, wie sie Feddersen beobachtete.

1) Für die Litteratur siehe Colley, Wied. Ann. **26.** p. 432. 1885.

Maxwell's death. The two main requirements were (a) a method of producing the waves, supposing that they existed, and (b) a method of detecting them once they were produced. Hertz found the first problem easy to solve. He used the oscillatory discharge of a condenser. Detection was much more difficult, because there then existed no means of detecting currents alternating at the high speed of these waves. Hertz in fact used an effect as old as the discovery of electricity itself – the electric spark. By inducing the waves to produce an electrical spark at a distance, with no apparent connection between the oscillator and the spark gap, and by moving the sparking apparatus so that the length of the spark varied, Hertz proved beyond question the passage of electric waves through space… The experiments were reported periodically from 1887 onward in *Annalen der Physik und Chemie*" (PMM). "This discovery [of electromagnetic waves] and its demonstration led directly to radio communication, television and radar" (Dibner). "In the early 1890's the young inventor Guglielmo Marconi read of Hertz's electric wave experiments in an Italian electrical journal and began considering the possibility of communication by wireless waves. Hertz's work initiated a technological development as momentous as it physical counterpart" (DSB). We can find no other copies of any of the papers [1]-[8] in auction records. The Smithsonian holds a copy of each of the offprints [1]-[7]; OCLC adds two other copies of [1], one in York (though not listed in their library catalogue), and one in Japan (not verified); two other copies of [2], one in Bern and one in Japan (not verified); and three other copies of [5], one in Bern and two in Yale.

The offered papers are as follows:

1. Ueber sehr schnelle electrische schwingungen [On very rapid electrical oscillations]. Offprint from *Annalen der Physik*, Bd. 31 (1887), pp. 421-448 and one folding plate. Contemporary wrappers, upper wrapper renewed with matching paper with manuscript title label, lower wrapper with publisher's imprint.

2. Ueber Inductionserscheinungen, hervorgerufen durch die elektrischen Vorgänge in Isolatoren [On electromagnetic effects produced by electrical disturbances in insulators]. Offprint from *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin*, 10 November, 1887, pp. 1-12. Original printed wrappers (creased vertically and horizontally for posting and with postmark and remains of stamp on rear cover).

3. Ueber die Einwirkung einer geradlinigen electrischen Schwingung auf eine benachbarte Strombahn [The action of a rectilinear electric oscillation on a neighbouring circuit]. Offprint from *Annalen der Physik*, Bd. 34 (1888), pp. 155-170 and one folding plate. Original printed wrappers.

4. Ueber elektrodynamische Wellen im Luftraume und deren Reflexion [On electrodynamic waves in air and their reflection]. Offprint from *Annalen der Physik*, Bd. 34 (1888), pp. 609-623. Original printed wrappers.

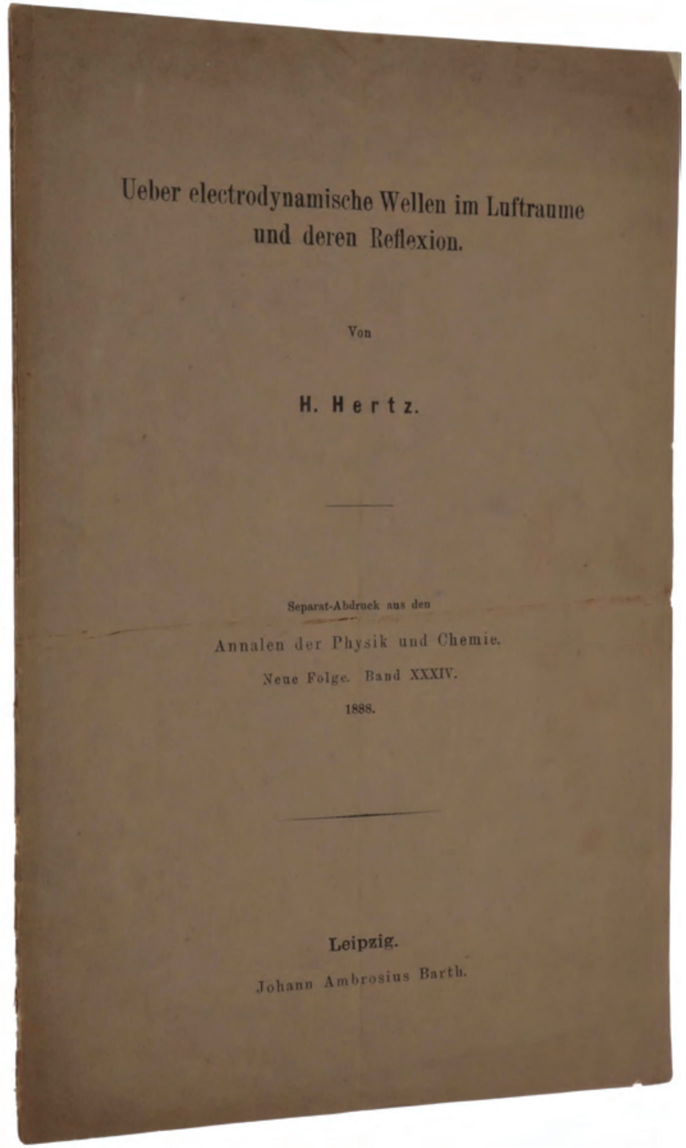
5. Uber Strahlen elektrischer Kraft [On rays of electric force]. Offprint from *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin*, 13 December, 1887, pp. 1-11. Original printed wrappers (creased vertically and horizontally for posting).

6. Die Krafte electrischer Schwingungen behandelt nach der Maxwell'schen Theorie [The forces of electric oscillations treated according to Maxwell's theory]. Offprint from *Annalen der Physik*, Bd. 36 (1888), pp. 1-22. Original printed wrappers.

7. Uber die Fortleitung electrischer Wellen durch Drähte [On the propagation of electric waves by means of wires]. Offprint from *Annalen der Physik*, Bd. 37 (1889), pp. 395-408 and one folding plate. Original printed wrappers, upper wrapper with English inscription by Hertz.

Hertz's work on electric waves began with Helmholtz's proposal in 1879 of a prize problem connected with the behaviour of unclosed circuits in Maxwell's theory. "Central to Maxwell's theory was the assumption that changes in dielectric polarization yield electromagnetic effects in precisely the same manner as conduction currents do. Helmholtz wanted an experimental test of the existence of these effects or, conversely, of the electromagnetic production of dielectric polarization. Although at the time Hertz declined to try the Berlin Academy problem because the oscillations of Leyden jars and open induction coils which he was familiar with did not seem capable of producing observable effects, he kept the problem constantly in mind; and in 1886 shortly after arriving in Karlsruhe, he found that the Riess or Knochenhauer induction coils he was using in lecture demonstrations he was using in lecture demonstrations were precisely the means he needed for undertaking Helmholtz' test of Maxwell's theory …

"He produced electric waves with an unclosed circuit connected to an induction coil, and he detected them with a simple unclosed loop of wire. He regarded his detection device as his most original stroke, since no amount of theory could have predicted that it would work. Across the darkened Karlsruhe lecture hall he could see faint sparks in the air gap of the detector. By moving it to different parts of the hall he measured the length of the electric waves; with this value and the calculated frequency of the oscillator he calculated the velocity of the waves. For Hertz his determination at the end of 1887 of the velocity – equal to the enormous velocity of light – was the most exciting moment in the entire sequence of experiments. He and others saw its significance as the first demonstration of



the finite propagation of a supposed action at a distance …

“Hertz followed up his determination of the finite velocity of electric waves by performing a series of more qualitative experiments in 1888 on the analogy between electric and light waves. Passing electric waves through huge prisms of hard pitch, he showed that they refract exactly as light waves do. He polarized electric waves by directing them through a grating of parallel wires, and he diffracted them by interrupting them with a screen with a hole in it. He reflected them from the walls of the room, obtaining interference between the original and the reflected waves. He focused them with huge concave mirrors, casting electric shadows with conducting obstacles. The experiments with mirrors especially attracted attention, as they were the most direct disproof of action at a distance in electrodynamics. They and the experiments on the finite velocity of propagation brought about a rapid conversion of European physicists from the viewpoint of instantaneous action at a distance in electrodynamics to Maxwell’s view that electromagnetic processes take place in dielectrics and that an electromagnetic ether subsumes the functions of the older luminiferous ether” (DSB).

In the important first paper of his study [1], Hertz describes the ingenious apparatus he had devised to produce, detect, and measure the electromagnetic waves, the key to all his later discoveries. He “observed for the first time ‘wire waves’, that is, regular alternating currents with a very high frequency in conductive wires. These were the only waves he had yet detected. He described them to his master Hermann von Helmholtz in Berlin: ‘In the meantime I have succeeded in several further experiments. By means of the oscillations I used in my previous work, I am now able to produce standing waves with many nodes in straight stretched wires…’ Today we know these ‘further experiments’ in great detail from Hertz’s Laboratory Notes. On Saturday November 5th, Hertz sent for publication in *Akademieberichte* his paper [2] proving the existence of ‘polarization currents’ in insulators (our ‘displacement currents’), by detecting their electrodynamic actions” (Baird *et al*., p. 73).

In [3], Hertz investigated the effects of electric waves on conductors by transmitting a wave inside a two-wire coaxial transmission line. “Early in 1888 he reported in the *Annalen* [in this paper] an indication of a ‘finite velocity of propagation of electric distance actions’, which gave him renewed confidence in his work” (Jungnickel & McCormmach, p. 87).

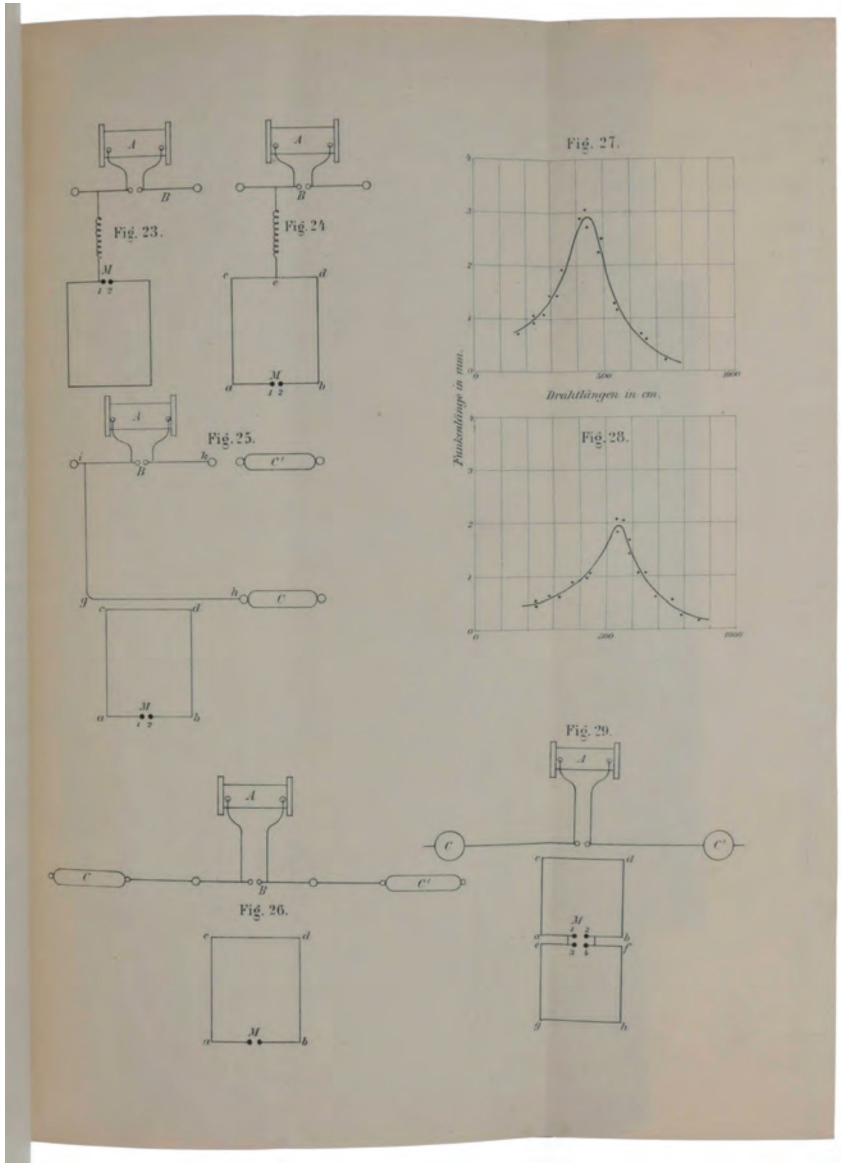
It is in the crucial paper [4] that Hertz first demonstrates the existence of electromagnetic waves in air (rather than in wires). “In March 1888, Hertz was able to demonstrate the wave nature of something new, his recently conjectured ‘air waves’, or what we call ‘Hertzian waves’. At this point he wrote Helmholtz as follows: ‘Electrodynamical waves in air are reflected from solid conducting walls; at normal incidence the reflected waves interfere with the incident and give rise to standing waves in the air …’ But of what do these air waves consist. What oscillates” This was so new for Hertz that the first times he described it he did nor even dare to use the same verb, ‘to oscillate’, used for wire waves … What it is that fluctuates Hertz called successively (in paper [4]): ‘inductive action’, ‘electrodynamic action’ and ‘electric force’. We note that these air waves are transverse; this is clearly shown in the figure of paper [4]: the direction in which the electric force fluctuates or oscillates is perpendicular to the direction of the wave propagation … From these two letters to Helmholtz, we can see Hertz’s conceptual shift, from the wire waves of November 1887 to the air waves of March 1888” (Baird *et al*., pp. 74-75).

In [5] Hertz shows that electric waves, like light waves, not only can be reflected but also exhibit the properties of interference and polarization. “In the paper ‘On electric radiation,’ which Hertz regarded as a ‘natural end’ of this series of researches, he reported that his experiments had removed ‘any doubt as to the identity of light, radiant heat, and electromagnetic wave motion’: electric rays are light rays of long wavelength” (Jungnickel & McCormmach, p. 88).

Paper [6] contains an important theoretical contribution by Hertz, his introduction of the retarded potential. This allowed him to derive Maxwell’s equations independently in a manner quite distinct from Maxwell’s. This paper also contains some beautiful diagrams illustrating the electric field outside his dipole antenna, and clearly illustrating how some of the energy escapes into space as an electric wave. “In one of the later papers, ‘The forces of electric oscillations, treated according to Maxwell’s theories,’ Hertz concluded that Maxwell’s theory explains all the facts he had investigated and is superior to all of the other theories … His theoretical orientation was now firmly Maxwell’s, for he viewed the production of electric waves as owing not only to the source but also to the condition of the surrounding space, the seat of the electromagnetic energy. By means of great parabolic mirrors, lenses, gratings and prisms, he showed that the electric force exhibits all of the main properties of light waves” (*ibid*., pp. 87-88).

The last paper [7] describes Hertz’s further investigations into the propagation of electric waves through wires. To his surprise, he found the velocity of propagation to be different from the speed in air, contrary to the predictions of theory. Hertz called on others to repeat the experiments and verify or refute his results. In Dublin, George Francis Fitzgerald and associates repeated and elaborated Hertz’s experimental discoveries. For wire transmission, their results were in good agreement with those of Hertz. On the other hand Édouard Sarasin and Lucien de la Rive of Geneva obtained the results required by theory.

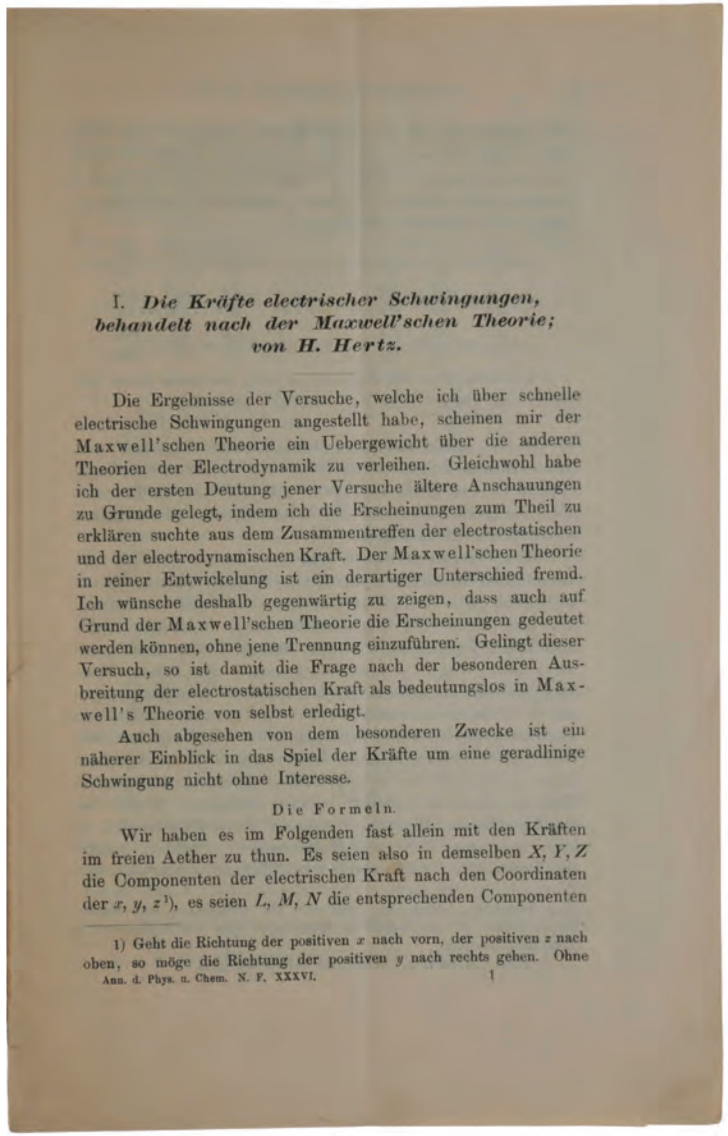
Born in Hamburg, Heinrich Hertz (1857-94) came from a prosperous and cultured family. After serving in the military for a year (1876-77), Hertz spent a



year at the University of Münich, where he decided to embark upon an academic career. In 1880 Hertz received a Ph.D. at the University of Berlin working under Helmholtz. He then taught at the University of Kiel and in 1885 was appointed professor of physics at the University of Bonn. His untimely death from blood poisoning, which occurred after several years of poor health, cut short a brilliant career. Hertz died before Guglielmo Marconi made the use of radio waves a practical means of communication. In his honour the unit of frequency is now called the hertz.

Hertz's papers on electromagnetism were published in book form as *Untersuchungen ueber die Ausbreitung der elektrischen Kraft* (Leipzig: Barth, 1892). The offprints offered here are Nos. 2, 6, 5, 8, 11, 9 and 10, respectively, in that collection.

Dibner 71; Honeyman 1668 (for the collected edition); Sparrow, *Milestones*, p. 47; PMM 377. Baird, Hughes & Nordmann, *Heinrich Hertz*: *Classical Physicist, Modern Philosopher*, 2013. Jungnickel & McCormmach, *Intellectual Mastery of Nature. Theoretical Physics from Ohm to Einstein*, Vol. 2, 1986.



I. ***Die Kräfte electrischer Schwingungen, behandelt nach der Maxwell'schen Theorie; von H. Hertz.***

Die Ergebnisse der Versuche, welche ich über schnelle electrische Schwingungen angestellt habe, scheinen mir der Maxwell'schen Theorie ein Uebergewicht über die anderen Theorien der Electrodynamik zu verleihen. Gleichwohl habe ich der ersten Deutung jener Versuche ältere Anschauungen zu Grunde gelegt, indem ich die Erscheinungen zum Theil zu erklären suchte aus dem Zusammentreffen der electrostatischen und der electrodynamischen Kraft. Der Maxwell'schen Theorie in reiner Entwickelung ist ein derartiger Unterschied fremd. Ich wünsche deshalb gegenwärtig zu zeigen, dass auch auf Grund der Maxwell'schen Theorie die Erscheinungen gedeutet werden können, ohne jene Trennung einzuführen. Gelingt dieser Versuch, so ist damit die Frage nach der besonderen Ausbreitung der electrostatischen Kraft als bedeutungslos in Maxwell's Theorie von selbst erledigt.

Auch abgesehen von dem besonderen Zwecke ist ein näherer Einblick in das Spiel der Kräfte um eine geradlinige Schwingung nicht ohne Interesse.

Die Formeln.

Wir haben es im Folgenden fast allein mit den Kräften im freien Aether zu thun. Es seien also in demselben X, Y, Z die Componenten der electrischen Kraft nach den Coordinaten der x, y, z 1), es seien L, M, N die entsprechenden Componenten

1) Geht die Richtung der positiven x nach vorn, der positiven z nach oben, so möge die Richtung der positiven y nach rechts gehen. Ohne

Ann. d. Phys. u. Chem. N. F. XXXVI. 1


Ueberreicht vom Verfasser.
H. Hertz.
Einwirkung ei
Schwingung a
Ueber
With the authors compliments
Ueber die Fortleitung electrischer Wellen durch Drähte.
Von
H. Hertz.
Separat-Abdruck aus den
Annalen der Physik und Chemie.
Neue Folge. Band XXXVII.
1889.
Leipzig.
Johann Ambrosius Barth.

HILBERT’S PROGRAM FOR THE FOUNDATION OF MATHEMATICS

HILBERT, David. *‘Neubegründung der Mathematik,’ pp. 157-177 in Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität, 1. Band, 2. Heft.* Hamburg: Verlag des mathematischen Seminars, 1922.

$1,250

8vo (240 x 162 mm), pp. 99-177, [1], 4, uncut and mostly unopened. Original printed wrappers, spine slightly sunned.

First edition, journal issue in the original printed wrappers, and a copy with excellent provenance, of the first major paper in the development of the ‘Hilbert programme,’ in which Hilbert put forward his proposal for a foundation for all of mathematics based on axiomatics and logic. Hilbert’s programme “was a proposed solution to the foundational crisis of mathematics, when early attempts to clarify the foundations of mathematics were found to suffer from paradoxes and inconsistencies. As a solution, Hilbert proposed to ground all existing theories to a finite, complete set of axioms, and provide a proof that these axioms were consistent. Hilbert proposed that the consistency of more complicated systems, such as real analysis, could be proven in terms of simpler systems. Ultimately, the consistency of all of mathematics could be reduced to basic arithmetic” (Wikipedia). “According to Reid [*Hilbert-Courant* (1970), p. 154), Hilbert was becoming, in the early 1920s, ‘increasingly alarmed by the gains that Brouwer’s conception of mathematics was making among the younger mathematicians. To him, the program of the Intuitionists represented quite simply a clear and present

danger to mathematics'. Hilbert interpreted intuitionism as requiring that all pure existence proofs, a large part of analysis and Cantor's theory of infinite sets would have to be given up. In particular, this would rebut some of Hilbert's own important contributions to pure mathematics. Hilbert was especially disturbed by the fact that Weyl, who was his most distinguished former student, accepted the radical views of Brouwer, who aroused in Hilbert the memory of Kronecker. In Reid's words, 'At a meeting in Hamburg in 1922 he came roaring back to the defence of mathematics' (Reid, p. 155). This was the first public presentation of Hilbert's program" (Raatikainen, p. 158). Hilbert's Hamburg lecture was published as the present paper. In it, "Hilbert sets out the basic ideas of his proof theory, describes a simple formal axiom system for a fragment of arithmetic, and proves its consistency; he also lays the groundwork for his later investigations of the foundations of set theory and real analysis ... Hilbert makes no attempt to supply a consistency proof for the transfinite part of his theory" (Ewald, p. 1116).

Provenance: Heinrich Behmann (1891-1970) (pencil signature on upper wrapper). Behmann studied mathematics in Tübingen, Leipzig and Göttingen. Hilbert supervised the preparation of his doctoral thesis at Göttingen, *Die Antinomie der transfiniten Zahl und ihre Auflösung durch die Theorie von Russell und Whitehead* (1918). This dissertation was primarily intended to give a clear exposition of the solution to the antinomies found in Whitehead & Russell's *Principia Mathematica*. Behmann continued to perform research in the field of set theory and predicate logic, proving in 1922 that the monadic predicate calculus is decidable. In 1938 he obtained a professorial chair in mathematics at Halle. See Mancosu, 'Between Russell and Hilbert: Behmann on the foundations of mathematics,' *Bulletin of Symbolic Logic* 5 (1999), 303-330.

Hilbert's work on the foundations of mathematics has its roots in his work on geometry of the 1890s, culminating in his influential *Grundlagen der Geometrie* (1899). Hilbert believed that the proper way to develop any scientific subject rigorously required an axiomatic approach, which would enable the theory to be developed independently of any need for intuition, and would facilitate an analysis of the logical relationships between the basic concepts and the axioms. Hilbert realized that the most important questions are the independence and the consistency of the axioms. For the axioms of geometry, consistency can be proved by providing an interpretation of the system in the real plane, and thus the consistency of geometry is reduced to the consistency of analysis. The foundation of analysis, of course, itself requires an axiomatization and a consistency proof. Hilbert provided such an axiomatization in *Über den Zahlbegriff* (1900), but it became clear very quickly that the consistency of analysis faced significant difficulties, in particular because the favoured way of providing a foundation for analysis in Dedekind's work relied on dubious assumptions akin to those that lead to the paradoxes of set theory. Hilbert thus realized that a direct consistency proof of analysis, i.e., one not based on reduction to another theory, was needed. He proposed the problem of finding such a proof as the second of his 23 mathematical problems in his address to the International Congress of Mathematicians in 1900 and presented a sketch of such a proof in his Heidelberg talk *Über die Grundlagen der Logik und der Arithmetik* in 1904, but further progress was delayed because of the lack of a properly worked-out logical formalism.

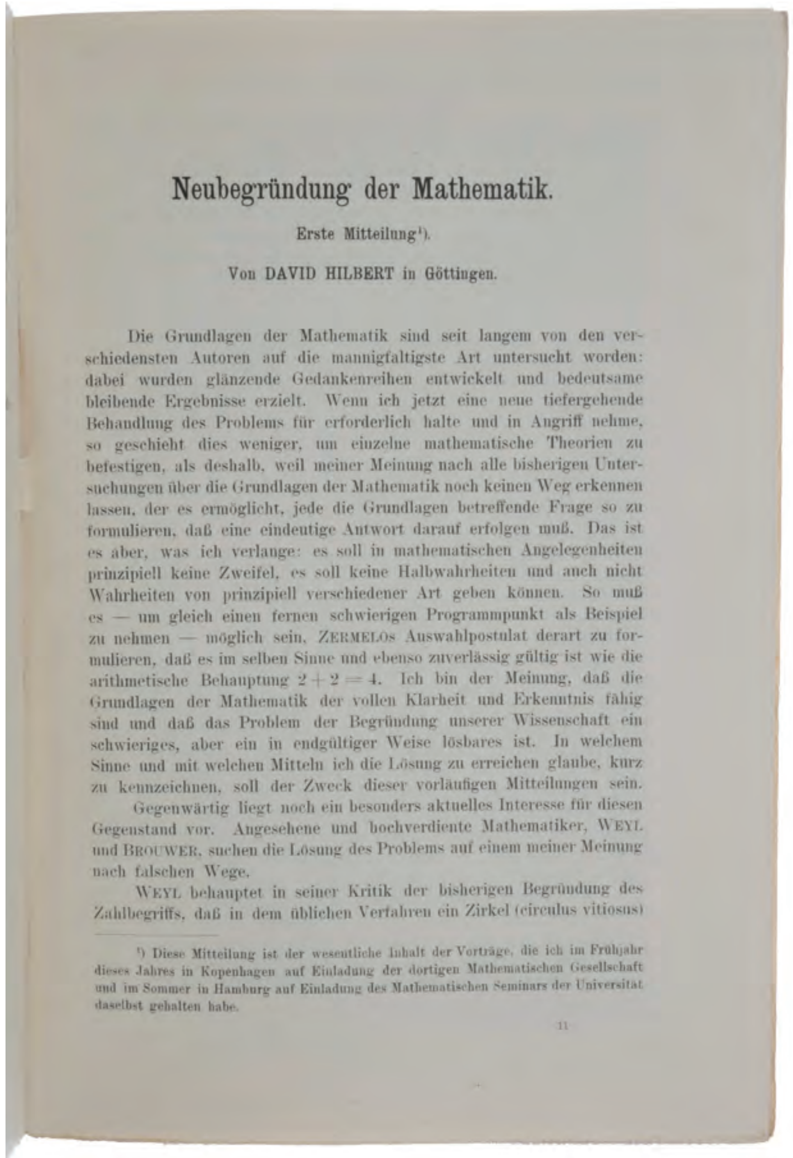
The publication of Russell and Whitehead's *Principia Mathematica* provided the required logical basis for a renewed attack on foundational issues. "In the interval since [*Über die Grundlagen der Logik und der Arithmetik*], numerous important developments had taken place in the foundations of mathematics. Hilbert's younger Göttingen colleague Ernst Zermelo had proved his well-ordering theorem. The paradoxes of set theory had become widely known, and had led to spirited discussions between Russell, Poincare, Richard, Konig, Zermelo, and Peano. Hilbert's axiomatic method, first employed in his *Grundlagen der Geometrie*

(1899), had been applied in numerous investigations in geometry, algebra, and mathematical physics. Poincare published his criticisms of [*Über die Grundlagen der Logik und der Arithmetik*] and of the logicists; Zermelo supplied axioms for the set theory of Dedekind and Cantor; Whitehead and Russell published *Principia Mathematica*; Brouwer began to develop his intuitionistic mathematics; Hilbert's gifted former pupil, Hermann Weyl, was drawn to Brouwer's ideas" (Ewald, pp. 1105-1106).

In September 1917, Hilbert delivered an address to the Swiss Mathematical Society entitled *Axiomatische Denken*, his first published contribution to mathematical foundations since 1905. In it, he again emphasizes the requirement of consistency proofs for axiomatic systems, and states his belief that this had been achieved by Russell's work in *Principia*. Nevertheless, other fundamental problems of axiomatics remained unsolved, including the problem of the "decidability of every mathematical question," which also traces back to Hilbert's 1900 address.

Within the next few years, however, Hilbert came to reject Russell's logicistic solution to the consistency problem for arithmetic. At the same time, Brouwer's intuitionistic mathematics gained currency. In particular, Hilbert's former student Hermann Weyl converted to intuitionism. Weyl's paper *Über die neue Grundlagenkrise der Mathematik* (1921) was answered by Hilbert in three talks in Hamburg delivered on July 25-27, 1921, published as *Neubegründung der Mathematik*.

"The first period [of Hilbert's work on the foundations of mathematics] is taken to extend from 1900 to 1905, the second from 1922 to 1931. The periods are marked by the dates of outstanding publications. Hilbert published in 1900 and 1905 respectively *Über den Zahlbegriff* and *Über die Grundlagen der Logik und der Arithmetik* … the considerations of the latter paper were taken up around 1921,



Neubegründung der Mathematik.

Erste Mitteilung[1]).

Von DAVID HILBERT in Göttingen.

Die Grundlagen der Mathematik sind seit langem von den verschiedensten Autoren auf die mannigfaltigste Art untersucht worden: dabei wurden glänzende Gedankenreihen entwickelt und bedeutsame bleibende Ergebnisse erzielt. Wenn ich jetzt eine neue tiefergehende Behandlung des Problems für erforderlich halte und in Angriff nehme, so geschieht dies weniger, um einzelne mathematische Theorien zu befestigen, als deshalb, weil meiner Meinung nach alle bisherigen Untersuchungen über die Grundlagen der Mathematik noch keinen Weg erkennen lassen, der es ermöglicht, jede die Grundlagen betreffende Frage so zu formulieren, daß eine eindeutige Antwort darauf erfolgen muß. Das ist es aber, was ich verlange: es soll in mathematischen Angelegenheiten prinzipiell keine Zweifel, es soll keine Halbwahrheiten und auch nicht Wahrheiten von prinzipiell verschiedener Art geben können. So muß es — um gleich einen fernen schwierigen Programmpunkt als Beispiel zu nehmen — möglich sein, ZERMELOs Auswahlpostulat derart zu formulieren, daß es im selben Sinne und ebenso zuverlässig gültig ist wie die arithmetische Behauptung $2+2=4$. Ich bin der Meinung, daß die Grundlagen der Mathematik der vollen Klarheit und Erkenntnis fähig sind und daß das Problem der Begründung unserer Wissenschaft ein schwieriges, aber ein in endgültiger Weise lösbares ist. In welchem Sinne und mit welchen Mitteln ich die Lösung zu erreichen glaube, kurz zu kennzeichnen, soll der Zweck dieser vorläufigen Mitteilungen sein.

Gegenwärtig liegt noch ein besonders aktuelles Interesse für diesen Gegenstand vor. Angesehene und hochverdiente Mathematiker, WEYL und BROUWER, suchen die Lösung des Problems auf einem meiner Meinung nach falschen Wege.

WEYL behauptet in seiner Kritik der bisherigen Begründung des Zahlbegriffs, daß in dem üblichen Verfahren ein Zirkel (circulus vitiosus)

[1]) Diese Mitteilung ist der wesentliche Inhalt der Vorträge, die ich im Frühjahr dieses Jahres in Kopenhagen auf Einladung der dortigen Mathematischen Gesellschaft und im Sommer in Hamburg auf Einladung des Mathematischen Seminars der Universität daselbst gehalten habe.

11

were quickly expanded into the proof theoretic program, and were exposed first in 1922 through Hilbert's *Neubegründung der Mathematik*" (Sieg, p. 3).

In the *Neubegründung* Hilbert attempts to rebut the critiques of classical mathematics by Brouwer and Weyl and to clear away the doubts engendered by the paradoxes they raised. "At the level of metamathematics, Hilbert adopted the intuitionistic criticisms of infinitary mathematics, and sought to use only reasoning that was intuitionistically acceptable; indeed, at this level, Hilbert's finitism went further than that of Brouwer himself. The disagreements stemmed rather from a difference of opinion about what constitutes a foundation for mathematics, and concerned, first, the desirability of formalized mathematics *uberhaupt;* second, the usefulness, legitimacy, and mathematical interest of the classical, infinitary modes of inference expressed in Hilbert's formal system.

"In contrast to [*Über die Grundlagen der Logik und der Arithmetik*], the present essay now draws a clear distinction between the logico-mathematical formalism and the *inhaltliche* metamathematical reasonings about it. This distinction allows Hilbert to answer the charge of circularity raised by Poincare … [who] had charged that Hilbert needed to presuppose the truth of mathematical induction in order to prove its consistency; but Hilbert can now distinguish (as he does in §31) between the strong principle of complete induction expressed in the formal language and the weaker principle used in the metalanguage" (*ibid.*).

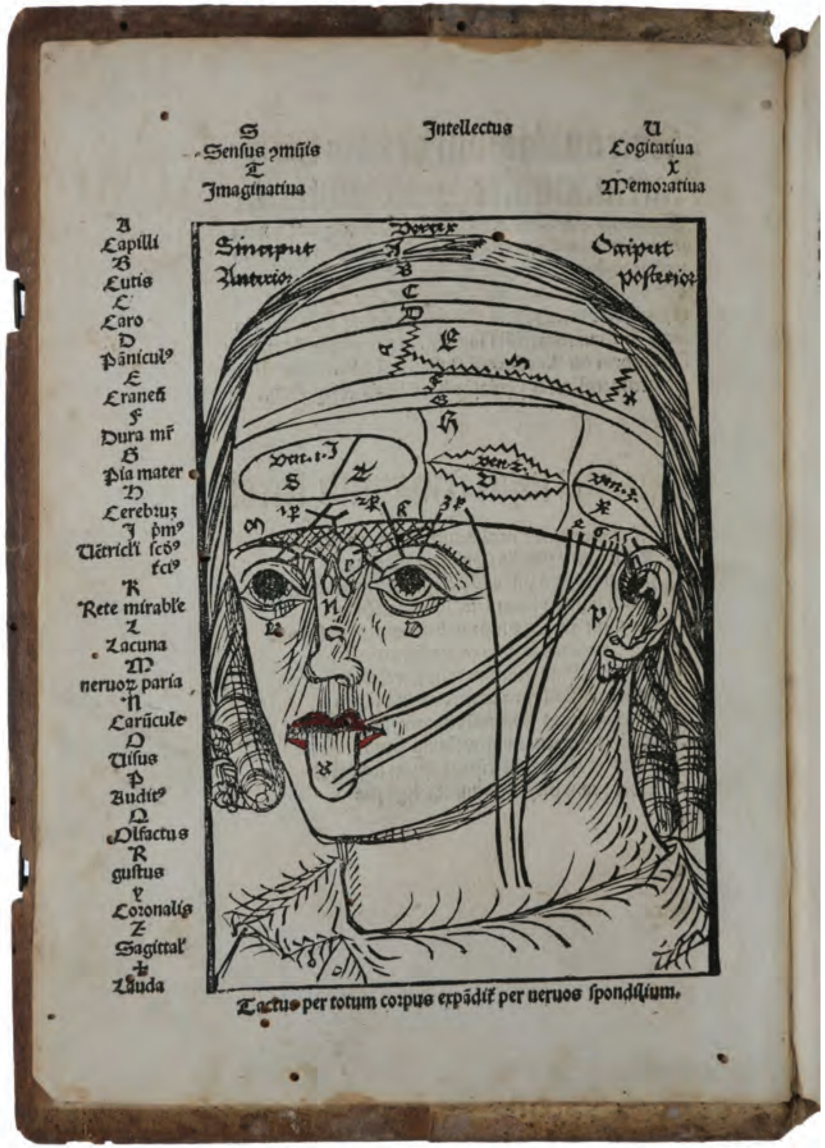
Ewald, *From Kant to Hilbert*, Vol. II, Oxford, 1996; Raatikainen, 'Hilbert's Program revisited,' *Synthese* 137 (2003), pp. 157-177. Sieg, 'Hilbert's Programs: 1917-1922,' *Bulletin of Symbolic Logic* 5 (1999), pp. 1-44.

SOME OF THE EARLIEST PRINTED ANATOMICAL ILLUSTRATIONS

HUNDT, Magnus. *Antropologium de hominis dignitate, natura et proprietatibus, de elementis, partibus et membris humani corporis.* Leipzig: Wolfgang Stöckel, 1507.

$165,000

4to (205 x 153 mm), 120 leaves, unpaginated. Contemporary German blind-tooled half pigskin over wooden boards, numerous annotations throughout the text. Some gatherings with light toning, but in general a beautiful copy. Highly scarce in such fine condition.



An outstanding copy, in untouched contemporary binding from the collection of Jean Blondelet, of one of the earliest works with anatomical illustrations, "includes the first illustrations of the viscera in a printed book" (GM). "Hundt's best-known work, *Antropologia de hominis dignitate, natura et proprietatibus de elementis*, published in 1501, is one of the three or four earliest printed books to include anatomic illustrations. At one time, Hundt's work was looked upon as the oldest printed book with original anatomic illustrations, but that is no longer believed to be the case. His *Antorpologia* included five full-page woodcuts, including two identical reproductions of the human head, which appeared on the back of the title page as well as later in the book. The woodcuts are crude and schematic and not done from nature, and although one of the woodcuts pictures the entire body and lists the various external parts, there is no attempt to equate the anatomical term with the actual representation. There is also a full-page woodcut of a hand with chiromantic markings, and of the internal organs of the throat and abdomen. Smaller woodcuts, including plates of the stomach, intestines, and cranium, are inserted throughout the text. The work gives a clear idea of anatomy prior to the work of Berengario da Carpi, and can be regarded as typifying late-fifteenth-century concepts. Hundt held that the stars exert more influence on the human body than on other composites of elements, and his book includes generalizations about human physiognomy and chiromancy as well as anatomy. He subscribed to the notion of the seven-celled uterus, which he apparently derived from Galen" (DSB). This is a very rare book on the market: APPC/RBH lists just the Norman copy, Christie's 1998 $85,000 modern binding; Swann Galleries 1979 $8,600 modern binding; Sotheby's 1974 $6,000 disbound.

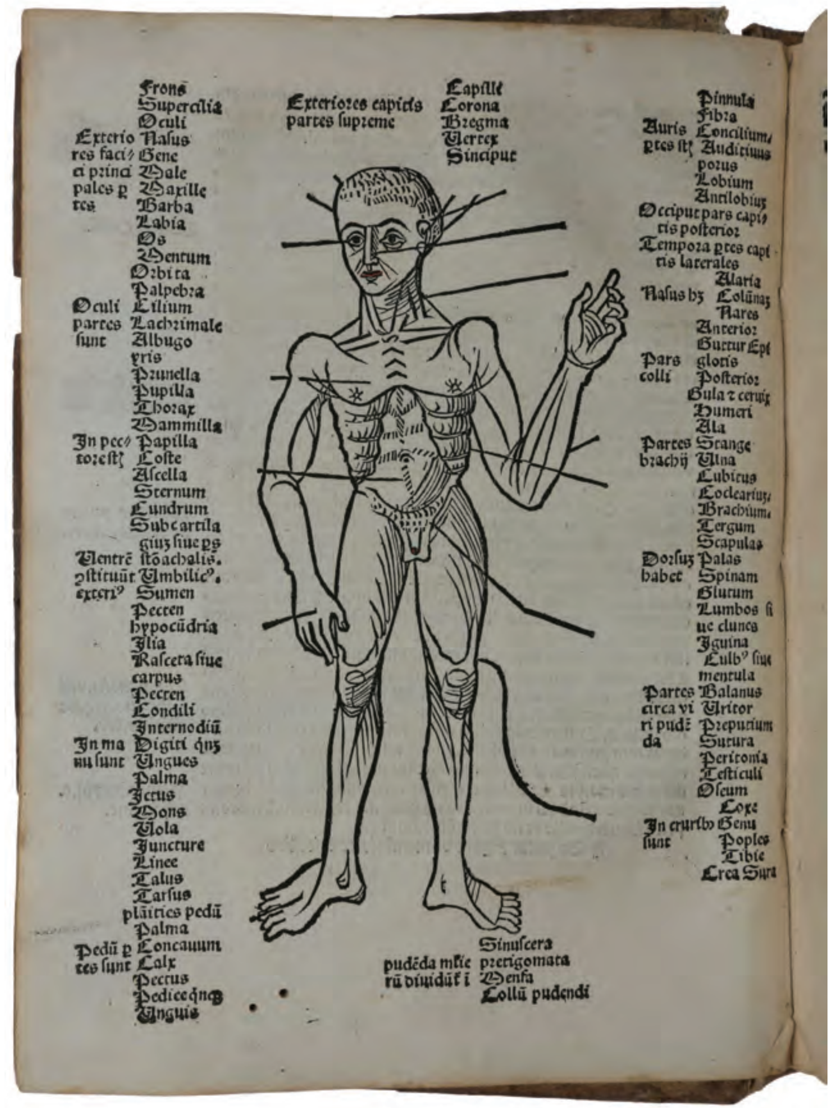
Provenance: Rear paste-down with the marking of Blondelet, and with his preferred custom morocco box by Duval. Numerous contemporary annotations throughout. "Jean Blondelet was probably the greatest, but least known, French collector of rare medical and scientific books in the 20th century" (Jeremy Norman).

"Hundt's best-known work, *Antropologia de hominis dignitate, natura et proprietatibus de elementis*, published in 1501, is one of the three or four earliest printed books to include anatomic illustrations. At one time, Hundt's work was looked upon as the oldest printed book with original anatomic illustrations, but that is no longer believed to be the case. His *Antropologia* included five full-page woodcuts, including two identical reproductions of the human head, which appeared on the back of the title page as well as later in the book. The woodcuts are crude and schematic and not done from nature, and although one of the woodcuts pictures the entire body and lists the various external parts, there is no attempt to equate the anatomical term with the actual representation. There is also a full-page woodcut of a hand with chiromantic markings, and of the

internal organs of the throat and abdomen. Smaller woodcuts, including plates of the stomach, intestines, and cranium, are inserted throughout the text. The work gives a clear idea of anatomy prior to the work of Berengario da Carpi, and can be regarded as typifying late-fifteenth-century concepts. Hundt held that the stars exert more influence on the human body than on other composites of elements, and his book includes generalizations about human physiognomy and chiromancy as well as anatomy. He subscribed to the notion of the seven-celled uterus, which he apparently derived from Galen" (DSB).

"The *Antropologium* … contains four large and several small woodcuts, which are accepted among the earliest of anatomical illustrations that are a little more than schematic representation. His work contains illustrations of the internal organs but without images of bones or muscles and this work seems to be the most comprehensive representation of all the internal parts up to that time. One of those illustrations shows that trachea on the right side of the neck, passing downward to the lungs; on the left side the oesophagus is represented. In the thorax are seen the lungs and the heart. The pericardium has been opened and the stomach and intestines are figured crudely. In addition, a figure of the uterus depicting the anatomy of the uterus with seven cells (Figura matricis) is noted. These illustrations also give a clear idea of pre-Berengarian anatomy and seem to be the aggregate of the views entertained in the fifteenth century as to the position and shape of the anatomic parts" (Gurunluoglu et al, 'The history and illustration of anatomy in the Middle Ages,' *Journal of Medical Biography* 21 (2013), 219-229).

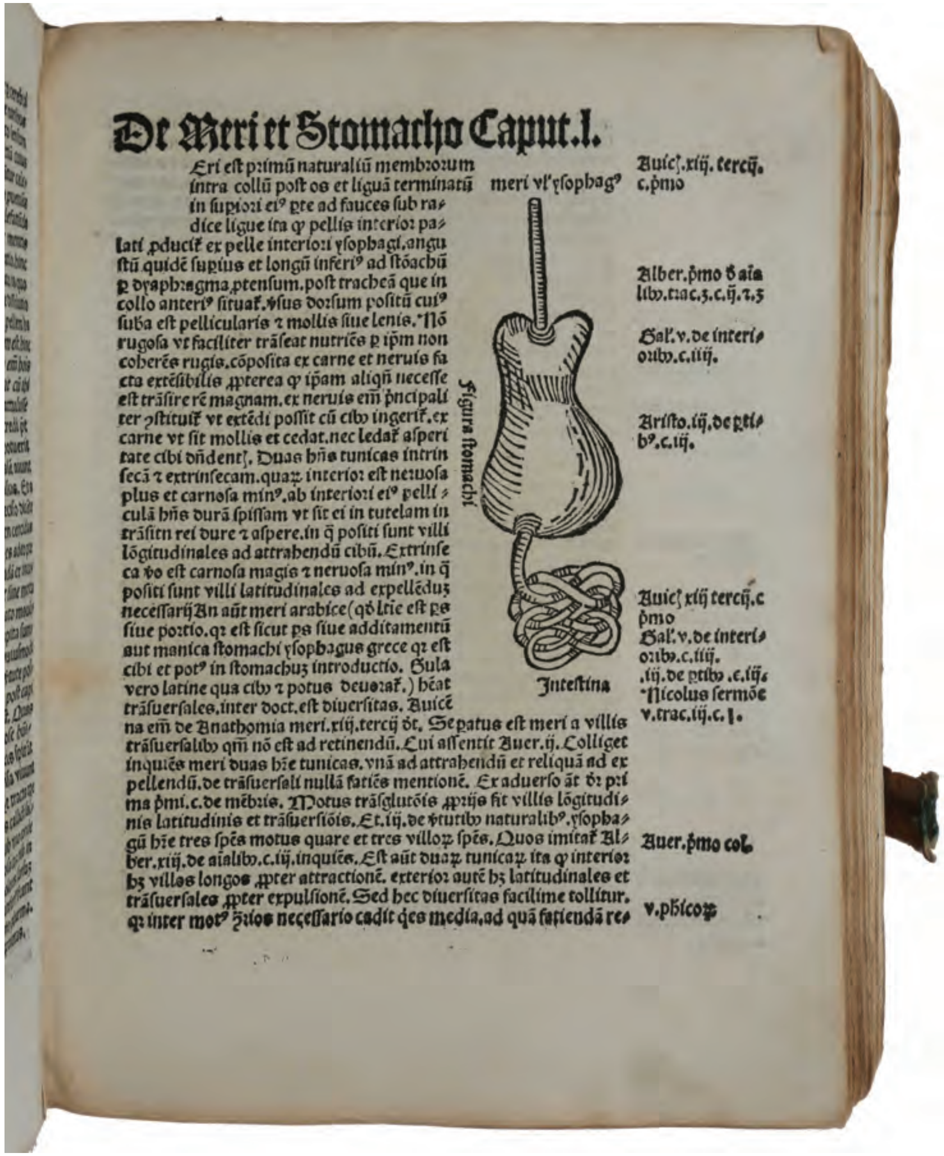
The representation of the head was reproduced in several later works. "It is believed that Hundt's scheme had its origin in the 1493 edition of Albertus Magnus' *Philosophia naturalis*. A similar representation can be found in the *Margarita philosophica* (1503) written by Gregor Reisch (ca. 1470-1523) … The figure was subsequently reproduced in a number of medical texts published in the



sixteenth century, such as Giovanni Battista Porta's (ca. 1535-1615) *De humani Physiognomia*, published in Padua in 1593 … In the later sixteenth century, a notable figure is Otto Casmann (d. 1607), also known as Otto Casmannus, a physician-theologian from Stade, near Hamburg in Lower Saxony, who published a number of texts such as *Psycholgia anthropologica* (1594) and *Somatologia* (1598), which appear to be elaborations of the "anthropology" of Hundt" (*History of Physical Anthropology*, Vol. 1, F. Spencer (ed.) (1997), p. 425).

"Hundt's *Antropologium* discussed anatomy and physiology in their premodern forms as well as the religious and philosophical aspects of humans. Thomas Bendyshe (1865) called the *Antropolgium* "a purely anatomical work", and Joseph Barnard Davis (1868) added that it was "ornamented with rude woodcuts, depicting gross inaccuracies". But this misrepresented Hundt's holistic attempt to explain the dual nature of humans (body and soul) from both an anatomical and a religious perspective. Convinced that humans were created in the image of God (*Homo est dei imago secundum animam*), Hundt regarded the spiritual component to be more important than the material one. He wanted to show people their dignity, as indicated in his book's title, by expanding on earlier views of humans at an intersection between the creator and the creation (*Homo est dei et mundi nodus*)" (H. F. Vermeulen, *Before Boas: The Genesis of Ethnography and Ethnology in the German Enlightenment* (2015), p. 360).

Magnus Hundt was born at Magdeburg in 1449 and died in Meissen in 1519. He was a renowned German theologian, physician and philosopher, and is generally regarded, jointly with Otto Casmann, as one of the founders of contemporary anthropology. In fact, the term was jointly coined and in due course popularized by both of them. Starting his studies at the age of 33 in Leipzig, Hundt received his Baccalaureate two years later. In 1487, the year he received an advanced degree, he was appointed Dean of the Faculty of Arts. Later in his illustrious career he was



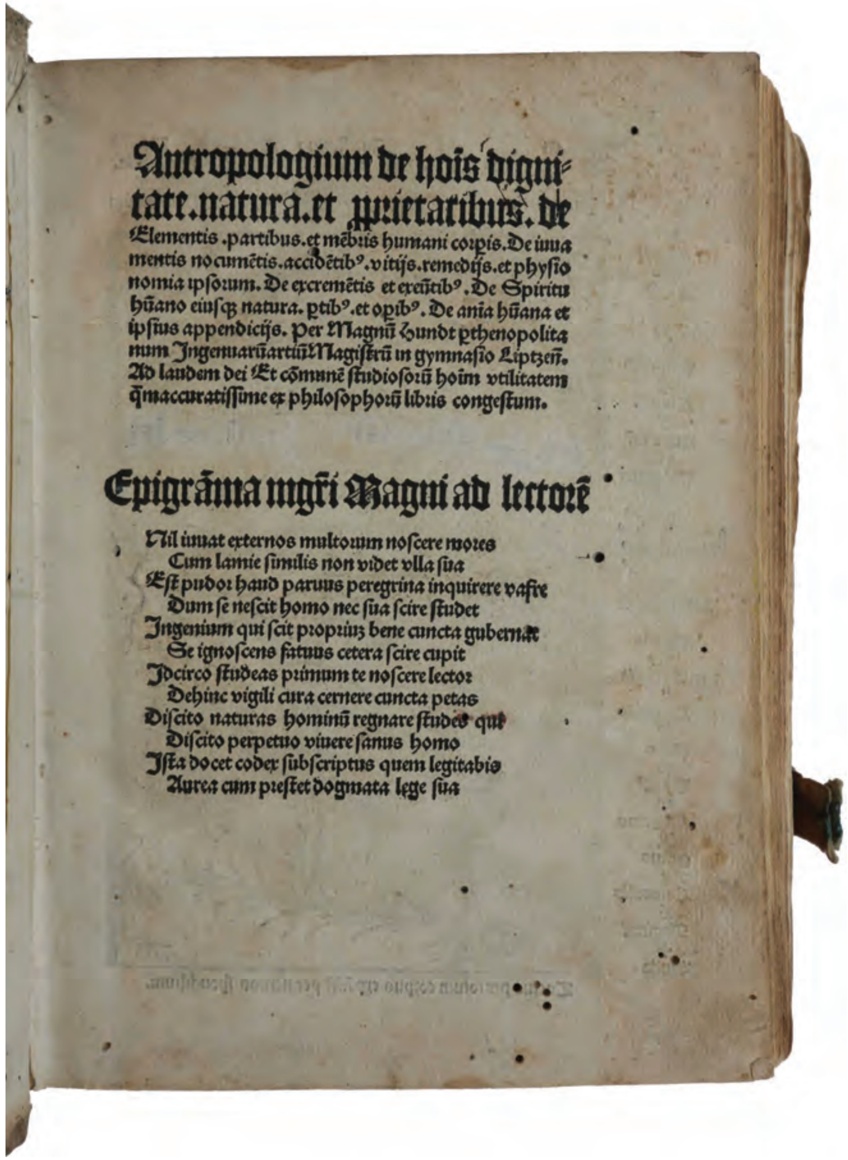
given the post of Rector at the University, and is also said to have served as the personal physician for Count Joachimsthal. Nevertheless, his personal interests went far beyond medicine. Thus, he earned a doctorate degree in theology in 1510 and consequently occupied a chair in this subject at the University of Meissen, a location to which the University of Leipzig was relocated after the plague.

Hundt's *Antropologium* is here bound with:

PINDER [or BINDER], Ulrich. Epiphanie medicorum. Speculum videndi urinas hominum. Clavis aperiendi portas pulsuum. Berillus discernendi causas & differentias febrium. [Nuremberg: Friedrich Peypus? for the author, 1506].

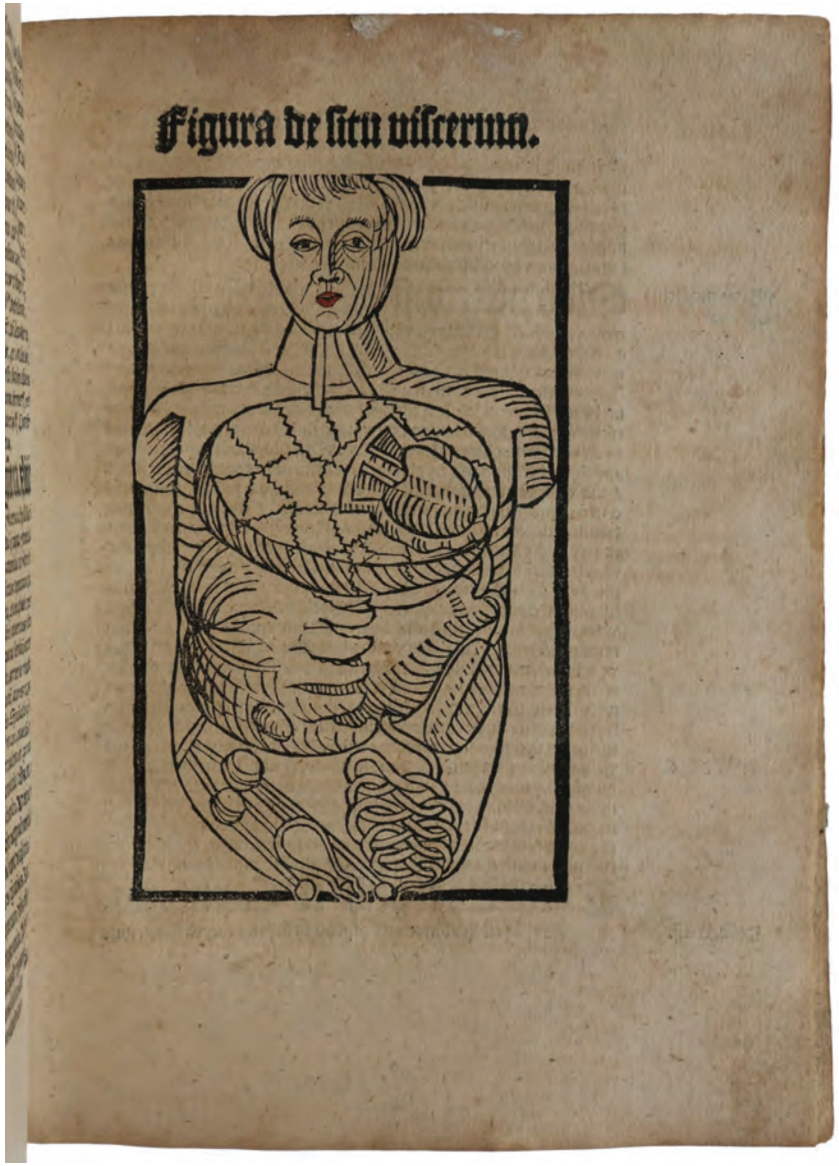
First edition, privately printed at the author's press. "The first and longest of Binder's three medical works in this collection treats of the practice of uroscopy, the diagnosis of illness by examination of the color and consistency of a patient's urine. The full-page woodcut preceding the work shows a uroscopic consultation, and is surrounded by a border of urine glasses. The remaining treatises deal with the movement of the heart and pulse, and with various types of fever. Durling states that Epiphanie medicorum was printed in the author's home in Nuremberg by the so-called 'Printer of the Sodalitas Celtica,' tentatively identified as Friedrich Peypus.

A native of Nordlingen, Pinder practiced medicine there from 1484-1489, before becoming in turn physician to the Elector Frederick of Saxony, and, in 1493, physician to the City of Nuremberg. This diagnostic treatise for the use of physicians, divided into three sections treating uroscopy, analysis of the pulse, and the various types of fever, was printed on a press that Pinder had installed in his house in 1505, probably by his future son-in-law Friedrich Peypus, who printed at least 11 editions there between 1505 and 1513, mostly of Pinder's works. The



types are those of the Printer of the Sodalitas Celtica, with whom Peypus may have learned printing. In 1515 Peypus moved the press - apparently part of his wife's dowry - to a new address; he remained active until 1534 (cf. Benzing pp. 332-333, nos. 12 and 15). The volume also includes Gilles de Corbeil's Carmina de urinarum judiciis, but omits the epilogue found in Choulant's edition of that text. "Pinder's edition is not listed in Choulant's bibliography of printed editions of Gilles, and contains a number of variant readings not recorded by him" (Durling).

[Hundt:] Norman 1115; Garrison-Morton 363.3; Stillwell 664; Flamm 15; Choulant-Frank pp. 125-126; Wellcome 3362a (lacking last 4ff).




Tabule
vrinarum
fo.
II
Albus color vt aqua fontis:
Glaucus color vt cornu lucidũ
Lacce9 color vt serum lactis.
Caropos color: vt vellus cameli.
Subpallidus color vt succus carnis semicoctus non remisse.
Remissus pallidus vt succ9 carnis semicoct9 remissi.
Subcitrinus vt pomi subcitrini non remissus
Citrin9 color vt pomi citrini remissi.
Subruffus color vt aurum remissum.
Ruffus vt aurũ purũ intensum
Subrubicũdus color vt croc9 occidentalis.
Rubeus vt crocus orientalis.
Subrubicũdus vt flãma ignis remissa.
Rubicundus vt flãma iguis nõ remissa.
Inops color vt epatis animalis.
Kyamos color: vt vinum bene nigrum.
Viridis color vt caulis viridis.
Liuid9 color vt plumbum.
Niger vt incaustum.
Niger vt cornu bene nigrum.
A ii

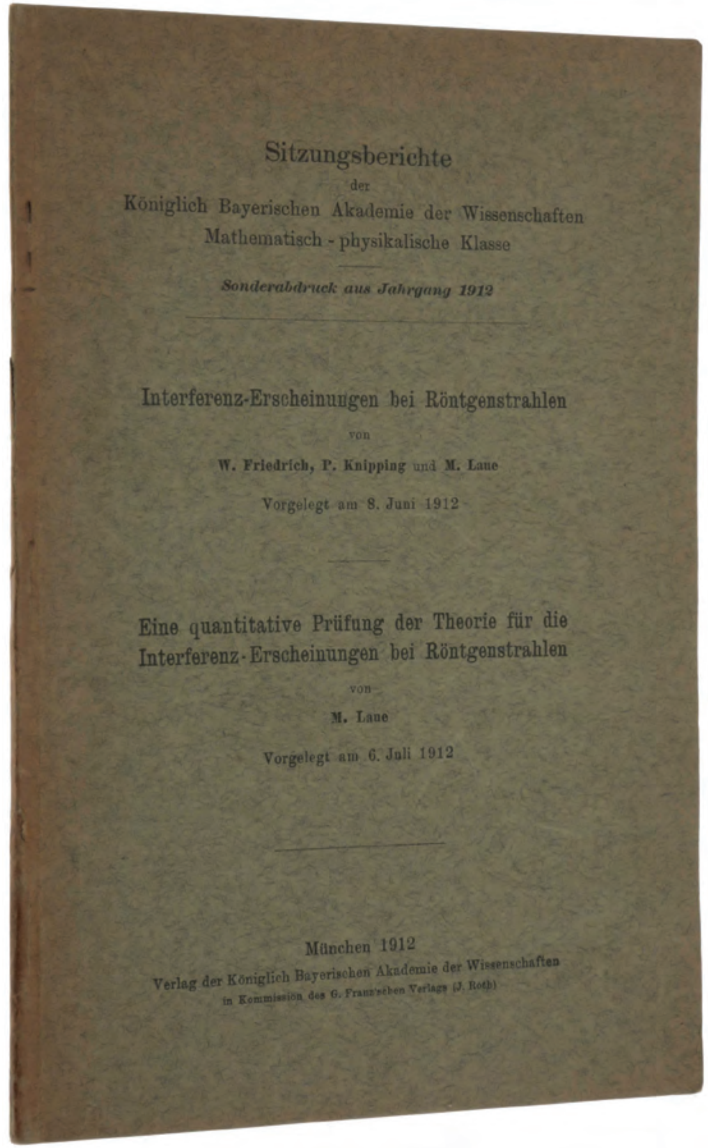
PMM 406 - "ONE OF THE MOST BEAUTIFUL DISCOVERIES IN PHYSICS" (EINSTEIN)

LAUE, Max von, Walter FRIEDRICH & Paul KNIPPING. *"Interferenz-Erscheinungen bei Röntgenstrahlen." - "Eine quantitative Prüfung der Theorie für den Interferenz-Erscheinungen bei Röntgenstrahlen." Offprint (containing both papers) from the Sitzungsberichte der Königlich Bayerischen Akademie der Wissenschaften Mathematisch-physikalische Klasse (1912).* Münich: F. Straub for the Verlag der Königlich Bayerischen Akademie der Wissenschaften, 1912.

$32,500

Large 12mo (220 x 144 mm), pp. 303-322 & 363-373, with three line block diagrams in text and five collotype plates. Stapled in original printed green wrappers, a very fine copy. Preserved in a clamshell cloth box.

First edition, very rare offprint issue, of Laue's Nobel Prize-winning report of "one of the most beautiful discoveries in physics" (Einstein). X-rays had been in wide use since their discovery in 1895 but their exact nature as electromagnetic waves of short wavelength was first elucidated by Laue and his collaborators in the present papers. Laue (1879-1960) had moved in 1909 from Berlin (where he was Planck's assistant) to Ludwig Maximillians University in Münich, where he was Arnold Sommerfeld's Privatdozent. In the spring of 1912 he was asked by Sommerfeld's doctoral student Paul Ewald a question about the arrangement of atoms in a crystal. In attempting to answer this question "Laue had the crucial idea of sending X-rays through crystals. At this time scientists were very far from



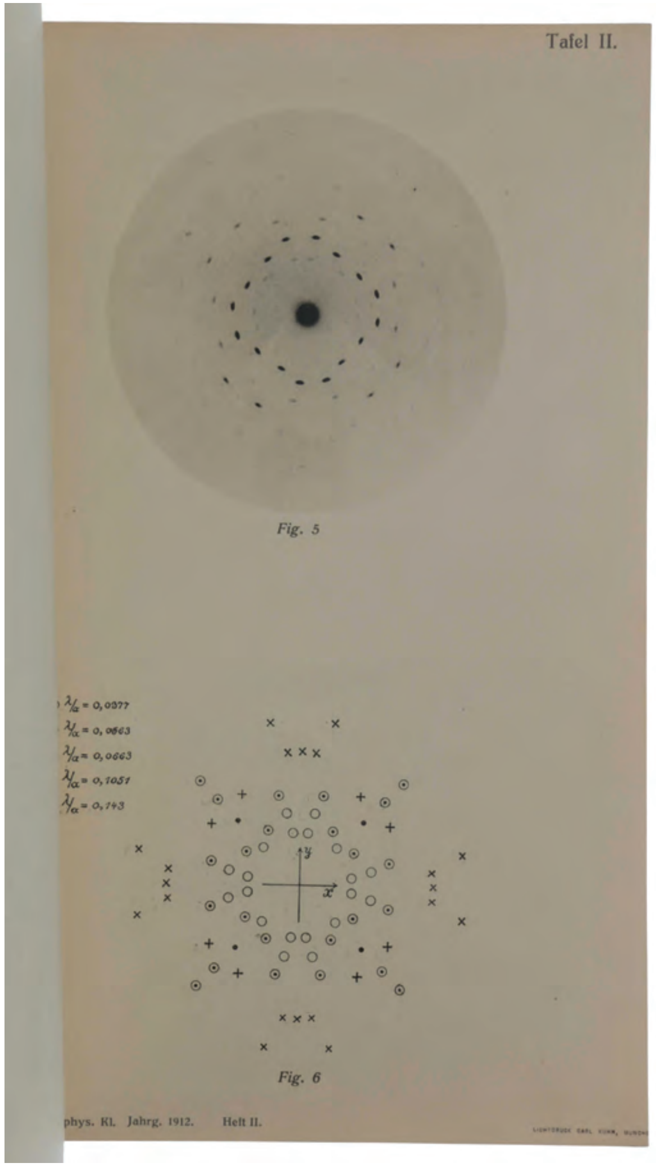
having proven the supposition that the radiation that Röntgen had discovered in 1895 actually consisted of very short electromagnetic waves. Similarly, the physical composition of crystals was in dispute, although it was frequently stated that a regular structure of atoms was the characteristic property of crystals. Laue argued that if these suppositions were correct, then the behavior of X-radiation upon penetrating a crystal should be approximately the same as that of light upon striking a diffraction grating" (DSB), an instrument used for measuring the wavelength of light, inapplicable to X-rays because their wavelength is too short. Sommerfeld was initially skeptical but Laue persisted, enlisting the help of Sommerfeld's experimental assistant Walter Friedrich (b. 1883) in his spare time as well as that of the doctoral student Paul Knipping. On April 12, 1912, Friedrich and Knipping succeeded in producing a regular pattern of dark spots on a photographic plate placed behind a copper sulphate crystal which had been bombarded with X-rays. Laue's second paper contains his complicated mathematical explanation of the phenomenon. "The awarding of the Nobel Prize in physics for 1914 to Laue indicated the significance of the discovery that Albert Einstein called 'one of the most beautiful in physics'. Subsequently it was possible to investigate X-radiation itself by means of wavelength determinations as well as to study the structure of the irradiated material. In the truest sense of the word scientists began to cast light on the structure of matter" (DSB). The following year the Prize was granted to the father and son team W. H. and W. L. Bragg for their exploration of crystal structure using X-rays. ABPC/RBH lists three other copies of this offprint (Christie's, 4 October 2002, lot 151, $5736; Sotheby's, 11 January 2001, lot 333, $10,200; Christie's 29 October 1998, lot 1161, $16,100).

In 1912, "the nature of the X rays discovered by Röntgen in 1895 was not known. Röntgen himself conjectured that they might be longitudinal ether waves as opposed to the transverse ones, the electromagnetic waves found by Hertz. Since in Röntgen's original experiment the X-rays originated from the point where cathode rays, i.e., electrons, hit matter, Wiechert and also Stokes suggested already in 1896 that X-rays were emitted by electrons while the latter were decelerated. In Maxwell's theory an electric charge with a velocity, which is not constant, emits electromagnetic waves. In the Hertzian dipole antenna the charges oscillate to and fro. In a Röntgen tube electrons lose their velocity hitting a piece of matter. The fact that interference effects, characteristic of all waves, in particular, light and Hertzian waves, were not observed for X-rays, did not preclude that they were electromagnetic waves. Their wavelength might be too small for the detection of interference …

"The nature of X-rays, homogeneous or heterogeneous, remained a mystery. They could be understood as electromagnetic waves of short wavelengths or as new neutral particles. The former standpoint was taken, for instance, by Barkla, the latter by William Henry Bragg. One of Bragg's arguments ran like this: X-rays, produced by electrons falling on matter, fly more or less in the same direction as the incident electrons. That is easily understood if one assumes them to be particles. The production of X-rays can then be seen as a collision process, just as one billiard ball hitting another. For some time the two scientists fought out the *Barkla–Bragg controversy* in the columns of *Nature*. Sommerfeld showed that, contrary to the expectations of Bragg and others, electromagnetic radiation is emitted mostly in forward direction if a fast electron suffers a sudden deceleration. The German term *bremsstrahlung* [breaking radiation] is still used commonly in the literature.

"It was the work of Laue and the experiment done by Friedrich and Knipping on his suggestion that cleared up the nature of X-rays once and for all and that, moreover, beautifully demonstrated that crystals are composed of atoms arranged in a regular lattice. Laue had studied mathematics and physics in Strasbourg, Göttingen, Münich, and Berlin, where in 1903 he took his Ph.D. with a thesis under Planck. Feeling that he still had to continue his studies he went for another two years to Göttingen. In 1905 Planck offered him a position as his assistant. Laue worked with Planck on the latter's speciality, the entropy of radiation. In the autumn of 1905 Planck gave a talk in the Berlin Physics Colloquium on Einstein's first paper *The Electrodynamics of Moving Bodies*. Laue was deeply impressed. In 1906, when on a mountaineering trip in Switzerland, as one of the first (possibly the very first) visitor from abroad, he looked up Einstein in the patent office in Bern. In 1907 he published a paper in which he showed that classic experiment by Fizeau, who had measured the velocity of light in a moving liquid, was in accordance with Einstein's theory. Laue became a Privatdozent in Berlin and, also in that capacity, moved to Münich University in 1909. In 1910 he wrote the first book on the theory of relativity …

"The towering figures in Münich were Röntgen (who, however, took only little part in research any more) and Sommerfeld. Early in 1912 Ewald, then a Ph.D. student of Sommerfeld, looked up Laue in his flat. Sommerfeld had asked him to study theoretically the behaviour of light waves in a spatial lattice of polarizable atoms. Laue could not help with the theory but it occurred to him that possibly a similar problem could be studied experimentally if one assumed that a crystal was a regular spatial lattice of atoms and if one passed X-rays through a crystal. (It had been conjectured in the nineteenth century, in particular by Bravais, that the regular shape of a crystal is due to the underlying regular lattice of atoms of which the crystal is composed. But that was a mere hypothesis and not widely discussed.) Assuming the wavelength of X-rays to be on the order of the distance between neighbouring atoms, one might be able to see interference effects. He mentioned this idea to Ewald and it soon got around in the closely-knit group of young physicists in Münich. Soon Friedrich, who had just obtained his Ph.D. with Röntgen and now was Sommerfeld's assistant, became interested. Sommerfeld had to be convinced that the experiment was important enough for Friedrich



to start it right away in spite of other assignments. Knipping, a Ph.D. student of Röntgen, joined in the effort.

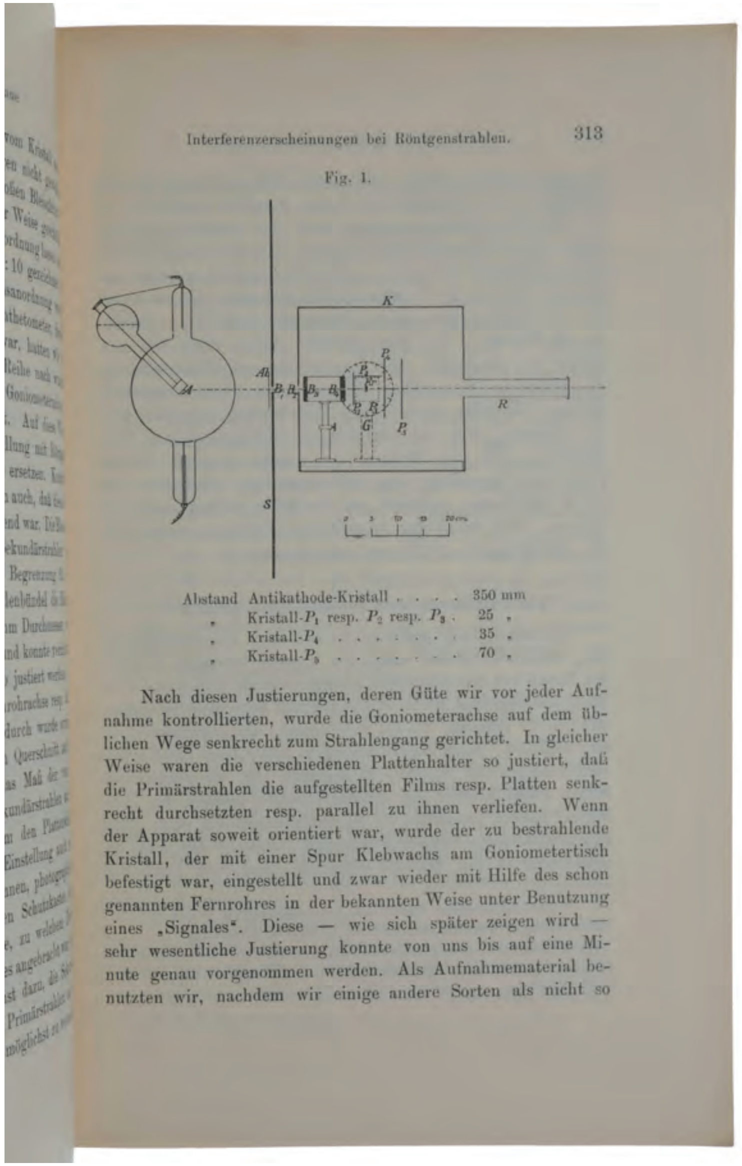
"Friedrich and Knipping sent a collimated beam of X-rays, 3 mm wide, onto a crystal of copper sulphate and placed a photographic plate at some distance behind the crystal. They observed a dark spot, where the undiffracted beam hit the plate. That spot was surrounded by a more or less regular pattern of further spots, which they attributed to diffraction by the crystal lattice. Only after Laue saw the plate did he start in earnest to analyse the problem of diffraction by a spatial lattice. Later he reminisced: 'In deep thought I went home by way of Leopoldstrasse after Friedrich had shown me the picture. And already near my flat, Bismarckstrasse 22, in front of the house Siegfriedstrasse 10, the thought came for the mathematical theory of the phenomenon.'

"Once the idea had come, the rest was simple. Laue advanced in three steps. First he recalled the laws of diffraction of light by a one-dimensional lattice or grid. If white light shines perpendicularly onto a grid made of fine wires, whose distance is on the order of the wavelength of this light, then on a screen behind the grid there will be a line of white light in the forward direction and a repetition of rainbow-like spectra to the left and to the right. If monochromatic light is used, there will be a series of regularly placed sharp lines. This is due to the fact that for a given wavelength only under certain angles light from equivalent points in the grid interferes constructively (i.e., has a phase difference corresponding to an integer number of wavelengths). He then turned to a two-dimensional lattice by replacing the grid by a mesh. Now two such conditions have to be met. Instead of a line pattern only a point pattern is observed for monochromatic light. For white light it is a pattern of patches each dis- playing side by side the colours of the rainbow. (The reader can easily do the experiment by looking at a street light through the cloth of her or his umbrella at night.) Up to here all was well known. But now Laue found that for a three- dimensional lattice a third condition had to be met. The spots are characterized by two angles (diffraction in horizontal and vertical direction). A third condition means that only a small number of spots is formed and only for certain wavelengths. Laue, of course, wrote his conditions in mathematical form, later called the *Laue equations*.

"Friedrich and Knipping improved their apparatus. They reduced the beam diameter, shielded their set-up by a lead box from stray radiation and, for the same reason, allowed the undiffracted beam to leave it through a lead tube. Instead of the original copper sulphate they used a carefully polished crystal of zinc blende. Also, they took great care to make the symmetry axis of the crystal coincide with the beam axis. As a result, they obtained photographs with beautifully symmetric sharp spots fitting Laue's theory. Such a photograph is now called a *Laue diagram* and the process leading to it is called *Laue diffraction*. A joint paper was written ['Interferenz-Erscheinungen bei Röntgenstrahlen'] with the theoretical part signed by Laue and the experimental part signed by Friedrich and Knipping and communicated by Sommerfeld on 8 June 1912 to the Bavarian Academy of Science. That same day Laue gave a talk on a session of the German Physical Society in Berlin 'at the same spot', as he proudly remembered, 'where in December 1900 Planck had first talked about his radiation law and the quantum theory'. Laue, Friedrich and Knipping assumed that the zinc blende crystal had a simple cubic lattice with zinc and sulphur atoms alternating on the corners of cubes of side length a. According to Laue's theory, the radiation responsible for one point in the diagram is monochromatic. All the points in the Laue diagram of zinc blende corresponded to just five different wavelengths. In July 1912 an extended numerical analysis of the diagram ['Eine quantitative Prüfung der Theorie für den Interferenz-Erscheinungen bei Röntgenstrahlen'] was published by Laue alone. He found that all the points in the diagram were explained by his theory but that not in every position in which his theory allowed for a point such a point was observed. He therefore assumed that the radiation emitted by the X-ray tube was a mixture of five monochromatic radiations, each with its own wave-length" (Brandt, pp. 80-83).

The two papers were first printed in 1912 in the *Sitzungsberichte der Königlich Bayerischen Akademie der Wissenschaften*; the offered offprint contains both papers. The two papers were reprinted the following year in *Annalen der Physik* (4 Folge, Bd. 41, pp. 971-988 & 989-1002).

PMM 406a; Norman 1283. Brandt, *The Harvest of a Century*, 2009.



Interferenzerscheinungen bei Röntgenstrahlen. 313

Fig. 1.

Abstand	Antikathode-Kristall	350 mm	
„	Kristall-P_1 resp. P_2 resp. P_3 .	25	„
„	Kristall-P_4	35	„
„	Kristall-P_5	70	„

Nach diesen Justierungen, deren Güte wir vor jeder Aufnahme kontrollierten, wurde die Goniometerachse auf dem üblichen Wege senkrecht zum Strahlengang gerichtet. In gleicher Weise waren die verschiedenen Plattenhalter so justiert, daß die Primärstrahlen die aufgestellten Films resp. Platten senkrecht durchsetzten resp. parallel zu ihnen verliefen. Wenn der Apparat soweit orientiert war, wurde der zu bestrahlende Kristall, der mit einer Spur Klebwachs am Goniometertisch befestigt war, eingestellt und zwar wieder mit Hilfe des schon genannten Fernrohres in der bekannten Weise unter Benutzung eines „Signales". Diese — wie sich später zeigen wird — sehr wesentliche Justierung konnte von uns bis auf eine Minute genau vorgenommen werden. Als Aufnahmematerial benutzten wir, nachdem wir einige andere Sorten als nicht so

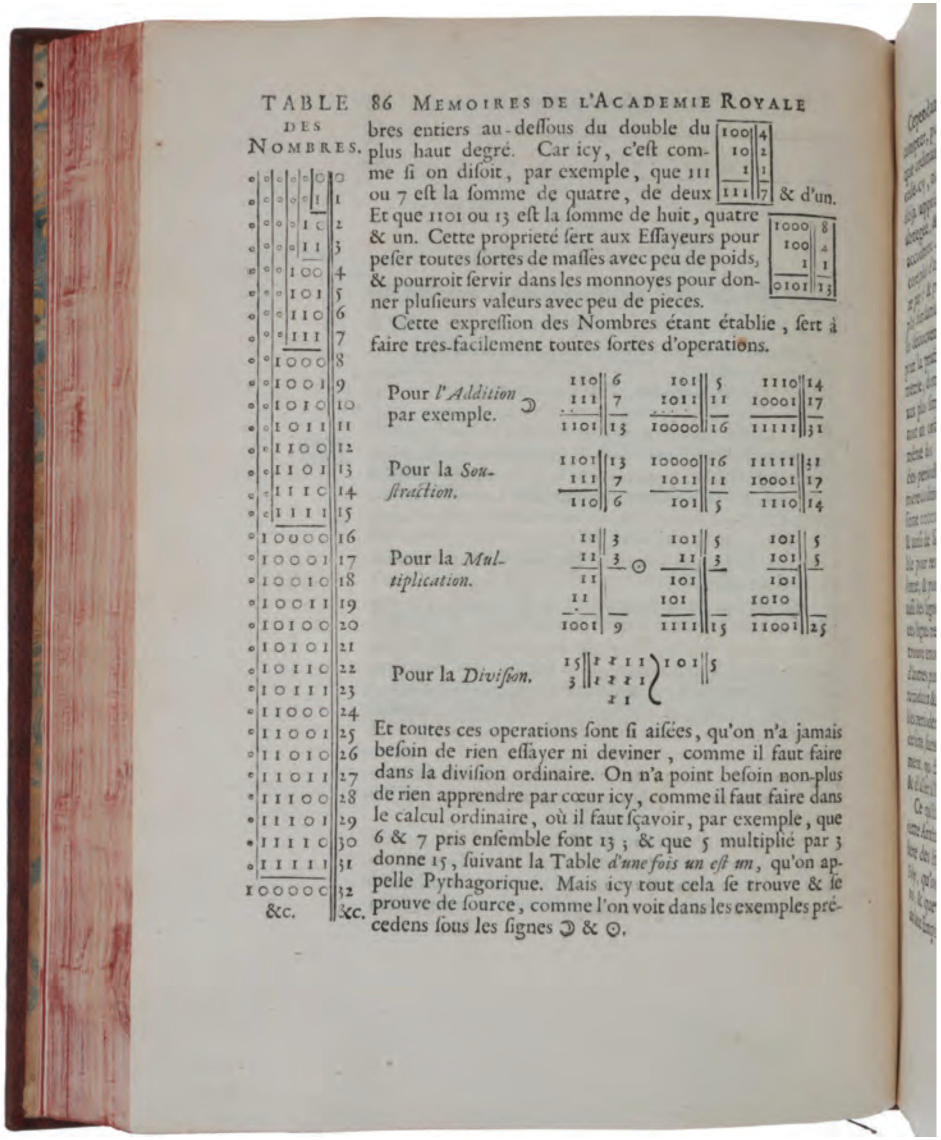
THE FOUNDATION OF THE ELECTRONIC COMPUTER INDUSTRY

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

$19,500

4to (243 x 182 mm), pp. [x], 148, 467, [2], with engraved frontispiece and 12 engraved plates (10 folding). Contemporary calf with richly gilt spine, hinges and capitals with some very well done leather restoration.

First edition, first issue, of Leibniz's invention of binary arithmetic, the foundation of the electronic computer industry. This is the second of Leibniz's great trilogy of works on mathematics and computation, following *Nova methodus pro maximis et minimis* (1684), his independent invention of calculus, and preceding *Brevis descriptio machinae arithmeticae* (1710), his (decimal) mechanical calculating machine. "A dated manuscript by Gottfried Wilhelm Leibniz, preserved in the Niedersachsische Landesbibliothek, Hannover, 'includes a brief discussion of the possibility of designing a mechanical binary calculator which would use moving balls to represent binary digits.' Though Leibniz thought of the application of binary arithmetic to computing in 1679, the machine he outlined was never built, and he published nothing on the subject until [the offered work]" (Norman).



Leibniz viewed binary arithmetic less as a computational tool than as a means of discovering mathematical, philosophical and even theological truths. It was a candidate for the *characteristica generalis*, his long sought-for alphabet of human thought. With base 2 numeration Leibniz witnessed a confluence of several intellectual strands in his world view, including theological and mystical ideas of order, harmony and creation. ABPC/RBH list only one copy of this first issue (Zisska & Schauer, May 4, 2011, lot 461, €5,616). The copy of the extracted leaves sold at the Hans Merkle sale (Reiss, Auktion 85, October 15, 2002, lot 696) realized €6500.

"In the domain of mathematics, Leibniz regarded binary notation as intrinsically superior to decimal notation. Over and above this advantage, however, he believed that it contained the key to resolving both the problem of conceptual primitives and the problem of adequate characters. If it could be established, as Leibniz speculated from about 1679 onwards, that the only truly primitive concepts were those of God and Nothingness (or Being and Privation), then the symbols *1* and *0* would form the basis for an adequate characteristic, whose simplest signs would stand in an immediate relation to the two conceptual primitives" (Jolley, pp. 236-237).

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

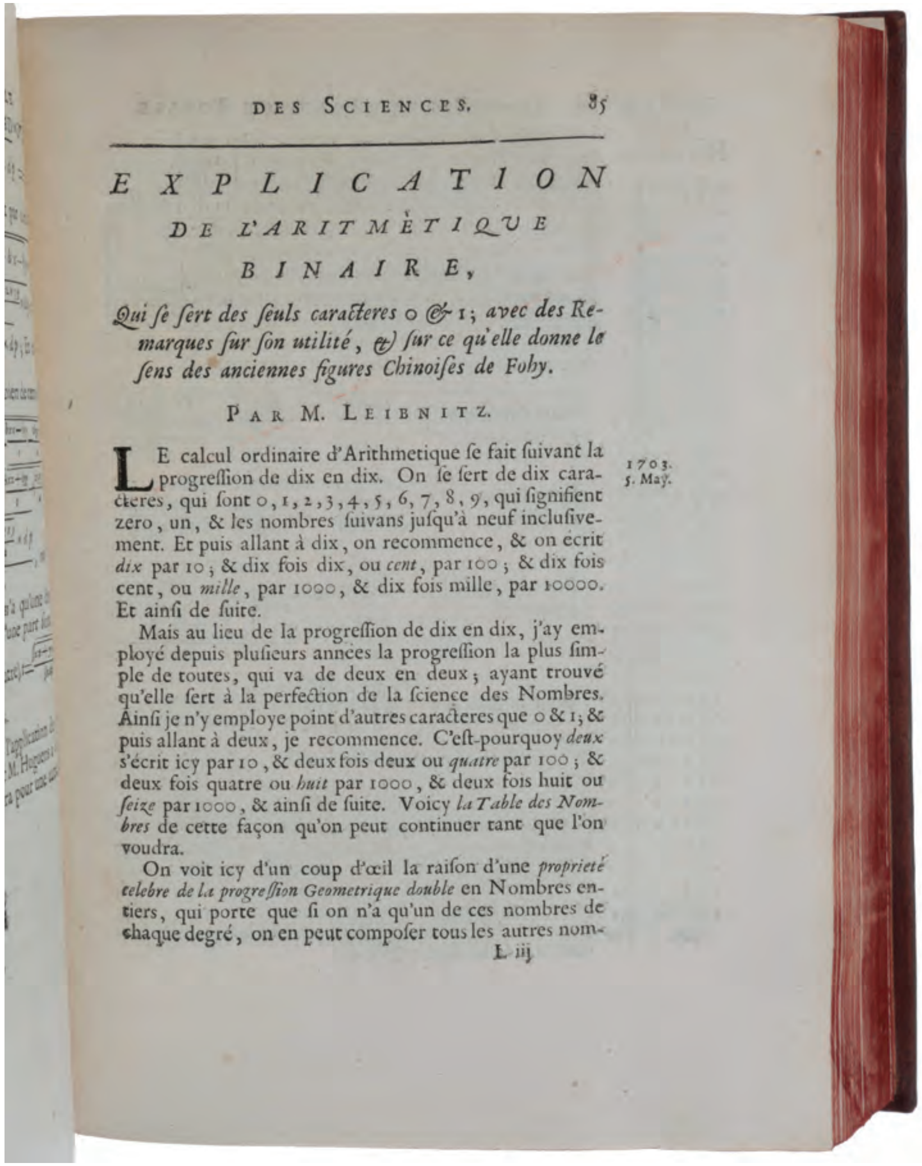
"… in April [1697] he [Leibniz] edited a collection of letters and essays by members of the Jesuit mission in China, entitled *Novissima Sinica* … One of the copies of the *Novissima Sinica* that Leibniz sent to to Verjus [Antoine Verjus, the leader of the mission] came into the hands of Joachim Bouvet,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

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

"Early in 1700 Leibniz was elected a foreign member of the reconstituted Royal Academy of Sciences in Paris. This brought him into correspondence with Fontenelle … In return for his election to the Academy, he contributed papers on the binary system of arithmetic [offered here]" (*ibid*., p. 218).

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

"The possession of Naudé's tables enabled Leibniz to compose his *Essay d'une nouvelle science des nombres*, which he sent from Wolfenbüttel on 26 February 1701 to the Paris Academy of Sciences to mark his election as a foreign member. In his essay, and also in his letter to Fontenelle, he explained that the new system of arithmetic was not intended for practical calculation but rather for the development of number theory. To Fontenelle he remarked that, before publication, there was perhaps a need to add something more profound and he hoped that some young scholar might be stimulated to collaborate with him to this



DES SCIENCES. 85

EXPLICATION DE L'ARITMÈTIQUE BINAIRE,

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

PAR M. LEIBNITZ.

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

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

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

L iij

end … Concerning his decision to communicate his binary system, although the applications had not been achieved, Leibniz explained [in a latter to L'Hospital] that, in view of his many commitments that prevented him from bringing his researches to completion, he feared that his continued silence might lead to the loss of an idea which seemed worthy of conservation.

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

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

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

"Owing to his separation from the real scholars of the time, who for political reasons shunned the Court circles on which he had to rely for his information, Bouvet had been mistaken in his belief in the antiquity of the Fu-Hsi arrangement of the hexagrams. For this order was the creation of Shao Yung, who lived in the eleventh century … In the *I ching* the hexagrams are arranged in a different order, attributed to King Wen (ca. 1050 BC) … This order lacks even a superficial resemblance to a number system.

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

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

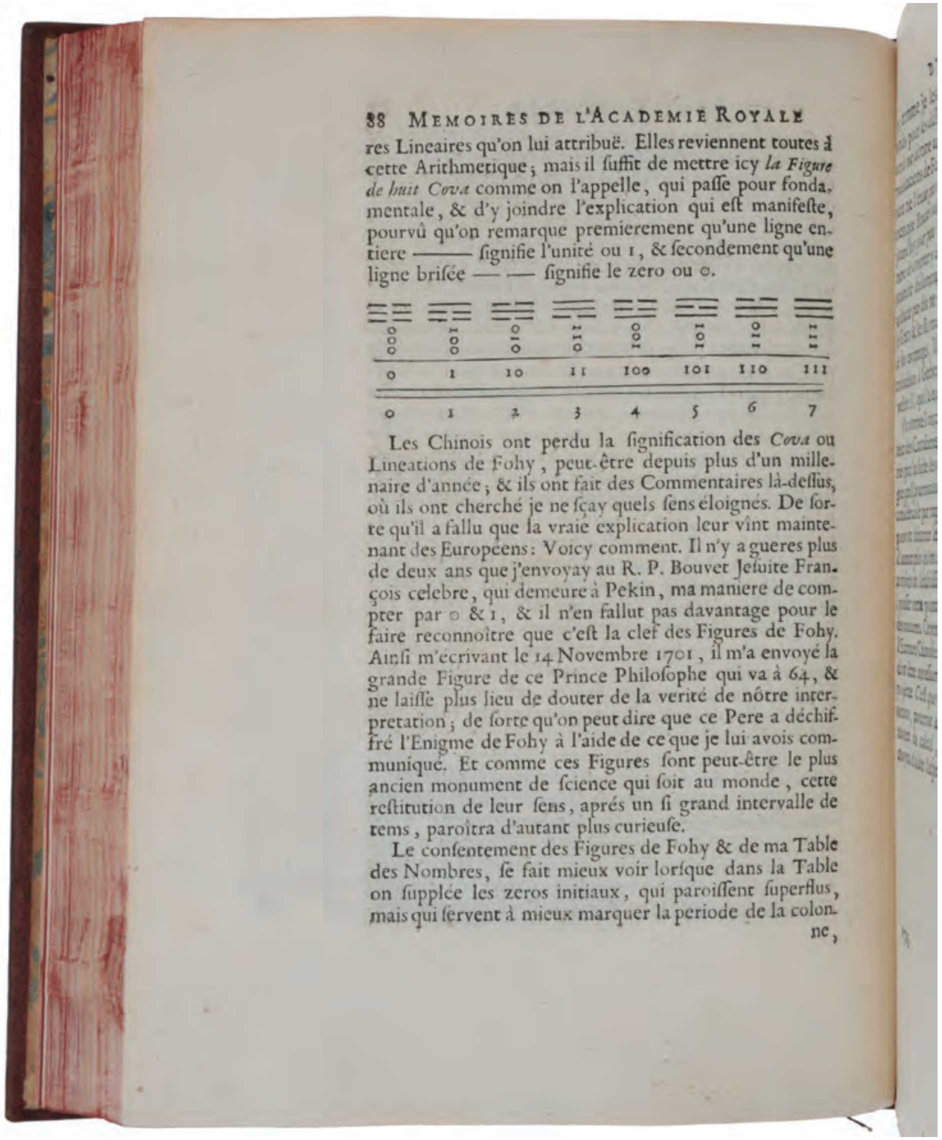
The article 'Nouvelle Arithmetique binaire' in the *Histoire* part of this volume is unsigned, but is actually by Bernard Le Bovier de Fontenelle. His article constituted an editorial comment on the 'Explication' of Leibniz.

"Fontenelle pointed out that ten need not be the base of our arithmetic, and that indeed certain other bases would have advantages over it. Base 12, for example, would simplify dealings with certain fractions such as 1/3 and 1/4. He also noted that numbers have two sorts of properties, essential ones and those dependent on the manner of expressing them. As an example of the former he cited the property that the sum of the first *n* odd numbers equals n^2, and of the latter that a number divisible by 9 has a digit sum also divisible by 9. This same property would hold for 11 in the case of base 12. He reported that Leibniz had worked with the simplest of all possible bases, base two. This base was not recommended for common use because of the excessive length of its number representations, but Leibniz considered it particularly suitable for difficult research and as possessing advantages absent from other bases. Fontenelle reported further that Leibniz had communicated this binary arithmetic in 1702, but had asked that no mention of it be made in the *Histoire* until he could supply an application. This application eventually came forth in the binary interpretation of the Figures of Fohy. The rest of Fontenelle's article is devoted to reporting that binary arithmetic was invented not only by Leibniz, but also by Professor Lagny at about the same time [i.e., Tomas Fantet de Lagny (1660-1734)]" (Glaser, p. 44).

Lagny "attempted to establish trigonometric tables through the use of transcription into binary arithmetic, which he termed 'natural logarithm' and the properties of which he discovered independently of Leibniz" (DSB). He "used binary arithmetic in his text *Trigonométrie française ou reformée* published in Rochefort in 1703" (MacTutor).

This volume of the *Histoire de l'Académie Royale des Sciences* was reissued at Paris in 1710 (this is the edition reproduced on Gallica), and later in octavo format at Amsterdam.

Aiton. *Leibniz: A Biography*, 1985. Glaser, *A History of Binary and Other Non-Decimal Numeration*, 1971. Jolley (ed.), *Cambridge Companion to Leibniz*, 1995. Van der Blij, 'Combinatorial Aspects of the Hexagrams in the Chinese Book of Changes,' *Scripta Mathematica* 28 (1967), pp. 37-49.



88 MEMOIRES DE L'ACADEMIE ROYALE

res Lineaires qu'on lui attribuë. Elles reviennent toutes à cette Arithmetique; mais il suffit de mettre icy *la Figure de huit Cova* comme on l'appelle, qui passe pour fondamentale, & d'y joindre l'explication qui est manifeste, pourvû qu'on remarque premierement qu'une ligne entiere ——— signifie l'unité ou 1, & secondement qu'une ligne brisée — — signifie le zero ou 0.

000	001	010	011	100	101	110	111
0	1	10	11	100	101	110	111
0	1	2	3	4	5	6	7

Les Chinois ont perdu la signification des *Cova* ou Lineations de Fohy, peut-être depuis plus d'un millenaire d'année; & ils ont fait des Commentaires là-dessus, où ils ont cherché je ne sçay quels sens éloignés. De sorte qu'il a fallu que la vraie explication leur vînt maintenant des Européens: Voicy comment. Il n'y a gueres plus de deux ans que j'envoyay au R. P. Bouvet Jesuite François celebre, qui demeure à Pekin, ma maniere de compter par 0 & 1, & il n'en fallut pas davantage pour le faire reconnoître que c'est la clef des Figures de Fohy. Ainsi m'écrivant le 14 Novembre 1701, il m'a envoyé la grande Figure de ce Prince Philosophe qui va à 64, & ne laisse plus lieu de douter de la verité de nôtre interpretation; de sorte qu'on peut dire que ce Pere a déchiffré l'Enigme de Fohy à l'aide de ce que je lui avois communiqué. Et comme ces Figures sont peut-être le plus ancien monument de science qui soit au monde, cette restitution de leur sens, aprés un si grand intervalle de tems, paroîtra d'autant plus curieuse.

Le consentement des Figures de Fohy & de ma Table des Nombres, se fait mieux voir lorsque dans la Table on supplée les zeros initiaux, qui paroissent superflus, mais qui servent à mieux marquer la periode de la colonne,

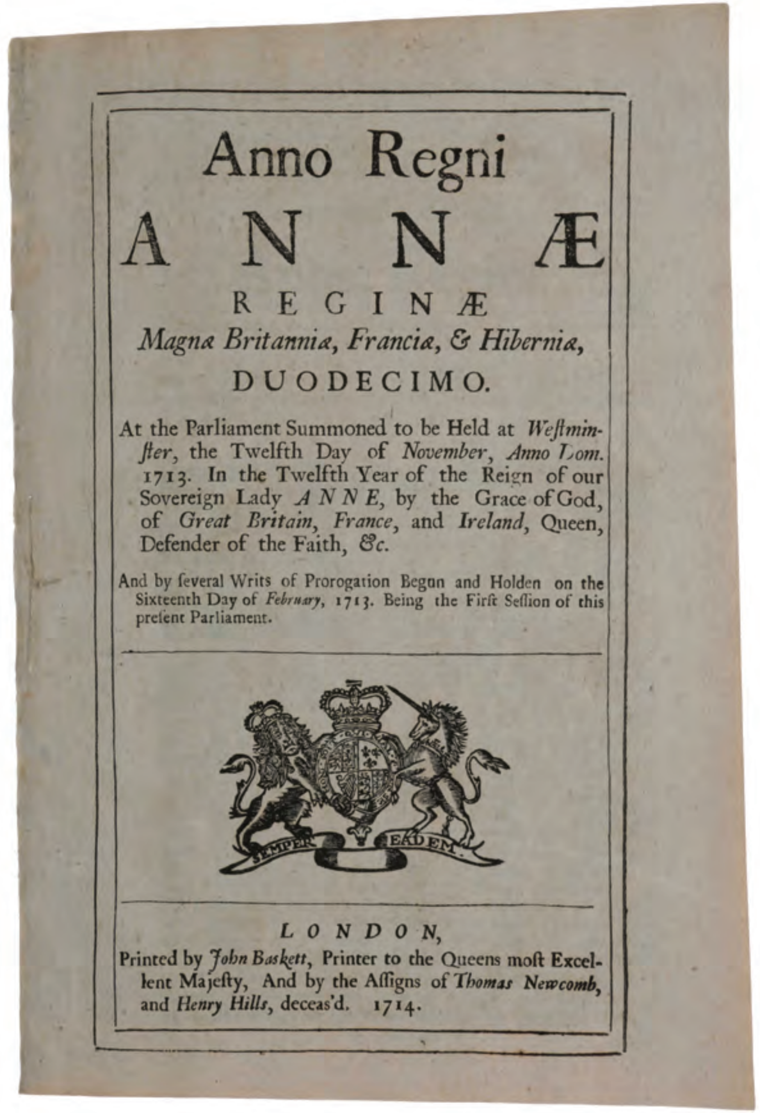
GROLIER/HORBLIT 42 - A REWARD FOR DETERMINING LONGITUDE AT SEA

[LONGITUDE ACT]. *An Act for Providing a Publick Reward for such Person or Persons as shall Discover the Longitude at Sea.* London: Printed by John Baskett … And by the Assigns of Thomas Newcomb and Henry Hills, 1714.

$12,500

Folio (278 x 186 mm), pp. [ii], 355-357, [1, blank], title with woodcut royal arms, factotum initial opening text. Stitched.

First edition, and a fine copy, of the Act of 1714 establishing a reward for the discovery of a method of determining longitude at sea. "An early example of a means adopted by a government for encouraging scientific discovery and progress" (Grolier/Horblit). John Harrison (1693-1776), among others, was so encouraged and eventually solved the longitude problem with the invention of his chronometer H4. "Harrison's chronometer not only supplied navigators with a perfect instrument for observing the true geographical position at any moment during their voyage, but also laid the foundation for the compilation of exact charts of the deep seas and the coastal waters of me world … There has possibly been no advance of comparable importance in aids to navigation until the introduction of radar" (PMM 208). "The Act of 1714 constituted 24 Commissioners either by name or office; if five or more thought a longitude proposal promising, they could direct the Commissioners of the Navy to have their Treasurer issue up to £2000 in total to conduct trials. After experiments were made, the Commissioners of the Longitude or 'the major part of them' were to determine whether the



tested proposal was 'Practicable, and to what Degree of Exactness'. The Act set up a three-tiered reward system for methods which were deemed successful" (Baker). This and the ensuing longitude acts passed between 1714 and 1828 set a precedent for government funding and, within fifty years, also gave rise to a unique standing body, the Board of Longitude, that encouraged and helped to define British science and technology at large. Although it is its conflict with John Harrison, who claimed the prize, which now most characterizes the Board of Longitude, and Parliament's longitude legislation, in the public mind, the Board involved itself in wide-ranging scientific, technological, and maritime activities – such as the annual publication of the *Nautical Almanac,* the improvement of diverse technologies, the establishment of observatories abroad, and voyages including those of Captain Cook and of Arctic exploration. The act was issued both separately, as here, and as part of the collected acts of Parliament for the 12th year of Queen Anne's reign.

"The *latitude* at sea could easily be determined from the altitude of celestial bodies, but the early sailors had no way to measure the longitude, other than by estimating the number of miles sailed east or west, which was often little more than inspired guesswork. The lack of method was increasingly felt in the seventeenth century and was, in fact, the main reason for the founding of the Royal Observatory at Greenwich in 1675.

"Two particular incidents accelerated the founding of the Board [of Longitude]. In 1707 a squadron under Sir Clowdisley Shovel ran aground off [the Isles of] Scilly with the loss of some 2,000 lives: Britain's worst maritime disaster. Then in 1713 the mathematicians William Whiston and Humphrey Ditton suggested a scheme for determining longitude by anchoring ships along the main sea-lanes and firing a shell timed to explode at a height of over a mile. The time between the flash and the corresponding sound would give the distance to any ship within range. "Although completely impractical it received widespread publicity and encouraged a petition to Parliament by several sea-captains and London merchants which suggested that Parliament should offer a prize for finding a solution. The government took this seriously and a Parliamentary committee was set up to report on the problem. Among others this included Isaac Newton, Edmond Halley and Samuel Clarke, and the committee recommended that a reward should be offered for finding longitude at sea.

"A Bill was presented in June 1714 'for Providing a Publick Reward for such Person or Persons as shall Discover the Longitude at Sea.' It received the Royal Assent by Queen Anne on July 20, 1714, only 12 days before she died.

"The Act offered rewards of up to £20,000 for discovering longitude at sea to within certain limits of accuracy: £10,000 if accurate to one degree of the great circle (60 nautical miles), £15,000 if to 2/3° (40 nautical miles) and £20,000 if to 1/2° (30 nautical miles). This sum was unprecedented by the standard of the day. By comparison, the Astronomer Royal's salary was originally only £100, rising to £300 under [Nevil] Maskelyne. It is estimated that £20,000 in 1714 is the equivalent of at least £1 million in the currency of the 1980s.

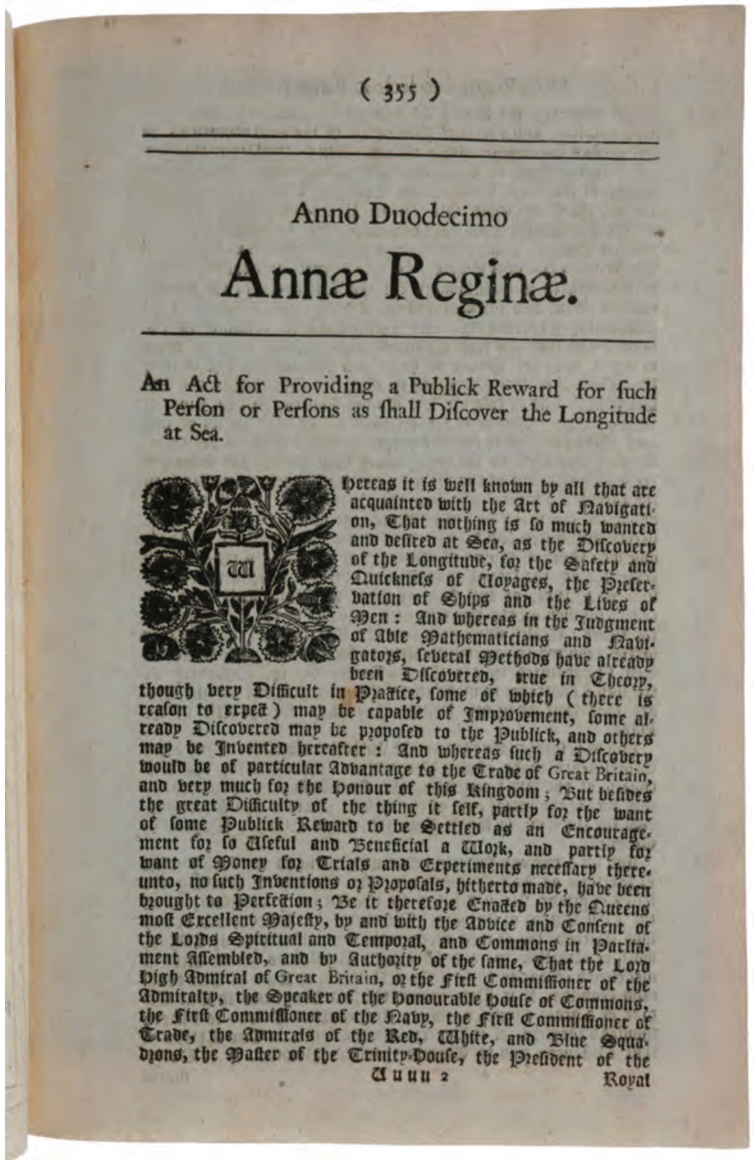
"Half the reward was to be paid if the method extended to 80 nautical miles from shore, the place of greatest danger, the other half if successful over a longer distance, such as the voyage to the West Indies. The method had to be 'practicable and useful at sea,' a vague term which was to be the subject of much controversy later.

"Small sums could be advanced towards schemes that seemed promising for further experimentation. If a method did not reach the listed limits of accuracy but was still considered to be useful, a smaller reward could be offered. These rewards were open to people of all nationalities and many applicants came from overseas, especially from France and Spain, the other two leading seafaring nations of the day. The reward also stimulated mathematicians and astronomers the world over to work on the problem.

"The Longitude Act appointed a group of Commissioners who came to be known as the Board of Longitude. Their function was to consider the suitability of schemes and pass on their opinions to the government. They obviously had to be selected so that they were competent in dealing with such scientific and technical matters. They comprised admirals, the Master of Trinity House, the President of the Royal Society, the Astronomer Royal, professors at Oxford and Cambridge, and ten members of Parliament [the Astronomer Royal had the final say as to the suitability of schemes and instruments] …

"The basic theory behind discovering longitude is simplicity itself. As each 15 degrees of longitude corresponds to a time difference of one hour, it is only necessary to compare the local time (using the Sun's highest altitude to determine noon) with the time at Greenwich (or any other reference meridian). One thus had to have on board a timekeeper from which Greenwich time could be found to the requisite precision. The difficulty was to design such a chronometer which could keep time accurately over months on board ship, with extremes of heat and cold, damp or drought.

"The successful inventor was John Harrison, the man who solved the longitude problem and who eventually won the £20,000 prize. The early years of the Board were dominated by Harrison and his chronometers, his problems, the testing of the timekeepers and his struggle to obtain the reward. The earliest surviving confirmed minutes of 1737 open with the words 'Mr John Harrison produced a new invented machine, in the nature of Clock Work, whereby he proposes to keep



(355)

Anno Duodecimo

Annæ Reginæ.

An Act for Providing a Publick Reward for ſuch Perſon or Perſons as ſhall Diſcover the Longitude at Sea.

Whereas it is well known by all that are acquainted with the Art of Navigation, That nothing is ſo much wanted and deſired at Sea, as the Diſcovery of the Longitude, for the Safety and Quickneſs of Voyages, the Preſervation of Ships and the Lives of Men: And whereas in the Judgment of Able Mathematicians and Navigators, ſeveral Methods have already been Diſcovered, true in Theory, though very Difficult in Practice, ſome of which (there is reaſon to expect) may be capable of Improvement, ſome already Diſcovered may be propoſed to the Publick, and others may be Invented hereafter: And whereas ſuch a Diſcovery would be of particular Advantage to the Trade of Great Britain, and very much for the Honour of this Kingdom; But beſides the great Difficulty of the thing it ſelf, partly for the want of ſome Publick Reward to be Settled as an Encouragement for ſo Uſeful and Beneficial a Work, and partly for want of Money for Trials and Experiments neceſſary thereunto, no ſuch Inventions or Propoſals, hitherto made, have been brought to Perfection; Be it therefore Enacted by the Queens moſt Excellent Majeſty, by and with the Advice and Conſent of the Lords Spiritual and Temporal, and Commons in Parliament Aſſembled, and by Authority of the ſame, That the Lord High Admiral of Great Britain, or the Firſt Commiſſioner of the Admiralty, the Speaker of the Honourable Houſe of Commons, the Firſt Commiſſioner of the Navy, the Firſt Commiſſioner of Trade, the Admirals of the Red, White, and Blue Squadrons, the Maſter of the Trinity-Houſe, the Preſident of the

Uuuu 2 Royal

time at sea with more exactness than by any other instrument or method hitherto contrived.' The trials went on for decades, Harrison's work being encouraged on many occasions by small payments from the Board.

"It was his fourth timekeeper (a large watch, called H4) which was taken on a trial to Jamaica and back in 1761-62. Although Harrison claimed that H4 more than complied with the 1714 Act, the Board disagreed and insisted on a second trial, in which H4 also performed very well.

"The Board was still not fully satisfied, half only of the reward was paid and on condition that Harrison disclosed the construction so that copies could be made. The first of these was built by Larcum Kendall (K1) and was sent with Captain Cook on his second voyage. Despite glowing praise from Cook, Harrison had to appeal to King George III before he was paid the full reward in 1772, at the age of 78. The often-heated exchanges between Harrison and the Board are well-preserved in the archives.

"At the same time as the Board was dealing with Harrison, another method of finding longitude was being acquired. The Moon moves quite slowly across the background stars and its position can in theory be used as a means of finding Greenwich time. The difficulty here was that the Moon's motions were not known sufficiently accurately for tables to be drawn up months or years in advance to determine longitude with sufficient precision.

"In 1755 the Board received accurate lunar tables from the German mathematician Tobias Meyer. They were derived from equations by Leonhard Euler and the observations of Mayer and James Bradley (Astronomer Royal 1742-62). These were improved by a second set of tables which Mayer bequeathed to the Board on his death in 1762. They allowed longitude to be found to within a few nautical miles and also permitted the position of the Moon to be calculated several years in advance.

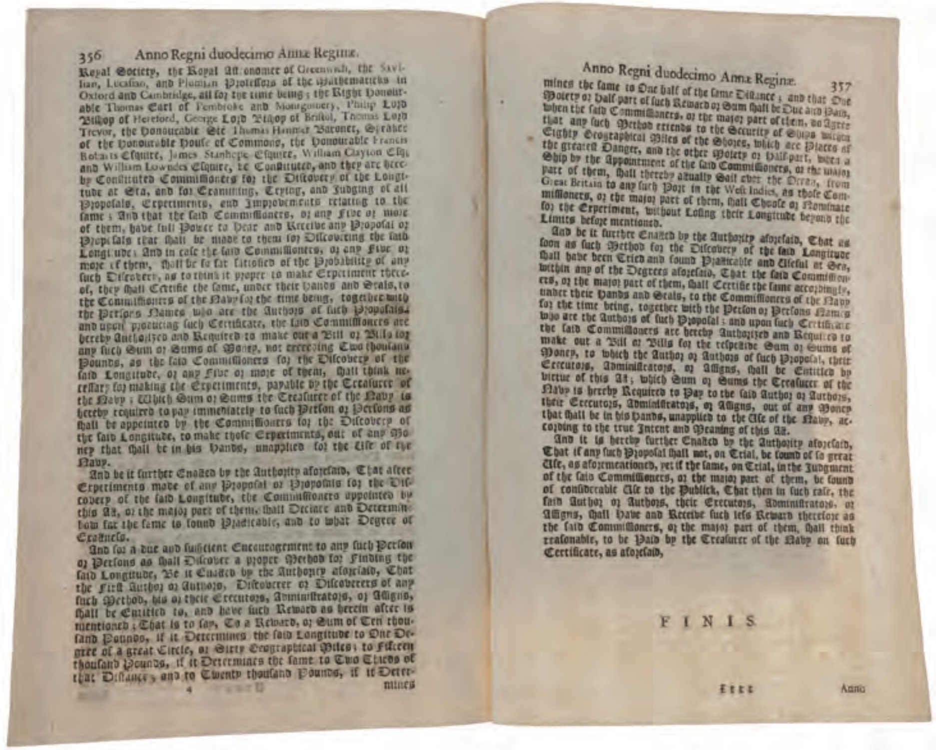
"The Board recommended publication of an annual almanac giving the position of the Moon every three hours, and other information, from which longitude could be found. The *Nautical Almanac* was first published in 1766, for 1767, most of the planning being done by the Astronomer Royal, Nevil Maskelyne. The computations of tables and the printing on the *Almanac* remained a major interest of the Board …

"With the award of the main prize to Harrison and the foundation of the *Nautical Almanac* the Board had fulfilled its role under the 1714 Act. However, it was kept in being by a new Act of 1774 which moved the emphasis away from longitude to navigation in general. The scope of the Board became much wider. Improvements and refinements of navigational instruments were a main concern. Chronometers were improved by men such as Thomas Mudge, John Arnold and Thomas Earnshaw, and sextants were refined using an artificial horizon. Greater accuracy was achieved in the technique of ruling the scale divisions on instruments, notably by Jesse Ramsden.

"The Board also entered into such areas as the accurate measurement of ships' tonnage, meteorology, magnetism and the production of accurate naval charts. It was mainly for the latter reason that the Board became increasingly linked with world exploration through the great voyages of discovery of the late 18th and early 19th centuries … The explorations round Australia, New Zealand and the Pacific owed much to the Board, most notably the second and third voyages of Captain Cook … the Board in 1819 offered rewards for discovering the north-west passage from the Atlantic to the Pacific, or for progressing towards the North Pole … The last major project undertaken by the Board was the foundation of the observatory at the Cape of Good Hope. Its aim was to increase the knowledge of the southern skies and improve navigation south of the equator. The first mention is at the Board meeting of 3 February, 1820 … By this time, though, the role of the Board was becoming even less well-defined and uncertain. It was increasingly linked with the pure astronomy at the Royal Observatory, Greenwich. The ardour of the great voyages of discovery had cooled … Areas of work into which the Board might have moved were taken over by two societies formed about this time, the Astronomical Society of London (later the Royal Astronomical Society) and the Royal Geographical Society. Its remaining responsibilities were subsumed into the work of the Royal Observatory …

"The Board of Longitude was finally dissolved on July 15, 1828 by Act of Parliament. It had been in existence 114 years. Nevertheless, in its time it had been a colourful and influential body. The rewards offered in 1714 stimulated many advances during the next 114 years over a very wide field, just as Parliament hoped would happen. Progress in astronomy worldwide, as well as in navigation and horology, was greatly furthered by the Board's activities. The Board of Longitude was certainly one of the most important and interesting bodies in the development of science and technology of that period" (Johnson).

Grolier/Horblit 42a. Baker, 'Longitude Acts,' Cambridge Digital Library (cudl.lib.cam.ac.uk/view/ES-LON-00023). Johnson, 'The Board of Longitude 1714-1828,' *Journal of the British Astronomical Association* 99 (1989), pp. 63-69.


356 Anno Regni duodecimo Annæ Reginæ.

Anno Regni duodecimo Annæ Reginæ. 357

F I N I S

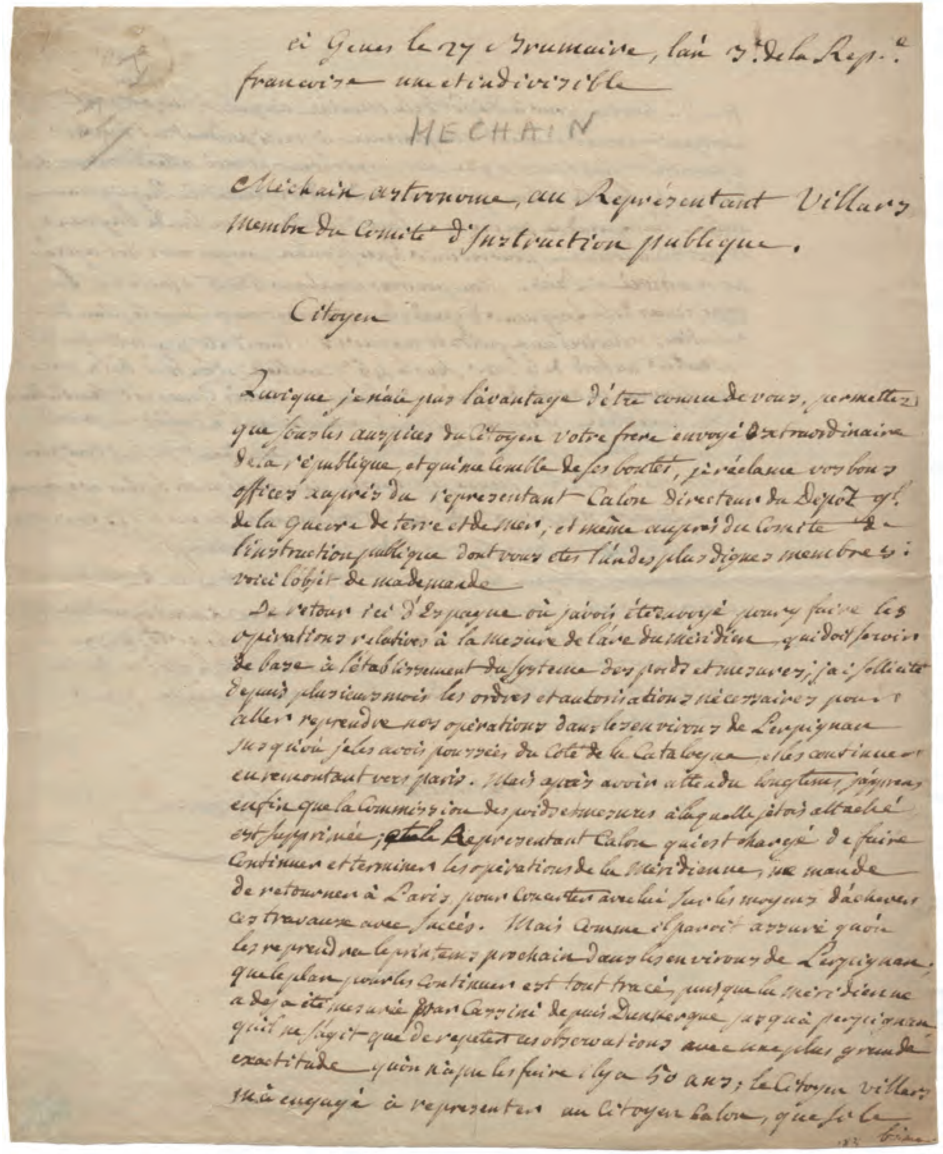
MEASURING THE MERIDIAN: AUTOGRAPH ACCOUNT

MÉCHAIN, Pierre François André. *Autograph letter signed 'Méchain', to the brother of the French Ambassador to Genoa, Jean-Baptiste Dorothée Villars (b. 1742), related to the on-going project to measure the length of the meridian arc between Dunkirk and Barcelona, and thereby establish the fundamental unit of length, the metre, the essential step in the establishment of the metric system.* Genoa: 27 October, 1794.

$2,500

Two pages in French (190 x 228 mm), usual folds, two-pin holes at top left hand corner where stapled, otherwise very good.

A fascinating glimpse into the details of the great expedition undertaken by Méchain and his collaborator Jean Baptiste Joseph Delambre to measure the length of the meridian, and thereby establish the length of the metre, the first step in the establishment of the metric system. Méchain writes to the brother of the French Ambassador to Genoa asking him to intervene with General Étienne-Nicolas de Calon (1726-1807), head of the Department of Military Topography in Paris, who had requested Méchain's return to Paris. Méchain had good reasons for not wishing to return – had he done so he could have become a victim of the Terror, as had several of his colleagues (his family had narrowly escaped it); and in Paris he would also have been forced to hand over his data, which would reveal a major error he had made which vitiated much of the work he had already carried out, and which he was hoping to keep secret. The letter sets out Méchain's


à Gênes le 27 e brumaire, l'an 3e de la Rep.e
françoise une et indivisible

MECHAIN

Méchain astronome, au Representant Villars
Membre du Comité d'Instruction publique.

Citoyen

Quoique je n'aie pas l'avantage d'être connu de vous, permettez
que sous les auspices du Citoyen votre frere envoyé extraordinaire
de la république, et qui me comble de ses bontés, je réclame vos bons
offices auprès du représentant Calon directeur du Depot g.l
de la guerre de terre et de mer, et même auprès du Comité de
l'instruction publique dont vous etes l'un des plus dignes membres;
voici l'objet de ma demande

De retour ici d'Espagne où j'avois été envoyé pour y faire les
opérations relatives à la mesure de l'arc du méridien qui doit servir
de base à l'établissement du systeme des poids et mesures, j'ai sollicité
depuis plusieurs mois les ordres et autorisations nécessaires pour
aller reprendre nos opérations dans les environs de Perpignan
jusqu'où je les avois poussées du Côté de la Catalogne, et les continuer
en remontant vers paris. Mais après avoir attendu longtems j'apprens
enfin que la Commission des poids et mesures à laquelle j'étois attaché
est supprimée; ~~et~~ le Representant Calon qui est chargé de faire
Continuer et terminer les opérations de la Méridienne, me mande
de retourner à Paris, pour concerter avec lui sur les moyens d'achever
ces travaux avec succès. Mais comme il paroit assuré qu'on
les reprendra le printems prochain dans les environs de Perpignan,
que le plan pour les continuer est tout tracé, puisque la méridienne
a deja été mesurée par Cassini depuis Dunkerque jusqu'à perpignan
qu'il ne s'agit que de repeter ces observations avec une plus grande
exactitude qu'on n'a pu les faire il y a 50 ans; le Citoyen Villars
m'a engagé à representer au Citoyen Calon, que si le

plan to remain in Genoa by offering to carry out a pendulum experiment to measure the metre there instead of at Bordeaux, as had already been planned; he also points out that to return to Paris would be difficult and expensive, a wasteful exercise in view of the fact that he would have to return the following spring. "For many centuries there were no general standards for measurement: every trade and craft had its own peculiar units and they differed even in various regions of the same country. Since the development of international trade in the Middle Ages this chaotic situation had become more and more tiresome, but all efforts towards standardization were strongly resisted by vested interest … We owe the introduction of an international metric system to the French Revolution. In 1790 the Académie des Sciences, at the request of Talleyrand, set up a commission to consider the question: among its members were J. C. Borda, Lagrange, Laplace, G. Monge and Condorcet. In 1791 they reported that the fundamental unit of length should be derived from a dimension of the earth: it should be the ten-millionth part of a quadrant of the earth's meridian extending between Dunkirk and Barcelona … The astronomers Jean Baptiste Joseph Delambre and Pierre Francois André Méchain were charged with the task of measuring accurately the newly adopted length along the meridian arc between Dunkirk and Barcelona. Owing to the disturbances of the revolutionary period their work was much impeded, but in 1799 their measurement was completed" (PMM). The final results of the project were reported in Système Mètrique Décimal, ou Mesure de l'Arc du Méridien compris entre les Parallèles de Dunkerque et Barcelone, exécutée en 1792 et Années suivantes, published at Paris in 1806-10, a famous work listed by PMM (260). No manuscripts by Méchain listed by ABPC/RBH.

À Genes le 27 Brumaire, l'an 3.ᵉ de la Rep.[ublique] francoise une et indivisible

Méchain astronome au Representant Villars Membre du Comité d'Instruction Publique

Citoyen

Quoique je n'aie pas l'avantage d'etre connu de vous, permettez que sous les auspices du Citoyen votre frere envoyé extraordinaire de la république, et qui me comble de ses bontés, je réclame vos bons offices auprès du representant Calon directeur du dépôt g[énéra]l de la guerre de terre et de mer, et même auprès du Comité de l'instruction publique dont vous etes l'un des plus dignes membres: voici l'objet de ma demande.

De retour ici d'Espagne où j'avois été envoyé pour y faire les opérations relatives à la mesure de l'axe du méridien, qui doit servir de base à l'établissement du systeme des poids et mesures, j'ai sollicité depuis plusieurs mois les ordres et autorisations nécessaires pour aller reprendre nos opérations dans les environs de Perpignan jusqu'où je les avois poussées du coté de la Catalogne, et les continuer en remontant vers Paris. Mais après avoir attendu longtemps, j'apprens enfin que la Commission des poids et mesures à laquelle j'étois attachée est supprimée ; et le representant Calon qui est chargé de faire continuer et terminer les opérations de la Méridienne, me mande de retourner à Paris pour concerter avec lui sur les moyens d'achever ces travaux avec succès. Mais comme il paroit assuré qu'on les reprendra le printemps prochain dans les environs de Perpignan, que le plan pour les continuer est tout tracé, puisque la méridienne a deja été mesurée par Cassini depuis Dunkerque jusqu'à Perpignan, qu'il ne s'agit que de repeter ces observations avec une plus grande exactitude qu'on n'a pu les faire il y a 50 ans ; le Citoyen Villars m'a engagé à representer au Citoyen Calon, que si le bien du service pour le dépôt de la Marine auquel je suis aussi attaché n'exigeait pas absolument ma présence à Paris pendant cet hyver il paraitroit plus à propos que mes coopérateurs et moi attendissions le retour de la belle saison pour nous rendre directement à Perpignan au lieu d'entreprendre un voyage long et difficile au milieu de l'hyver et très dispendieux, pour revenir à Perpignan presque aussitôt apres notre arrivée à Paris. Nous pourrons employer le tems à faire ici les *expériences de la longueur du pendule, qui entrent aussi dans le plan des opérations relatives aux poids et mesures ; il avoit été prescrit de les exécuter au bord de la mer sous le 45e parallele et au bord de la mer en conséquence on avoit designé Bordeaux; mais Genes est semblablement situé sur le globe, et en y remplissant actuellement cet objet on seroit dispensé d'elles exprès à Bordeaux. Cependant je suis tout près à retourner à Paris, si ce que je propose n'est pas le parti le plus avantageux; notre départ n'en sera retardé que de trois semaines ou un mois environ si l'on veut bien me faire passer la décision sur le champs.*

Je vous aurois, Citoyen, la plus grande obligation, si vous voulez bien conférer sur cela avec le Citoyen Calon. Pardonnez à mon importunité; j'ai osé espérer, qu'aimant protégeant les sciences et les arts; consacré uniquement à tout ce qui peut contribuer à l'utilité à la gloire de la Nation, vous accueillerez ma demande surtout appuyée par votre cher frère.

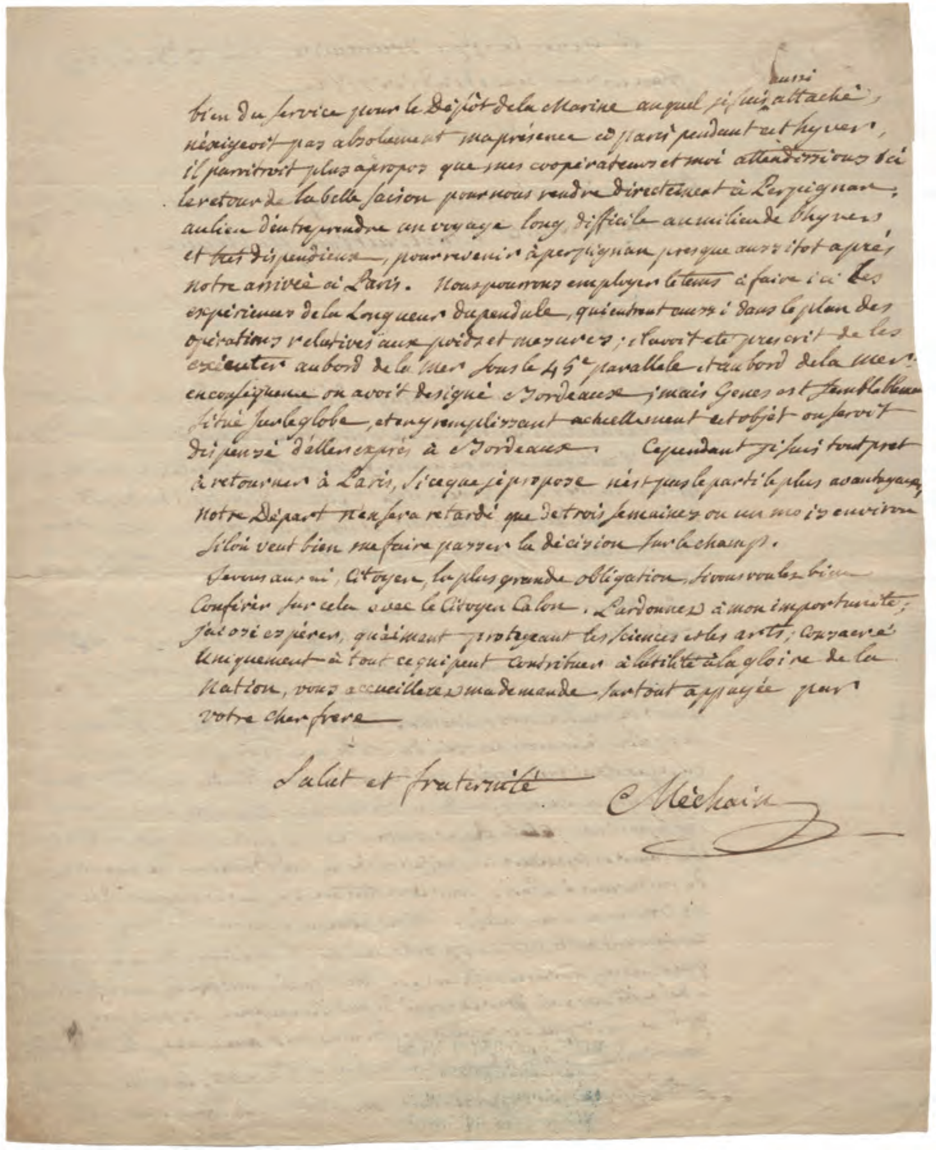
Salut et fraternité

Méchain

Translation:

Méchain astronomer to Representative Villars of the Committee of Public Instruction

Although I do not have the advantage of being known to you, allow that under the auspices of the Citizen your brother, extraordinary envoy of the republic, and who fills me with his kindness, I ask your good offices with the representative Calon, director of the the deposit of war of land and sea, and even with the Committee of Public Instruction, of which you are one of the most worthy members; here is the object of my request.


bien du service pour le Dépôt de la Marine auquel je suis aussi attaché,
n'exigeoit pas absolument ma présence à Paris pendant cet hyver,
il paroitroit plus à propos que mes coopérateurs et moi attendissions ici
le retour de la belle saison pour nous rendre directement à Perpignan,
au lieu d'entreprendre un voyage long, difficile au milieu de l'hyver,
et très dispendieux, pour revenir à Perpignan presque aussitôt après
notre arrivée à Paris. Nous pourrons employer le tems à faire ici les
expériences de la Longueur du pendule, qui entrent aussi dans le plan des
opérations relatives aux poids et mesures; il avoit été prescrit de les
exécuter au bord de la mer sous le 45e parallele et au bord de la mer
en conséquence on avoit designé Bordeaux; mais Genes est semblablement
situé sur le globe, et en y remplissant actuellement cet objet on seroit
dispensé d'elles exprès à Bordeaux. Cependant je suis tout prêt
à retourner à Paris, si ce que je propose n'est pas le parti le plus avantageux;
Notre Départ n'en sera retardé que de trois semaines ou un mois environ
si l'on veut bien me faire passer la décision sur le champs.
Je vous aurois, Citoyen, la plus grande obligation, si vous voulez bien
conférer sur cela avec le Citoyen Calon. Pardonnez à mon importunité;
j'ai osé espérer, qu'aimant protégeant les sciences et les arts; consacré
uniquement à tout ce qui peut contribuer à l'utilité à la gloire de la
Nation, vous accueillerez ma demande surtout appuyée par
votre cher frère

Salut et fraternité Méchain

Returning here from Spain where I had been sent to carry out operations relating to the measurement of the axis of the meridian, which must serve as a basis for the establishment of the system of weights and measures, for several months I have asked for the orders and authorizations necessary to resume our operations in the neighborhood of Perpignan, to where I had pushed them on the side of Catalonia, and to continue them towards Paris. But after having waited a long time, I finally learn that the Weights and Measures Commission to which I was attached is suppressed; and that representative Calon, who is in charge of continuing and terminating the operations of La Meridienne, asks me to return to Paris to consult with him on the means of completing this work successfully. But as it seems certain that they will be resumed next spring in the vicinity of Perpignan, that the plan for continuing them is mapped out, since the meridian has already been measured by Cassini from Dunkerque to Perpignan, that it is only a matter of repeating these observations with greater accuracy than was possible 50 years ago; Citizen Villars asked me to represent to Citizen Calon, that if the good of the service for the depot of the Navy to which I am also attached did not absolutely require my presence in Paris during this winter, it seems more appropriate than my co-workers and I wait for the return of the fine season to go directly to Perpignan instead of taking a very expensive, long and difficult journey in the middle of the winter to return to Perpignan almost immediately after our arrival in Paris. We may use the time here to make experiments on the length of the pendulum, which also enter into the plan of operations relating to weights and measures; it had been prescribed to execute them at the seashore under the 45th parallel, and at the seashore, as a result, Bordeaux had been designated; but Genoa is similarly situated on the globe, and by filling this object at present they would be dispensed from them expressly at Bordeaux. However, I am very near to return to Paris, if what I propose is not the most advantageous solution; our departure will be delayed by only three weeks or a month if we want to make the decision in the field.

I would have you, Citizen, the greatest obligation, if you will confer on it with Citizen Calon. Forgive my importunity; I dared to hope, that a loving protecting the sciences and the arts; devoted solely to all that can contribute to the utility to the glory of the Nation, you will receive my request, especially supported by your dear brother

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

"In place of this Babel of measurement, the savants imagined a universal language of measures that would bring order and reason to the exchange of both goods and information. It would be a rational and coherent system that would induce its users to think about the world in a rational and coherent way. But all the savants' grand plans would have remained fantasy had not the French Revolution -- history's great utopian rupture -- provided them with an unexpected chance to throw off the shackles of custom and build a new world upon principled foundations. Just as the French Revolution had proclaimed universal rights for all people, the savants argued, so too should it proclaim universal measures" (Alder, pp. 2-3).

"In 1790 the National Assembly approved an Academy proposal to establish a decimal system of measures, and Méchain and Delambre were designated to carry out the fundamental geodetic measurements for a new unit of length. This unit, the meter, was intended to be one ten-millionth part of the distance from the terrestrial pole to the equator, and it was to be based on an extended survey from Dunkerque to Barcelona, Méchain was assigned the shorter but more difficult southern zone, the previously unsurveyed region across the Pyrenees.

"The new repeating transit became the fundamental instrument of the survey, but not until June 1792 was the new equipment, including parabolic mirrors for reflecting signals, ready. By this time the Revolution was engulfing France and the monarchy was tottering; Méchain with his suspicious array of instruments was arrested at Essonnes just south of Paris as a potential counterrevolutionary. Only with much difficulty was he located and released two months later, so that he could continue his journey to Spain. In September and October he swiftly carried out the triangulation between Perpignan and Barcelona … His plans were abruptly interrupted at the beginning of spring in 1793 when, invited by a friend to inspect a new hydraulic pump in the outskirts of Barcelona, he was involved in an accident. While trying to start the machine, the friend and an assistant were caught in the mechanism. Méchain, rushing to aid them, was struck by a lever that knocked him violently against the wall, breaking some ribs and a collarbone … During Méchain's convalescence, open war had broken out between Spain and France, and he was denied a passport to return home. Profiting from his captivity, he determined the latitude of Montjouy, just south of Barcelona, and surveyed the triangle connecting these points. He then noticed a 3" discrepancy in the latitude previously obtained for Barcelona. Anguished by his failure to find the cause, and blaming himself for the error, he kept the discrepancy a carefully guarded secret … Eventually Méchain obtained a passport for Italy, and he managed to reach Genoa in September 1794. Saddened by the guillotining of several of his colleagues and in poor health, he delayed his return to France, not embarking for Marseilles until the following year" (DSB, under Méchain).

"Méchain's spirits were not improved by the greeting sent by his colleagues on the Commission of Weights and Measures. At the end of August they finally sent him a copy of the law of 1 August 1793, which had established the metric system … Moreover, they informed him that Delambre had been purged from the Commission, with no one assigned to take his place. Méchain drew his own conclusion: the meridian expedition, he decided, had been 'definitively abandoned' Then, the very next week, came the good news … His supposition had been premature: the meridian project was being revived … General Calon had been had been placed at the head of the department of military topography, with Méchain appointed chief of naval cartography … Official news of this appointment arrived from Calon himself two weeks later, with this request: that Méchain return immediately to Paris so that they might discuss the future of the meridian project together …

"Calon was ordering him to return to Paris, where he would surely have to turn over his data … Worse, the revival of the meridian project meant that his mistake mattered again, that the discrepancy in the Mount-Jouy data was once more an affront to the accuracy of the metre, 'the most important mission with which man has ever been charged' … Most unsettling of all was the news that accompanied the announcement of the meridian project's revival. Lalande confirmed that Lavoisier, Condorcet and several other colleagues had been guillotined. Worse, Lalande informed him that the Terror had struck even closer to home – in the grounds of the Observatory, where his family still lived … Had Méchain been living in Paris at the time, the consequences would have been too awful to contemplate … Yet even after telling him Méchain all this, Lalande still expected him to return to France that month.

"In early October the French ambassador to Genoa was recalled to Paris to account for his Jacobin sympathies, and his replacement, Ambassador Villars, arrived with funds and passports to speed the team's return … Calon still insisted that he report to Paris. 'For you are not destined to return to Italy, but to extend the measure of the meridian in concert with Delambre.'

"So Méchain concocted another plan to justify his sojourn in Genoa … As Méchain pointed out, Genoa was near the 45th parallel, halfway between the equator and the pole. Bordeaux was not the only site suitable for a pendulum experiment to determine the length of the metre. If Calon would just send him the Observatory's platinum-bob pendulum, Méchain would save the Commission the trouble of transporting a scientific team to Bordeaux. Or, alternatively, he might conduct observations at Genoa to supply new refraction corrections (and secretly resolve the discrepancies in his own Barcelona data)" (Alder, pp. 177-182).

It was this plan that Méchain put forward in the offered letter.

"In the end, the new French Ambassador rescued Méchain by taking matters out of his hands … He advised Méchain to request further instructions from Paris – no one could ever fault a public servant for seeking to clarify his orders – and in the interim he would refuse to issue Méchain a passport, thereby taking responsibility for the delay upon his own head. All Méchain had to do was to write the request. On Monday morning, Villars told him he expected the official request by two that afternoon, before the mail-boat set out. When Méchain arrived at the post office at half-past three … the outgoing mail was already in the satchel and Villars and the carrier were waiting impatiently. Villars snatched the letter out of Méchain's hand, and stuffed it in the satchel …

"Villars' manoeuvre worked … Méchain spent the rest of the winter on the Italian Riviera" (Alder, pp. 182-183).

After returning to France, Méchain travelled to Perpignan and resumed his work. This continued through 1796 and 1797, and in the spring of 1798 he reluctantly returned to Paris. There he was made director of the observatory, but he yearned to return to Spain, "and eventually the Bureau of Longitudes approved the extension of the meridian to the Balearic Islands, a project that would render his imperfect latitude of Barcelona unnecessary … The expedition left Paris on 26 April 1803, but encountered unexpected delays in Spain. When the ship at last departed for the islands, an epidemic of yellow fever broke out on board. Méchain eventually reached Ibiza, but he discovered that his mainland station at Montsia could not be sighted from the island. Thus he was obliged to change the pattern of triangles and survey a greater distance southward in the mountains along the Spanish coast. Exhausted by the work and further weakened by fever and a poor diet, he collapsed and died on 20 September 1804. Several years later the extension of the meridian was completed by Biot and Arago" (DSB).

Alder, *The Measure of All Things*, 2002. See PMM 260.

THE MÖBIUS BAND

MÖBIUS, August Ferdinand. *'Ueber die Bestimmung des Inhaltes eines Polyëders', pp. 31-68 in Berichte der Königlichen Sächsischen Gesellschaft der Wissenschaften zu Leipzig. Mathematisch-Physisiche Classe 17 (1865). [With:] 'Theorie der elementaren Verwandtschaft', pp. 18-57 in ibid. 15 (1863).* Leipzig: S. Hirzel, 1866[-1864].

$3,200

Two complete journal issues, 8vo (226 x 140 mm). [1865:] pp. [iv], xii, 116, with one folding plate (coloured); [1863:] pp. 81. Original printed wrappers, unopened (front wrapper of 1865 issue slightly soiled, very minor wear to ends of spine). Fine copies.

First edition, journal issues in the original printed wrappers, of two of the most important papers in the early history of topology, including the introduction (and illustration) in the 1865 paper of the famous 'Möbius band' (or 'Möbius strip'). "August Möbius was one of the nineteenth century's most influential mathematicians and astronomers" (Fauvel et al.). "Möbius first described the 'Möbius band' in a paper presented to the Paris Academy in 1861 as an entry to a competition on the theme "Improve in some important point the geometric theory of polyhedra." Möbius' paper, written in bad French and containing many new ideas, was not understood by the jury, and like the other papers submitted to the competition, was not awarded the prize. The contents of the paper were published by Möbius in his articles 'Theorie der elementaren Verwandtschaft' (1863) and 'Ueber die Bestimmung des Inhaltes eines Polyëders' (1865)" (Kolmogorov & Yushkevich, p. 101). From an examination of Möbius's notebooks it is known that he discovered the Möbius strip in 1858; it was discovered independently in the same year by Johann Listing (who had coined the term 'topology' in 1847). On his

discovery of a one-sided surface, Ian Stewart writes (Fauvel et al, p. 159): "It was typical that Möbius should notice a simple fact that anyone could have seen in the previous two thousand years – and typical that nobody did". Norman Biggs (*ibid*., p. 112) speculates that both Listing and Möbius may have been influenced in their discovery by the great Carl Friedrich Gauss (1777-1855). Gauss, Listing and Möbius all worked for many years at Göttingen; Möbius studied under Gauss and Listing freely acknowledges that he was trying to develop the topological ideas of Gauss, who himself never published anything on the subject.

The Möbius band is a surface obtained by taking a strip of paper, giving one of the two ends a half twist, and then gluing together the two ends. Unlike the cylinder, obtained by joining the ends of the strip without a twist, the Möbius band has only one side. This is sometimes illustrated by saying that an ant walking around a Möbius band will return to its starting position but will be on the opposite side. This is famously illustrated in M. C. Escher's woodcut 'Möbius Strip II (Red Ants).' A further surprising property of the Möbius band is that, whereas a cylinder has two boundary curves, the Möbius band has only one: if you start at any point on the boundary and move along you will pass through every point on the boundary before returning back to your starting place. The 1865 paper contains the first published illustration of a Möbius band.

In his 1865 paper, "Möbius pointed out that 'having only one side,' while intuitively clear, is difficult to make precise, and proposed a related property that could be defined in complete rigour. This property was 'orientability'. A surface is 'orientable' if you can cover it with a network of triangles, with arrows circulating round each triangle, so that whenever two triangles have a common edge the arrows point in opposite directions. If you draw a network on a plane, for example, this is what happens. On a Möbius band, no such network exists" (Stewart). By gluing the circular edge of a disc to the single edge of his band, Möbius constructed the first example of a non-orientable 'closed' surface (i.e., one without boundary curves); this is now called a 'projective plane' and is the simplest of an infinite number of such surfaces. Möbius also showed that there is no well-defined concept of volume ('Inhalt') for a non-orientable closed surface.

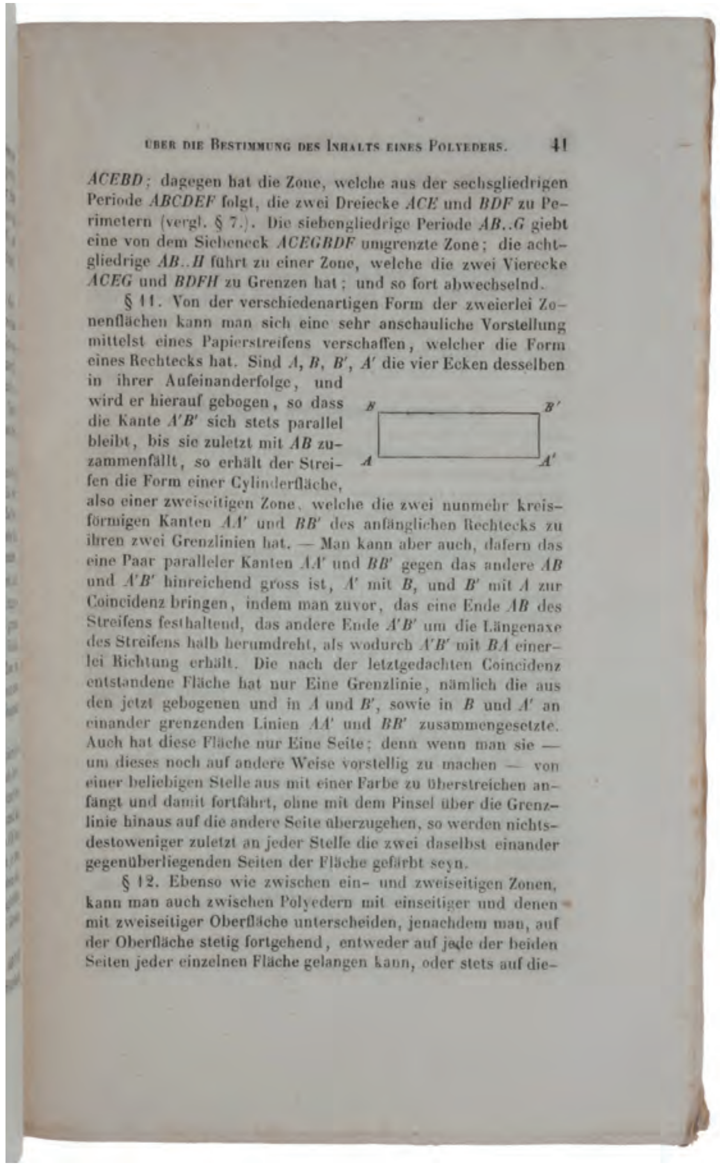
Möbius's 1863 paper gave a classification of closed orientable surfaces. Such surfaces had been studied by Bernhard Riemann as part of his theory of complex functions (they are now called 'Riemann surfaces' in that context), and he had grasped intuitively that they are classified by the number of 'holes' (zero for a sphere, one for the surface of a doughnut, etc.), a concept that had been introduced by Simon L'Huilier in 1813, building on work of Leonhard Euler in 1752. Möbius gave the first rigorous proof of this important result. For this it was first necessary to agree when two such surfaces are to be considered 'equivalent'. Möbius introduced the idea of *elementaren Verwandtschaften* ('elementary relationships') between two surfaces, in which 'each point of one corresponds to a point of the other, in such a way that two infinitely neighbouring points always correspond to two infinitely neighbouring points' (such transformations are now called 'homeomorphisms'). Möbius showed that if two closed orientable surfaces have the same number of holes, there is an 'elementary relationship' from one to the other (i.e., they are 'equivalent'). His proof was remarkably modern. "He classified singular points of a 'height' function into 'elliptic' and 'hyperbolic' points and developed what from a 20th century point of view reads as a geometric presentation of the Morse theory of differentiable closed orientable surfaces" (James, p. 37). In his 1865 paper, Möbius showed that the classification theorem no longer holds for closed surfaces that are not necessarily orientable.

August Ferdinand Möbius (November 17, 1790 – September 26, 1868) was born in Schulpforta, Saxony-Inhalt, and was descended on his mother's side from religious reformer Martin Luther. He studied mathematics under Carl Friedrich

Gauss and Johann Pfaff. His early work was devoted to astronomy, culminating in his *Die Elemente der Mechanik des Himmels* (1843), which gives a through treatment of celestial mechanics without the use of higher mathematics. His best-known book, *Der barycentrysche Calcul* (1827) is celebrated for the introduction of homogeneous coordinates into projective geometry. Many mathematical concepts are named after him, including the Möbius transformations, important in projective geometry, and the Möbius function and the Möbius inversion formula in number theory.

"August Möbius influenced mathematics on many levels. Specific ideas – his famous one-sided surface, his inversion formula, his number-theoretic function, his transformations of the complex plane, his geometrical nets – bear his name. But, in addition, and perhaps more importantly, Möbius was aware of the big ideas, the general principles, the major areas of research … What is Möbius's modern legacy? It is a large part of today's mathematical mainstream. The concepts that attracted his attention, and the methods that he helped to develop, play a central role in modern mathematics" (Ian Stewart in Fauvel et al, p. 120).

Biggs, 'The development of topology,' pp. 106-119 in J. Fauvel, R. Flood & R. Wilson (eds.), *Möbius and his band* (1993); I. M. James (ed.), *History of Topology* (1999); A. N. Kolmogorov & A. A. Yushkevich, *Mathematics in the 19th Century*, Vol. II (1996); I. Stewart, *Visions of Infinity: The Great Mathematical Problems* (2011), Chapter 10.

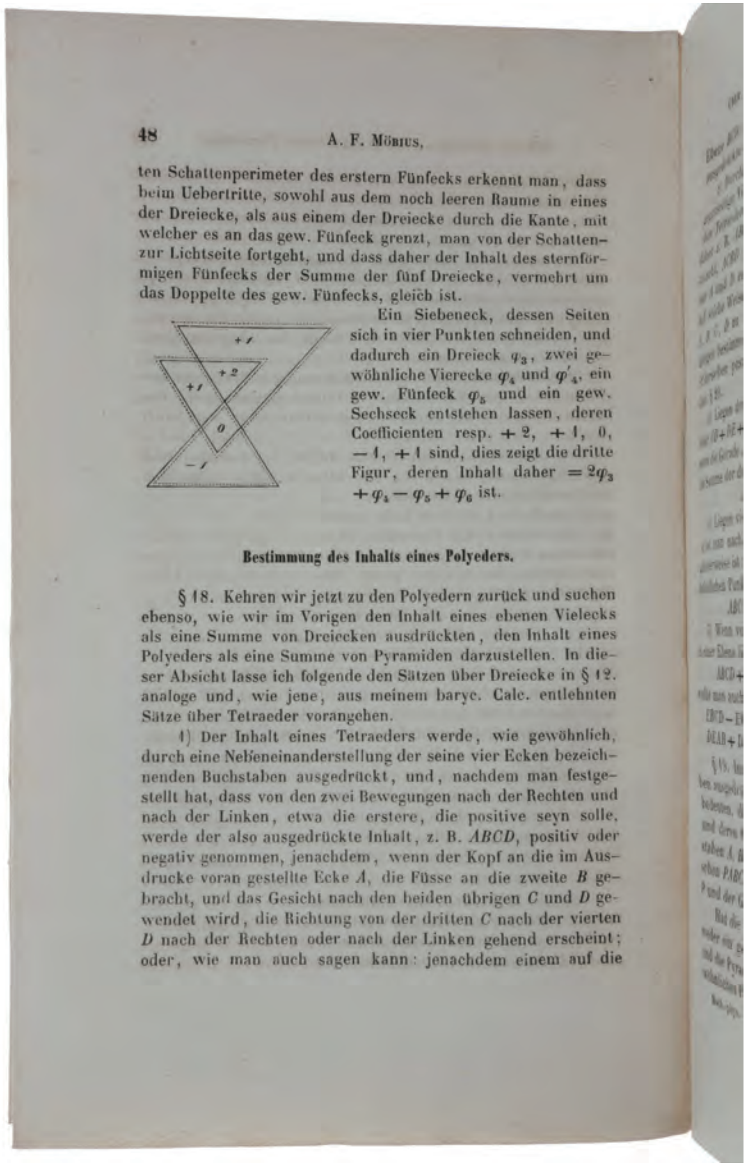


UEBER DIE BESTIMMUNG DES INHALTS EINES POLYEDERS. 41

ACEBD; dagegen hat die Zone, welche aus der sechsgliedrigen Periode *ABCDEF* folgt, die zwei Dreiecke *ACE* und *BDF* zu Perimetern (vergl. § 7.). Die siebengliedrige Periode *AB..G* giebt eine von dem Siebeneck *ACEGBDF* umgrenzte Zone; die achtgliedrige *AB..H* führt zu einer Zone, welche die zwei Vierecke *ACEG* und *BDFH* zu Grenzen hat; und so fort abwechselnd.

§ 11. Von der verschiedenartigen Form der zweierlei Zonenflächen kann man sich eine sehr anschauliche Vorstellung mittelst eines Papierstreifens verschaffen, welcher die Form eines Rechtecks hat. Sind A, B, B', A' die vier Ecken desselben in ihrer Aufeinanderfolge, und wird er hierauf gebogen, so dass die Kante $A'B'$ sich stets parallel bleibt, bis sie zuletzt mit AB zusammenfällt, so erhält der Streifen die Form einer Cylinderfläche, also einer zweiseitigen Zone, welche die zwei nunmehr kreisförmigen Kanten AA' und BB' des anfänglichen Rechtecks zu ihren zwei Grenzlinien hat. — Man kann aber auch, dafern das eine Paar paralleler Kanten AA' und BB' gegen das andere AB und $A'B'$ hinreichend gross ist, A' mit B, und B' mit A zur Coincidenz bringen, indem man zuvor, das eine Ende AB des Streifens festhaltend, das andere Ende $A'B'$ um die Längenaxe des Streifens halb herumdreht, als wodurch $A'B'$ mit BA einerlei Richtung erhält. Die nach der letztgedachten Coincidenz entstandene Fläche hat nur Eine Grenzlinie, nämlich die aus den jetzt gebogenen und in A und B', sowie in B und A' an einander grenzenden Linien AA' und BB' zusammengesetzte. Auch hat diese Fläche nur Eine Seite; denn wenn man sie — um dieses noch auf andere Weise vorstellig zu machen — von einer beliebigen Stelle aus mit einer Farbe zu überstreichen anfängt und damit fortfährt, ohne mit dem Pinsel über die Grenzlinie hinaus auf die andere Seite überzugehen, so werden nichtsdestoweniger zuletzt an jeder Stelle die zwei daselbst einander gegenüberliegenden Seiten der Fläche gefärbt seyn.

§ 12. Ebenso wie zwischen ein- und zweiseitigen Zonen, kann man auch zwischen Polyedern mit einseitiger und denen mit zweiseitiger Oberfläche unterscheiden, jenachdem man, auf der Oberfläche stetig fortgehend, entweder auf jede der beiden Seiten jeder einzelnen Fläche gelangen kann, oder stets auf die-



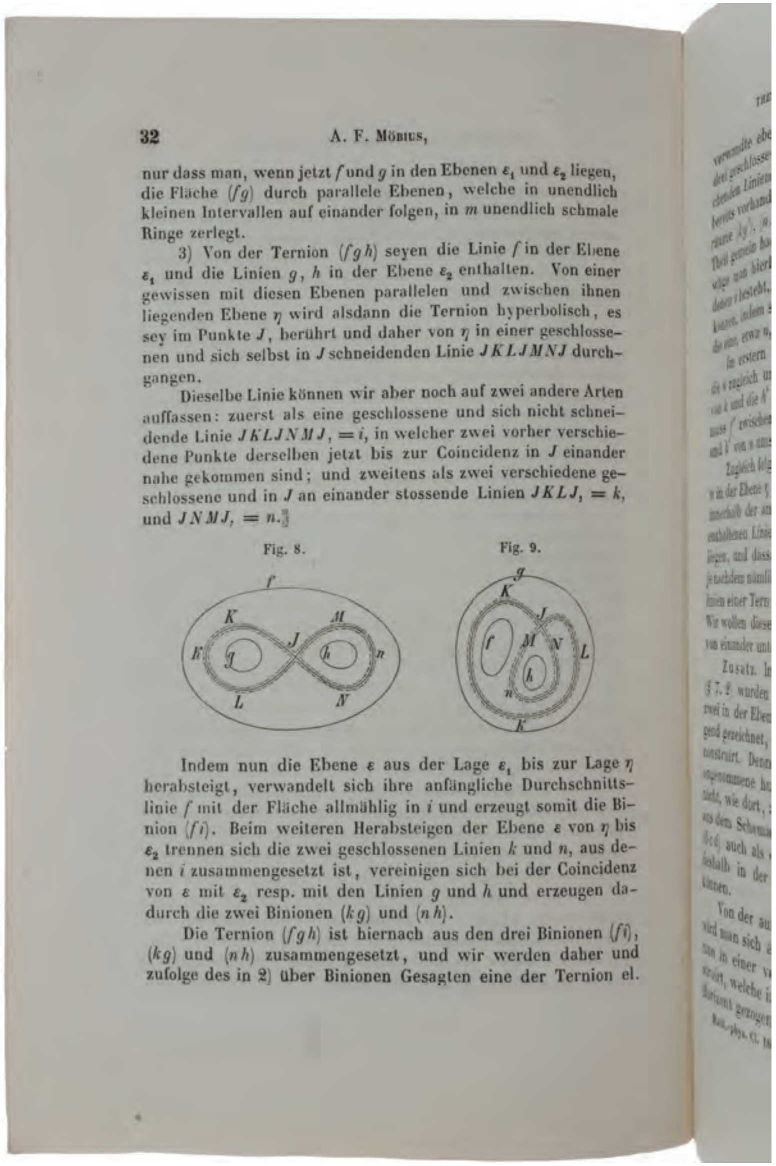
ten Schattenperimeter des erstern Fünfecks erkennt man, dass beim Uebertritte, sowohl aus dem noch leeren Raume in eines der Dreiecke, als aus einem der Dreiecke durch die Kante, mit welcher es an das gew. Fünfeck grenzt, man von der Schatten- zur Lichtseite fortgeht, und dass daher der Inhalt des sternförmigen Fünfecks der Summe der fünf Dreiecke, vermehrt um das Doppelte des gew. Fünfecks, gleich ist.

Ein Siebeneck, dessen Seiten sich in vier Punkten schneiden, und dadurch ein Dreieck φ_3, zwei gewöhnliche Vierecke φ_4 und φ'_4, ein gew. Fünfeck φ_5 und ein gew. Sechseck entstehen lassen, deren Coefficienten resp. $+2$, $+1$, 0, -1, $+1$ sind, dies zeigt die dritte Figur, deren Inhalt daher $= 2\varphi_3 + \varphi_4 - \varphi_5 + \varphi_6$ ist.

Bestimmung des Inhalts eines Polyeders.

§ 18. Kehren wir jetzt zu den Polyedern zurück und suchen ebenso, wie wir im Vorigen den Inhalt eines ebenen Vielecks als eine Summe von Dreiecken ausdrückten, den Inhalt eines Polyeders als eine Summe von Pyramiden darzustellen. In dieser Absicht lasse ich folgende den Sätzen über Dreiecke in § 12. analoge und, wie jene, aus meinem baryc. Calc. entlehnten Sätze über Tetraeder vorangehen.

1) Der Inhalt eines Tetraeders werde, wie gewöhnlich, durch eine Nebeneinanderstellung der seine vier Ecken bezeichnenden Buchstaben ausgedrückt, und, nachdem man festgestellt hat, dass von den zwei Bewegungen nach der Rechten und nach der Linken, etwa die erstere, die positive seyn solle, werde der also ausgedrückte Inhalt, z. B. $ABCD$, positiv oder negativ genommen, jenachdem, wenn der Kopf an die im Ausdrucke voran gestellte Ecke A, die Füsse an die zweite B gebracht, und das Gesicht nach den beiden übrigen C und D gewendet wird, die Richtung von der dritten C nach der vierten D nach der Rechten oder nach der Linken gehend erscheint; oder, wie man auch sagen kann: jenachdem einem auf die



nur dass man, wenn jetzt f und g in den Ebenen ε_1 und ε_2 liegen, die Fläche (fg) durch parallele Ebenen, welche in unendlich kleinen Intervallen auf einander folgen, in m unendlich schmale Ringe zerlegt.

3) Von der Ternion (fgh) seyen die Linie f in der Ebene ε_1 und die Linien g, h in der Ebene ε_2 enthalten. Von einer gewissen mit diesen Ebenen parallelen und zwischen ihnen liegenden Ebene η wird alsdann die Ternion hyperbolisch, es sey im Punkte J, berührt und daher von η in einer geschlossenen und sich selbst in J schneidenden Linie $JKLJMNJ$ durchgangen.

Dieselbe Linie können wir aber noch auf zwei andere Arten auffassen: zuerst als eine geschlossene und sich nicht schneidende Linie $JKLJNMJ$, $= i$, in welcher zwei vorher verschiedene Punkte derselben jetzt bis zur Coincidenz in J einander nahe gekommen sind; und zweitens als zwei verschiedene geschlossene und in J an einander stossende Linien $JKLJ$, $= k$, und $JNMJ$, $= n$.

Fig. 8. Fig. 9.

Indem nun die Ebene ε aus der Lage ε_1 bis zur Lage η herabsteigt, verwandelt sich ihre anfängliche Durchschnittslinie f mit der Fläche allmählig in i und erzeugt somit die Binion (fi). Beim weiteren Herabsteigen der Ebene ε von η bis ε_2 trennen sich die zwei geschlossenen Linien k und n, aus denen i zusammengesetzt ist, vereinigen sich bei der Coincidenz von ε mit ε_2 resp. mit den Linien g und h und erzeugen dadurch die zwei Binionen (kg) und (nh).

Die Ternion (fgh) ist hiernach aus den drei Binionen (fi), (kg) und (nh) zusammengesetzt, und wir werden daher und zufolge des in 2) über Binionen Gesagten eine der Ternion el.

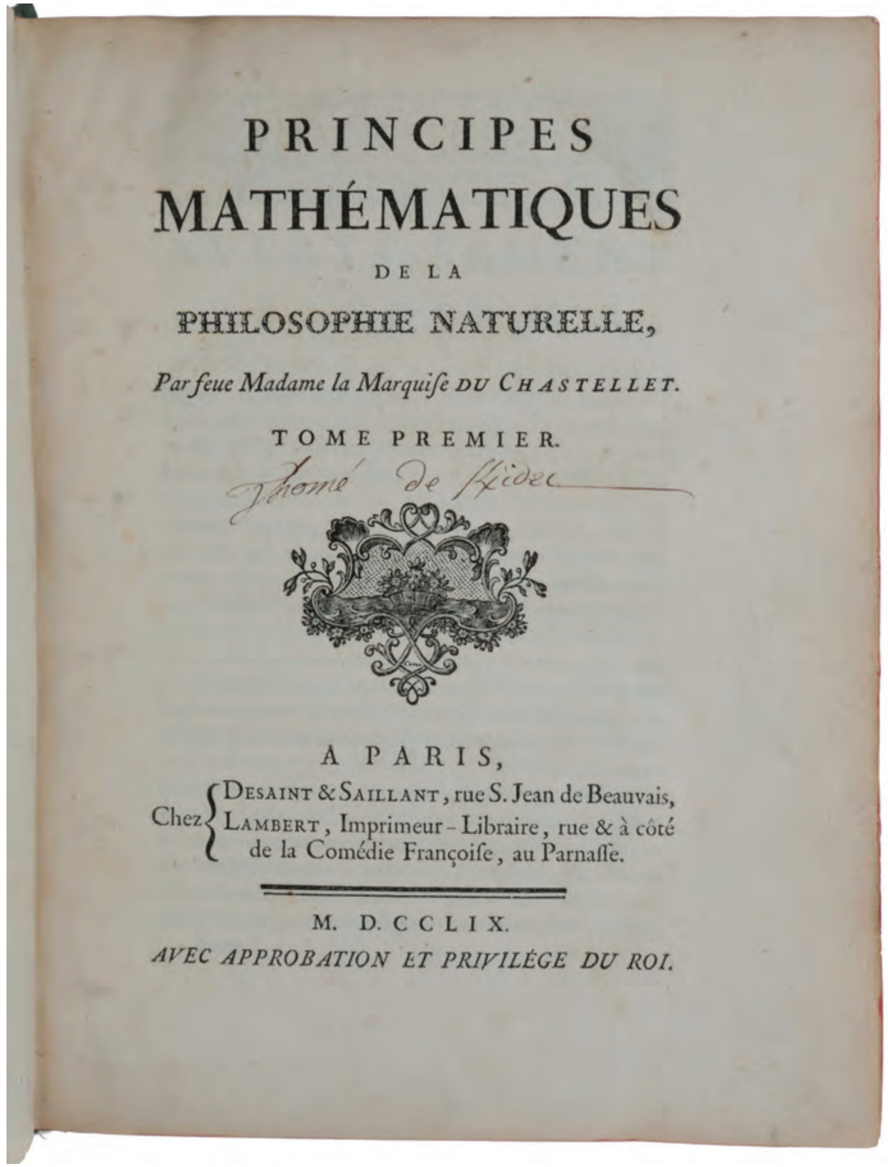
DU CHÂTELET'S FRENCH TRANSLATION OF THE 'PRINCIPIA'

[NEWTON, Sir Isaac] DU CHÂTELET, Gabrielle-Émilie le Tonnelier de Breteuil, Marquise. *Principes Mathématiques de la Philosophie Naturelle, par feue Madame la Marquise du Chastellet.* Paris: Desaint & Saillant, 1759.

$5,500

Two vols., 4to (251 x 188 mm), Volume I: pp. [iv], xxxix, [1, errata], [4] (Voltaire's poem), 243, [1, blank] (Book I), [1], 246-437, [1] (Book II), with 9 folding engraved plates; Volume II: pp. [iv], 180 (Book III), 297, [2, Privilege] (Du Châtelet's commentary), with 5 folding engraved plates. Bound in uniform contemporary calf, red title lables with gilt lettering, some wear to capitals, hinges starting.

First edition in French of Newton's *Principia*, "the greatest work in the history of science" (PMM), translated by Voltaire's mistress, Madame du Châtelet, with the assistance of Alexis-Claude Clairaut. Begun in 1745 but published posthumously, this is her most important work. It was the first translation into a language other than English and is "still the only French translation ever made … The first volume contains, in addition to Books I and II [of *Principia*], a 'preface historique' and a poem by Voltaire. The second volume contains Book III and, separately paginated, 'Exposition abrégée du système du monde, et explication des principaux phénomenes astronomiques tirée des Principes de M. Newton' (pp. 1-116). This was supplied by Clairaut. There was also 'Solution analytique des principaux problèmes qui concernent le système du monde' (pp. 117-286)" (Gjertsen, *Newton Handbook*, p. 480). Voltaire's 'preface historique' is partly an



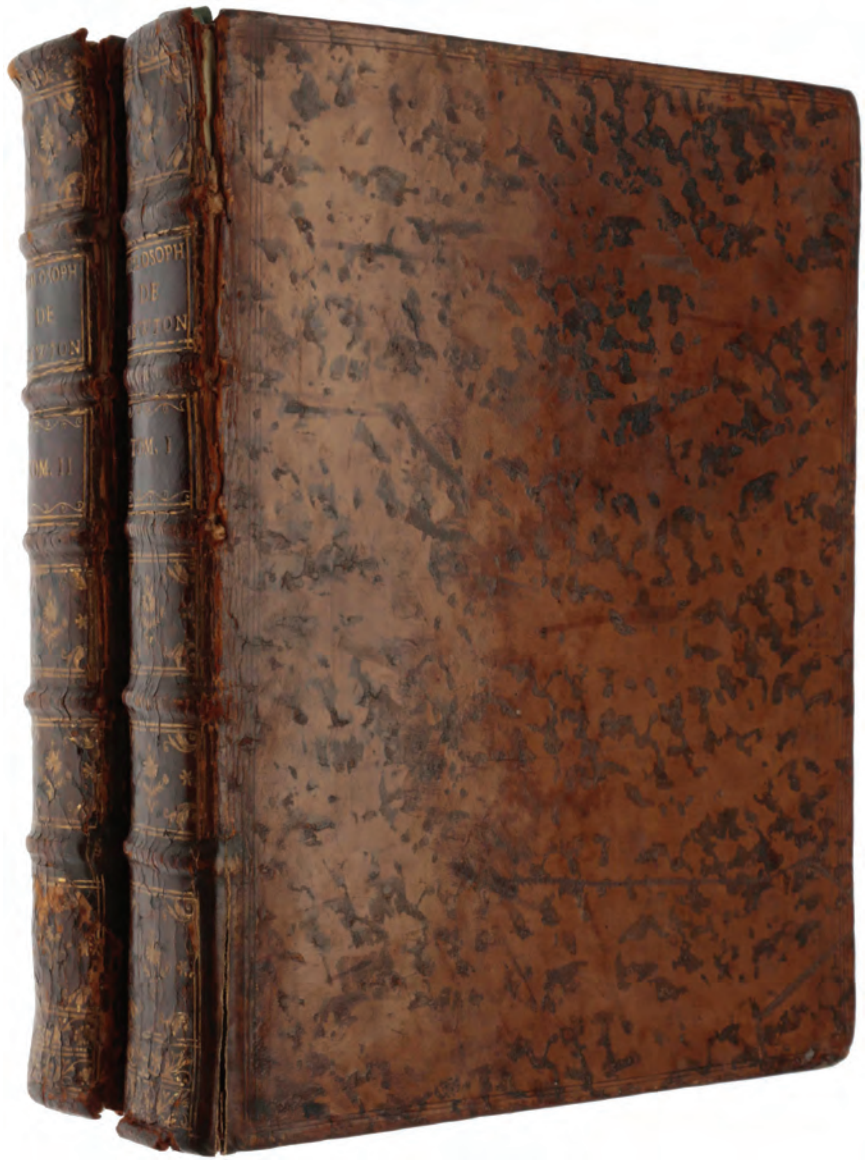
extravagant *Éloge* of his late mistress but also a survey of Newton's philosophical position and the problems of translation. "Known throughout intellectual Europe as Émilie, the name popularized by Voltaire, Mme. du Châtelet—beyond the influence that she had for some fifteen years on the orientation of Voltaire's work and on his public activity—contributed to the vitality of French scientific life and to the parallel diffusion of Newtonianism and Leibnizian epistemology. Her affairs entertained the fashionable world of her period, yet her last moments revealed the sincerity of her scientific vocation. Although she limited her efforts to commentary and synthesis, her work contributed to the great progress made by Newtonian science in the middle of the eighteenth century" (DSB). "For a long time, Emilie du Châtelet's French translation and her commentary on Newton's *Philosophiae naturalis principia mathematica* provided the only access to Newton's principles of natural philosophy in French and, together with her commentary on Leibniz's philosophy in the *Institutions de physique*, it thus greatly influenced scientific discourse in the eighteenth century and can be said to have furthered both transnational and European scientific collaboration" (Winter, p. 174). A few copies of this work were issued with a date of 1756. "Although the first issue does have the date 1756 on its title page, it is very rare and may well have been, according to Cohen, 'a preliminary edition, not made available for general sale to the public.' He managed to trace only twelve" (Gjertsen). This copy is complete with Cotes's preface, which is sometimes lacking.

The *Principia*, first published in 1687, elucidated the universal physical laws of gravitation and motion which lie behind phenomena described by Newton's great predecessors Copernicus, Galileo and Kepler. Newton established the mathematical basis for the motion of bodies in unresisting media (the law of inertia); the motion of fluids and the effect of friction on bodies moving through fluids; and, most importantly, set forth the law of universal gravitation and its unifying role in the cosmos. "For the first time a single mathematical law could explain the motion of objects on earth as well as the phenomena of the heavens … It was this grand conception that produced a general revolution in human thought, equalled perhaps only by that following Darwin's *Origin of Species*" (PMM). In 1713 a second edition was published, edited by Roger Cotes. Numerous changes were made to the text, and Cotes also provided an important preface in which he undertook to explain and defend Newton's account of gravity. The definitive third edition, the last to be corrected by Newton himself, was edited by Henry Pemberton and was published in 1726. A few changes were made but, compared to 1713, there were, in Cohen's phrase, no 'bold and exciting innovations.' An English translation, by Andrew Motte, was published in 1729, and the so-called 'Jesuits' edition, edited by F. Jacquier and T. Le Seur, appeared in three volumes (in Latin) in 1739-1742. No further editions were published until Mme. du Châtelet's French translation.

On 22 June 1725 Gabrielle-Émilie married Florent-Claude, marquis du Châtelet and count of Lomont, who, after spending several years with her when he was governor of the city of Semur-en-Auxois, pursued a military career and visited her only briefly. After returning to Paris in 1730, Émilie du Châtelet led a glittering existence and had several affairs before becoming intimate, in 1733, with Voltaire, who had just completed his *Lettres philosophiques*. Several of these *lettres* dealt with Newton's philosophy and were given for review to Pierre-Louis Maupertuis, the author of the first French work devoted to the Newtonian world system, the *Discours sur la figure des astres* (1732). Mme. du Châtelet in her turn developed a very cordial friendship with Maupertuis and with another ardent Newtonian, Alexis-Claude Clairaut. The mathematics lessons that she received from Maupertuis at the beginning of 1734 awakened her scientific inclinations.

In June 1734 Voltaire, threatened with arrest, withdrew to one of Mme. du Châtelet's properties, the château at Cirey in Champagne, the restoration of which he undertook. The marquise spent a few months there at the end of 1734 and then made several prolonged stays. Devoting their time variously to their literary endeavors, metaphysical, philosophical, and scientific discussions, and a very refined worldly existence, she and Voltaire made the château at Cirey one of the most brilliant centres of French literary and philosophical life.

The stay at Cirey, at the end of 1735, of Francesco Algarotti, who was preparing a popularization of Newtonian optics, *II newtonianismo per le dame*, which appeared in 1737, incited Voltaire and Mme. du Châtelet to undertake a work propagandizing Newtonian science, and she began a systematic study of Newton's work, writing an *Essai sur l'optique*, of which a fragment is preserved, and participating in the elaboration of the *Éléments de la philosophie de Newton*, published by Voltaire in 1738. It is to this book that she devoted her "Lettre sur les élémens de la philosophie de Newton", published in the *Journal des sçavants* in 1738, a report on and defense of that part of the work which discusses Newtonian attraction. Over the next several years, however, she took up Leibnizian epistemology, under the influence of Johann I Bernoulli and Maupertuis, and it was not until 1745 that she returned to Newtonianism, deciding to dedicate all of her scientific activity to perfecting a French translation of Newton's *Principia*. It was to be enriched by a commentary on the work inspired by the one accompanying the Latin edition of T. Le Sueur and F. Jacquier and by theoretical supplements drawn essentially from the most recent works of Clairaut. As early as 1746 she obtained Clairaut's collaboration as adviser, as reviser of her translation and her commentaries, and as author of theoretical supplements to her work. In the spring of 1747 the definitive plan was settled upon, the translation completed, and the printing begun. But Clairaut then found himself involved in a major discussion on the modifications to the law of universal gravitation which were apparently needed in order to explain an anomaly observed in the movement of the moon's apogee, and it was not until February 1749 that she came to Paris to finish her book in collaboration with



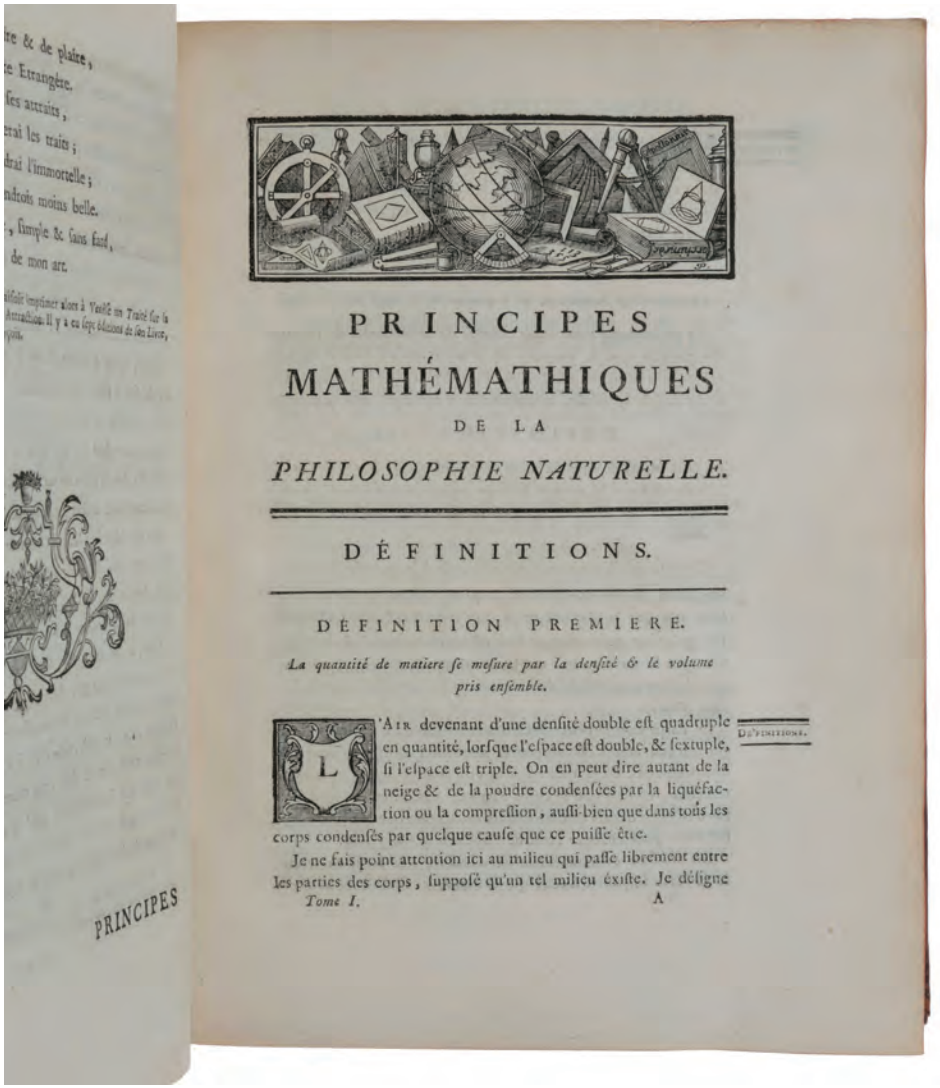
Clairaut. The revelation of an unexpected and late pregnancy increased her desire to complete the project before the confinement that she dreaded. At the end of June, fleeing indiscreet stares, she left for Lunéville, where she died of childbed fever. Before her death she entrusted the manuscript of her annotated translation of the *Principia* to the librarian of the Bibliothèque du Roi in Paris.

The work consists of two parts. The first is a translation of the text of the *Principia* in the 1726 edition. Mme. du Châtelet added clarifications and amplifications where she thought it necessary. The second part is a commentary on Newton's system of the world. This is in turn divided into two parts. The first of these begins with *Du Système du Monde*, which sets out the basic physical principles, followed by an *Exposition abregée duSystème du Monde,* which aims to give an account of the principal astronomical phenomena, on Newtonian principles, in as straightforward and non-technical manner as possible. The second, more technical part, *Solution analytique des principaux Problèmes qui concernent le Système du Monde*, analyses some of the most important problems relating to the system of the world: the nature of the orbits of the planets under various hypotheses about the nature of the force of attraction; the attraction of bodies of various shapes; the theory of the shape of the earth and the tides; and an account of the refraction of light based on the principles of attraction. Much of this is drawn from the works of Clairaut, and from Daniel Bernoulli's prize essay on the tides ('Traité sur le flux et reflux de la mer,' *Piéces qui ont remporté les prix de l'Académie royale des sciences* (Paris), 1740). There was no discussion of fluid mechanics.

"In her commentary on the *Principia*, Du Châtelet shifts her focus from Newton's theories of natural philosophy to his astronomical theories. Du Châtelet in fact excludes Newton's natural philosophy in favour of an in-depth discussion of his astronomy. In her 'Exposition abrégéé du Système du monde', Newton's theories on absolute time and space, his notion of God and God's influence on cosmic structure and his concept of the 'causae finales' are neither mentioned nor explicitly commented on. Newton's concepts of mass and inertia, which form the foundation of all of his definitions of matter, are also only mentioned briefly … Du Châtelet's commentary on the *Principia* surprisingly only deals with Newton's physical laws and their respective mathematical calculations, to which she now adds a number of her own calculations. This shift in focus is aptly supported by her subtitle '*Explication des principaux phénomènes astronomiques. Tirée des principes de m. Newton*', which shifts the emphasis from Newton's extensive paradigm of nature to descriptions and calculations of physical laws. What this shows is that Du Châtelet understands her commentary on Newton to be a discourse in its own right; her work seeks to uphold an objective view of Newton's work, while at the same time introducing her own definitions and terms. Consequently, Du Châtelet also dedicates the beginning of her work to her own definition of gravitation: 'Au reste, *je déclare ici,* comme M. Newton a fait lui-mème, qu'en me servant du mot d'*attraction*, je n'entend que la force qui fait tendre les corps vers un centre, sans prétendre assigner la cause de cette tendance' … On her account, Newton's theories are limited to the physical and astronomical world and are not understood as definite findings, as the cosmic law *per se*, as was the case with some of the English commentators. In presenting Newton's ideas that way, Du Châtelet understands his theories as a considerable step towards scientific progress while at the same time reminding her readers to keep an open mind regarding their subsequent modifications and corrections" (Winter, pp. 181-184).

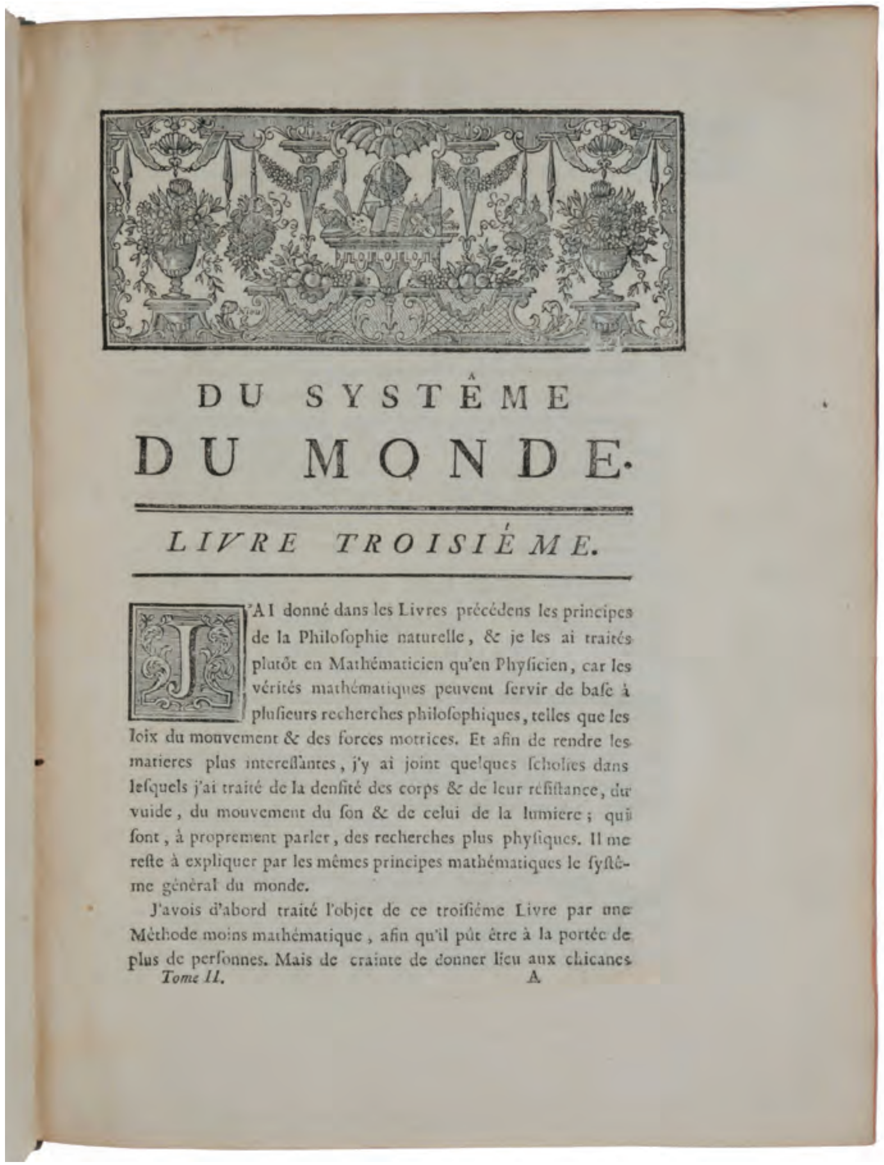
The genesis of Mme. du Châtelet's translation and commentary has still not been fully elucidated. From her correspondence, we know that the first version of the translation was completed on 21 March 1746. On 15 February 1749 she wrote to Jacquier, "I hope everything will be finished by May." The manuscript of the translation, together with a fragment of the second part of the commentary, was given to the librarian of the Bibliothèque du Roi the day before she died on 10 September 1749. A preliminary edition was published in 1756, possibly designed for presentation only, with the regular edition appearing three years later. Two outstanding questions remain: (i) why was there a delay of more than a decade from the completion of the translation, apparently in late 1745, to the first publication in 1756; (ii) why was there a further delay of three years until the publication of the regular edition of 1759. We do not today have the manuscript from which the translation was printed (this is not the manuscript deposited in the Bibliothèque du Roi), nor do we have any complete manuscript of the commentary (a partial manuscript, containing only the *Exposition abregée,* was offered by Christie's Paris, 29 October 2012, lot 16, but this seems not to have been subjected to scholarly study).

"The textual differences between the printed work and Mme. du Chastellet's manuscript are both stylistic and technical, and they show how much revision was needed in order to ready her translation for publication. We do not know how much of this revision was done by Mme. du Chastellet herself, and how much was done by Clairaut; nor are we able to say whether the bulk of Clairaut's work on this text may have been done before the death of the Marquise. Possibly the delay in publication may have been caused by the slowness with which that final revision was completed. That the translation was revised by Clairaut is put out of any doubt by a declaration of the publisher in the 'Avertissement de l'éditeur': 'A l'égard de la confiance que le Public doit avoir dans cette traduction, il suffit de dire qu'elle a été faite par feue Madame la Marquise *du Chastellet,* & qu'elle a été révue par M. *Clairaut*'. In the *Preface Historique* at the beginning of vol. I of the translation, written by Voltaire, nothing is said of any revision of the translation. With respect to the commentary however, Voltaire did state definitely that the great mathematician Clairaut had taken a hand. Voltaire admitted that many of the ideas in the commentary were Clairaut's, that – although Mme. Du Chastellet



had made the calculations by herself – Clairaut went over each chapter as she finished it and corrected it: 'M. Clairaut faisoit encoire revoir par un tiers les calculs.' Finally, according to Voltaire, at the time of the death of the Marquise, 'Elle n'avoit pas encore … termine le Commentaire.' Possibly the delay in publication was due to the need of time for Clairaut to complete the commentary and ready it for publication, rather than to work on the translation" (Cohen, pp. 264-6).

Bernard Cohen, 'The French Translation of Isaac Newton's *Philosophiae Naturalis Principia Mathematica* (1756, 1759, 1966),' *Archives internationales d'Histoire des Sciences*, Vol. 21 (1968), pp. 262-290. M. Toulmond, "Le Commentaire des Principes de la Philosophie naturelle' in *Émilie du Châtelet, éclairages & documents nouveaux*, Publications du Centre international d'étude du XVIII^e siècle 21, 2008. Winter, 'From translation to philosophical discourse – Emilie du Châtelet's commentaries on Newton and Leibniz,' pp. 173-206 in *Emilie du Châtelet between Leibniz and Newton* (Hagengruber, ed.), 2012.



DU SYSTÊME

DU MONDE.

LIVRE TROISIÉME.

J'AI donné dans les Livres précédens les principes de la Philoſophie naturelle, & je les ai traités plutôt en Mathématicien qu'en Phyſicien, car les vérités mathématiques peuvent ſervir de baſe à pluſieurs recherches philoſophiques, telles que les loix du mouvement & des forces motrices. Et afin de rendre les matieres plus intéreſſantes, j'y ai joint quelques ſcholies dans leſquels j'ai traité de la denſité des corps & de leur réſiſtance, du vuide, du mouvement du ſon & de celui de la lumiere; qui ſont, à proprement parler, des recherches plus phyſiques. Il me reſte à expliquer par les mêmes principes mathématiques le ſyſtême général du monde.

J'avois d'abord traité l'objet de ce troiſiéme Livre par une Méthode moins mathématique, afin qu'il pût être à la portée de plus de perſonnes. Mais de crainte de donner lieu aux chicanes

Tome II. A

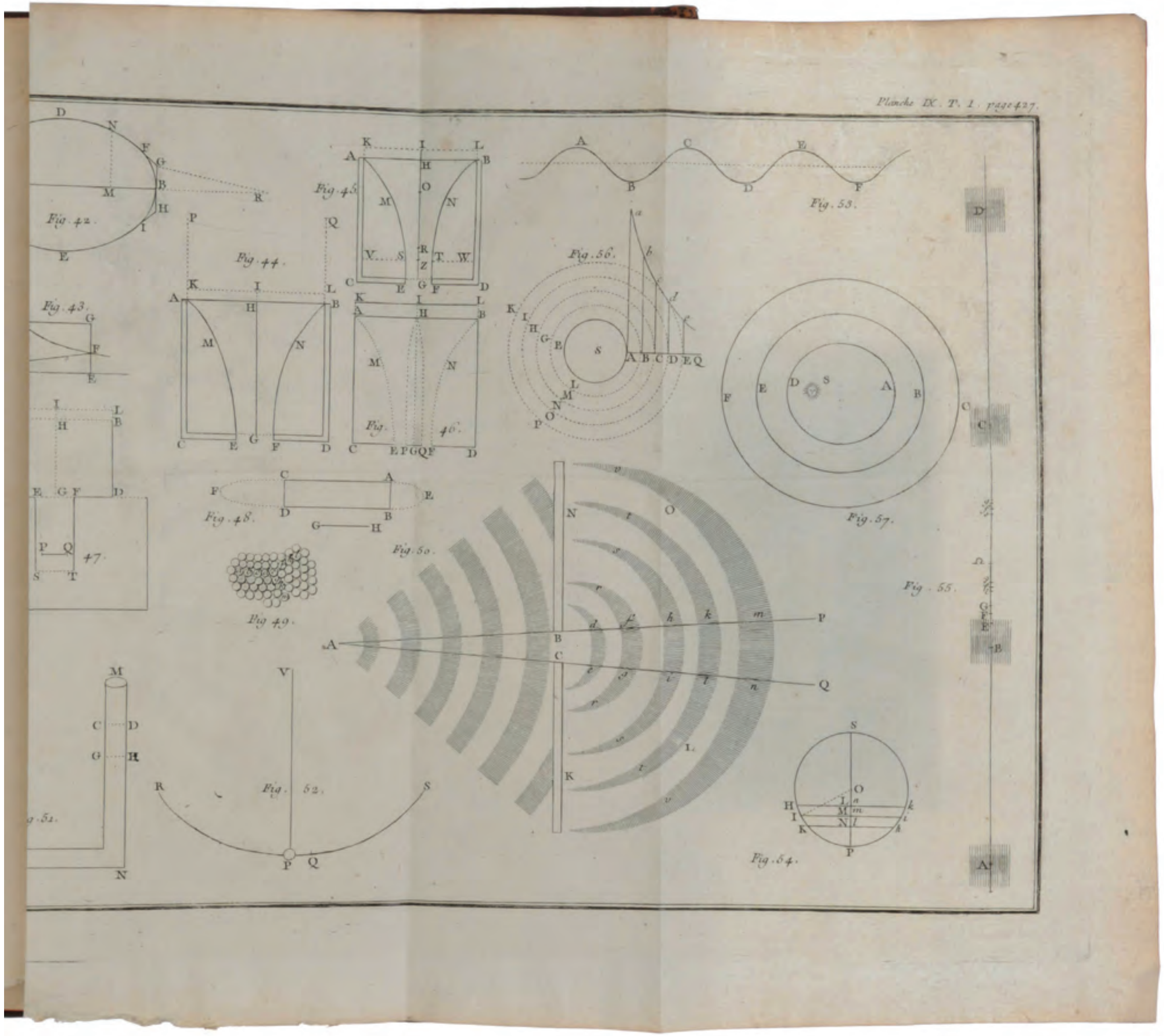


Planche IX. T. 1. page 427.
Fig. 42.
Fig. 43.
Fig. 44.
Fig. 45.
Fig. 46.
Fig. 48.
Fig. 49.
Fig. 50.
Fig. 52.
Fig. 53.
Fig. 54.
Fig. 55.
Fig. 56.
Fig. 57.

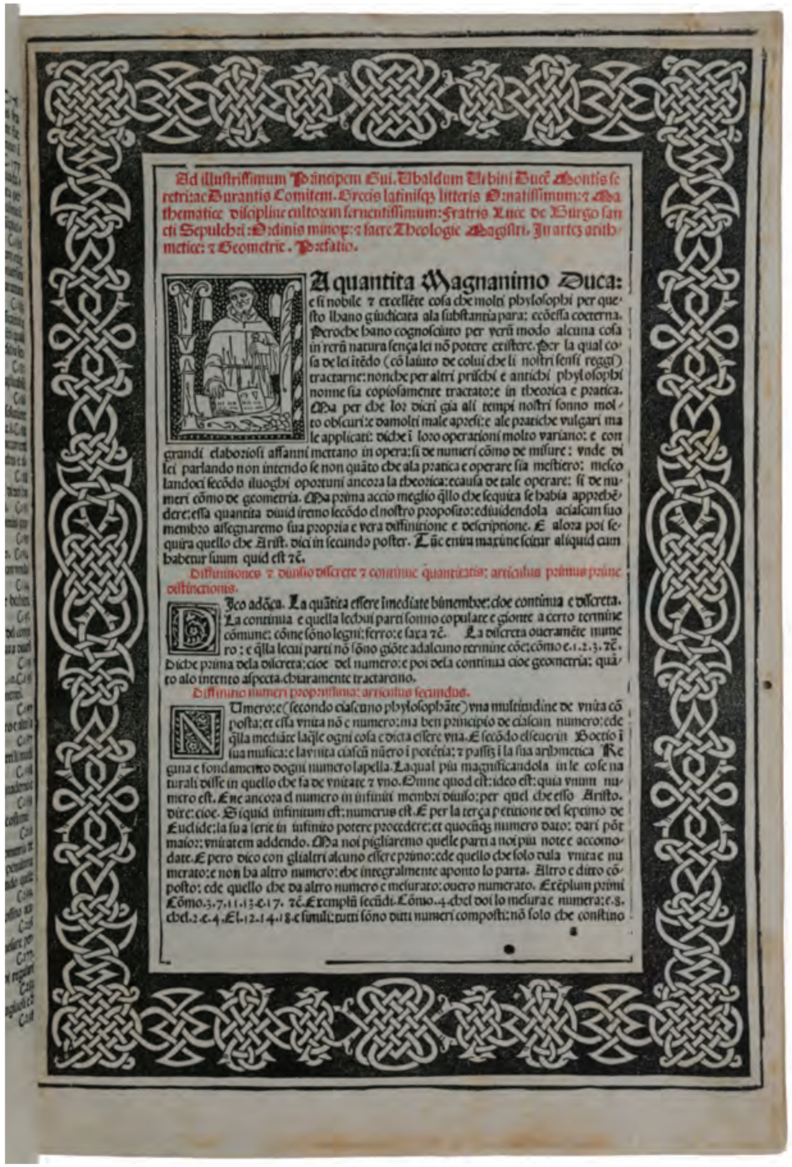
THE MOST INFLUENTIAL WORK IN THE HISTORY OF CAPITALISM

PACIOLI, Luca [Lucas de Burgo S. Sepulchri]. *Su[m]ma de arithmetica geometria proportioni et proportionalità.* [Colophon:] Venice: Paganinus de Paganinis, 10-20 November 1494.

$1,350,000

Two vols. in one, folio (319 x 217 mm), ff. [8], 224; 76, with large white-on-black woodcut initials (including a repeated depiction of Pacioli with a pair of dividers and a copy of the Summa before him), full-page woodcut 'tree of proportion' printed in red and black, full-page woodcut showing finger symbolism for numbering, numerous woodcut mathematical and geometrical diagrams, and illustrations showing instruments and methods of measuring, in margins; a few minute wormholes in a few gatherings, not affecting text, a fine, large copy, with numerous deckle edges, in its original Italian [Venetian?] binding of quarter leather with blind-ruled panels on sides, over bevelled wooden boards, with four embossed brass catchplates and remains of clasps.

First edition, first issue (see below), very rare, this is a remarkable copy in entirely original condition and with a distinguished provenance, having been in the famous Giustiniani family from publication to the present day. The *Summa* is a work of enormous importance on several levels. It is the first mathematical encyclopaedia of the Renaissance, 'the first great general work on mathematics printed' (Smith, *Rara arithmetica*, p. 56), and the first printing of any of the works of the great thirteenth-century mathematician Leonardo of Pisa, called Fibonacci



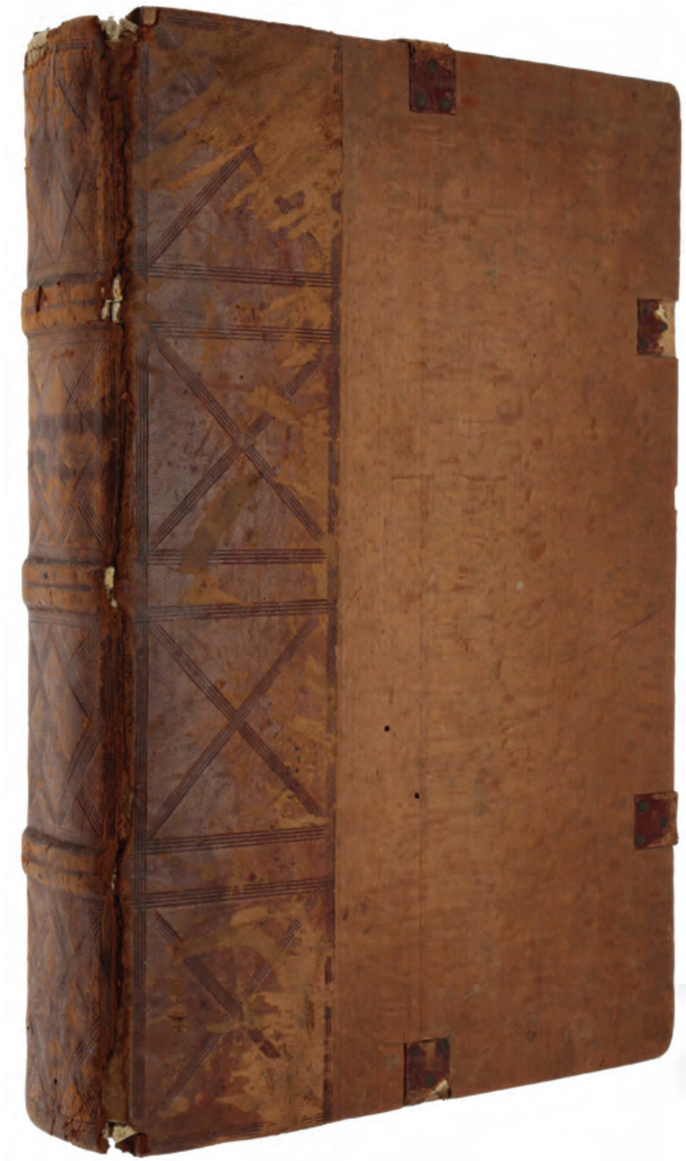
(c. 1175-c. 1250), and of Pacioli's friend, the brilliant mathematician and artist Piero della Francesca (1416-92). The first part of the *Summa* is the first printed comprehensive treatment of algebra and arithmetic, based largely on Fibonacci's 1202 *Liber Abaci* which famously introduced Arabic numbers to the West, and which was itself in part a translation of the treatises on algebra and arithmetic of the Persian mathematician and astronomer Muhammad ibn Mūsā al-Khwārizmī (c. 780-c. 850) (the word algorithm derives from his name). The second part, on geometry, is based on Fibonacci's *Practica Geometriae*, but includes at the end a section on stereometric geometry and regular solids taken from the *Trattato d'abaco* of Piero della Francesca. The first part of the *Summa* also contains sections illustrating the applications of arithmetic and algebra to problems in business, notably including Pacioli's original treatise *Particularis de Computis et Scripturis* ('Details of Accounting and Recording') (ff. 197v-210v). This is the first printed text to set out the method of double-entry bookkeeping, the single most influential work in European accounting history, which earned Pacioli the title 'Father of Accounting'; it has been called "the most influential work in the history of capitalism". *De Computis* introduces the 'rule of 72' for predicting an investment's future value, anticipating the development of the logarithm by more than a century. The business section of the *Summa* also contains the earliest discussion of mathematical probability in print (this was not in *Liber abaci*). "The oldest known printed source for the treatment of the problem of points is Luca Pacioli's *Summa*" (Schneider, p. 230) – modern probability theory is generally regarded as having begun with the exchange of letters discussing the 'problem of points' between Fermat and Pascal in the mid-1650s. In its iconic full-page woodcut of finger counting (f. 36v), from which our modern 'digital computing' took its name, the *Summa* contains the earliest printed representation of computation. Sangster *et al.* argue that the *Summa* was, in fact, mainly intended as a reference text for merchants – it synthesised the three major mathematical traditions – medieval European, Arab and ancient Greek – but its use of the vernacular opened it up to businessmen, students, artists, technicians and scholars alike. The *Summa* is also a work central to the development of Leonardo da Vinci (1452-1519). Pacioli came to Milan where he held the chair of mathematics from 1496 to 1499, during which years he lodged with Leonardo, and taught him mathematics. Leonardo owned a copy of the *Summa* (he paid 119 soldi for it *ca.* 1494, as noted in the *Codex Atlanticus* f. 288 recto) and refers to it in his notebooks (see below for more on Leonardo and Pacioli). Only three other copies of the first edition of the *Summa* are recorded at auction in the last 50 years, of which only one was in its original binding, a copy of the second issue sold at Christie's New York in 2019 for $1,215,000.

Provenance: contemporary inscription on front free endpaper recto: 'Di Giacomo Giust.[inia]no fu di m[esser] Lor.[enzo] Genovese e de soi amici'; Giacomo records the gifting of the book from his father Lorenzo Giustiniani, a member of the Genoan branch of the Giustiniani family; on verso: 'sexqui altera sexqui tertia sexqui quinta // questo sexqui, via, fia in c.[arte] 27 // algebra, algebratica vuol dire speculativa'; i.e. referring to passages concerning the superparticular ratios sesquialtera (3:2), sesquitertia (4:3) and sesquiquinta (6:5). Coincidentally, the ancestor of Lorenzo, his namesake Saint Lorenzo Giustiniani (1381–1456), is depicted in a funerary statue in the St Pietro di Castello church in Venice, holding a book with a very similar binding (thanks to Nicholas Pickwoad for pointing this out).

The *Summa*, the writing of which had been completed by 1487, is divided into two volumes, the first dealing with arithmetic and algebra, the second with geometry. The first volume is divided into nine chapters (*distinctiones*): chapters 1 to 7 on arithmetic (222 pages), chapter 8 on algebra (78 pages), and chapter 9 on business (150 pages). The second volume comprises chapters 1-8 (151 pages), on geometry; it has separate signatures and foliation and a caption title. There is a brief colophon at the end of part 1 referring to the full colophon at the end of part 2.

"In the dedication to duke Guidobaldo of Urbino [1472-1508], Pacioli gives a list of his sources. For the first part these include first and foremost Fibonacci's *Liber Abaci*, followed by Jordanus [fl. 13th century], Blasius of Parma [1355-1416] and Prosdocimo de' Beldomandi [1375-1428], among others …The geometrical part is taken from Fibonacci's *Practica geometriae* and Archimedes … In making this vast compilation, Pacioli availed himself of many libraries including the ducal collection at Urbino. There he consulted al-Khwārizmī's *Algebra* [which was translated into Italian at least by 1464, the date of a manuscript copy in the Plimpton Collection] …, as well as Piero della Francesca's *Libellus de Quinque Corporibus Regularibus*. At the Venetian monastery of S. Antonio, he annotated a manuscript of Fibonacci's *Liber Abaci* later seen by Cardano, and at Florence he used a text of Witelo's *Perspectiva* [composed c. 1274]" (Rose, pp. 143-144).

Pacioli particularly emphasizes his indebtedness to Fibonacci. Fibonacci was born around 1175, the son of a Pisan merchant, and in his early life travelled widely around Italy to Genoa and Venice, but also to Barbary, Egypt, Syria, Greece and France. On these trips he learned the Arabic ways of arithmetic and computation. From 1200 he settled back in Pisa and began his mathematical writings; the first and most important work was the *Liber Abaci* (The Book of Calculation) written in 1202. No copy of this first version has survived, but around 1228 a second version of the text was completed, with additional chapters. "Virtually all subsequent Renaissance algebraists cite Fibonacci and Pacioli in the same breath and indeed it was difficult to cite Fibonacci independently since his works were not published until the nineteenth century" (Rose, p. 145). The first volume of the *Summa* follows the *Liber abaci* closely.



"*Liber abaci* is an encyclopedic work treating much of the known mathematics of the thirteenth century on arithmetic, algebra, and problem solving. It is, moreover, a theoretical as well as practical work; the methods employed in *Liber abaci* [Fibonacci] firmly establishes with Euclidean geometric proofs … General methods are established by using the geometric algebra found principally in Book II of the *Elements*. [Fibonacci] turns to Book X for a foundation of a theory of quadratic irrational numbers. Throughout *Liber abaci* proofs are given for old methods, methods acquired from the Arabic world, and for methods that are [Fibonacci's] original contributions. Leonardo also includes those commonplace non-algebraic methods established in the mediaeval world for problem solving, at the same time giving them mathematical legitimacy with his proofs. Among others they include checking operations by casting out nines, various rules of proportion, and methods called single and double false position" (Sigler, pp. 4-5).

"It was [Fibonacci's] purpose to replace Roman numerals with the Hindu numerals not only among scientists, but in commerce and among the common people" (*ibid*.). "When Fibonacci's *Liber abaci* first appeared, Hindu-Arabic numerals were known to only a few European intellectuals through translations of the writings of the 9th-century Arab mathematician al-Khwārizmī. The first seven chapters dealt with the notation, explaining the principle of place value, by which the position of a figure determines whether it is a unit, 10, 100, and so forth, and demonstrating the use of the numerals in arithmetical operations" (Britannica).

In this part Pacioli gives the first printed example of a set of plus and minus signs that were to become standard in Italian Renaissance mathematics: 'p' with a tilde above for 'plus' and 'm' with a tilde for 'minus'.

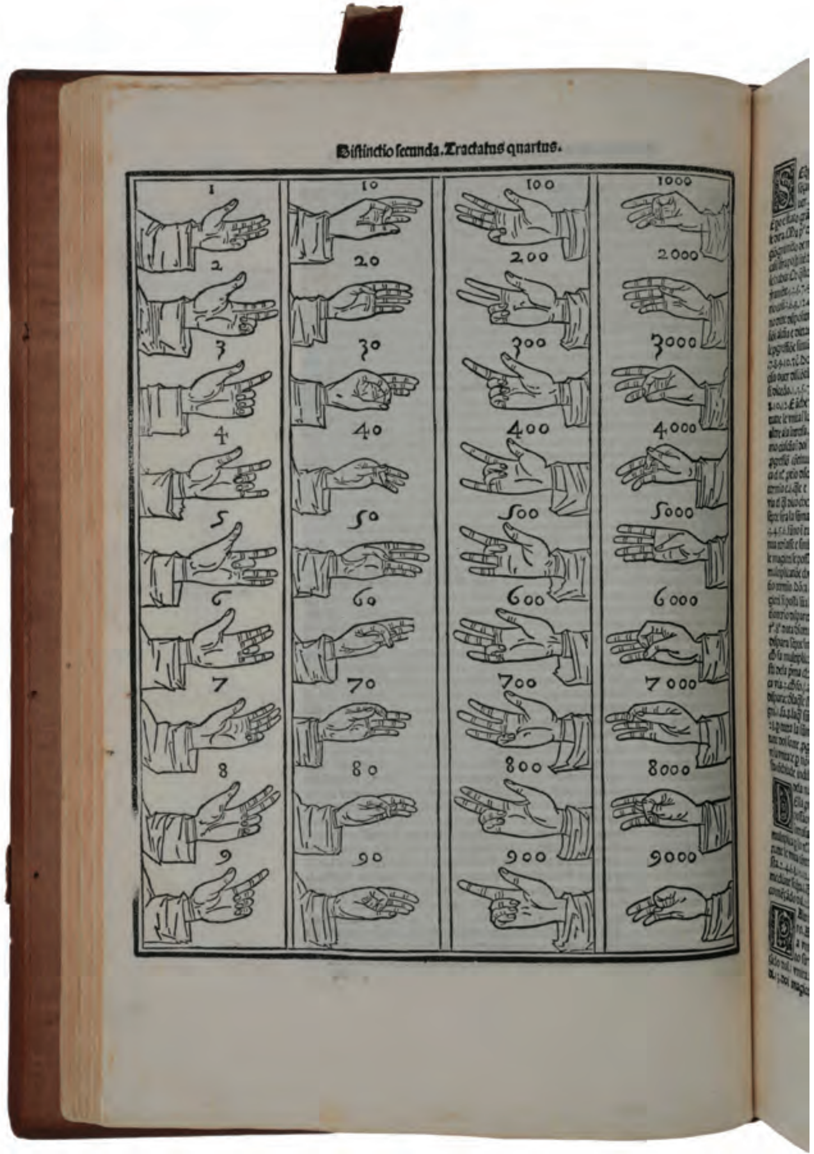
"In addition to teaching all of the necessary methods of arithmetic and algebra, Leonardo includes in *Liber abaci* a wealth of applications of mathematics to all kinds of situations in business and trade, conversion of units of money, weight, and content, methods of barter, business partnerships and allocation of profit, alloying of money, investment of money, simple and compound interest" (Sigler, p. 5). This material comprises chapter 9 of the *Summa*, which is divided into 12 sections (*Tractati*), the first ten on various items relevant to business (including barter and bills of exchange), the eleventh on bookkeeping (27 pages), and the twelfth on weights and measures and exchange rates. Pacioli interestingly observes that the term for the modern mathematics of merchants, '*abbaco*', was likely derived from the phrase '*in modo Arabico*' ('In the Arab manner') (see f. 19r), not from the abacus counting device (see Sangster *et al*., p. 116).

Within these chapters on business, the eleventh section entitled *Particularis de computis et scripturis* describes the accounting methods then in use among northern-Italian merchants, including double-entry bookkeeping, trial balances, balance sheets and various other tools still employed by professional accountants. "Five hundred years ago, in November 1494, one of the world's earliest printed texts included a section on accounting … Five hundred years later, the ideas printed in that accounting manual continue to provide the guidelines for recording economic activity in all the world's great financial centers. The author of that first accounting manual is Luca Pacioli. The *Summa de Arithmetica, Geometria, Proportioni at Proportionalita* – which includes this first published discourse on accounting – is a work of genius" (Cripps, p. ix). Richard Brown has said [in his *History of Accounting and Accountants (*1905), p. 119] that "The history of bookkeeping during the next century consists of little else than registering the progress of the *De computis* through the various countries of Europe."

"Pacioli defines double-entry bookkeeping broadly, as 'nothing else than the expression in writing of the arrangement of [a merchant's] affairs.' If a merchant follows the system Pacioli sets out, then he will always know 'all about his business and will know exactly whether his business goes well or not' … Pacioli's formulation of Venetian double-entry bookkeeping is one of the great advances in the history of business and commerce. He recommends this method, which had been practised in Venice for two hundred years, as the best … Rather than mingling debt and credit entries under each other down a single column or page – as did the Florentine merchants before they began keeping their books *alla viniziana* – Venetian ledgers separate debits and credits, dividing them into two columns, which is exactly how we organise our ledgers today …

"In Pacioli's view, three things are needed by 'anyone who wishes to carry on business carefully. The most important of these is cash or any equivalent' … The second thing necessary in business 'is to be a good bookkeeper and ready mathematician.' The third 'and last thing is to arrange all the transactions in such a systematic way that one may understand each one of them at a glance, i.e., by the debit and credit method.' Not much has changed today … Pacioli does not go into detail, as he makes clear in his introduction: 'Although one cannot write out every essential detail for all cases, nevertheless a careful mind will be able, from what is given, to make the application to any particular case.' Nor does he give sample pages of worked examples, as writers on bookkeeping would begin to do in the next century. Instead, he assumes his readers are merchants and their sons, with some working knowledge of bookkeeping …

"The first thing a merchant must do, says Pacioli, is to make an inventory of everything he owns … Once the inventory is made, a merchant needs three books in which to record his business transactions. The first is the *memoriale*, or memorandum, which acted like a diary and was a temporary record of the merchant's transactions … The second book required is the *giornale*, or journal. After entering his inventory into the journal, the merchant uses this book to write up in a neat and orderly fashion the details of each transaction that has been



recorded in the memorandum. In Pacioli's Venetian system, every item entered into the journal must be preceded by one of two key words: *per* (which means 'from' and indicated that the ledger account must be debited) and *a* (which means 'to' and indicated that the ledger must be credited) … The third book is the *quaderno*, or ledger. The ledger is made up of pages ruled in two columns (the simplest form of which is now known as a T-column) and it records – twice – every journal entry. For every entry made in the journal there will be two in the ledger: one, a debit, entered on the left side of the T-column; and the other, a credit, on the right. The ledger with its two columns marked a great advance in account-keeping. By using this system the merchant could at any moment see at a glance the precise state of his assets and his debts. It also allowed him to find mistakes in his bookkeeping relatively easily, because if his books did not balance – his debits were not equal to his credits – he had made a mistake somewhere and would have to scrutinize his books to find it. The ledger with its 'double-entries' was an innovation made by the merchants of Venice and is the reason that Venetian bookkeeping is now known as double entry" (Gleeson-White, pp. 92-100).

The section of the *Summa* on business contains what is almost certainly the first example of a logarithm in print. On f. 181r, Pacioli claims that the number of years necessary to double a capital placed at compound interest, is the number resulting from the division of the fixed number 72 by the rate of interest as a percentage. This was the 'rule of 72', which succeeding mathematicians including Tartaglia failed to explain. In modern notation, we want the number n such that

$$(1 + r/100)^n = 2,$$

where r is the rate of interest. Our solution is $n = \log 2/\log(1 + r/100)$, which if r is small is approximately $(100 \log 2)/r$. Thus, Pacioli was giving the approximate value 0.72 of the natural logarithm of 2, more than a century before logarithms were invented, and with an error of less than 3%.

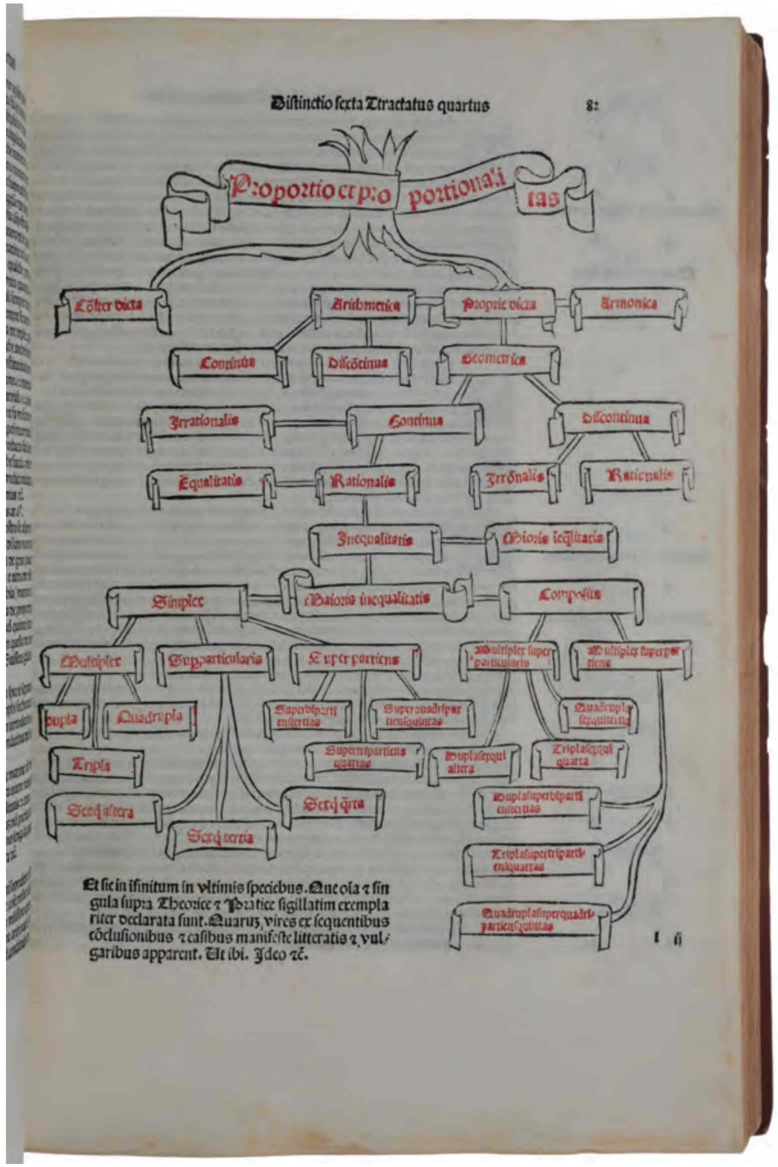
The *Summa* can claim to be the earliest printed book on probability, thanks to its discussion of the famous 'problem of points', or division of stakes. On ff. 197r and 198v, Pacioli states two problems. The first of these is as follows (the other is similar):

'A company [two players] plays a ball game to 60 and each goal is 10. They stake 10 ducats in all. It happens by certain incidents that they are not able to finish; and one party has 50 and the other 20. One asks what portion of the stake is due to each party.'

Pacioli gives three solutions, each of which amounts to dividing the stakes in proportion to the number of points already won. Schneider notes that although Pacioli's solution is incorrect, he had contributed to the problem of points in two significant ways. First, by simply offering a solution, Pacioli had put forward a problem that would later be brought to Cardano's attention. Secondly, Pacioli advocated the view that a definite and unique solution exists for the problem of points. In Cardano's *Practica arithmetice* (1539), he questions Pacioli's solution, noting that it does not take into account the number of games yet to be won by the players. In section 20, titled 'Error di Fra Luca dal Borgo,' of his *General trattato di numeri* (1556-60), Tartaglia notes that 'His rule seems neither agreeable nor good, since, if one player has, by chance, ten points and the other no points, then following this rule, the player who has the ten points would take all the stakes which obviously does not make sense.' These criticisms were well founded, and although Cardano and Tartaglia proposed their own solutions, it was Pascal and Fermat who, in their correspondence beginning in 1654, first correctly solved the problem of points, using a combinatorial technique that was not available to earlier authors.

The problem of points is clearly related to the division of shares in a trading company, which perhaps explains why Pacioli included it in the *Summa*. "Games, and especially games of chance, became a favourite pastime of the sixteenth-century merchant. Betting and economic enterprises became intertwined in a way that can explain why gaming was understood as a process which recapitulated the activities of merchant adventurers in a condensed time span. This very close connection between gaming and the economy in the sixteenth century explains very nicely why the economy was the source of inspiration for the solution of the problem of points" (Schneider, p. 220). Pacioli actually wrote a book devoted to games of chance, *De ludis*, but this has not survived.

The second volume of the *Summa* is largely, though not entirely, a version of Fibonacci's *Practica geometriae*, composed in 1220 or 1221. 'Practical Geometry' is the name of the craft of medieval land-measurers, known as *agrimensores* in Roman times, and as surveyors in modern times. Fibonacci wrote *Practica geometriae* for these artisans, a fitting complement to *Liber abaci*. The first chapter of the second volume of the *Summa* contains a summary of the books of Euclid's *Elements* on fundamental geometric constructions, calculations of areas, and similarity theory. The second is concerned with special lines in a triangle. The third treats right triangles and the associated solution of quadratic equations (theorem of Pythagoras). In contrast to the writings of al-Khwārizmī, here the solution of quadratic equations is presented theoretically, not by means of examples. In addition, Pacioli dealt with equations of the third and fourth degree, which he held to be as unsolvable (*impossibile*) as the quadrature of the circle, an assertion that not long afterwards was refuted by Scipione del Ferro (1515) (Rose points out that Pacioli may have stimulated Scipio's discovery – he was one of



Pacioli's colleagues when Pacioli was lecturing Bologna in 1501-1502). The fourth chapter concerns the theory of the circle. Tables of chords give information on the lengths of chords and their associated arcs. For π, Pacioli gives the approximation 3 33/229. In the fifth chapter, the division of geometric figures is discussed (theory of ratios). The sixth chapter explains how to calculate the surface area and volume of geometric solids. The seventh chapter introduces apparatus and methods of measurement. The eighth chapter contains a number of applications of different types: calculation of the volume of a barrel (approximately described as two frusta of cones), calculations on regular solids, and the inscribing of several equal circles of maximal size in a triangle and in a circle. Finally, Pacioli provides an overview of the coinage and weights and measures of the various Italian city-states.

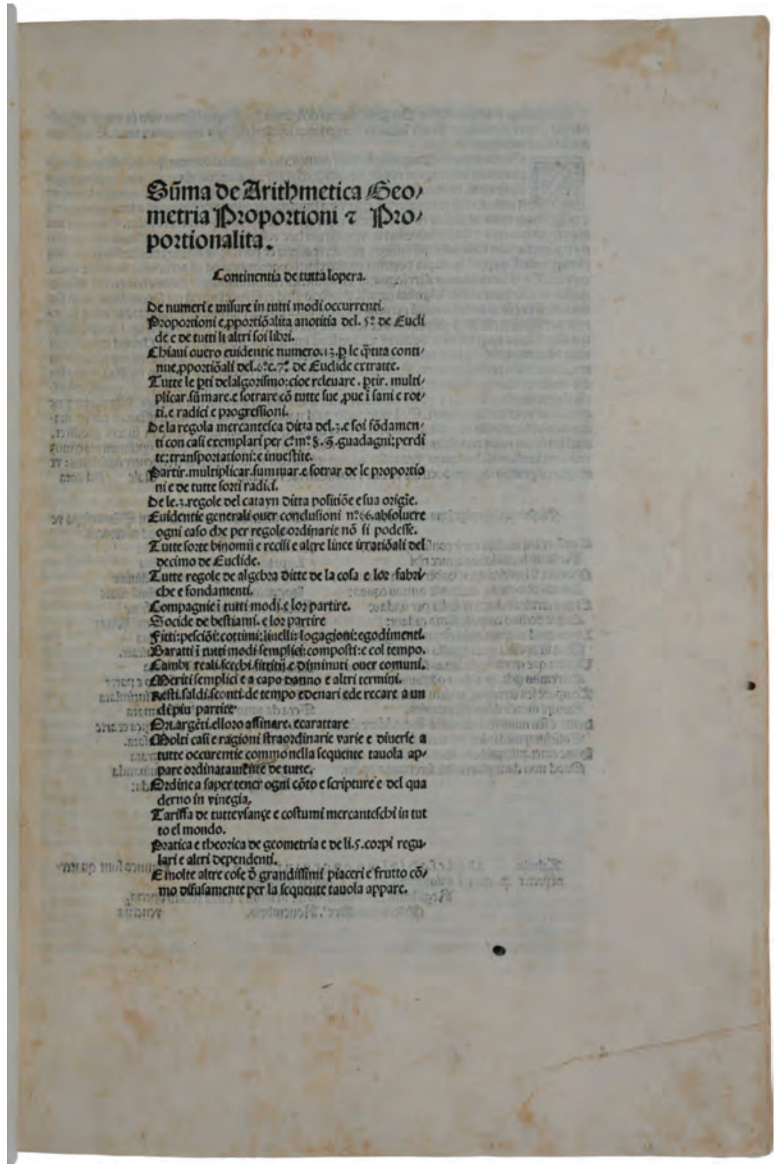
In both the algebraic and geometric volumes of the *Summa*, Pacioli includes material from the *Trattato d'abaco* of Piero della Francesca (which was not published until the 20th century). The *Trattato*, composed between 1460 and 1470, building on the work of Fibonacci, "belongs to the so-called abacus books, which, despite their name, presented not the abacus but mathematics on an elementary level for future merchants, bank clerks, artisans, and artists. The books dealt primarily with arithmetic, but often included some algebra and practical geometry as well – which is also true of Piero's *Trattato*. In addition, Piero treated some more advanced geometrical objects, such as the regular polyhedra. He returned to these solids in the last of his books *Libellus de quinque corporibus regularibus*, in which he also included five semi-regular, also called Archimedean, polyhedral [convex polyhedra that have regular polygons of more than one kind as faces] … In *Trattato* [Piero] described two of these, one – later called a cuboctahedron – having eight equilateral triangles and six squares as faces, obtained by cutting off the corners of a cube through the midpoints of the edges. The second was a truncated tetrahedron that Piero constructed by cutting off the vertices of a tetrahedron through points situated one third of the edge length from the corners. In *Libellus* he came back to the truncated tetrahedron and then added the truncated versions of the remaining four regular polyhedra. Pacioli later incorporated the *Libellus* into his *Divina proportione* (1509). As to the rest of the contents of *Trattato* and *Libellus*, Piero benefited a great deal from earlier treatises – the material for which mainly dates back to Euclid, al-Khwārizmī, and Leonardo da Pisa" (DSB, under Piero della Francesca). Pacioli included more than a hundred problems of algebra and arithmetic from the *Trattato* in the first volume of the *Summa*; much of the geometrical content of the *Trattato* appears at the end of the second volume, including new theorems of Euclidean geometry, stereometric problems (e.g., the formula for the volume of a tetrahedron in terms of the lengths of its sides), as well as the material on polyhedra.

"The *Summa* was widely circulated and studied by the mathematicians of the sixteenth century. Cardano, while devoting a chapter of his *Practica arithmetice* to correcting the errors in the *Summa*, acknowledged his debt to Pacioli. Tartaglia's *General trattato de' numeri et misure* was styled on Pacioli's *Summa*. In the introduction to his *Algebra* [1570], Bombelli says that Pacioli was the first mathematician after Leonardo Fibonacci to have thrown light on the science of algebra" (DSB, under Pacioli).

Born 1446-1448 in Borgo Sansepolcro, Pacioli was first schooled in Venice in the *abbaco* system, an applied, commercial education focusing on mercantile mathematics. He later studied mathematics under the great Florentine polymath mathematician and architect Leon Battista Alberti. Pacioli became a Franciscan friar in the early 1470s and shortly thereafter was named Perugia's first public lecturer in *abbaco* arithmetic. Pacioli soon rose to teach mathematics at the university level and so was highly unusual in mastering both practical and theoretical mathematics. Prior to its publication in 1494, Pacioli had been working on the *Summa* for a period of thirty years. Pacioli regretted the low ebb to which teaching had fallen and he thought that the fault lay in the use of improper methods and in the scarcity of available subject matter. He sought to correct these faults in the *Summa*.

Pacioli, who had studied chiefly in Venice, published his *Summa* there in 1494, two years before his extensive travels as a teacher brought him to Milan, summoned by the Duke Lodovico Sforza. There he held the chair of mathematics from 1497–99. In Milan he met Leonardo da Vinci, also in the service of Duke Lodovico Sforza 'il Moro'. They became friends and immersed themselves in common intellectual pursuits: 'the remarkable convergence of their thought after 1496 was such that many of Luca's published opinions would sit easily in Leonardo's notebooks' (M. Kemp, *Leonardo da Vinci: the marvellous works of nature and of man,* p 148). A note in the *Codex Atlanticus* reads: "Learn the multiplication of roots from Maestro Luca" (f. 331r), and both the Madrid and Forster codices contain notes on the *Somma* – in particular, sections on proportions and proportionality, including a mirror-version of the *arbor proportionis et proportionalitatis* on f. 82r of Pacioli's work. Pacioli familiarized Leonardo with Euclidean mathematics and "Luca's presence in Milan served to encourage Leonardo to pursue far more explicit and fundamental investigations into the mathematical order which had formed the implicit basis of much of his earlier art and science" (*ibid*). On the 8th of February 1498 Pacioli argued in a court disputation for the status of painting as a liberal art involving mathematical knowledge.

Unemployed as a result of the fall of the Sforza regime in 1499, Pacioli and Leonardo resumed their peripatetic careers, ending up together in Florence where they found protection under its republican ruler Piero Soderini. There Pacioli embarked upon compiling his *De divina proportione*, illustrated by Leonardo, and a work best understood as the product of their collaboration and intellectual friendship. Pacioli's acknowledgement of Leonardo in the introduction is



Sũma de Arithmetica Geometria Proportioni & Proportionalita.

Continentia de tutta lopera.

fulsome, referring as he does to the supreme beauty of his 'figures of the platonic and mathematical regular solids … made and formed by that ineffable left hand accommodated to all the disciplines', comparing Leonardo to the greatest of the ancient artists, and lauding the happy time they spent together at the Sforza court in Milan. He also refers to Leonardo's never-to-be-completed treatises on painting, on local motion, on weight and on percussion, and praises his recently painted *Last Supper* in the refectory of Sta. Maria delle Grazie in Milan and the colossal clay model of the unexecuted equestrian statue of Lodovico il Moro, the bronze for the casting of which was used by the Duke to make cannon in his last-ditch (and unsuccessful) attempt to defend his city against the invading armies of Louis XII of France.

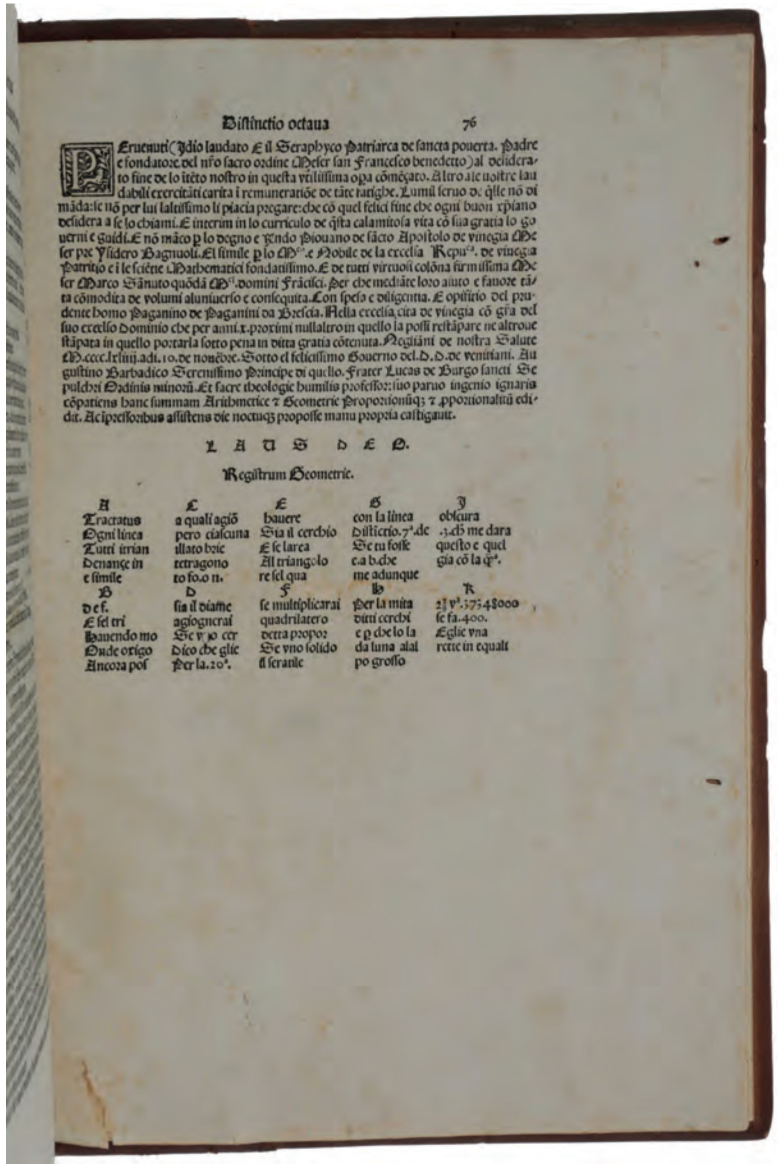
One large repeated initial depicts the author standing over an illustrated mathematical text, presumably the *Summa*, with a pair of dividers in his hand. In the portrait of Pacioli by Jacopo de' Barbari (1495; also attributed to Jacometto Veneziano) in the Capodimonte, Naples, Pacioli has his left hand on an open copy of Euclid, as "he demonstrates the eighth proposition from book XIII of the *Elements* of Euclid to a disciple, dressed according to the aristocratic fashion of the time, and identified as the duke Guidubaldo da Montefeltro, who instead turned his eyes towards the viewer. The friar had dedicated to the young duke the *Summa de Arithmetica, geometry, proportions and proportionality*, printed in Venice in 1494 and depicted in the painting right in front of the gentleman, with the inscription Li[ber] R[egularum] Luc[ae] Bur[gensis]" (*Il ritratto di Luca Pacioli*, Capodimonte website). Placed on top of the *Summa*'s red leather binding is a pentagonal dodecahedron, while a large crystal rhombicuboctahedron, half filled with water, is suspended beside him; this polyhedron has been attributed to the hand of Leonardo by some scholars. Pacioli also appears in the Brera Madonna by Piero della Francesca, who has used his features in the depiction of Peter Martyr.

This copy is the first issue; for reasons still unexplained the work was reissued twice (the second reissue can be dated to 1507–1509); for example, the copy in the British Library has some leaves in Boncompagni's setting B (IGI 7134). For details see Clarke p 91. Of the first issue, Volume 2 was completed on November 10, 1494, and the introduction to Volume 1 was completed on November 20, 1494. Pacioli tells us that he was present during 1493 and 1494 to oversee the printing, but it is clear that this did not include proof-reading as there are numerous typographical errors. The Venice printer, Paganino de' Paganini (c. 1450-1538), ran a fairly small print shop at the time the *Summa* was published, although his business grew in the early 16th century when he was joined by his son Alessandro. Paganini also published Pacioli's *Divina proportione* and his Italian translation of Euclid's *Elements*, both in 1509.

The *Summa* was an extremely large book for the period, and sold for 119 soldi, about one week's wages for a university teacher at the time. Sangster *et al*. estimate that between 1000 and 2000 copies of the *Summa* were printed, of which about 160 have survived. Olschki (*Geschichte der Neusprachlichen Wissenschaftlichen Literatur*, 1918) wrote that, for fifty years after its publication, the *Summa* was the most widely read mathematics work in Italy, and Favier [*Gold and Spices*: *The Rise of Commerce in the Middle Ages*, 1998], then president of the French Bibliothèque Nationale and author of many books on the Middle Ages, averred that the *Summa* was "an instant success and [was] for many years used by the business world" and that "merchants from every country rushed to buy this guide to accountancy" (Sangster *et al*., p. 142).

ISTC il00315000 (all three issues); IGI 7132; GM 18913; BMC V pp. 457–8; Essling 779; Goff L315; Goldsmiths 5; HC 4105; ICA p. 1; Klebs 718.1; Riccardi ii, 226; Smith, *RaraArithmetica* p. 54; Essling 779; Mortimer, *Italian 16th Century Books II* 2358 (second issue); Sander 5367; Stillwell 203. Jeremy Cripps, *Particularis de*

Computis et Scripturis … A Contemporary Interpretation (Seattle: Pacioli Society, 1994); Alan Sangster, *et al.*, 'The Market for Luca Pacioli's *Summa arithmetica*', *Accounting Historians Journal*, vol. 35, 2008, pp. 111–134; P. L. Rose, *The Italian Renaissance of Mathematics* (Geneva: Librairie Droz, 1975), pp. 143–50; Derek Ashdown Clarke, 'The First Edition of Pacioli's "Summa de Arithmetica" (Venice, Paganinus de Paganinis, 1494)', *Gutenberg Jahrbuch* (1974), pp. 90-92; J. Gleeson-White, *Double Entry*: *How the Merchants of Venice Created Modern Finance* (New York: W. W. Norton, 2012); Jayawardene, 'The 'Trattato d'abaco' of Piero della Francesca', in C. Clough, ed., *Cultural aspects of the Italian Renaissance, essays in honour of P.O. Kristeller* (Manchester, 1979); Ivo Schneider, 'The market place and games of chance in the fifteenth and sixteenth centuries,' in *Mathematics from Manuscript to Print*, ed. Cynthia Hay (New York: Oxford University Press, 1988); L. E. Sigler, *Fibonacci's Liber Abaci* (New York: Springer, 2002); A. Ciocci, *Luca Pacioli tra Piero della Francesca e Leonardo*, (Sansepolcro: Aboca Museum, 2009); A. Ciocci, *Luca Pacioli e la mathematizzazione del sapere nel Rinascimento* (Bari: Cacucci, 2003); E. Giusti and C. Maccagni, *eds*, *Luca Pacioli e la mathematica del Rinascimento* (Florence: Giunti, 1994).



Distinctio octaua 76

LAUS DEO.

Registrum Geometrie.


Distinctio octaua Casus primus.
Distinctio octaua.

The portrait of Pacioli by Jacopo de' Barbari (1495; also attributed to Jacometto Veneziano) in the Capodimonte, Naples, Pacioli has his left hand on an open copy of Euclid, as 'he demonstrates the eighth proposition from book XIII of the *Elements* of Euclid to a disciple, dressed according to the aristocratic fashion of the time, and identified as the duke Guidubaldo da Montefeltro, who instead turned his eyes towards the viewer. The friar had dedicated to the young duke the *Summa de Arithmetica, geometry, proportions and proportionality*, printed in Venice in 1494 and depicted in the painting right in front of the gentleman, with the inscription Li[ber] R[egularum] Luc[ae] Bur[gensis]' (*Il ritratto di Luca Pacioli*, Capodimonte website). Placed on top of the *Summa*'s red leather binding is a pentagonal dodecahedron, while a large crystal rhombicuboctahedron, half filled with water, is suspended beside him; this polyhedron has been attributed to the hand of Leonardo by some scholars. Pacioli also appears in the Brera Madonna by Piero della Francesca, who has used his features in the depiction of Peter Martyr.

THE NATURE OF SPACE

PATRIZI, Francesco. *Della Nuova Geometria Libri XV.* Ferrara: Vittorio Baldini, 1587 [but 1586].

$16,750

4to (199 x 150 mm), pp. [8], 227, [1, errata], with woodcut device on title and woodcut diagrams in the text. Contemporary vellum with manuscript lettering to spine, some wear but entirely unrestored.

First edition, very rare, of Patrizi's important work on the concept of 'space', which is "one of the most significant and important documents for the history of mathematical *epistemology* in the Renaissance, and might indeed almost be considered the turning point and dividing line between ancient and modern geometry" (Prins, p. 56). "Patrizi's importance in the history of science rests primarily on his highly original views concerning the nature of space, which have striking similarities to those later developed by Henry More and Isaac Newton. His position was first set out in *De rerum naturae libri II priores, alter de spacio physico, alter de spacio mathematico* (Ferrara, 1587) [although this was probably published *after* the *Nuova geometria* – see below]. Rejecting the Aristotelian doctrines of *horror vacui* and of determinate 'place,' Patrizi argued that the physical existence of a void is possible and that space is a necessary precondition of all that exists in it. Space, for Patrizi, was 'merely the simple capacity (*aptitudo*) for receiving bodies, and nothing else.' It was no longer a category, as it was for Aristotle, but an indeterminate receptacle of infinite extent. His distinction between 'mathematical' and 'physical' space points the way toward later philosophical and scientific theories. The primacy of space (*spazio*) in Patrizi's

system is also seen in his *Della nuova geometria* (Ferrara, 1587), the essence of which was later incorporated into the *Nova de universis philosophia*. In it Patrizi attempted to found a system of geometry in which space was a fundamental, undefined concept that entered into the basic definitions (point, line, angle) of the system. The full impact of Patrizi's works on later thought has yet to be evaluated" (DSB). "Patrizi's works seem to have been widely known throughout Europe and directly influenced some of the Cambridge Platonists, notably Joseph Glanville and Henry More. Henry More can be seen as a link between Patrizi and Sir Isaac Newton. Patrizi's long arguments for an isotropic, unchanging, immobile and infinite space, his vehement denunciation of the Aristotelian concept, and his establishment of space as a new philosophical term can finally be said to have taken root when Newton was able to discuss absolute space after writing: I do not define space as being well known to all" (John Christopher Henry, Francesco Patrizi and the concept of space, doctoral thesis, University of Leeds, 1977, pp. 167-168). OCLC records eight US locations, at Chicago, Columbia, Wisconsin, Burndy, Michigan, Illinois, Temple, and Cornell, and a copy at the Fisher Library, Toronto. ABPC/RBH list just three other copies: Swann, 21 October 2014, lot 346, $20,000; Sotheby's, 4 November 2004 (Macclesfield, part of a sammelband), £6250; Sotheby's, 11 April 2002 (de Vitry), £2585.

"Patrizi's new geometry primarily presents a comprehensive axiomatic deductive system based on his own intuitive definitions of geometric concepts. As a religious mathematical realist, Patrizi holds that through the realization of these God given intuitions, or innate ideas, the mathematical structure of the universe can be obtained. First of all, he develops his geometric system in order to replace that of Euclid … Euclid's *Elements* had disregarded the concepts of continuous quantity and infinity, proceeding from basic assumptions, axioms, and postulates to develop a strictly deductive geometric system. Even though Euclid's axioms and postulates are faultless, Patrizi declares that the underlying philosophical foundation of Euclidean theory is not a suitable foundation for a science of geometry. He argues that although Euclid had defined all basic geometric concepts, including the point, line, surface and solid, he failed to formulate a proper philosophical system for defining all the other geometric concepts. In the history of thought, this was already often noted as a major logical shortcoming of Euclid's geometry. There is indeed an unbridgeable gap between Euclid's first seven definitions (of concepts such as point, line, and surface) in the first book of his *Elements* and the remaining definitions of the treatise. The former definitions concern only geometric concepts, which must be understood, in accordance with Aristotle, as abstractions from real things in the world. In line with this aversion to anything Aristotelian, Patrizi holds that such a system of definitions does not meet all formal logical requirements for a scientific geometry. In addition, he argues, as a mathematical realist against Aristotelian scholars, that the mathematics of the natural world is not something merely abstracted from bodies by the human mind, but that bodies themselves are constituted of mathematical space. Therefore, mathematics should precede natural philosophy.

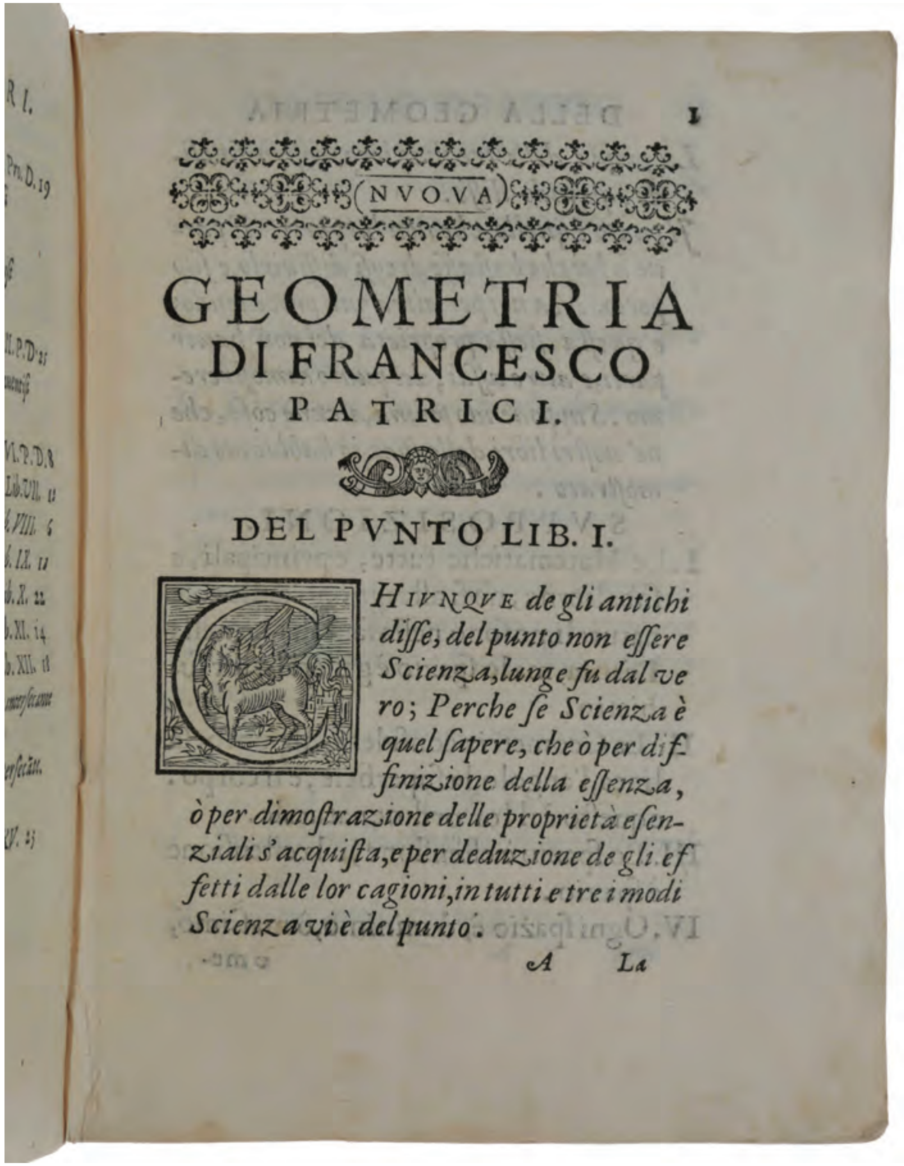
"In his *Nuova geometria* Patrizi defines points, lines, angles, surfaces, and solids as the subject matter of the discipline of geometry. He then goes on to argue that every space must have a minimum, maximum and mean. At this point in his explanation, he comes to the conclusion that each space must be one-dimensional (length), two-dimensional (length and breadth), or three-dimensional (length, breadth and depth) … Patrizi conceives of space as the first and most important undefined geometric concept which is lacking in Euclid's *Elements*. Points, lines, angles, surfaces, and solids must all be related to space as their founding concept … Patrizi holds that space, rather than the structure of the World-Soul, must be the prime subject of mathematics" (Prins, pp. 259-260).

"Aristotle's philosophy of mathematics, which was the widest and most elaborate to be developed in the Classical Age and, what is more, the most influential for the following centuries, held the proper object of geometry to consist in *magnitude*, that is, in continuous quantity; in other words, it considered all the shapes and figures which elementary geometry studies (triangles, circles, and spheres) as exemplifications and instances of the common genus 'magnitude'. Euclid's *Elements* themselves … appeared to straightforwardly endorse the notion that it was just these magnitudes that formed the proper object, and the matter of the inquiry, of the science of geometry. The concept of (continuous) quantity, however, did not seem to have any connection with that of space (nor with that of position), and it was thus inconceivable that geometry would have to deal with such a concept …

"[For the Neoplatonists, however,] magnitude is not a property of substance, but of matter itself. This thesis liberated, in principle at least, the consideration of magnitudes from that of individual, determinate bodies. Thus, it carried mathematics beyond the status of a theory of individual shapes or figures. This marked the birth of the idea of a quantified *environment*, in which the determinate figures and magnitudes which geometry examines are located …

"The principal element of originality in Patrizi's metaphysics of space consists in space's ontological independence from bodies and from matter, and in the fact that this space is quantified and is, in effect, the foundation of the quantification and extension of everything which exists in it. From this, clearly, it directly follows that mathematics must be concerned with space itself, rather than with bodies or with extended corporeal matter. It is thus no surprise that Patrizi was eventually to arrive at the formulation of a new epistemology of geometry as the science of space.

"The thesis that space is directly quantified but the matter located in it only


1

(NVOVA)

GEOMETRIA
DI FRANCESCO
PATRICI.

DEL PVNTO LIB. I.

CHIVNQVE de gli antichi disse, del punto non essere Scienza, lunge fu dal vero; Perche se Scienza è quel sapere, che ò per diffinizione della essenza, ò per dimostrazione delle proprietà essenziali s'acquista, e per deduzione de gli effetti dalle lor cagioni, in tutti e tre i modi Scienza vi è del punto.

A La

indirectly so had vast implications for the general conception of science and nature. In particular, this leads to the idea that the world is mathematizable, and that it is possible to investigate its quantitative features precisely because it is in space. That is to say, geometry can be applied to the world because it can be applied to space, and space is the foundation of all bodies' extension … We witness here, so to speak, a kind of 'dress rehearsal' of the epistemology of modern science …

"This definition of geometry as the science of space may seem obvious today, but in the sixteenth century it was not. Patrizi was perhaps the *very first* to formulate it. It was a definition which gained broader acceptance in the course of the following century, although it met with some resistance too, and was at times defended precisely in Patrizi's name. It then finally won the approval not only of philosophers, but also of geometers, who were able to substantiate with proofs and theorems the new geometry which Patrizi had left, in the moment of its birth, largely undefended. Finally, it succeeded in imposing itself as the commonly-accepted definition of the discipline, to such a point that it is nowadays difficult to imagine that geometry could be (and could have been for centuries) anything else than the science of space. In this sense, Patrizi gave birth to a new geometry, or rather laid the first epistemological and ontological foundations for such a geometry to be, first, fully theorized and then, eventually, actually implemented …

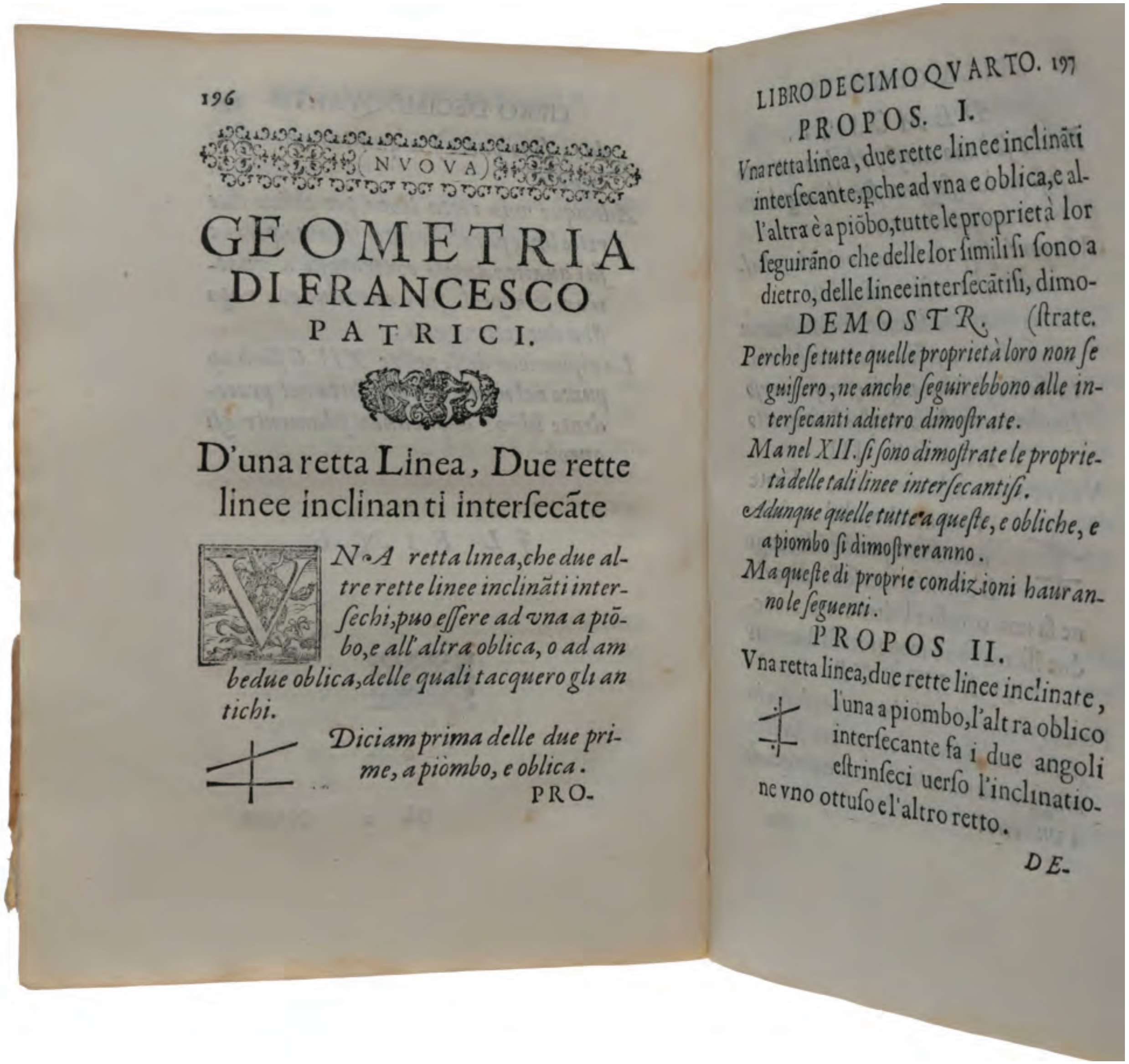
"In France, [Patrizi's] positions gained ground principally through the writings of Gassendi, who certainly makes a wide use of them (though without ever mentioning Patrizi) in the exposition of his theory of space, so similar to Patrizi's, in the *Syntagma philosophicum* from 1649 … on the other hand, Mersenne made the effort to mount a complete refutation of Patrizi's doctrines [in his *Quaestiones celeberrimae in Genesim*, 1623]. Most of all, however, Patrizi's metaphysics later gained currency in Great Britain, and Gilbert, Bacon, Fludd, Digby, and Harriot and Warner, were acquainted with him while Hobbes appreciated him highly; his good fortune, however, was chiefly due to the large numbers of English Neoplatonists who embraced his spatial theories enthusiastically: Herbert of Cherbury, Joseph Glanvill, and certainly Henry More … Newton's mentor, Isaac Barrow, certainly held modern positions about space, but in geometry he remained a classical Neoplatonist. On the other hand, Newton himself, who had studied all these English authors, especially More, and then Gassendi and scores of other readers of Patrizi, never once doubted that geometry is performed in space. Already his early writings betray a mathematical epistemology clearly Patrizian in spirit. Newton's extraordinary fame was certainly instrumental in the definitive acceptance of the geometry of space by mathematicians and philosophers.

"In the same years, moreover, Leibniz was developing a different theory of space and of geometry, but certainly agreed that the two needed to proceed hand in hand. It is not easy to ascertain the influence of Patrizi's *Nuove geometria* on Leibniz's attempts to develop an *analysis situs*. We do know that Leibniz read, admired – but also in certain respects scorned – Patrizi's book; that he shared with him some epistemological views about mathematics; and that many geometrical concepts and definitions appear in almost identical forms in both works. We don't know, however, when exactly Leibniz read the *Nuova geometria* and whether he was actually influenced by it in the making of his own (far superior) geometry of space …

"The frontispiece [of the *Nuova geometria*] bears the date of 1587, but we know from Patrizi's correspondence with the mathematician Giambattista Benedetti that, as early as July 1586, he was in a position to send him the first printed pages, and that the whole book had appeared by early December of that year. It is remarkable, however, that Patrizi affirms in the treatise on geometry that he presupposes therein some principles which he has, so he claims, already proven in his two books on space, since the latter had probably not yet appeared in print by the end of 1586. We may assume, therefore, that he had already written his metaphysical essays by the time of the composition of the *Nuova geometria*. Conversely, in the final sentence of the two booklets *De spacio physico & mathematico*, Patrizi noted that the Italian treatise *Della nuova geometria* was intended as the continuation of the essays on space" (Prins).

Patrizi (1529-1597) studied at Ingolstadt, at the University of Padua (1547-1554), and at Venice. While in the service of various noblemen in Rome and Venice he made several trips to the East, where he perfected his knowledge of Greek, and to Spain. He lived for a time at Modena and at Ferrara, before being appointed to a personal chair of Platonic philosophy at the University of Ferrara by Duke Alfonso II d'Este in 1578. He remained there until 1592, when Pope Clement VIII summoned him to a similar professorship in Rome, a post he held until his death.

Macclesfield 870; Poggendorff II, col. 374; Riccardi I 254 ("Bella e rara ediz. ommessa dal Brunet"); Sommerville p. 3; STC Italian p. 493; not in Adams. De Risi, 'Francesco Patrizi and the New Geometry of Space,' pp. 55-106 in Regier & Vermeir (eds.), *Boundaries, Extents and Circulations*, 2016. Prins, *Echoes of an Invisible World*: *Marsilio Ficino and Francesco Patrizi on Cosmic Order and Music Theory*, 2014.



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(NVOVA)

GEOMETRIA
DI FRANCESCO
PATRICI.

D'una retta Linea, Due rette linee inclinanti intersecãte

VNA retta linea, che due altre rette linee inclinãti intersechi, puo essere ad vna a piõbo, e all'altra oblica, o ad ambedue oblica, delle quali tacquero gli antichi.

Diciam prima delle due prime, a piombo, e oblica.

PRO-

LIBRO DECIMO QVARTO. 197

PROPOS. I.

Vna retta linea, due rette linee inclinãti intersecante, pche ad vna e oblica, e all'altra è a piõbo, tutte le proprietà lor seguirãno che delle lor simili si sono adietro, delle linee intersecãtisi, dimostrate.

DEMOSTR.

Perche se tutte quelle proprietà loro non seguissero, ne anche seguirebbono alle intersecanti adietro dimostrate.

Ma nel XII. si sono dimostrate le proprietà delle tali linee intersecantisi.

Adunque quelle tutte a queste, e obliche, e a piombo si dimostreranno.

Ma queste di proprie condizioni hauranno le seguenti.

PROPOS II.

Vna retta linea, due rette linee inclinate, l'una a piombo, l'altra oblico intersecante fa i due angoli estrinseci uerso l'inclinatione vno ottuso e l'altro retto.

DE-

PMM 40 - AN EXCEPTIONALLY FINE COPY IN STRICTLY CONTEMPORARY BINDING

PTOLEMY. Johannes REGIOMONTANUS (Johannes MÜLLER) [and Georg PEURBACH]. *Epytoma in Almagestum Ptolemaei.* Venice: Johannes Hamman, 31 August 1496.

$265,000

Folio (314 x 220 mm), ff 108, [including final blank], xylographic title, full-page woodcut of Ptolemy and Regiomontanus seated beneath an armillary sphere, within a fine white-on-black woodcut border, 279 woodcut diagrams (including repeats), 6-, 7-, and 14-line white-on-black floriated and historiated woodcut initials, woodcut printer's device on colophon leaf; minute marginal wormhole just touching a few letters from quire g to end, faint marginal dampstain to a few quires, a few early or contemporary corrections in ink to text along with an added manuscript diagram and manuscript additions to a printed diagram on f2, an extraordinarily fresh, untouched, unpressed, and very large copy (the largest we have been able to find measurements of), with some deckle edges, in its original Italian binding comprising a 13th-century vellum leaf, from a manuscript of Justinian's Digest, over paper boards, with ties, absolutely unrestored (for further details see below).

First edition, and a beautiful, large and fresh copy in an outstanding contemporary Italian binding preserved in its original state, of this epochal work by the leading astronomer of the fifteenth century. "The importance of this book lies in the fact that it enshrines, within the editor's commentary, the first appearance in print, in

a Latin translation from the Greek, of the monumental compendium of Claudius Ptolemaeus of Alexandria known as the *Almagest*" (PMM), the foundation of ancient astronomy. "The *Almagest* of Ptolemy (90-180 AD) had been known since the late 1100s only through a poor Arabic translation until Cardinal Bessarion charged the eminent Georgius Peuerbach with the task of properly editing the text. Only the first four books were completed when Peuerbach died in 1461 and the work was finished by his pupil Regiomontanus. This handsome volume again brought Greek astronomy and the accepted version of the universe before the Western world in Latin, a language all learned men could read. The xylographic portrait of Regiomontanus is considered authentic" (Dibner, *Heralds*, 1). "The *Epitome* of the *Almagest* was a new astronomical treatise. It paved the way for future investigations on the basis of fundamental observations and findings of the past, and his pointing out defects stimulated new research. Both Copernicus and Galileo used it as their textbook" (Zinner, *Regiomontanus: his life and work,* p. 54). The *Epitome* is "a model of clarity and includes everything essential to a working understanding of mathematical astronomy and even manages to clarify sections in which Ptolemy omits steps or is somewhat obscure. It has not been superseded even by the excellent modern commentaries on the *Almagest*, and the mathematical astronomy of the sixteenth century is in places unintelligible without it" (DSB, under Peurbach). "At the end of the fifteenth century, Ptolemy's achievement remained at the pinnacle of astronomical thought; and by providing easier access to Ptolemy's complex masterpiece, the Peuerbach-Regiomontanus *Epitome* contributed to current scientific research rather than to improved understanding of the past. Moreover, the *Epitome* was no mere compressed translation of the *Syntaxis* [i.e., the *Almagest*], to which it added later observations, revised computations, and critical reflections—one of which revealed that Ptolemy's lunar theory required the apparent diameter of the moon to vary in length much more than it really does. This passage (book V, proposition 22) in the *Epitome*, which was printed in Venice, attracted the attention of Copernicus, then a student at the University of Bologna. Struck by this error in Ptolemy's astronomical system, which had prevailed for over 1,300 years, Copernicus went on to lay the foundations of modern astronomy and thus overthrow the Ptolemaic system" (DSB, under Regiomontanus). "In his writings challenging the Ptolemaic system, Copernicus ... often referred to this book by Peurbach and Regiomontanus. Indeed, the *Epitome Almagesti* did not just serve as the foil to Copernicus's new theories; on the contrary, it contained proofs that were fundamental to his development of a heliocentric system" (Jason Dean, introduction to Dr Henry Zepeda's lecture on the *Epitome* at Linda Hall Library). Regiomontanus had hoped to publish the *Epitome* at his own short-lived Nuremberg press (active 1473-1475), but his premature death at the age of 40 in 1476 delayed its appearance for more than twenty years.

Binding: 'an exactly contemporary Italian long–stitch binding sewn through a full primary cover of blue cartonnage with a secondary cover of medieval manuscript waste, with alum–tawed skin ties at head, tail, and fore–edges' (thanks to Professor Nicholas Pickwoad, who notes that the use of the blue cartonnage 'is less common – though used for the covers of some of the little long–stitch notebooks used by Leonardo da Vinci in the same decade (1490s')

Provenance: 1601 inscription on front free endpaper, recording the exchange of a copy of 'La Pratica' of a Maggi or Maggio at a value of 25-somethings for this work: 'A di 6 Aprile 1601 Barat[t]ato cum il Gatta in Cambio li det [t]i La Pratica del Maggio est modo del instrumentare qual mi valeua 25'; there are various possible candidates for Maggi or Maggio's 'La Pratica' , the most likely being Maggi and Castriotto's treatise on military perspective, *Della Fortificatione delle Citta*, Venice 1564. There is one early (16th century?) manuscript geometrical diagram in the text (in the margin of f2r).

Born in Austria, Georg Peurbach (1423-61) matriculated for the baccalaureate at the University of Vienna in 1446 and received the bachelor's degree in the Arts Faculty on 2 January 1448. Two years later he probably became a licentiate, and on 28 February 1453 he received the master's degree and was enrolled in the Arts Faculty. It is unclear with whom, or indeed if, he formally studied astronomy. The last notable astronomer at Vienna, John of Gmunden, had died in 1442, prior to Peurbach's arrival, but it is possible that he had access to astronomical books and instruments collected by John of Gmunden. During the period 1448–1453, Peurbach traveled through Germany, France, and Italy, at the end of which he appears to have acquired an international reputation. After his return to Vienna, Peurbach became court astrologer to King Ladislaus V of Hungary, the young nephew of Emperor Frederick III. At some later time, Peurbach became court astrologer to the emperor himself, since Regiomontanus refers to him as 'astronomus caesaris' in the dedication of the *Epitome* and cites his service to Frederick in the lecture given at Padua. During this period, Peurbach's responsibilities at the university were concerned mostly with humanistic rather than astronomical studies.

"Regiomontanus (1436-76) enrolled in the University of Vienna on 14 April 1450 as 'Johannes Molitoris de Künigsperg [Johannes Müller of Königsberg]'. Since the name of his birthplace means 'King's Mountain,' he sometimes Latinized his name as 'Joannes de Regio monte,' from which the standard designation Regiomontanus was later derived. He was awarded the bachelor's degree on 16 January 1452 at the age of fifteen; but because of the regulations of the university, he could not receive the master's degree until he was twenty-one. On 11 November 1457 he was appointed to the faculty, thereby becoming a colleague of Peurbach, with whom he had studied astronomy. The two men became fast friends and worked closely together as observers of the heavens.

"The course of their lives was deeply affected by the arrival in Vienna on 5 May 1460 of Cardinal Bessarion (1403-72), the papal legate to the Holy Roman Empire" (DSB, under Regiomontanus). "His mission was to intervene in the continuing dispute between Frederick III and his brother Albert VI of Styria and to seek aid in a planned crusade against the Turks for the recapture of Constantinople. In Vienna he met both Peurbach and Regiomontanus. Bessarion was a figure of considerable importance in the transmission of Greek learning to Italy, and his interests were sufficiently diverse to include the exact sciences. He collected a large number of very fine Greek manuscripts that he later left to the city of Venice, where they form the core of the manuscript collection of the Biblioteca Marciana. One of his plans evidently involved a new translation of the *Almagest* from the Greek to replace Gerard of Cremona's version from the Arabic and to improve upon the inferior translation from the Greek made by George of Trebizond in 1451. He also desired an abridgment of the *Almagest* to use as a textbook. Although Peurbach was unfamiliar with Greek, according to Regiomontanus he knew the *Almagest* almost by heart (quem ille quasi ad litteram memorie tenebat) and so took on the task of preparing the abridgment. Further plans were made for Peurbach and Regiomontanus to accompany Bessarion to Italy and there work with him, using Bessarion's Greek manuscripts as the basis of the new translation. Peurbach, however, had completed only the first six books of the abridgment when he died, not yet thirty-eight years old. On his deathbed he made Regiomontanus promise to complete the work, which the latter did in Italy during the next year or two. This account is given by Regiomontanus in his preface to the *Epitome* of the *Almagest*. The completed work was dedicated to Bessarion by Regiomontanus, probably in 1463, in a very careful and beautifully executed copy (Venice, lat. 328, fols, 1–117).

"Peurbach's early death was a serious loss to the progress of astronomy, if for no other reason than that the collaboration with his even more capable and industrious pupil Regiomontanus promised a greater quantity of valuable work than either could accomplish separately. Of their contemporaries, only Bianchini, who was considerably their senior, possessed a comparable proficiency and originality. The equally early death of Regiomontanus in 1476 left the technical development of mathematical astronomy deprived of substantial improvement until the generation of Tycho Brahe …

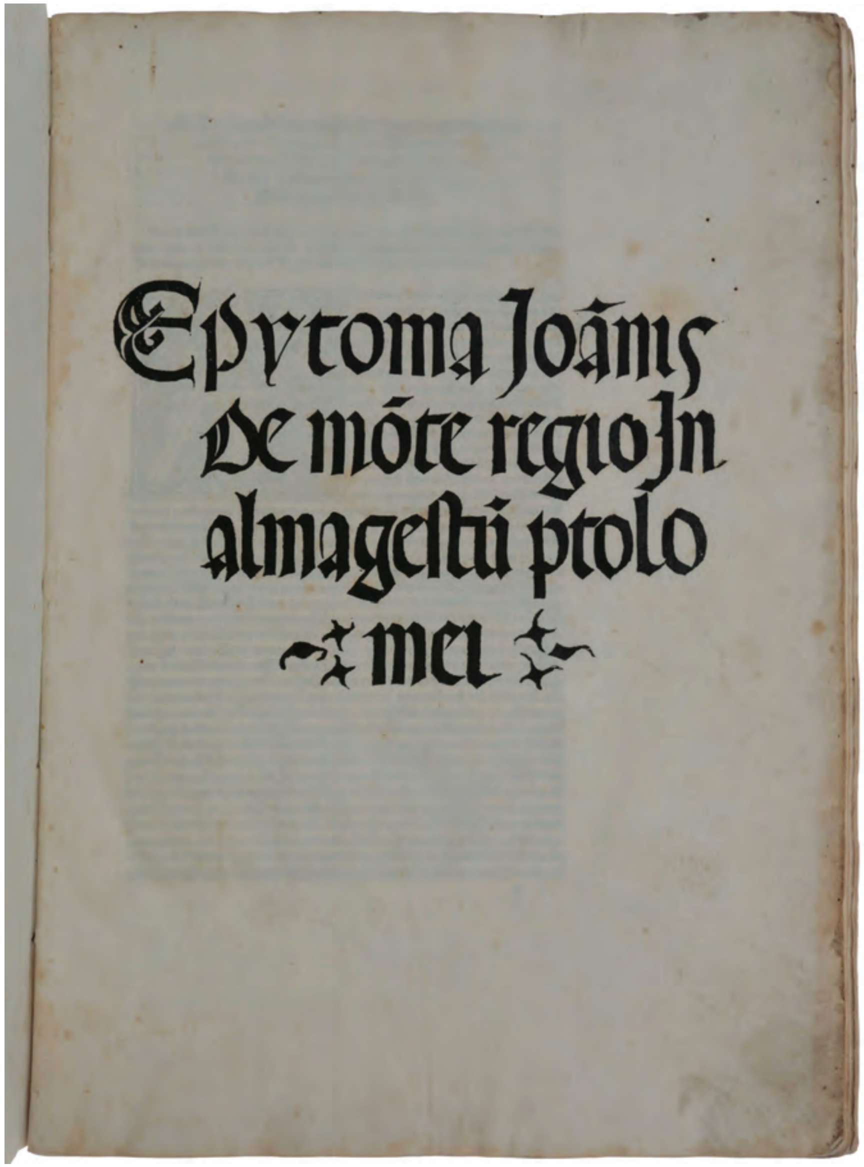
"According to Regiomontanus, Peurbach was responsible for the first six books of *Epitoma Almagesti Ptolemaei* (also known by slight variants of this title), the most important and most advanced Renaissance textbook on astronomy, while books VII-XIII were completed by Regiomontanus after Peurbach's death. But this account of the division of labor and credit probably requires some modification. The introduction and first six propositions of book I, giving the general arrangement of the universe, are in part translated and in part paraphrased from the Greek *Almagest* and must be the work of Regiomontanus, possibly with assistance from Bessarion …

"Aside from the introductory section, books I through VI are closely based upon the so-called *Almagesti minoris libri VI*, a doubtless unfinished textbook, apparently of the late thirteenth century, that supplements Ptolemy with information and procedures drawn from al-Battānī, Thābit ibn Qurra, Jābir ibn Aflah, az-Zarqāl, and the Toledan Tables. The *Almagestum minor* divides Ptolemy's sometimes lengthy chapters into individual propositions showing the proof of a geometrical theorem, the derivation of a parameter, or the carrying out of a procedure, and there are occasional digressions adding the work of post-Ptolemaic writers. The *Epitome* adopts exactly this arrangement and sometimes follows the *Almagestum minor* nearly word for word, including all of its supplements to Ptolemy. Evidently Peurbach based the *Epitome* upon the earlier work; and, with all due respect to Regiomontanus' account of his teacher's contribution, one may legitimately ask to what extent the present state of the first six books is really the result of Regiomontanus' revision of what Peurbach may have left as little more than a close paraphrase of the *Almagestum minor*" (DSB, under Peurbach).

"The derivation of planetary paths [in the *Almagest*] was very awkward, and the necessary mathematical formulas were held to be obsolete, ever since Geber had introduced the Law of Sines for easy solution of spherical triangles, back in the twelfth century. Faced with these defects, Peuerbach took a new tack: he abandoned the restoration of the tables and the lengthy derivation of paths, reproducing the contents of many sections in abbreviated form. In many cases, especially when representing the mathematical relationships between divisions of the heavens, he gave a series of theorems with a well-established foundation; in this way, he introduced his readers to the difficult field of spherical astronomy.

"His technique showed itself in the first book: he briefly restated the first six axioms on the place of the earth in the world, following with the theorems in individual sections. He had already discussed the first six sections in his derivation of the table of sines. The 18th section of Book I shows the application of sines, replacing old calculations with arcs and chords … in Book I, Section 17 he describes a quadrant with a scale having two sighting holes, as opposed to Ptolemy's quadrant, in which a shadow was cast by rods …

"Book II (spherical astronomy) is completely altered, but in Book III, he again follows Ptolemy's way. Nonetheless he does not refrain from pointing out, in the first section, the uncertainty of observing the time of the solstice, saying that it is more suitable to observe the equinoxes. In Section 14 he refers to his own representation of the sun's entrance into the equinoctial points and neighboring signs and gives a rule for finding the beginnings of the four seasons. However the observations were not communicated. In the following sections he refers to al-



Battani's results again and again.

"Book V contains several important sections. In Section 13 he describes the 'Dreistab', calling it the *Regula Ptolomaei*, but unfortunately he omits any statements about its exact arrangement; it is well known that later on, Regiomontanus made it into a fundamental observational instrument. The end of section 22 contains the crucial remark that it is wonderful that the moon does not occasionally appear four times its usual size, as the Ptolemaic theory requires. This reference to a flagrant defect in the prevailing theory must have made an impression on a youthful Copernicus …

"[Regiomontanus's] reworking of Book VII is worthy of comment. In this book Ptolemy describes the motion of the firmament relative to the zodiac and includes a star catalog. Regiomontanus omits the catalog; otherwise he preserves the reasoning, insofar as he verified the unchangeable positions of the stars relative to each other through his own observations. He then reported on the motion of the firmament as it follows from the observations of Ptolemy and his successors, and refers to the uncertainty of these occasionally contradictory points of view as follows:

'The inexactness of the instruments may have caused these differences; Nature may have assigned some unknown motion to the stars; it is now and will henceforth be very difficult to determine the amount of this motion due to its small size. For if our predecessors were deceived by their instruments, so necessarily would we, too, for our observations will prove nothing if we do not compare them with those of antiquity. But if we ascribe an unknown motion to the stars, then we must keep the stars firmly under observation and rid the future generations of this tradition.'

"This reference to the necessity of a new determination of stellar motion (the precession) was also significant, in view of the subsequent sections in which Regiomontanus gives instructions for calculating stellar coordinates, if the positions of two stars are known and relative distances between stars are fixed. The two stars' coordinates are found from the positions of the sun and the moon by means of an armillary sphere, i.e., an instrument composed of several circles. At this point, everything necessary for a new survey of the stars and the production of a new star-catalog was in place. It was now only a matter of beginning with the observations …

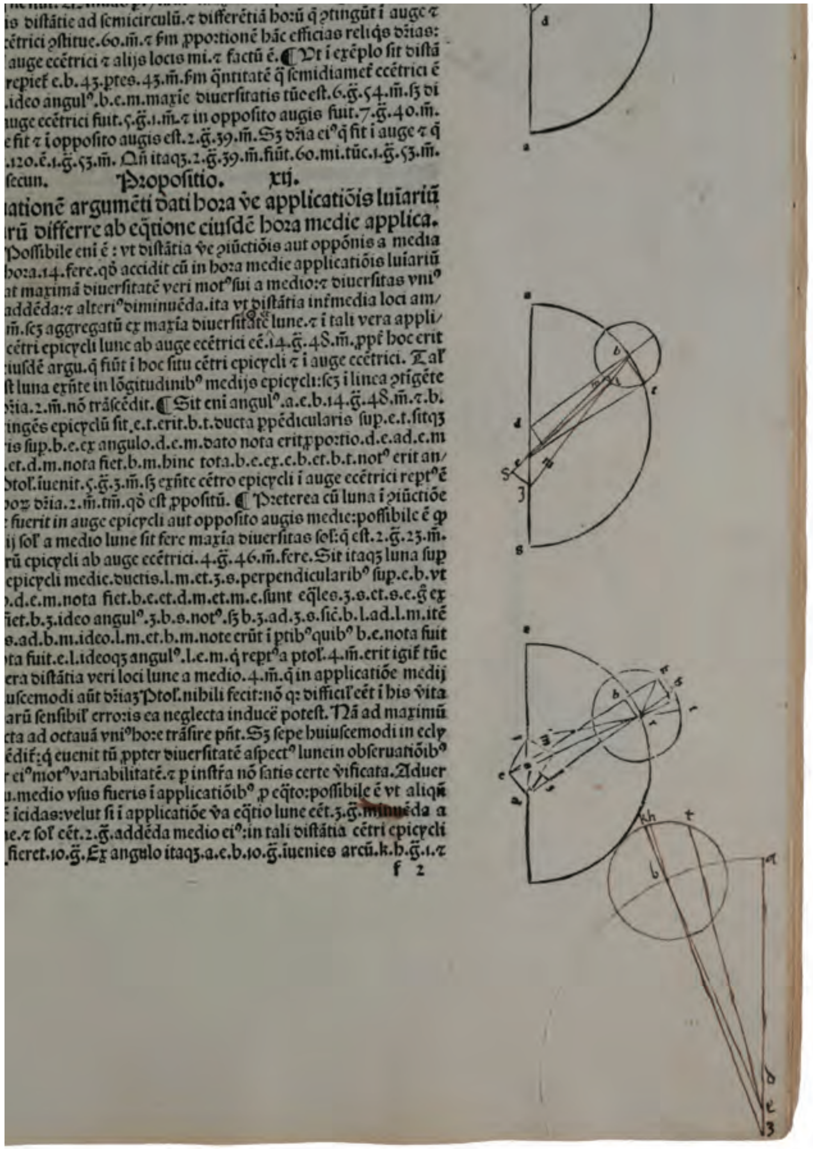
"The ninth book, on the motion of Venus and Mercury, gave him occasion to make several comments. In Section 1 he mentioned the old assumption that the ratio of the apparent diameters of the sun and Venus was 10:1. Consequently, the sun's disk equals one hundred of Venus's, so that a transit of Venus could not be seen … The last four books, concerned with Mars, Jupiter, Saturn, and changes in latitude, did nor prompt him to make any comments …

"Now, on which translation of the *Almagest* was the *Epitome* based? There are two Latin translations from that time: Gerard of Cremona's translation of 1175 and George Trebizond of Crete's translation. The latter translated the original Greek text of the Almagest for Pope Nicholas V and added his own commentary. This version, swiftly written between March and December 1451, was so objectionable that the pope banned Trebizond from Rome. Regiomontanus had obtained both translations while in Vienna. Peuerbach had copied Cremona's for himself: it was mainly this translation on which Peuerbach and Regiomontanus based their work" (Zinner, pp. 52-54).

"At the end of the fifteenth century, Ptolemy's achievement remained at the pinnacle of astronomical thought; and by providing easier access to Ptolemy's complex masterpiece, the Peuerbach-Regiomontanus *Epitome* contributed to current scientific research rather than to improved understanding of the past. Moreover, the *Epitome* was no mere compressed translation of the *Syntaxis*, to which it added later observations, revised computations, and critical reflections—one of which revealed that Ptolemy's lunar theory required the apparent diameter of the moon to vary in length much more than it really does. This passage (book V, proposition 22) in the *Epitome*, which was printed in Venice, attracted the attention of Copernicus, then a student at the University of Bologna. Struck by this error in Ptolemy's astronomical system, which had prevailed for over 1,300 years, Copernicus went on to lay the foundations of modern astronomy and thus overthrow the Ptolemaic system" (DSB, under Regiomontanus).

In a very few copies (for example two out of the thirty-four listed in Goff) there are two inserted leaves containing a letter from the astrologer Giovan Baptista Abioso. It is completely irrelevant to the text, simply comprising prognostications, and "appears to be a later supplement" (Horblit).

HC *13806; BMC V, 427; CIBN R-60; BSB-Ink R-67; Bod-inc R-040; IGI 5326; Klebs 841.1; Essling 895; Sander 6399; Stillwell Science, 103; Dibner, *Heralds* 1; Grolier/Horblit 89; Norman 1565; Schäfer/Arnim 192; PMM 40; Goff R-111.



THE 'AVERAGE MAN'

QUETELET, Lambert Adolphe Jacques. *Sur l'Homme et le Développement de ses Facultés, ou Essai de Physique Sociale.* Paris: Bachelier, 1835.

$14,500

Two vols., 8vo (222 x 139 mm), pp. [4], xii, 327, [1], with two folding plates; [4], viii, 327, [1] , with four folding plates (four plates engraved, the other 2 lithographed). Original yellow printed wrappers, previous owner's signature stamp to front wrappers, small rectangular piece cut from the rear wrapper of vol. 2 (probably a price). Entirely unrestored in its original state. Some scattered relatively mild spotting (this work is usually found rather foxed). Very rare in such fine condition.

First edition of Quetelet's principal work in which he presented his conception of the *homme moyen* ("average man") as the central value about which measurements of a human trait are grouped according to the normal distribution (this was the first time the normal distribution had been used other than as an error law). "With Quetelet's work of 1835 a new era in statistics began. It presented a new technique of statistics, or, rather, the first technique at all. The material was thoughtfully elaborated, arranged according to certain pre-established principles, and made comparable. There were not very many statistical figures in the book, but each figure reported made sense. For every number, Quetelet tried to find the determining influences, its natural causes, and the perturbations caused by man. The work gave a description of the average man as both a static and dynamic phenomenon. This work was a tremendous achievement, but Quetelet had aimed at a much higher goal: social physics, as the subtitle of the work said; the same title under which, since 1825, Comte had taught what he later called sociology.

Terms and analogies borrowed from mechanics played a great part in Quetelet's theoretical explanation. To find the laws that govern the social body, one has to do what one does in physics: to observe a large number of cases and then take averages. Quetelet's average man became a slogan in nineteenth-century discussions on social science" (DSB). "A rare, three-part review in the *Athenaeum* concluded by remarking: 'We consider the appearance of these volumes as forming an epoch in the literary history of civilization'" (Stigler, p. 170). This work occasionally appears on the market, but we have not been able to locate any copy in original printed wrappers sold at auction.

"It was in writings published in the 1830s that Quetelet (1796-1874) established the theoretical foundations of his work in moral statistics or, to use the modern term, sociology. First there was the idea that social phenomena in general are extremely regular and that the empirical regularities can be discovered through the application of statistical techniques. Furthermore, these regularities have causes: Quetelet considered his averages to be "of the order of physical facts," thus establishing the link between physical laws and social laws. But rather than attach a theological interpretation to these regularities—as Sussmilch and others had done a century earlier, finding in them evidence of a divine order—Quetelet attributed them to social conditions at different times and in different places. This conclusion had two consequences: It gave rise to a large number of ethical problems, casting doubt on man's free will and thus, for example, on individual responsibility for crime; and in practical terms it provided a basis for arguing that meliorative legislation can alter social conditions so as to lower crime rates or rates of suicide.

"On the methodological side, two key principles were set forth very early in Quetelet's work. The first states that 'Causes are proportional to the effects produced by them'. This is easy to accept when it comes to man's physical characteristics; it is the assumption that allows us to conclude, for example, that one man is 'twice as strong' as another (the cause) simply because we *observe* that he can lift an object that is twice as heavy (the effect). Quetelet proposed that a scientific study of man's moral and intellectual qualities is possible only if this principle can be applied to them as well. The second key principle advanced by Quetelet is that large numbers are necessary in order to reach any reliable conclusions—an idea that can be traced to the influence of Laplace, Fourier, and Poisson …

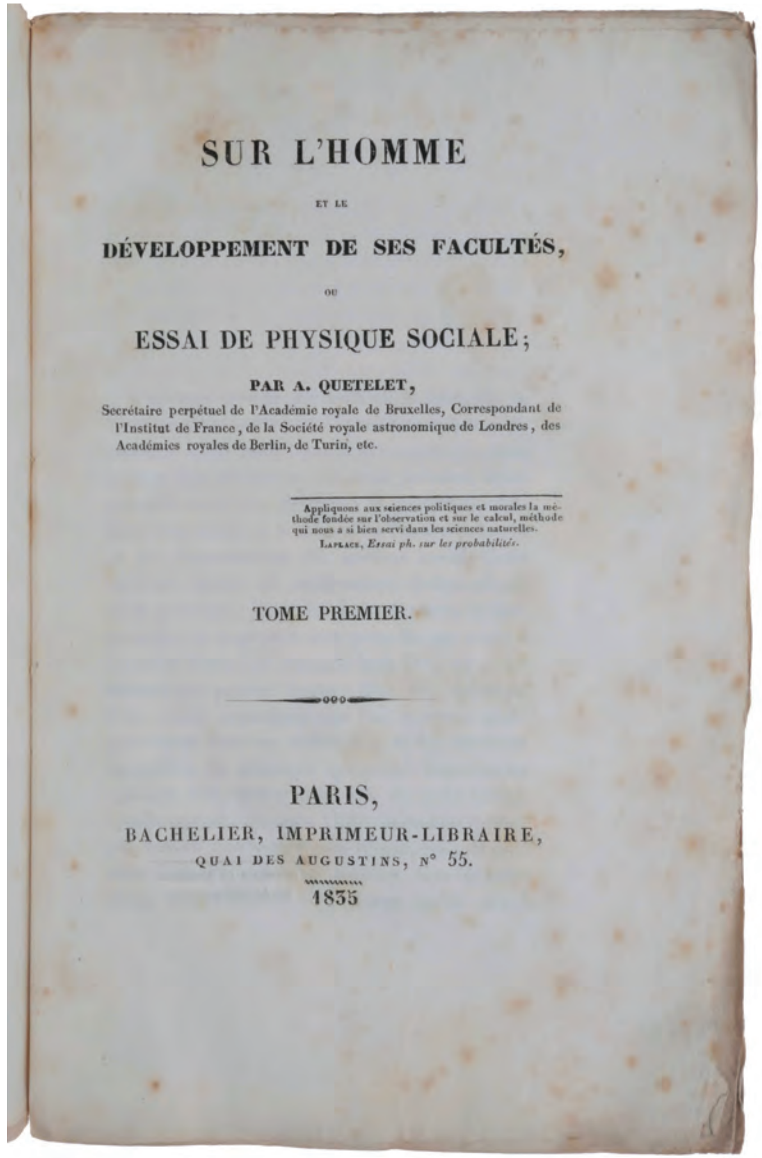
"Quetelet was greatly concerned that the methods he adopted for studying man in all his aspects be as 'scientific' as those used in any of the physical sciences. His solution to this problem was to develop a methodology that would allow full application of the theory of probabilities. For in striking contrast to his contemporary Auguste Comte, Quetelet believed that the use of mathematics is not only the *sine qua non* of any exact science but the measure of its worth …

"The two memoirs which form the basis for all of Quetelet's subsequent investigations of social phenomena appeared in 1831. By then he had decided that he wanted to isolate, from the general pool of statistical data, a special set dealing with human beings. He first published a memoir entitled *Recherches sur la loi de la croissance de I'homme*, which utilized a large number of measurements of people's physical dimensions. A few months later he published statistics on crime, under the title *Recherches sur le penchant au crime aux differens ages*. While the emphasis in these publications is on what we would call the life cycle, both of them also include many multivariate tabulations, such as differences in the age-specific crime rates for men and women separately, for various countries, and for different social groups … In 1833 Quetelet published a third memoir giving developmental data on weight, *Recherches sur le poids de Vhomme aux differens ages*" (DSB).

"Quetelet made two important advances toward the statistical analysis of social data: the first of these was formulating the concept of the average man, the second the fitting of distributions. Quetelet's first awakening to the variety of relationships latent in society may have come with his investigation of population data, but his interests soon spread. From 1827 to 1835 he examined scores of potentially meaningful relationships through the compilation of tables and the preparation of graphical displays… He examined birth and death rates by month and city, by temperature, and by time of day. He calculated the month of conception from the birth month and tried to relate it to marriage statistics. He investigated mortality by age, by profession, by locality, by season, in prisons, and in hospitals. He considered other human attributes: height, weight, growth rate, and strength. Quetelet's interests also extended to moral qualities: statistics on drunkenness, insanity, suicides, and crime. In 1835 he collected a number of earlier memoirs and added to them to form the two-volume book that was to gain him an international reputation as a social scientist: *Sur l'homme et le developpement de ses facultes ou Essai de physique sociale*. It was translated into English in 1842 as *A Treatise on Man and the Development of his Faculties*.

"There was no mistaking Quetelet's aim in this book: to lay the ground-work for social physics, to conduct a rigorous, quantified investigation of the laws of society that might some day stand with astronomers' achievements of the previous century. His beginning was only tentative, and he was careful (sometimes to the point of being apologetic) not to claim more success than he could defend; but he was eloquent and his zeal caught the public eye" (Stigler, pp. 169-70).

"From 1819 Quetelet lectured at the Brussels Athenaeum, military college, and museum. In 1823 he went to Paris to study astronomy, meteorology, and the management of an astronomical observatory. While there he learned probability from Joseph Fourier and, conceivably, from Pierre-Simon Laplace. Quetelet


SUR L'HOMME

ET LE

DÉVELOPPEMENT DE SES FACULTÉS,

OU

ESSAI DE PHYSIQUE SOCIALE;

PAR A. QUETELET,

Secrétaire perpétuel de l'Académie royale de Bruxelles, Correspondant de l'Institut de France, de la Société royale astronomique de Londres, des Académies royales de Berlin, de Turin, etc.

Appliquons aux sciences politiques et morales la méthode fondée sur l'observation et sur le calcul, méthode qui nous a si bien servi dans les sciences naturelles.

LAPLACE, *Essai ph. sur les probabilités.*

TOME PREMIER.

PARIS,

BACHELIER, IMPRIMEUR-LIBRAIRE,

QUAI DES AUGUSTINS, N° 55.

1835

founded (1828) and directed the Royal Observatory in Brussels, served as perpetual secretary of the Belgian Royal Academy (1834–74), and organized the first International Statistical Congress (1853). For the Dutch and Belgian governments, he collected and analyzed statistics on crime, mortality, and other subjects and devised improvements in census taking. He also developed methods for simultaneous observations of astronomical, meteorological, and geodetic phenomena from scattered points throughout Europe" (Britannica).

Kress C.4017; DSB XI: 236-8; S. M. Stigler, *The History of Statistics*, 1986, Chapter 5.

THE FIRST THEORY OF NEURAL NETWORKS

ROSENBLATT, Frank. *The perceptron: a probabilistic model for information storage and organization in the brain. Offprint from Psychological Review, vol. 65, no. 6, November 1958, pp. 386-408. [With:] The design of an intelligent automaton. Offprint from [Office of Naval Research] Research Reviews, October 1958, pp. 5-13. [With:] Two theorems of statistical separability in the perceptron. Offprint (preprint?) from Proceedings of the Symposium on the Mechanization of Thought, National Physical Laboratory, Teddington, UK, November 1958, Vol. I, pp. 421-456. Together with two further offprints on the physiological basis of memory".* Washington and Teddington: American Psychological Association, Office of Naval Research, National Physical Laboratory, 1958.

$9,500

Five offprints, 8vo. Stapled as issued in original printed wrappers. All in very fine condition.

First edition, very rare offprints, of the first published account of Rosenblatt's invention of the 'perceptron', the first modern neural network; this is one of the most important early works on Artificial Intelligence (AI). "Rosenblatt's paper ('The perceptron: a probabilistic model for information storage and organization in the brain') introduced the perceptron, the first precisely specified, computationally oriented neural network. ('Neural network' is the term used to describe a system deliberately constructed to employ some of the organizational principles believed to be used in the human brain). The perceptron had the

ability to recognize patters and to associate new patters with ones it had 'seen' before on the basis of similarity; like the brain, it could function in the presence of 'noise' and with damaged or missing connections. Later models were capable of trial-and-error learning and could be taught to emit ordered sequences of responses. Rosenblatt's perceptron theory took an empiricist or 'connectionist' position with regard to the central questions of how information is stored or remembered, and how information contained in storage or memory influences recognition and behavior. Disagreeing with those theorists who saw the brain as simply an imperfectly designed logic machine, Rosenblatt insisted that noise and randomness present in the nervous system were essential to the kinds of computations that brains performed, and thus had to be taken into account … The perceptron had a major impact on several different disciplines." (Hook & Norman, *Origins of Cyberspace*, 870). No copies of any of the offprints on OCLC or in auction records. These offprints were preceded only by internal Cornell Aeronautical Laboratory reports.

Provenance: All three perceptron offprints inscribed 'Harry Blum // Nov 59 // The Hague'. Harry Blum was an AI pioneer who worked at the Air Force Cambridge Research Laboratories at Hanscom Air Force Base, in Bedford, Massachusetts.

"The very first mathematical model of an artificial neuron was the Threshold Logic Unit proposed by Warren S. McCulloch (1898–1969, American neurophysiologist) and Walter H. Pitts Jr. (1923–1969, American logician) in 1943 … The operating principle of a biological neuron can be summarized as follows. First, it takes inputs from its dendrites (i.e., from other neurons). In a second step, a weighted sum of these inputs is performed within the soma. The result is then passed on to the axon hillock. If this weighted sum is larger than the threshold limit, the neuron will fire. Otherwise, it stays at rest. The state of our neuron (on or off) then propagates through its axon and is passed on to the other connected neurons via its synapses...

"Based on this basic understanding of the neuron's operating principle, McCulloch & Pitts proposed the very first mathematical model of an artificial neuron in their seminal paper *A logical calculus of the ideas immanent in nervous activity* back in 1943. Although very simple, their model has proven extremely versatile and easy to modify. Today, variations of their original model have now become the elementary building blocks of most neural networks, from the simple single layer perceptron all the way to the 152 layers-deep neural networks used by Microsoft to win the 2016 ImageNet contest.

"Almost fifteen years after McCulloch & Pitts, the American psychologist Frank Rosenblatt (1928–1971), inspired by the Hebbian theory of synaptic plasticity (i.e., the adaptation of brain neurons during the learning process), came up with the perceptron, a major improvement over the MCP neuron model. This invention granted him international recognition and, to this date, the Institute of Electrical and Electronics Engineers (IEEE), "the world's largest professional association dedicated to advancing technological innovation and excellence for the benefit of humanity", names its annual award in his honor.

"Rosenblatt's major achievement has been to show that, by relaxing some of the MCP's rules (namely the absolute inhibition, the equal contribution of all inputs as well as their integer nature), artificial neurons could actually learn from data. More importantly, he came up with a *supervised* learning algorithm for this modified MCP neuron model that enabled the artificial neuron to figure out the correct weights directly from training data by itself …

"Let us quickly discuss the type of problems that can be addressed by the perceptron. Binary (or binomial) classification is the task of classifying the elements of a given set into two groups (e.g. classifying whether an image depicts a cat or a dog) based on a prescribed rule … Rosenblatt's perceptron can handle only classification tasks for 'linearly separable classes' [i.e., classes which, in a suitable graphical representation, can be separated by a straight line, or more generally a hyperplane]...

"The simplicity and efficiency of this learning algorithm for linearly separable problems is one of the key reasons why it got so popular in the late 1950s and early 1960s. This popularity however caused Rosenblatt to oversell his perceptron's ability to learn, giving rise to unrealistic expectations in the scientific community and/or also reported by the media … this perceptron indeed suffers from major limitations greatly restricting its applicability to real-life problems. The *coup de grâce* came from Marvin Minsky (1927–2016, American cognitive scientist) and Seymour Papert (1928–2016, South African-born American mathematician) who published in 1969 the notoriously famous book *Perceptrons: an introduction to computational geometry*. In this book, the authors have shown how limited Rosenblatt's perceptron (and any other single layer perceptron) actually is and, notably, that it is impossible for it to learn the simple logical XOR function. Some argue that the publication of this book and the demonstration of the perceptron's limits triggered the so-called AI winter of the 1980s" (towardsdatascience.com/rosenblatts-perceptron-the-very-first-neural-network-37a3ec09038a).

The 'winter' of connectionist research came to an end in the middle 1980s, when the work of John Hopfield, David Rumelhart and others revived large-scale interest in neural networks. Today, many believe Rosenblatt has been vindicated. The principles underlying the perceptron helped spark the modern artificial intelligence revolution. Deep learning and neural networks – which can classify online images, for example, or enable language translation – are transforming society.

Rosenblatt's three papers on the perceptron are offered with the following works on the physiological basis of memory:

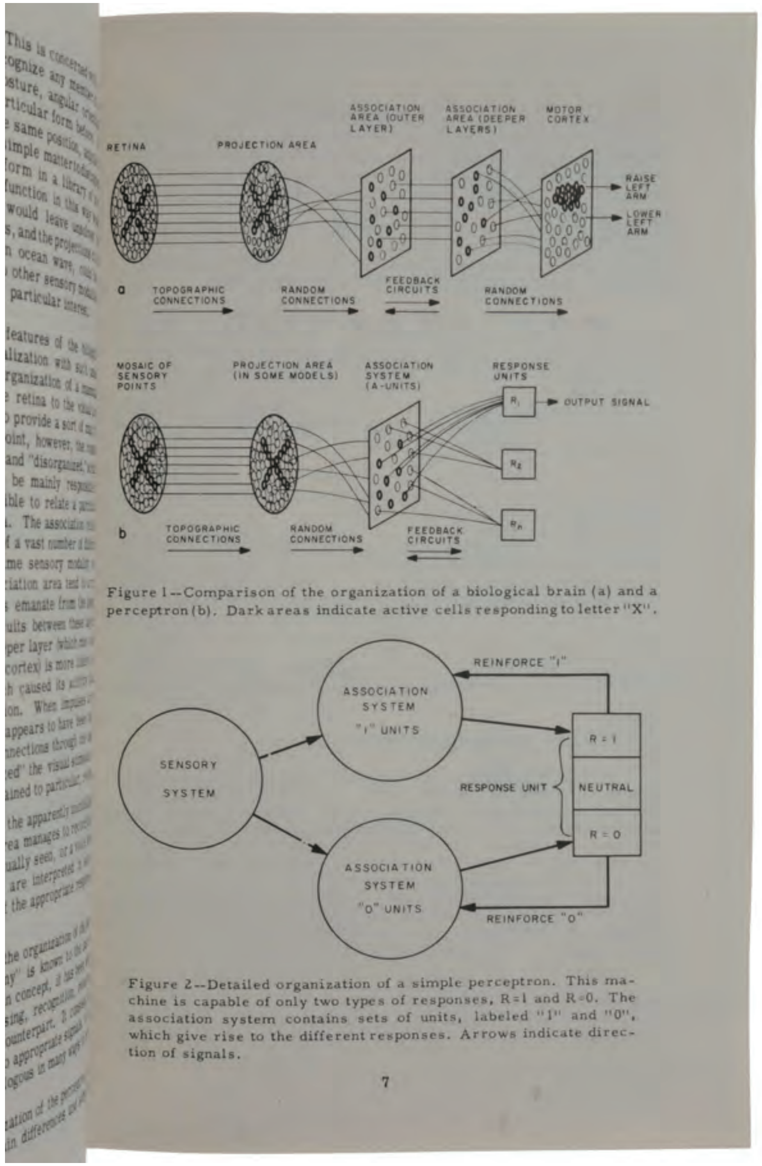


Figure 1--Comparison of the organization of a biological brain (a) and a perceptron (b). Dark areas indicate active cells responding to letter "X".

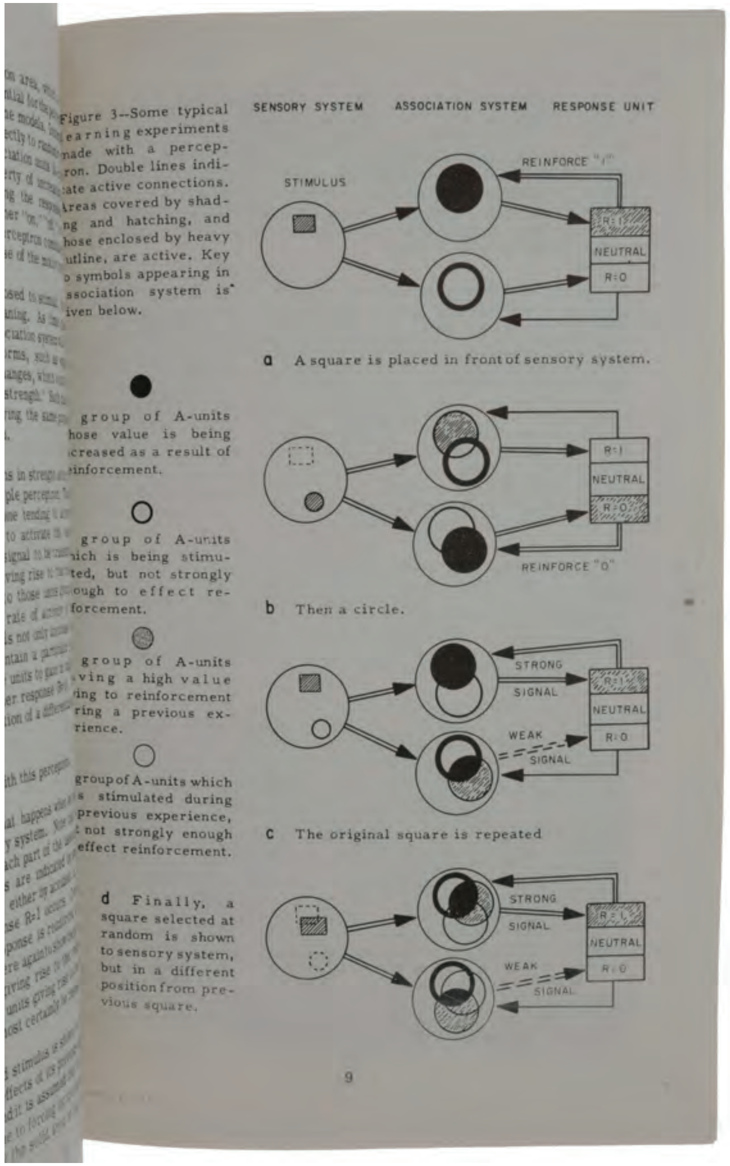
Figure 2--Detailed organization of a simple perceptron. This machine is capable of only two types of responses, R=1 and R=0. The association system contains sets of units, labeled "I" and "0", which give rise to the different responses. Arrows indicate direction of signals.

7

ROSENBLATT, Frank, FARROW, John T. & HERBLIN, William. Transfer of conditioned responses from trained rats to untrained rats by means of a brain extract. Offprint from *Nature*, vol. 209, no. 5018, January 1, 1966, pp. 46-48.

ROSENBLATT, Frank, FARROW, John T. & RHINE, Sam. The transfer of learned behaviour from trained to untrained rats by means of brain extracts, I, II. Offprint from *Proceedings of the National Academy of Sciences*, vol. 55, nos. 3, 4, pp. 548-555, March, pp. 787-792, April, 1966.

"Frank Rosenblatt was born in New Rochelle, New York as son of Dr Frank and Katherine Rosenblatt. After graduating from The Bronx High School of Science in 1946, he attended Cornell University, where he obtained his A.B. in 1950 and his Ph.D. in 1956. He then went to Cornell Aeronautical Laboratory in Buffalo, New York, where he was successively a research psychologist, senior psychologist, and head of the cognitive systems section. This is also where he conducted the early work on perceptrons, which culminated in the development and hardware construction of the Mark I Perceptron in 1960. This was essentially the first computer that could learn new skills by trial and error, using a type of neural network that simulates human thought processes. Rosenblatt's research interests were exceptionally broad. In 1959 he went to Cornell's Ithaca campus as director of the Cognitive Systems Research Program and also as a lecturer in the Psychology Department. In 1966 he joined the Section of Neurobiology and Behavior within the newly formed Division of Biological Sciences, as associate professor. Also in 1966, he became fascinated with the transfer of learned behavior from trained to naive rats by the injection of brain extracts, a subject on which he would publish extensively in later years. In 1970 he became field representative for the Graduate Field of Neurobiology and Behavior, and in 1971 he shared the acting chairmanship of the Section of Neurobiology and Behavior. Frank Rosenblatt died in July 1971 on his 43rd birthday, in a boating accident in Chesapeake Bay" (Wikipedia).



tend to gain an advantage over the other, and will be the one which occurs. If such a system is to be capable of learning, then it must be possible to modify the A-units or their connections in such a way that stimuli of one class will tend to evoke a stronger impulse in the R_1 source-set than in the R_2 source-set, while stimuli of another (dissimilar) class will tend to evoke a stronger impulse in the R_2 source-set than in the R_1 source-set.

It will be assumed that the impulses delivered by each A-unit can be characterized by a value, V, which may be an amplitude, frequency, latency, or probability of completing transmission. If an A-unit has a high value, then all of its output impulses are considered to be more effective, more potent, or more likely to arrive at their endbulbs than impulses from an A-unit with a lower value. The value of an A-unit is considered to be a fairly stable characteristic, probably depending on the metabolic condition of the cell and the cell membrane, but it is not absolutely constant. It is assumed that, in general, periods of activity tend to increase a cell's value, while the value may decay (in some models) with inactivity. The most interesting models are those in which cells are assumed to compete for metabolic materials, the more active cells gaining at the expense of the less active cells. In such a system, if there is no activity, all cells will tend to remain in a relatively constant condition, and (regardless of activity) the net value of the system, taken in

TABLE 1

COMPARISON OF LOGICAL CHARACTERISTICS OF α, β, AND γ SYSTEMS

	α-System (Uncompensated Gain System)	β-System (Constant Feed System)	γ-System (Parasitic Gain System)
Total value-gain of source set per reinforcement	N_{a_r}	K	0
ΔV for A-units active for 1 unit of time	+1	K/N_{a_r}	+1
ΔV for inactive A-units outside of dominant set	0	K/N_{A_r}	0
ΔV for inactive A-units of dominant set	0	0	$\frac{-N_{a_r}}{N_{A_r}-N_{a_r}}$
Mean value of A-system	Increases with number of reinforcements	Increases with time	Constant
Difference between mean values of source-sets	Proportional to differences of reinforcement frequency $(n_{s_{r_1}}-n_{s_{r_2}})$	0	0

Note: In the β and γ systems, the total value-change for any A-unit will be the sum of the ΔV's for all source-sets of which it is a member.

N_{a_r} = Number of active units in source-set
N_{A_r} = Total number of units in source-set
$n_{s_{r_i}}$ = Number of stimuli associated to response r_i
K = Arbitrary constant

its entirety, will remain constant at all times. Three types of systems, which differ in their value dynamics, have been investigated quantitatively. Their principal logical features are compared in Table 1. In the alpha system, an active cell simply gains an increment of value for every impulse, and holds this gain indefinitely. In the beta system, each source-set is allowed a certain constant rate of gain, the increments being apportioned among the cells of the source-set in proportion to their activity. In the gamma system, active cells gain in value at the expense of the inactive cells of their source-set, so that the total value of a source-set is always constant.

For purposes of analysis, it is convenient to distinguish two phases in the response of the system to a stimulus (Fig. 3). In the *predominant phase*, some proportion of A-units (represented by solid dots in the figure) responds to the stimulus, but the R-units are still inactive. This phase is transient, and quickly gives way to the *postdominant phase*, in which one of the responses becomes active, inhibiting activity in the complement of its own source-set, and thus preventing the occurrence of any alternative response. The response which happens to become dominant is initially random, but if the A-units are reinforced (i.e., if the active units are allowed to gain in value), then when the same stimulus is presented again at a later time, the same response will have a stronger tendency to recur, and learning can be said to have taken place.

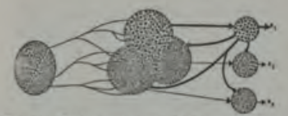


FIG. 3A. Predominant phase. Inhibitory connections are not shown. Solid black units are active.

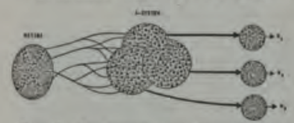


FIG. 3B. Postdominant phase. Dominant subset suppresses rival sets. Inhibitory connections shown only for R_1.

FIG 3. Phases of response to a stimulus.

ANALYSIS OF THE PREDOMINANT PHASE

The perceptrons considered here will always assume a fixed threshold, θ, for the activation of the A-units. Such a system will be called a *fixed-threshold model*, in contrast to a *continuous transducer model*, where the response of the A-unit is some continuous function of the impinging stimulus energy.

In order to predict the learning curves of a fixed-threshold perceptron, two variables have been found to be of primary importance. They are defined as follows:

P_a = the expected proportion of A-units activated by a stimulus of a given size,

P_c = the conditional probability that an A-unit which responds to a given stimulus, S_1, will also respond to another given stimulus, S_2.

It can be shown (Rosenblatt, **15**) that as the size of the retina is increased, the number of S-points (N_s) quickly ceases to be a significant parameter, and the values of P_a and P_c approach the value that they would have for a retina with infinitely many points. For a large retina, therefore, the equations are as follows:

$$P_a = \sum_{e=\theta}^{x} \sum_{i=0}^{\min(y,\, e-\theta)} P(e,i) \qquad (1)$$

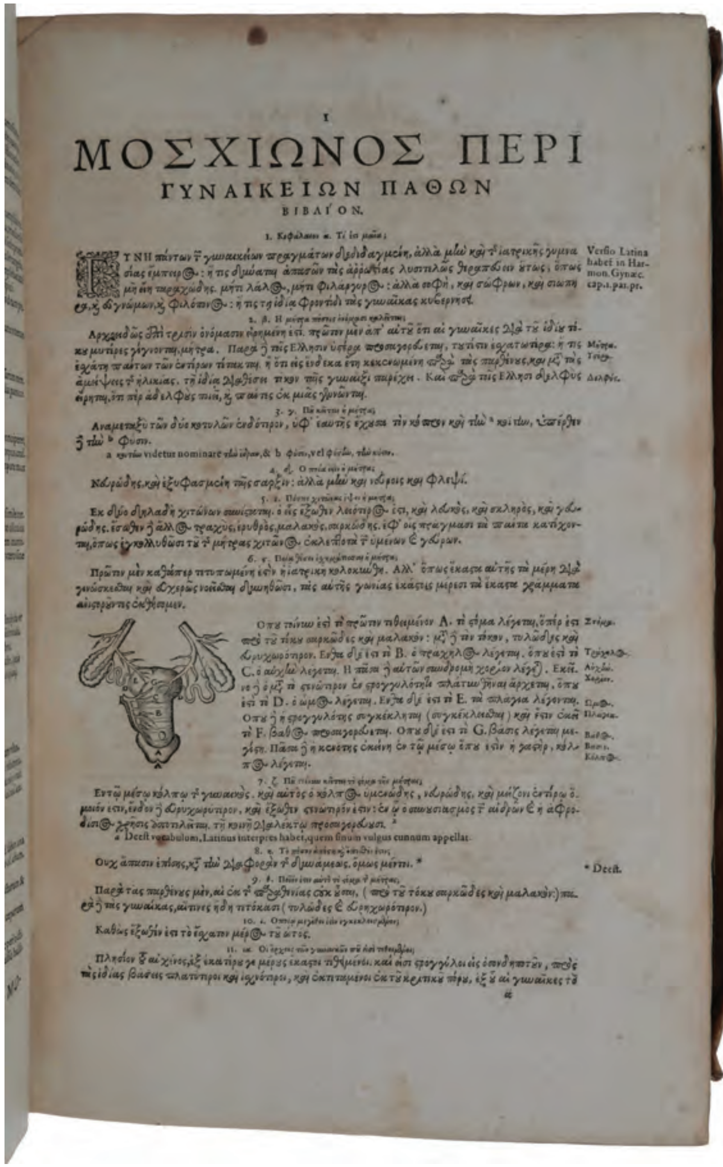
RENAISSANCE ENCYCLOPEDIA OF GYNAECOLOGY

SPACH, Israel. *Gynaeciorum, sive de Mulierum turn Communibus, turn Gravidarum, Parientium, et Puerperarum Affectibus & Morbis, Libri Graecorum, Arabum, Latinorum Veterum et Recentium quotquot extant,...* Strassburg: Lazarus Zetzner, 1597.

$4,000

Folio (330 x 212 mm), pp. [xxx], 28; 1080 [recte 1082], [34], title in red and black, with large woodcut printer's device, and numerous woodcut illustrations in text; title with light water stain to outer margin, light browning, a very nice copy of a work that usually suffers from heavy browning, in contemporary vellum.

First edition under Spach's editorship, and the most complete edition of this celebrated collection of gynaecological writings, incorporating the best that had been written on the subject. "The increased interest in diseases of women during the Renaissance is evidenced by the appearance of what we may call encyclopedias of gynaecology. Conrad Gesner (1516-1565) of Zürich, a physician of remarkable erudition and the author of the famous *Bibliotheca Universalis*, prepared a collection of what were considered the best treatises on diseases of women and proposed to edit them in a single volume. Before the work was finished, Gesner died, but he had designated his friend and successor at the University of Zürich, Caspar Wolff (1532-1601), to complete the task … In 1586, twenty years after the publication of the first *Gynaecia*, a second four-volume edition appeared. Considerably augmented, it was the work of Caspar Bauhin



(1560-1624), professor of anatomy, botany, medicine and Greek at the University of Basle … The third and final edition of the *Gynaecia* appeared eleven years later in 1597, issued in a single volume by Israel Spach of Strassbourg" (Ricci, pp. 254-255). "Spach's compilation of gynecological and obstetrical texts was the largest such collection of its day, reprinting most of the works collected in Caspar Wolff's *Volumen gynaeciorum* (1566) and Gaspard Bauhin's *Gynaeciorum sive de mulierum affectibus* (1586-1588), and adding several other treatises. The collection includes works by Plater (whose anatomy of the female genitalia heads the collection), Moschion, Le Bon, Montanus, Paré and many others. Some of the anatomical woodcuts are taken from Vesalius's *Fabrica*, in particular his 'masculinized' representation of the female reproductive organs; also illustrated are various surgical and gynecological instruments, including a speculum" (Norman). This edition of the *Gynaeciorum* is the first to contain Bauhin's *Libellus variorum historianum* (on Caesarean section) and Martin Akakia's *De morbis muliebribus*, the latter published here for the first time; this edition is also the only one to contain a substantial index. This is a rather clean copy in an attractive contemporary binding – this book was printed on poor quality paper, and most copies are badly browned.

"Spach's massive volume of more than a thousand pages, exclusive of an elaborate index, contains the contributions of twenty-one different authors. Felix Platter (1536-1614), anatomist and physician to the Margrave of Baden, and the earliest systematic nosologist, contributed the first monograph. The major portion is devoted to the anatomy of the female genitalia, including five pictures, all taken from Vesalius. Two pictures present the female torso with the abdominal contents exposed and labelled; the second of these shows the kidneys, uterus, bladder, tubes, ovaries and their blood supply; the remaining pictures portray the separate genital organs, including the vagina. Platter rounded out his text with brief notations on sterility, retention of dead foetus in utero, a case of abdominal pregnancy with an umbilical fistula, super- foetation, fluor, ulcerations of the cervix and bladder, uterine prolapse, etc. He made the erroneous statement that the tubes had a wide communication surrounded by vessels in the center. The historian Kossmann was of the opinion that Platter was referring to the infundibulo-pelvic ligaments. The Moschion text in Greek follows — a summary of the gynaecology of Soranos. Wolff or Gesner added Vesalius' picture of the uterus with the utero-ovarian blood supply.

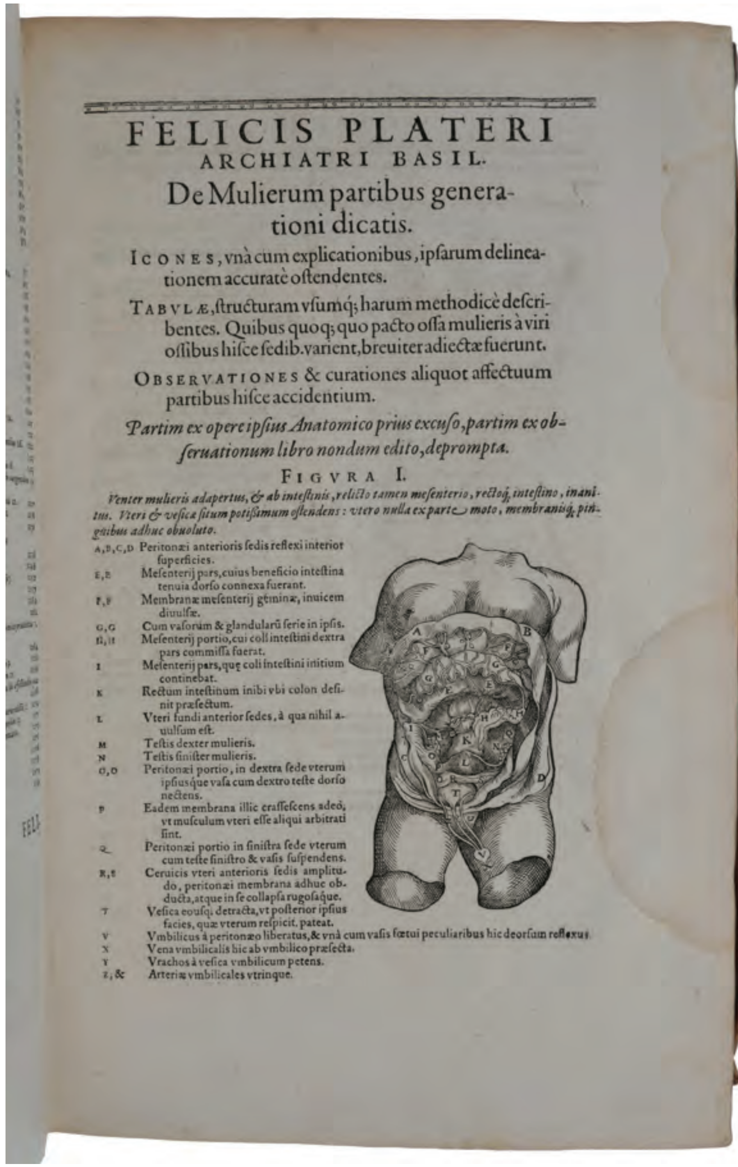
"The *Harmonia Gynaeciorum* was contributed by Caspar Wolff. It is in the form of questions and answers, in which respect he imitated Moschion. Wolff's material is not original but was gathered from Theodorus Priscianus, Moschion and a certain Cleopatra of whom nothing is known. Galen quoted the same Cleopatra on cosmetics without giving his source … Eloy believed that she lived shortly before Christ, and a book entitled *De Morbis Mulierum* has been attributed to her. Indeed, with the exception of the chapters on the anatomy of the genitalia, the bulk of Wolff's work is second century gynaecology. Chapter II deals with virginity; and the author states that 'saluberrima autem est perpetua virginitas' [perpetual virginity is most wholesome]. He gives advice on how to conceal a spacious vaginal orifice — by the application of parsnip juice one hour before coition.

"To test for virginity, a fumigation of sorrel flowers and charcoal is prescribed; the virgin does not grow pale on breathing the fumes. Wolff attributes sterility to both the male and the female; and is the first to mention hypospadias as a possible cause. If a woman becomes dizzy after the use of a vaginal suppository of galbanum, she will bear children. The method of ascertaining whether the husband or the wife is at fault is unique: pour their respective urines in separate earthen vessels containing barley seeds and place the vessels in a cool place for seven days. The absence of sprouting allocates the fault in sterility cases. Wolff enumerates a dozen vaginal suppositories which aid conception. For contraceptive purposes he recommends the use of *nitrum* in the vagina after coitus.

"Another contribution, from the surgical treatise of Albucasis, deals for the most part with obstetrics, save for some brief notations on the treatment for obstruction of the vulval orifice, either of congenital nature or resulting from disease. Trotula's text on gynaecology follows, although Wolff attributed it to a certain Eros, physician to Julia, daughter of Caesar Augustus …

"Nicolaus Rocheus (fl. 1540), a French physician, contributed a lengthy resume from the works of Hippocrates, Aristotle, Pliny, Galen, Aetios, Paulus Aegineta, the Arabians, and from two sixteenth century physicians, Alexander Benedictus (1460-1525) and Leonard Fuchs (1501-1566). The text contains an introduction dated at Paris January 26, 1542, in which he states that the author based the treatise partly on his own experience; but it is entirely lacking in originality, save perhaps in his extensive therapy. The contribution of Luigi Bonacciuoli (d. 1540) deals with conception, miscarriage, delivery, the puerperium, the care of the infant and allied subjects. He was professor of medicine and logic at the University of Ferrara and dedicated his treatise to Lucrezia Borgia, the duchess of Ferrara. The contents of this treatise formed the basis of a series of lectures delivered at the University of Padua. Bonacciuoli's text was later published under the title *Enneas Muliebris*, conjointly with *De Virginitatis Notis* of Severinus Pinaeus (1550-1619). There is not a single item of interest in the gynaecological chapters, except for his belief in astrological influences on female diseases.

"The commentary on menstrual disorders is by Jacobus Sylvius (Jacques Dubois) (1478-1555), professor of anatomy at the University of Paris. It is the only non-obstetrical treatise in the *Gynaecia*. It is indeed an elaborate essay on menstruation with all its irregularities, ending with a few pages on leucorrhoea, prolapse,



suffocation, sterility and conception. The subjects are intelligently discussed, and the author shows a wide range of ancient knowledge. His authorities are Hippocrates, Galen, Aetios and Savonarola. In keeping with ancient gynaecology, Sylvius advises violent coughing spells to remedy an amenorrhoea.

"Rueff's text deals largely with obstetrics, with a few sketchy references to the more common diseases affecting women. Hieronymus Mercurialis (1530-1606) wrote a treatise *De Morbis Muliebribus* (1591) — a lengthy and gossipy discussion consisting of four books. The first deals with sterility and uterine tumors, the second with abortions, the third with the puerperium, and the fourth with diseases of the uterus. Each entity is named and defined, the aetiology is considered, the symptoms are discussed, the prognosis is given and the therapy follows. For example, in Book IV, chapter VIII, we note *De Pruritu Matricis, Causae Signa, Prognostica, et Curatio*. It is maintained that the state of the uterus can best be determined both by touch and by inspection. And for this reason the vaginal speculum is advocated. The author was professor of medicine at Padua, Pisa and Bologna. He was by far a greater dermatologist than a gynaecologist, since he wrote the first book on diseases of the skin (*De Morbis Cutaneis*). He is often referred to as the founder of the milk theory of puerperal fever, since he was the first to state that the retention of milk was the cause of uterine inflammation. This theory was accepted by Horatius Augenius di Monte Santo (1527-1603) in his *Epistolarum et consult ationum medicinalium libri XXIV*. Mercuriale was the first to suggest packing the cervix with a sponge strip to dilate it. He was also the first to refer to the lack of fertility among the learned class.

"Giovanni Battista del Monte (Montanus) (1498-1552) begins his contribution with an elaborate discussion of the menses and leucorrhoea, wherein he attempts to differentiate leucorrhoea from gonorrhoea; and although he vaguely flirts with the idea of a venereal disease, he falls into the usual error. In a certain case of sterility he stated that the husband had gonorrhoea, but drew no conclusions. The treatise ends with ten brief case reports on gynaecological subjects. He lectured at the University of Padua, and was the teacher of the famous John Kaye, the Dr. Gains of the *Merry Wives of Windsor*. Del Monte described a case of vaginal rupture by a midwife, and also reported a case of destruction of the clitoris caused by syphilis.

"Vittore Trincavella (1496-1568) contributed three case reports (*Consilia III*), covering eight pages of the massive volume. The first deals with a woman who was unable to nourish her children; the second, with a patient subject to peculiar manifestation of the menses; and the third, with a patient troubled with white and pinkish discharge and severe pains. Apparently these *Consilia* were lectures delivered at the University of Padua, where Trincavella succeeded Montanus as professor …

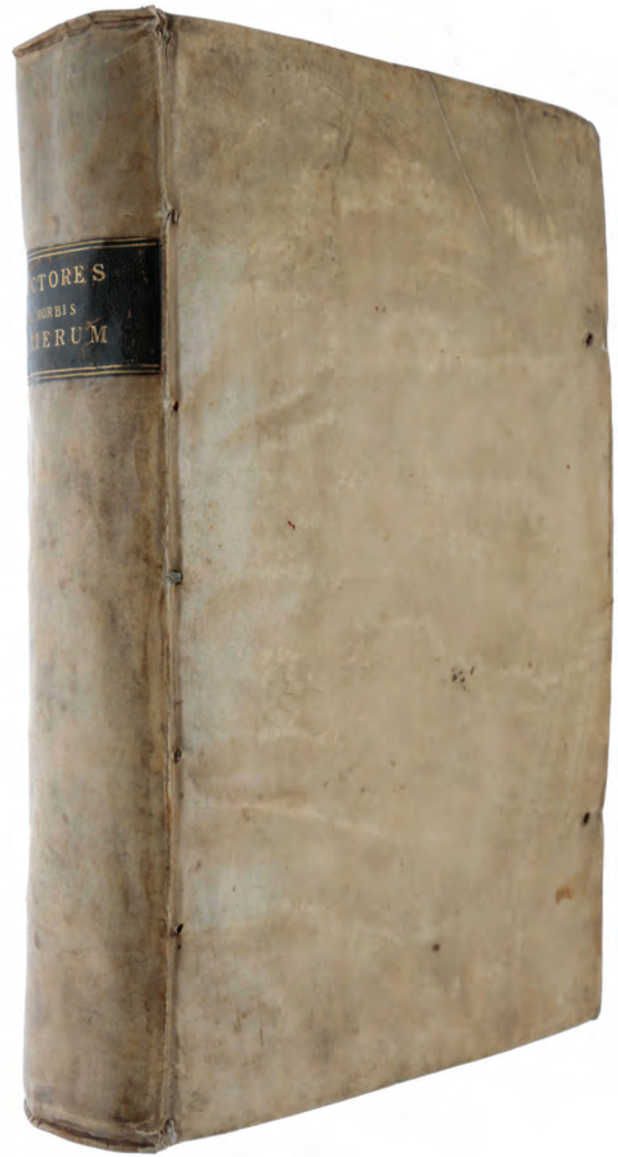
"Alberto Bottoni (?-1596) wrote a lengthy dissertation on diseases of women, including thirty brief chapters on the physiology of menstruation, the irregularities of the menses and the treatment thereof. In accounting for the absence of a menstrual flow in man, the author stated that menstrual blood was produced only in women. The remaining two subjects discussed by this author were gonorrhoea (the flow of the seed) and uterine prolapse. Bottoni was the first physician at the University of Padua to institute bedside teaching. Jean Le Bon (?-1578) includes but one gynaecological reference in his *Therapia Puer per arum* — laceration of the perineum at delivery. A large number of drugs, mostly oils and unguents are prescribed, but no mention whatsoever is made of sutures.

"One of the most interesting treatises in the *Gynaecia* is that of the celebrated French surgeon, Ambroise Paré (1509-1590). It is the only monograph of the Spach edition with pictures of gynaecological instruments, including several of the vaginal speculum. His text also includes the first picture of a submucous fibroid …

“Of interest is Paré’s reference to a third degree laceration and the use of sutures for repair. He stated that sometimes after a forcible delivery the genital parts of the mother were torn asunder, converting two openings into one; in which cases, ‘We should, by means of some stitches, unite the parts unnaturally separated, and treat the wound according to art.’ This suggestion was accepted by his pupil, Guillemeau, who actually repaired a case successfully by using interrupted sutures. Paré recorded the fact that his wife was subject to rectal bleeding at each menstrual period. He described three different types of cervical growths: those resembling mulberry fruit he called *morales*; those resembling grapes, *uvales*, and those like warts, *vernicales*. In discussing ventral hernia he said that a cure could be attained by excision of the peritoneum. This must have been a dangerous procedure in Paré’s time.

“Paré was very ingenious in his gynaecological therapy. He was the first to use oval-shaped pessaries of hammered brass and pessaries made of waxed cork for prolapsed uterus. To keep the vaginal canal open, in order to permit free drainage, Paré designed a special instrument, which was also used to apply aromatic drugs directly to the vaginal parts for the treatment of what he called strangulation of the uterus. The apparatus was made of either gold, silver or brass and was kept in situ by means of a belt around the waist. Paré was also the first surgeon to suggest amputation of the cervix for malignant growths of the parts, but there is no definite proof that he performed the operation. He relates a case where, with the aid of another surgeon, an inverted uterus was cut away, and the patient survived. She died three months later of pleurisy and at autopsy the uterus was not found.

“Francois Rousset’s *Hysterotomotokie* originally appeared in French, after which



Bauhin translated it into Latin, and it is the Latin version which appears in the *Gynaeciorum*. Bauhin also added a supplement, entitled *Appendix varies et novas histories continens*, which includes case reports on so-called Caesarean section, bladder incisions, and retained dead foetus in utero (blighted ovum).

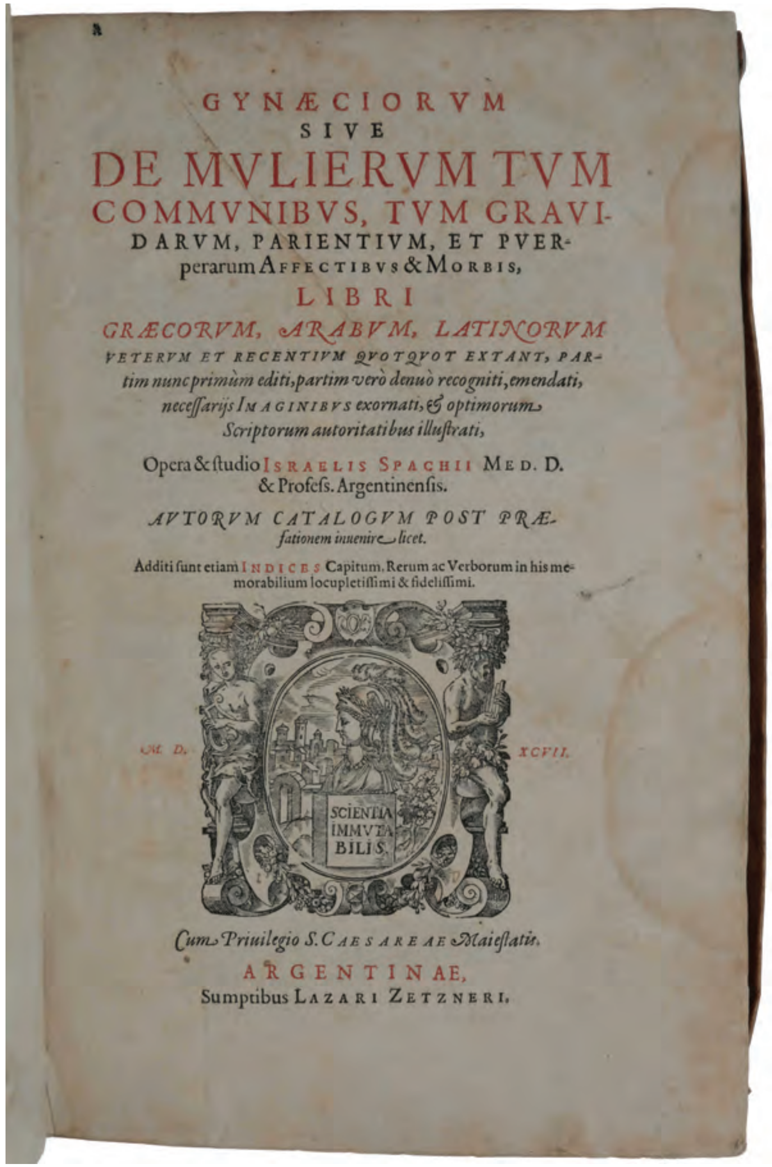
"In Bauhin's supplement there is a brief and illuminating chapter on pessaries, with an illustration. These were made in different shapes – oval or round, some asymmetrical, and some fashioned like a heart. They were held by a cord which was tied to the thigh. Bauhin likewise described a pessary which his mother-in-law, who was well versed in medical subjects, had found useful in prolapse. It was made from the root of a wild grape plant, fashioned like a ball, dipped into a solution of wax, resin and a little turpentine, and shaped to the size desired. Bauhin used the terni pessary in its present connotation. Prior to his time the term was also used to designate vaginal tampons and suppositories. A closing notation states that his brother, John Bauhin, while giving an anatomical demonstration, had observed a bicornate uterus, as is seen in dogs, in a young girl (1565).

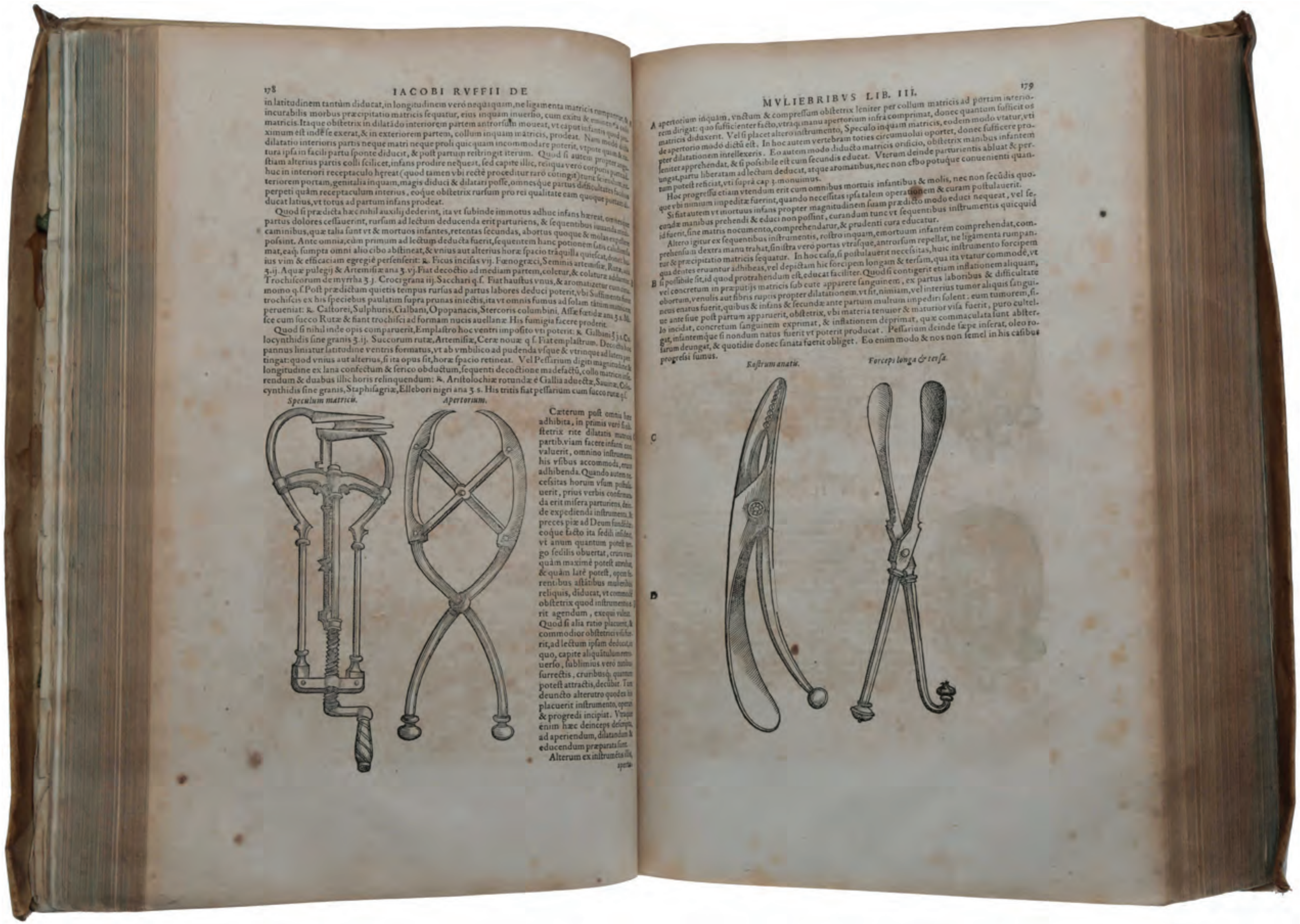
"Mauriceus Cordaeus (fl. 1570), professor of medicine at the University of Paris and an able Greek scholar, translated into Latin and annotated the Greek text *Diseases of Women*. The author, a French Huguenot, got into difficulties with the Catholic authorities and was imprisoned. Cordaeus' text, as it appears in the *Gynaecia* of Spach, contains both the original Greek and his Latin commentaries. In his commentaries we find a remarkable instance of an extra-uterine gestation. The patient did not conceive until she was forty years of age. At term the phenomenon of parturition occurred without delivery. The pains stopped, and she recovered. At the age of sixty-eight she became bed-ridden and died three years later. At autopsy a foetus was removed enclosed in a stony crust in the lower abdomen. Similarly, Cordaeus reported the story of the famous *Lithopedion* of Sens. The story is mentioned by Rousset who added a picture; 'Readers, we herewith include the picture of the *Lithopedion* or petrified embryo of the city of Sens. The details of the case are given in full by Cordaeus, whereas the picture is given here, so that we may not fall short of satisfying you.'

"The *Gynaecia* ends with the bulky contribution *De Communibus Mulierum Affectionibus* of Ludovico Mercado (1520-1606). It is by far the largest single treatise on diseases of women from the Hippocratic age until the publication of Jean Astruc's *Traite des maladies des femmes* in 1761. It contains enough material to fill a book the size of Graves' *Gynaecology*, without a single illustration. But despite its bulk and comprehensiveness, there is only the rare item of originality. He was the first to mention pregnancy and fibroids as a combined entity. Book II of his treatise on diseases of women, entitled 'Maladies of Virgins and Widows,' treats of uterine suffocation, uterine melancholia, uterine epilepsy, and uterine furor. He spoke of *uteri fistulae*, though in reality he was referring to vesico-vaginal lesions (chapter XXV). He described two strange cases of vicarious bleeding: (a) from the canthus of the eye, (b) from the small and ring finger of an otherwise amenorrhoic nun. He spoke of *febris alba* or *morbus virgineus*, which corresponds to chlorosis. He devoted special chapters to diseases of the stomach, head and back due to disturbances of the uterus. He tried to identify *hydrops uteri* of the ancients with hydatid mole. Scirrhus and cancer of the uterus were due to disorders of menstrual blood. Among the diseases of the external genitals he spoke of ulcerations due to syphilis. He left the treatment of sterility caused by sorcerers to prayers and priests. Mercado was physician to Emperor Philip II of Spain and became one of the most famous practitioners of the sixteenth century" (Ricci, pp. 255-261).

The editor of the present work, Israel Spach (1560-1610), studied medicine at Paris under Jean Riolan the Elder, one of the most fervent defenders of Hippocrates against the attacks of the chemists of that time. After obtaining a doctorate in medicine at Tübingen in 1581, Spach returned to Strasbourg to practise medicine. In 1589 he took over the teaching of physics and Hebrew, as well as medicine, at the Strasbourg Academy (predecessor of the University of Strasbourg). Apart from the present work, Spach's most important publication is the *Nomenclator scriptorum philosophicorum atque philologicorum* (Strasbourg, 1598). Covering the works of over 4,000 authors arranged under 400 subject headings, including esoteric subjects like gladiatorial combat, glory, and sobriety, with an emphasis on contemporary writers, this was the most significant subject bibliography of the sixteenth century.

Adams S1517; Cutter and Viets, pp. 29-30; Durling 2254; Garrison and Morton 6013; Norman 1977; Waller 9096; Wellcome 6030. Ricci, *The Genealogy of Gynaecology. History of the Development of Gynaecology throughout the Ages 2000 BC – 1800 AD* (2nd edition), 1950.

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Speculum matricis.
Apertorium.
Rostrum anatis.
Forceps longa & tersa.

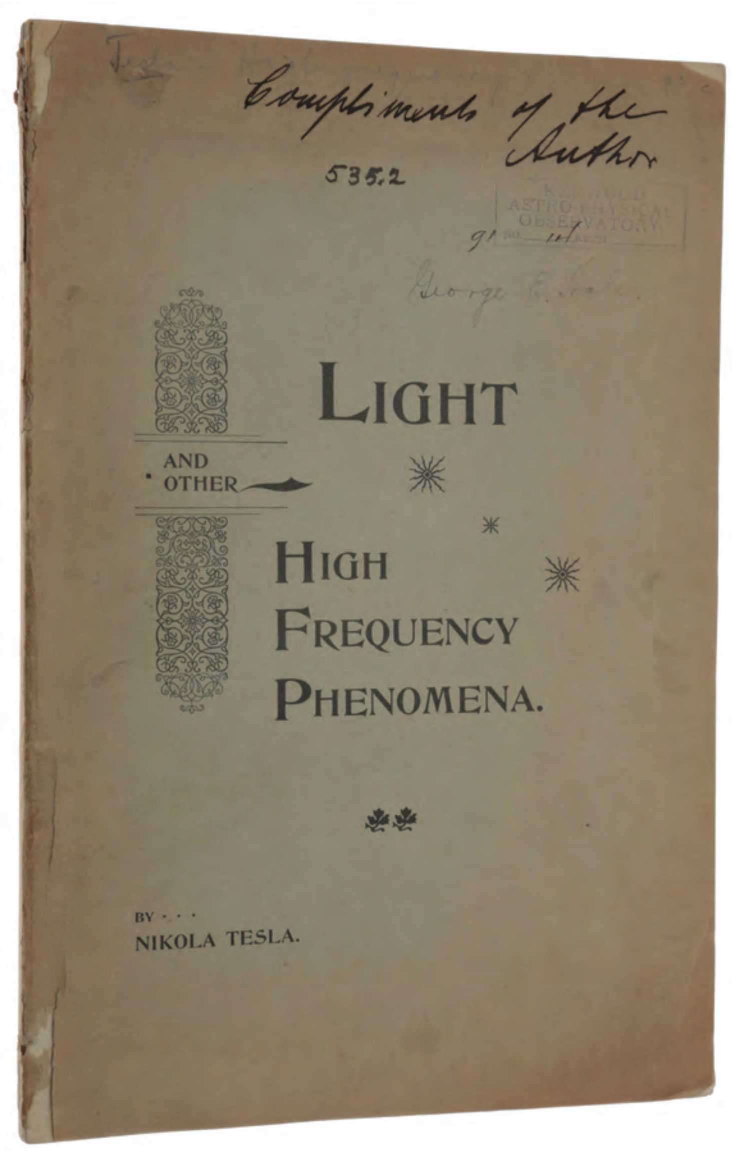
INSCRIBED OFFPRINT OF TESLA'S FAMOUS LECTURE

TESLA, Nikola. *Light and other High Frequency Phenomena. A lecture delivered before the National Electric Light Association at its Sixteenth Convention held at St. Louis, MO, February 28th, March 1st and 2d, 1893.* [New York: James Kempster Printing Company for the National Electric Light Association, 1893].

$38,500

Large 8vo (240 x 160 mm), pp. 114, with text illustrations throughout. Original printed wrappers, spine with some wear, front wrapper starting to seperate, preserved in a clamshell box.

First edition, extremely rare offprint, **inscribed by Tesla to the great American astronomer George Ellery Hale**, of this famous lecture. "At St. Louis [Tesla] made the first public demonstration ever of radio communication, although Marconi is generally credited with having achieved this feat in 1895" (Cheney, p. 68). "What Tesla described in this lecture should be taken to be the foundation of radio engineering" (Sarkar, p. 271). By virtue of this 1893 lecture, Tesla was recognized by the Institute of Electrical and Electronics Engineers as discovering radio: "In a lecture-demonstration given in St. Louis in the same year – two years before Marconi's first experiments – Tesla also predicted wireless communication; the apparatus that he employed contained all the elements of spark and continuous wave that were incorporated into radio" (Pratt, p. 1107). Tesla also anticipated in this lecture the 1902 discovery of the ionosphere by Heaviside and its use for radio propagation (Seifer, p. 105). "He was an inventor, an engineer, a scientist



and an oddball … more than any one man, Nikola Tesla is responsible for the twentieth century" (Hunt, introduction to *Nikola Tesla*: *My Inventions and Other Writings* (2011)). In his speech presenting Tesla with the Edison medal in 1917, B. A. Behrend, Vice President of the American Institute of Electrical Engineers, stated: "Were we to seize and eliminate from our industrial world the result of Mr. Tesla's work, the wheels of industry would cease to turn, our electric cars and trains would stop, our towns would be dark and our mills would be idle and dead. His name marks an epoch in the advance of electrical science." The offered work is a separately-paginated offprint from the *Proceedings of the National Electric Light Association*, 1893 (journal pagination 191-302). It was reprinted (in parts) in May-June of the same year in *Electrical Engineer* (New York), *Electrical Engineer* (London), *Electrical Review*, *The Electrician*, and *Electricity*, in July-December in the *Journal of the Franklin Institute*, and in the following year as Chapter 28 in *The Inventions, Writings and Researches of Nikola Tesla*, as well as in numerous later publications (in several languages). We know of no other inscribed book or pamphlet of Tesla's having appeared in commerce, nor of any other copy of this offprint. OCLC lists copies at the American Philosophical Society, Huntington, Library of Congress, Linda Hall, New York Public Library, and New York University (no listings outside US).

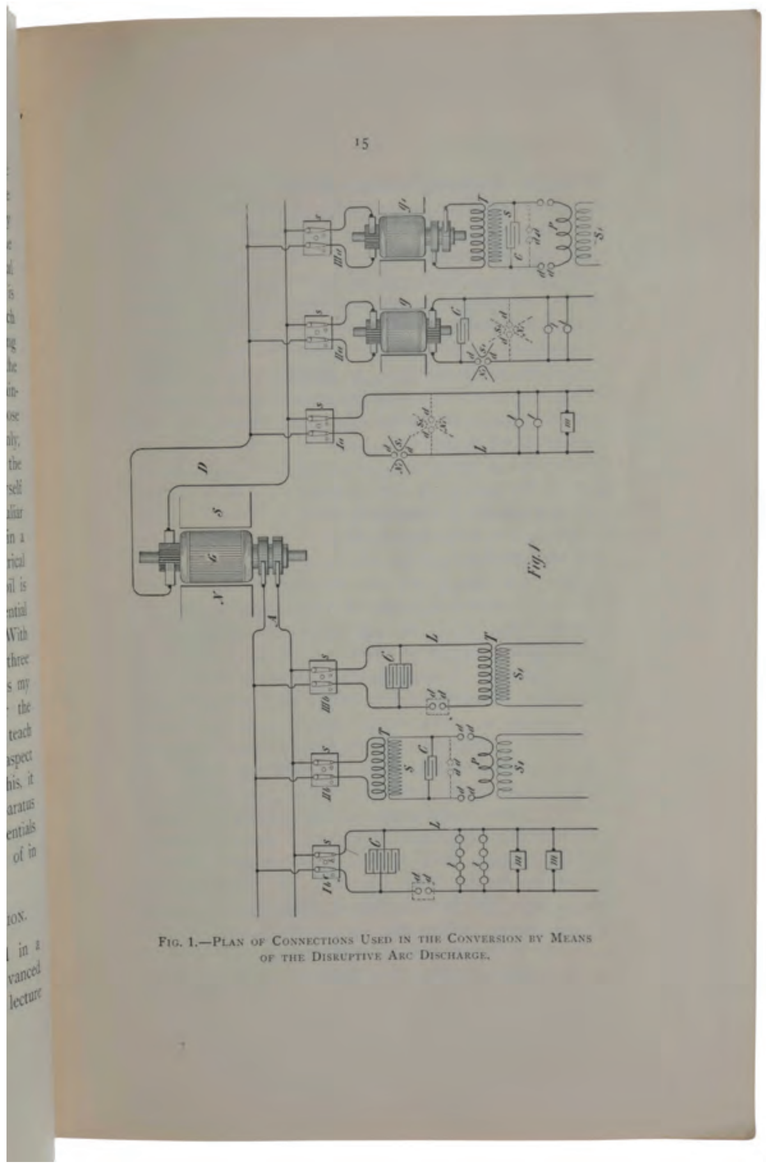
Provenance: Inscribed on front wrapper 'Compliments of the Author' in Tesla's hand to George Ellery Hale (Hale's signature and ink stamp of the Kenwood Astrophysical Observatory in Chicago, of which Hale was director, also on front wrapper). Tesla and Hale met on at least one occasion. In a letter to Hale of June 4, 1908, Tesla wrote: "I have greatly regretted that since our meeting at Chicago years ago, we have never been able to get again together. Your work interests me very much, and I am heartily in sympathy with you" (hale.archives.caltech.edu/islandora/object/hale:19006#page/1/mode/1up). It is surely probable that this meeting was on the occasion of the World's Fair, held May-October 1893 in Chicago – Tesla also gave a demonstration of his wireless experiments at the Fair and Hale had recently been appointed director of the Kenwood Observatory and professor of astrophysics at the newly founded University of Chicago – and the date of the Exposition suggests that Tesla could have presented this offprint to Hale at that meeting.

"Nikola Tesla was born in Smiljan, Lika [now Croatia] in 1856 as the fourth child of Milutin and Djuka Tesla. His father was a well-educated priest of the Serbian Orthodox Church. Nikola's mother was also intelligent and talented and he often said that his mother influenced his life as an inventor. His technical education was limited to two years polytechnic studies at Gratz, Styria [now Austria], where he devoted himself to mathematics, physics and mechanical engineering. From Gratz he went to Prague with the object of completing his scientific education and on philosophical studies at the University. From Prague he went to Budapest to work in a new telephone company. It was there that in 1882 he invented his induction motor and an alternating-current system of power transmission. Seeking better opportunities to find people who were interested in his invention, he accepted a position of electrical engineer for a French company in Paris, where he remained for two years. Another important step in his life was acceptance of the position of designer to build direct current dynamos and motors for the Edison Company in New York, where he arrived in 1884 in hope of finding the "the land of golden promise". Edison was not interested in Tesla's alternating currents system and Tesla soon left Edison, after a bitter struggle. In 1885, the Tesla Electric Light Company was formed but Tesla had to work on electric arc light. It was not until he formed the new Tesla Electric Company [that he was able] to realize his inventions and develop working models of motors, generators and transformers. During the years 1887 and 1888 Tesla applied and was granted more than 30 patents for his inventions. In 1888, the American Institute of Electrical Engineers invited Tesla to give a lecture on his work on the alternating-current system. After that lecture Tesla became famous. In 1889 George Westinghouse approached Tesla and soon they completed an agreement for transferring exclusive license of Tesla's polyphase current patents to the Westinghouse Company … At the beginning of 1892, [Tesla] visited London and Paris talking about his future experiments with alternating currents of high potential and high frequency. He disclosed his new achievements in obtaining better operation of his high frequency spark generator by producing rapid succession of sparks, either by employment of a magnet, simple or multiple air gaps or by various designs of mechanical interrupters. Many of these inventions were later 'reinvented' by others without referring to Tesla" (Sarkar, pp. 267-269).

"But before he could get too far along with new experiments, Tesla agreed to lecture again, first before the Franklin Institute [in Philadelphia] on 25 February 1893 and again the following week at the National Electric Light Association in St. Louis. In this lecture, Tesla followed a strategy similar to the one he employed in his performances in London and Paris, offering American audiences both his philosophical musings on the relationship between electricity and light along with sensational demonstrations.

"In St. Louis Tesla lectured at the Exhibition Theatre, which seated four thousand, but the hall was packed to suffocation as another several thousand people crowded in, most of whom came to see Tesla's spectacular demonstrations. The demand for seats was so great that tickets were being scalped outside the hall for three to five dollars.

"Tesla did not disappoint this huge crowd. In his first demonstration he allowed 200,000 volts to pass through his body; as he described in the published lecture: 'I now set the coil to work and approach the free terminal with a metallic object [most likely a ball] held in my hand, this simply to avoid burns. As I approach the metallic object to a distance of eight or ten inches, a torrent of furious sparks breaks forth from the end of the secondary wire, which passes through the rubber



columns. The sparks cease when the metal in my hand touches the wire. My arm is now traversed by a powerful electric current, vibrating at about the rate of one million times a second. All around me the electrostatic force makes itself felt, and the air molecules and particles of dust flying about are acted upon and are hammering violently against my body. So great is this agitation of particles, that when the lights are turned out you may see streams of feeble light appear on some parts of my body. When such a streamer breaks out on any part of the body, it produces a sensation like the pricking of a needle. Were the potentials sufficiently high and the frequency of vibration rather low, the skin would probably be ruptured under the tremendous strain, and the blood would rush out with great force in the form of fine spray or jet so thin as to be invisible … I can make these streams of light visible to all, by touching with the metallic object one of the terminals as before, and approaching my free hand to the brass sphere [connected to the coil's other terminal] … [T]he air … is more violently agitated, and you see streams of light now break forth from my fingertips and from the whole hand … The streamers offer no particular inconvenience, except that in the ends of the finger tips a burning sensation is felt' [the offered work, pp. 36-37].

"In the rest of the lecture, Tesla reviewed systematically the different means by which electricity could produce light using effects based on electrostatics, impedance, resonance, and high frequencies. Waving differently shaped tubes in the strong electromagnetic field created by his oscillating transformer, Tesla produced 'wonderfully beautiful effects … the light of the whirled tube being made to look like the white spokes of a wheel of glowing moonbeams.' Near the end of the performance, Tesla held up in his hand one of the phosphorescent bulbs and announced that he would illuminate this lamp by touching his other hand to his oscillating transformer. When this lamp burst into light, recalled Tesla, the audience was so startled that 'there was a stampede in the two upper galleries and they all rushed out. They thought it was some part of the devil's work, and ran out. That was the way my experiments were received' …

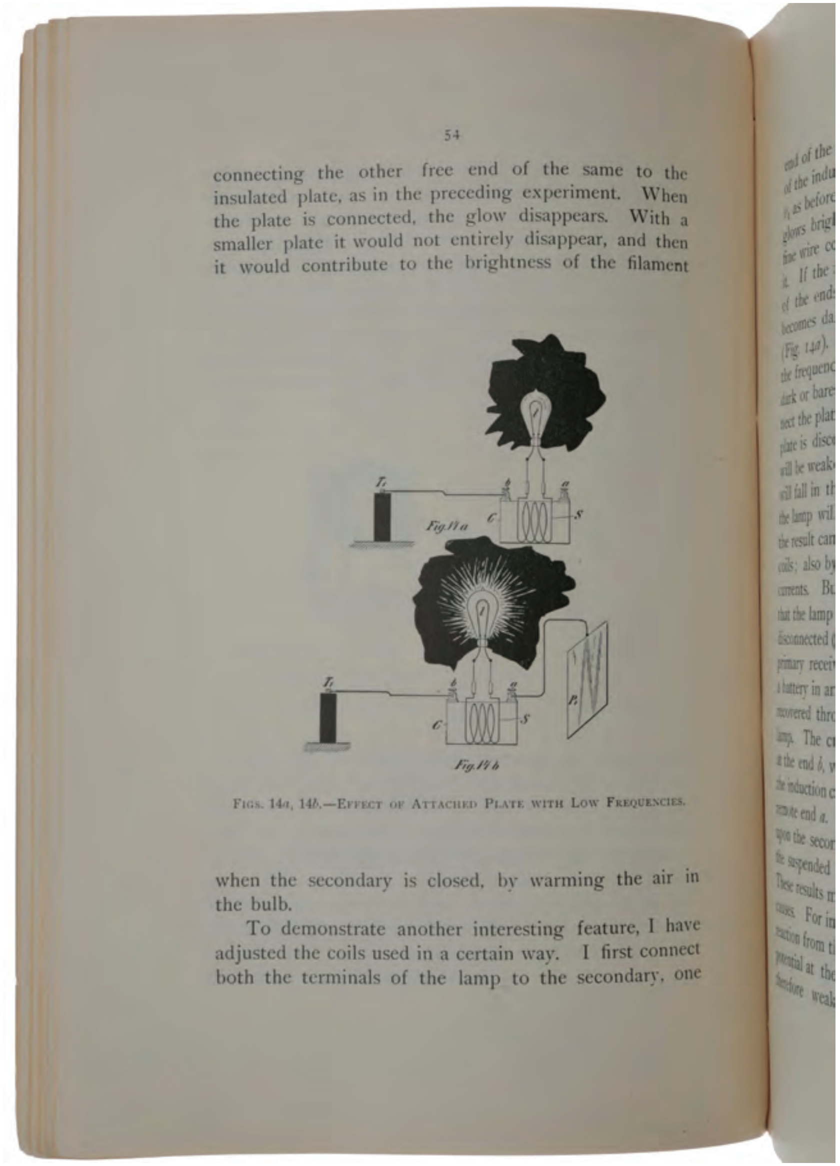
"Although Tesla's 1893 lecture covered many of the same topics as his previous lectures, what was new was that Tesla outlined for the first time his hopes for wireless transmission" (Carlson, pp. 176-178).

Tesla began his discussion of wireless transmission with a prescient prediction of the necessity of developing renewable energy sources. "Looking at the world around him, Tesla realized that it was a finite place and that the natural resources which gave humans the fuel to produce electricity would eventually run out. 'What will man do when the forests disappear, when the coal deposits are exhausted?' he asked … 'Only one thing, according to our present knowledge, will remain: that is to transmit power at great distances. Man will go to the waterfalls, [and] to the tides' [p. 13]. Tesla speculated that these, unlike coal and oil reserves, are replenishable.

"Having set up the premise that it could be possible to derive inexhaustible amounts of energy with properly constructed equipment, that is, 'to attach our engines to the wheelwork of the universe,' Tesla described, for the first time ever, his invention of wireless transmission. Cloaking his true goals in more palatable language, he announced, 'I … firmly believe that it is practicable to disturb by means of powerful machines the electrostatic conditions of the earth and thus transmit intelligible signals and perhaps power.' Taking into consideration the speed of electrical impulses, with this new technology, 'all … ideas of distance must … vanish,' as humans will be instantaneously interconnected. 'First we must know what capacity the earth is, and what charge it contains.' Tesla also speculated that the earth was 'probably a charged body insulated in space' and thus has a 'low capacity.' The upper strata, much like the vacuum created in his Geissler tubes, would probably be an excellent medium for transmitting impulses [p. 76]. We see here the precursor to the discovery by Heaviside and Kennelly of the ionosphere …

"With pure resonance, Tesla suggested, wires become unnecessary since impulses can be 'jumped' from sending device to receiver. Naturally the receiving instruments would have to be tuned to the frequency of the transmitter. 'If ever we can ascertain at what period the earth's charge, when disturbed [or] oscillates with respect to an oppositely electrified system or known circuit, we shall know a fact possible of the greatest importance to the welfare of the human race' [p. 76]" (Seifer, pp. 105-107).

Tesla's demonstration of wireless transmission in the St. Louis lecture was described in 1976 by William Broughton, as told to him by his father H. P. Broughton, who had been Tesla's assistant at the lecture. "On the auditorium stage a demonstration was set up by using two groups of equipment. In the transmitter group on one side of the stage was a 5-kva high-voltage pole-type oil-filled distribution transformer connected to a condenser bank of Leyden jars, a spark gap, a coil, and a wire running up to the ceiling. In the receiver group at the other side of the stage was an identical wire hanging from the ceiling, a duplicate condenser bank of Leyden jars and coil – but instead of the spark gap, there was a Geissler tube that would light up like a modern fluorescent lamp bulb when voltage was applied. There were no interconnecting wires between transmitter and receiver. The transformer in the transmitter group was energized from a special electric power line through an exposed two-blade knife switch. When this switch was closed, the transformer grunted and groaned, the Leyden jars showed corona sizzling around their foil edges, the spark gap crackled with a noisy spark discharge, and an invisible electromagnetic field radiated energy into space from the transmitter antenna wire. Simultaneously, in the receiver group, the Geissler tube lighted up from radio-frequency excitation picked up by the receiver antenna wire. *Thus wireless was born*. A wireless message had been transmitted by the 5-kilowatt spark



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connecting the other free end of the same to the insulated plate, as in the preceding experiment. When the plate is connected, the glow disappears. With a smaller plate it would not entirely disappear, and then it would contribute to the brightness of the filament

Figs. 14a, 14b.—Effect of Attached Plate with Low Frequencies.

when the secondary is closed, by warming the air in the bulb.

To demonstrate another interesting feature, I have adjusted the coils used in a certain way. I first connect both the terminals of the lamp to the secondary, one transmitter, and instantly received by the Geissler tube receiver thirty feet away" (Cheney, p. 68). Tesla's account of this demonstration in the published lecture is illustrated with a diagram (Fig. 21) showing how to set up the aerials, receivers, transmitter, and ground connection.

"Although the St. Louis demonstration was no 'message sent round the world' as Tesla would doubtless of course have preferred it to be, he had nevertheless demonstrated all the fundamental principles of modern radio: 1. An antenna or aerial wire; 2. A ground connection; 3. An aerial-ground circuit containing inductance and capacity; 4. Adjustable inductance and capacity (for tuning); 5. Sending and receiving sets tuned to resonance with each other; and 6. Electronic tube detectors" (Cheney, p. 69).

Some of Tesla's contemporaries found the idea of wireless transmission too speculative and worried that what they saw as his far-fetched claims might discourage investors in Tesla's polyphase AC system. "In Tesla's autobiography, written a quarter of a century later, the inventor informs the reader that there was such opposition to his discussion of wireless telegraphy at that time that 'only a small part of what I had intended to say was embodied [in the lecture] … This little salvage from the wreck has earned me the title 'Father of the Wireless'.' Tesla stated that it was Joseph Wetzler [editor of *Electrical Engineer*] who told him to deemphasize his work in wireless in this lecture. Wetzler probably edited out a number of key passages which, in the long run, could have helped Tesla establish more easily his priorities in the field" (Seifer, p. 107).

"In developing his 1893 system, 'using a single or no wire for electrical energy transmission', Tesla was slowed down because of many other side activities, such as the inauguration of the polyphase system built by George Westinghouse at the World Fair in Chicago, which opened to the public from May 1, 1893 till October 30, 1893. At this fair, Tesla had his own stand showing his inventions in the area of low and high frequency currents … On March 13, 1895 Tesla's laboratory in South Fifth Avenue [New York] was burned, and that stopped his research in the field of high frequency currents for some time" (Sarkar, p. 272). It was not until 1897 that Tesla filed his first patents for wireless transmission.

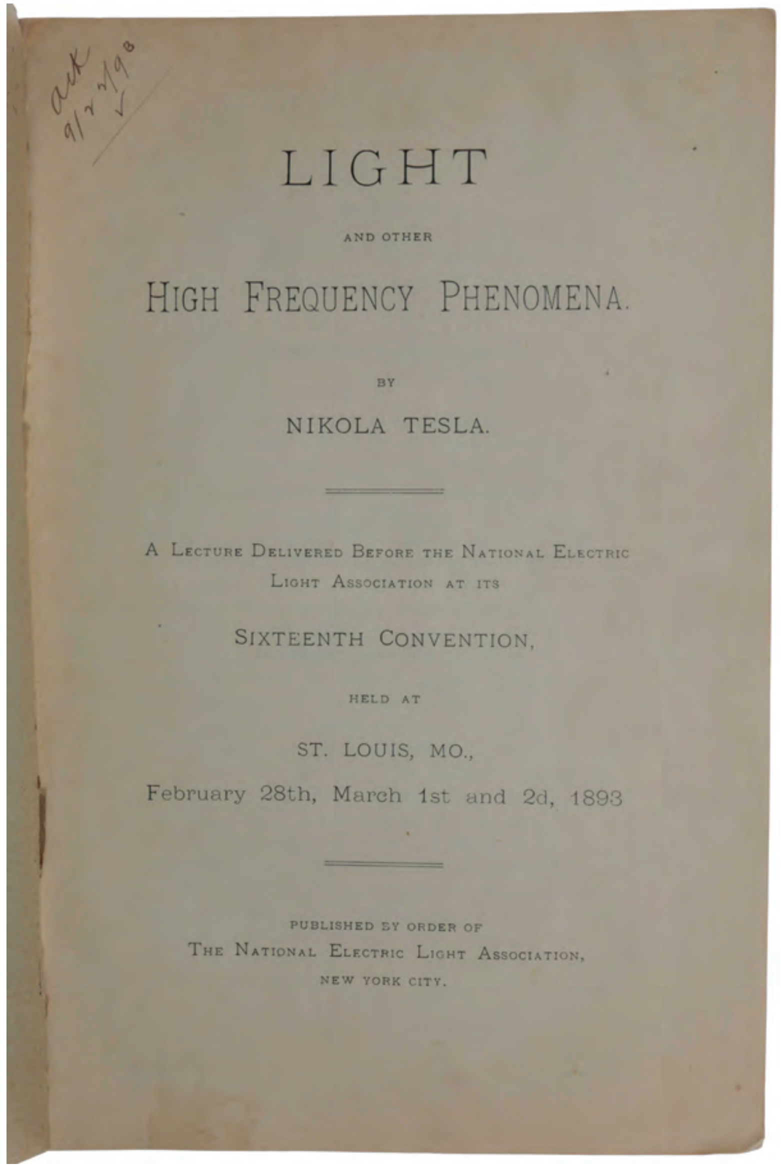
"The scientist who, next to Tesla, most deserved credit for pioneering radio was Sir Oliver Lodge, for in 1894 he demonstrated the possibility of transmitting telegraph signals wirelessly by Hertzian waves a distance of 150 yards. Two years later, young Marchese Guglielmo Marconi arrived in London with a wireless set identical to Lodge's … He [had] a ground connection and antenna or aerial wire with which he had made crude experiments in Bologna. As it happened, this equipment was exactly what Tesla had described in his widely published lecture in 1893, which had been translated into many languages. Later … Marconi was to deny that he had ever read of Tesla's system, and the US Patent Examiner was to brand his denial patently absurd" (Cheney, p. 69).

"On September 2, 1897 Tesla filed the patent application No. 650,343, subsequently granted as patent No. 645,576 of March 20, 1900 and patent No. 649,621 of May 15, 1900. The two patents by which Tesla protected his system and apparatus for wireless transmission are known as 'system of four tuned circuits'. This fact is particularly important in the history of radio. They were the subject of a long lawsuit brought by the Marconi Wireless Telegraph Company of America against the United States of America, alleging that they have used wireless devices that infringed on Marconi patent No. 763,772 of June 28, 1904 … After 25 years, the United States Supreme Court on June 21, 1943 invalidated the fundamental American radio patent of Marconi No. 763,772 … The Supreme Court cited Tesla's system in its deliberations:

"*The Tesla patent No. 645,576, applied for September 2, 1897 and allowed March 20, 1900, disclosed a four circuit system, having two circuits each at transmitter and receiver, and recommended that all four circuits be tuned to the same frequency. Tesla's apparatus was devised primarily for transmission of energy of any form of energy-consuming device by using the rarefied atmosphere at high elevations as a conductor when subjected to the electrical pressure of a very high voltage. But he also recognized that his apparatus could, without change, be used for wireless communication, which is dependent upon the transmission of electrical energy. His specifications declare:* 'The apparatus which I have shown will obviously have many other valuable uses – as, for instance, when it is desired to transmit intelligible messages to great distances …'" (Sarkar, pp. 274-275).

"Having become obsessed with the wireless transmission of energy, around 1900 Nikola set to work on his boldest project yet: to build a wireless global communication system – to be transmitted through a large electrical tower – for sharing information and providing free electricity throughout the world. With funding from a group of investors that included financial giant J. P. Morgan, in 1901 Tesla began work on the project in earnest, designing and building a lab with a power plant and a massive transmission tower on Long Island, New York, that became known as Wardenclyffe. However, doubts arose among his investors about the plausibility of Tesla's system. As his rival, Guglielmo Marconi – with the financial support of Andrew Carnegie and Thomas Edison – continued to make great advances with his own radio technologies, Tesla had no choice but to abandon the project … Two years later Tesla declared bankruptcy … Poor and reclusive, Nikola Tesla died on January 7, 1943, at the age of 86, in New York City, where he had lived for nearly 60 years" (Hunt).

Tesla was the first to publish the idea of radio communication, but it was Marconi who successfully commercialized it and was rewarded with the Nobel Prize



LIGHT

AND OTHER

HIGH FREQUENCY PHENOMENA.

BY

NIKOLA TESLA.

A LECTURE DELIVERED BEFORE THE NATIONAL ELECTRIC LIGHT ASSOCIATION AT ITS

SIXTEENTH CONVENTION,

HELD AT

ST. LOUIS, MO.,

February 28th, March 1st and 2d, 1893

PUBLISHED BY ORDER OF

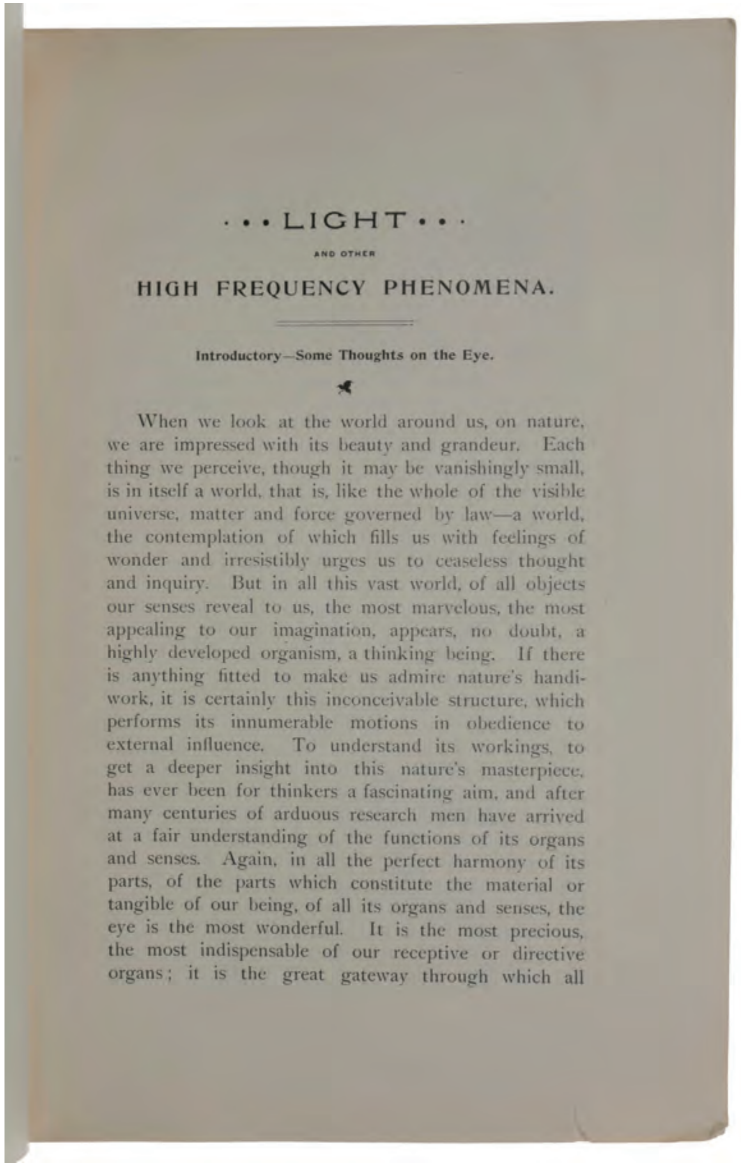
THE NATIONAL ELECTRIC LIGHT ASSOCIATION,

NEW YORK CITY.

in Physics 1909, shared with Karl Ferdinand Braun "in recognition of their contributions to the development of wireless telegraphy." A persistent rumour is that Edison and Tesla were to be jointly awarded the 1915 Prize but that each refused to share it with the other and so neither received it (the 1915 Prize was actually awarded to the Braggs, father and son).

The recipient of this offprint, George Ellery Hale (1868-1938), was an American astronomer, best known for his discovery of magnetic fields in sunspots, and as the key figure in the planning or construction of several world-leading telescopes: namely, the 40-inch refracting telescope at Yerkes Observatory, the 60-inch Hale reflecting telescope at Mount Wilson Observatory, the 100-inch Hooker reflecting telescope at Mount Wilson, and the 200-inch Hale reflecting telescope at Palomar Observatory. In 1890, he was appointed director of the Kenwood Astrophysical Observatory in Chicago, founded by his father William E. Hale. He was professor of astrophysics at the University of Chicago (1892-1905). He also played a key role in developing the California Institute of Technology into a leading research university. He was coeditor of *Astronomy and Astrophysics*, 1892-95, and after 1895 editor of the *Astrophysical Journal*. In October 1913, Hale received a letter from Albert Einstein, asking whether certain astronomical observations could be made that would test Einstein's hypothesis concerning the effects of gravity on light. Hale replied in November, saying that such observations could be made only during a total eclipse of the sun. In 1919 F.W. Dyson and A.S. Eddington made observations during a total solar eclipse which verified Einstein's hypothesis.

Carlson, *Tesla. Inventor of the Electrical Age*, 2013. Cheney, *Tesla. Man Out of Time*, 2001. Pratt, '*Nikola Tesla 1856-1943*',*Proceedings of the IRE*, vol. 44 (1956), pp. 1106-1108. Sarkar *et al*, *History of Wireless*, 2006. Seifer, *Wizard. The Life and Times of Nikola Tesla. Biography of a Genius*, 1998.



. . . LIGHT . . .

AND OTHER

HIGH FREQUENCY PHENOMENA.

Introductory—Some Thoughts on the Eye.

When we look at the world around us, on nature, we are impressed with its beauty and grandeur. Each thing we perceive, though it may be vanishingly small, is in itself a world, that is, like the whole of the visible universe, matter and force governed by law—a world, the contemplation of which fills us with feelings of wonder and irresistibly urges us to ceaseless thought and inquiry. But in all this vast world, of all objects our senses reveal to us, the most marvelous, the most appealing to our imagination, appears, no doubt, a highly developed organism, a thinking being. If there is anything fitted to make us admire nature's handiwork, it is certainly this inconceivable structure, which performs its innumerable motions in obedience to external influence. To understand its workings, to get a deeper insight into this nature's masterpiece, has ever been for thinkers a fascinating aim, and after many centuries of arduous research men have arrived at a fair understanding of the functions of its organs and senses. Again, in all the perfect harmony of its parts, of the parts which constitute the material or tangible of our being, of all its organs and senses, the eye is the most wonderful. It is the most precious, the most indispensable of our receptive or directive organs; it is the great gateway through which all

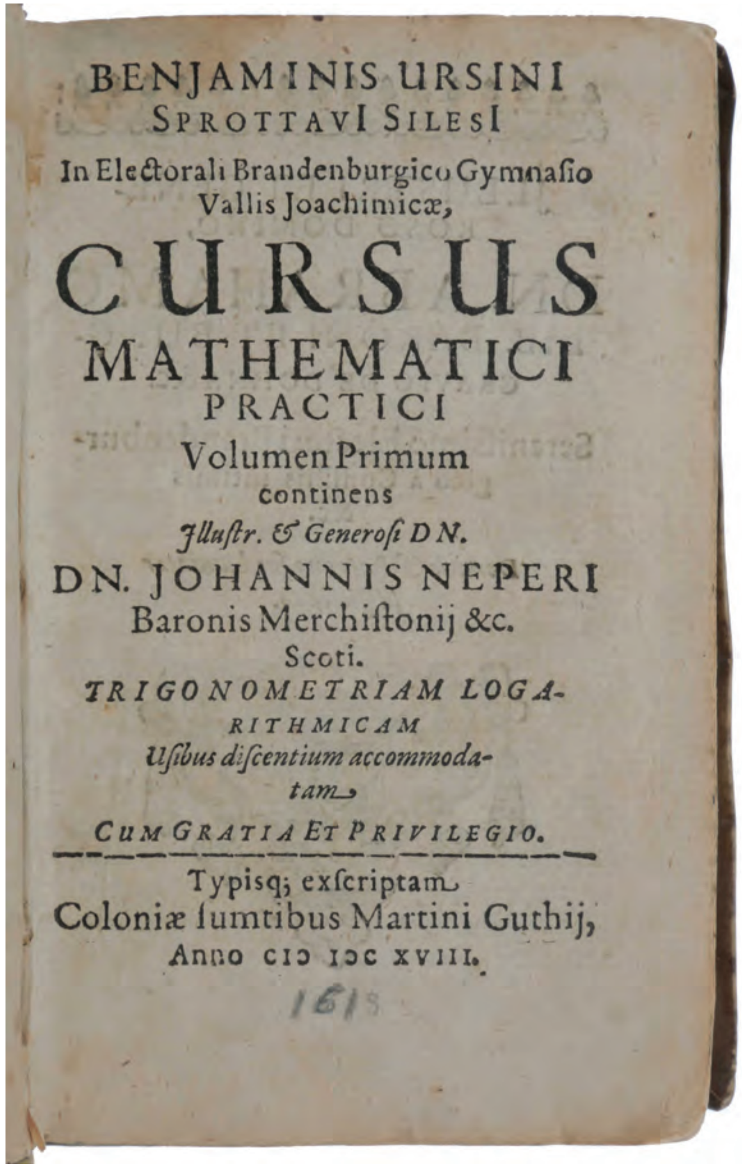
THE FATHER OF KEPLER'S RUDOLPHINE TABLES

URSINUS, Benjamin. *Cursus Mathematici Practici Volumen Primum [all published]: continens Illustr. & Generosi Dn. Dn. Johannis Neperi Baronis Merchistonii &c. Scoti. Trigonometriam Logarithmicam.* Cölln an der Spree: Martin Guthius, 1618.

$27,500

8vo (158 x 97 mm), pp. [164], with geometric figures and tables in text (light dampstain in upper inner margin at the beginning of the volume). Contemporary German yapped vellum over boards, blue edges (soiled, extremities worn, front hinge cracked but firm, armorial bookplate on front paste-down, old shelf-mark pasted onto front free endpaper, later ownership signature on front paste-down and blind-stamp on front free endpaper), internally very good. A very good and genuine copy in original condition.

First edition, exceptionally rare, of the book that introduced logarithms to Continental Europe; in particular, it was through this work that Johannes Kepler, in 'a happy calamity,' as he called it, became aware of Napier's epoch-making work, a discovery that enabled him to complete his great *Rudolphine Tables* (1627), "the foundation of all planetary calculations for over a century" (Sparrow). "The earliest publication of Napier's logarithms on the Continent was in 1618, when Benjamin Ursinus included an excerpt from the canon, shortened by two places, in his *Cursus mathematici practici*. Through this work Kepler became aware of the importance of Napier's discovery and expressed his enthusiasm in a letter



to Napier dated 28 July 1619, printed in the dedication of his *Ephemerides* (1620)" (DSB, under Napier). Ursinus assisted Kepler with the computations for the *Rudolphine Tables*, and Kepler presented and inscribed a copy to Ursinus (Honeyman 1800 – this copy is now held by the Adler Planetarium in Chicago); in the inscription, Kepler calls Ursinus and Tycho Brahe the scientific fathers of the tables. "The [*Rudolphine*] *Tables* was far more accurate than its predecessors – its margin of error staying within 10 seconds compared to up to 5 degrees with earlier tables. Instead of providing a sequence of planetary positions for specified days (which Kepler did in his *Ephemerides*), the *Rudolphine Tables* were set up to allow calculations of planetary positions for any time in the past or future. The finding of the longitude of a given planet at a given time was based on Kepler's equation and he exploited logarithms for this tabulation. The precise geocentric positions had to be worked out from combining the heliocentric positions of the planets and the earth that were calculated separately. Logarithmic tabulations were used again to facilitate calculation" (www.sites.hps.cam.ac.uk/starry/keplertables.html). Ursinus was for several years Kepler's assistant: in Prague, he made observations with Kepler of the newly discovered satellites of Jupiter, published in his *Narratio* (1611), and later, after Kepler had moved to Linz, lived there in Kepler's house for a year (1613/1614). A second issue of the *Cursus Mathematici Practici* was published in 1619 (same place and publisher). OCLC lists four copies (British Library, Chicago, Columbia, Göttingen); KVK adds no further copies outside Germany. As far as we can determine ours is the only copy of the first issue to have appeared in commerce; the Macclesfield copy of the second issue (Sotheby's, October 26, 2005, lot 2027), in an 18th century binding, realised £9600 ($16942).

Provenance: Patrick Hume, 1st Earl of Marchmont (engraved armorial bookplate on front paste-down). Sir Patrick Hume (1641-1724) was a Scottish Presbyterian statesman and a supporter of William of Orange. He began his long political career in opposition during the reigns of Charles II and James VII and II. Because of his involvement in the 1685 anti-Catholic rebellion, Hume spent several years in exile in the Netherlands. He returned after the revolution of 1688 when he accompanied the Protestant William of Orange to Britain. His forfeited estates were returned to him and in 1696 he was appointed Lord Chancellor. Created Earl of Marchmont in 1697, he opposed the claims of the Jacobites and voted for Parliamentary union between Scotland and England.

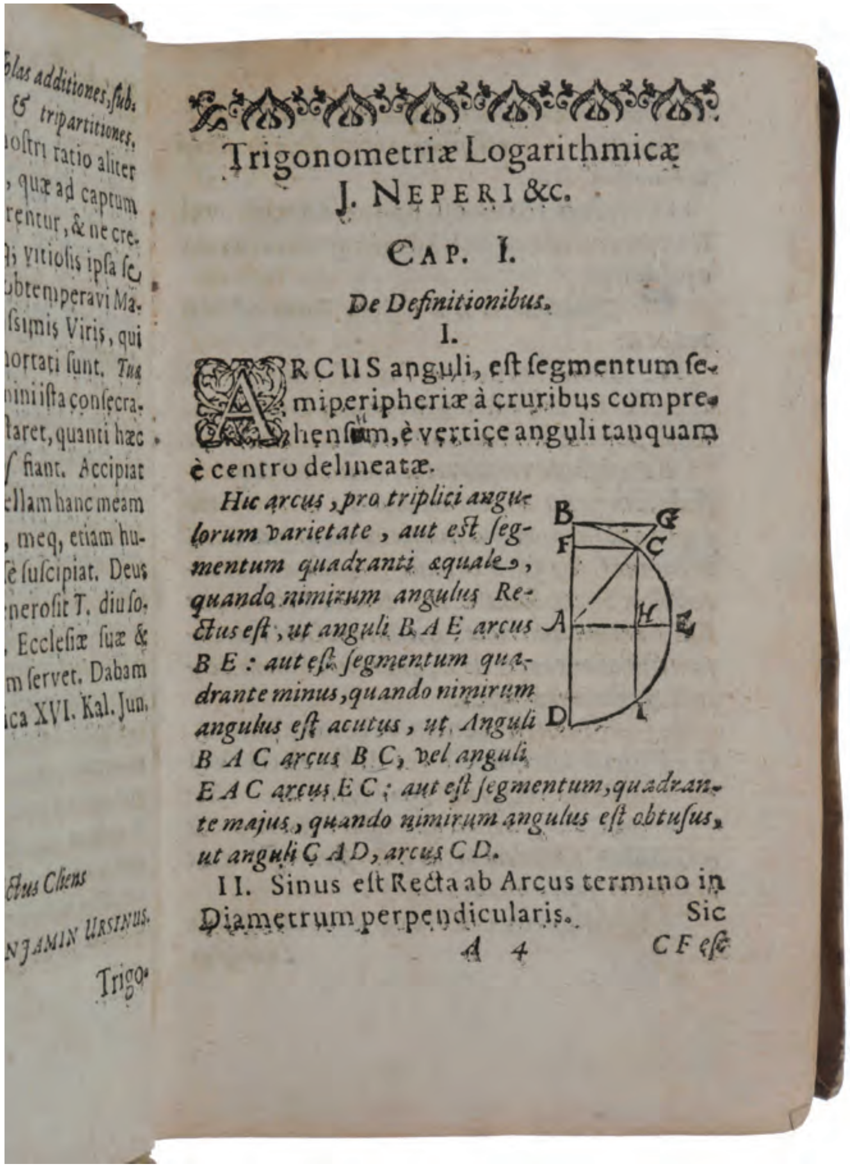
Benjamin Ursinus (originally Benjamin Behr, Latinized Ursinus), was born on July 15, 1587 in Sprottau in Silesia (now in Poland). Ursinus was a private tutor in Prague and then high school teacher at the Gymnasium of the Unity of the Bohemian brothers in Sobieslau and in Beuthen. From 1615 he taught at the Elector of Brandenburg's Gymnasium in Joachimsthal near Berlin, a school for gifted boys founded in 1607. From 1630 he was mathematics professor at the University of Frankfurt an der Oder, where he died in 1633 or 1634.

We do not know exactly when Ursinus first came into contact with Kepler (some sources suggest that Ursinus was Kepler's student), but certainly by 1610 Ursinus was acting as Kepler's assistant. Following the publication of *Sidereus nuncius* (1610), Kepler, then Imperial Court Astronomer to Rudolph II, was keen to test Galileo's observations. At the end of August, "the Elector of Cologne passed through Prague and lent Kepler the very instrument earlier sent to him by Galileo. Consequently, in just over one week (from August 30 to September 8), Kepler was able to observe what he now called for the first time the 'satellites' of Jupiter, and he was careful to do so with the testimony of various named and carefully described witnesses. Presumably, these were the kind of testimonials that Kepler had expected from Galileo. The first was Benjamin Ursinus, 'a diligent student of astronomy who, from the start, because he loves this art and has decided to practice philosophizing in it, never dreams of ruining the credit necessary to a future astronomer by false witness.' But there was more to Ursinus's reliability than concern for his future reputation. Kepler explained: 'We adopted the following method: with a piece of chalk and out of sight of each other, each of us drew on a wall what he had been able to observe; afterward, each of us went at the same time to see the other's picture to see if it was in agreement. This [method] is also to be understood for the following [observations]' … From August 30 to September 5, Benjamin Ursinus was Kepler's principal co-witness" (Westman, p. 480). Kepler acknowledged Ursinus's assistance in the preface of his *Narratio De Observatis a se quatuor Iouis satellitibus erronibus* (1611).

"In 1611 the political situation in Prague took an abrupt turn, ending Kepler's exhilarating atmosphere of intellectual freedom. The gathering storm of the Counter-Reformation reached the capital, and brought about the abdication of Rudolph II. As warfare and bloodshed surged around him, Kepler sought refuge in Linz, where he was appointed provincial mathematician … The Linz authorities charged him first of all to 'complete the astronomical tables in honor of the Emperor and the worshipful Austrian House, for the profit of … the entire land as well as also for his own fame and praise.' After Rudolph's death in 1612, his successor Matthias confirmed Kepler as court mathematician and agreed to his new residence away from Prague. But Kepler realized that as long as the *Rudolphine Tables* were unfinished, he would be tied to Linz. Thus the work on the tables became part of his fate" (Gingerich).

Despite Kepler's move, Ursinus evidently remained close to him – indeed, he lived in Kepler's house in Linz at Hofgasse 7 from October 1613 until autumn 1614 (Meyer, p. 4). It was presumably during this period that Ursinus assisted Kepler with the computation of the tables.

At about the same time as Ursinus left Kepler's house in Linz, Napier's great



work was published. There is evidence that the Scottish polymath John Napier of Merchiston (1550-1617) started working on logarithms in 1594, but his completed work, *Mirifici logarithmorum canonis descriptio*, was not published until 1614. "This is one of the most influential mathematical books ever published. It introduced the world to the concept of logarithms and their use. By simplifying arduous calculation, that is, by reducing multiplication and division to addition and subtraction, logarithms became the fundamental principle behind most of the methods of, and aides to, computation prior to the invention of the electronic computer. They also proved to be a fundamental component of many mathematical systems" (Tomash and Williams). The importance of Napier's work was immediately recognized, but it was not until 1618, in the offered work, that any account of logarithms was published outside Great Britain.

In his dedication of the *Cursus Mathematici Practici*, after singing the praises of Tycho Brahe, Kepler, and Galileo, whose telescope has made it possible for us to see what he and Kepler describe in their works, the author goes on to say that his 'versatile friend' Georg Vechner (1590–1647) had communicated to him Napier's book, and that he had not ceased until he had made it public to his pupils in the Gymnasium in Joachimsthal. Although primarily a theologian and philosopher, the versatile Vechner published in 1613 a posthumous biography of Bartholomaeus Pitiscus (1561-1613), the German mathematician and astronomer who first coined the word trigonometry. Ursinus's work was published at Cölln, the twin city of Old Berlin (Altberlin) from the 13th century to the 18th century, and now part of modern Berlin (and not at Cologne, as some sources would have it).

The structure of Ursinus's book is similar to that of Napier's *Descriptio*, although it is by no means simply a translation. After giving a general description of the logarithmic tables, and explaining that multiplying or dividing numbers corresponds to adding or subtracting their logarithms, Ursinus, like Napier, goes on to give examples of the application of the tables to solving various trigonometrical problems (a few of the examples are repeats of examples given by Napier, but most are not). He begins with right-angled plane triangles, before treating oblique-angled triangles in the plane, and then spherical triangles. This last case receives the most detailed treatment, as befits its importance to astronomical calculations. The text is followed by two tables. First, a *Tabula Proportionalis* giving the decimal equivalents of simple fractions, together with examples illustrating its use; and finally *J. Neperi … Mirificus Canon Logarithmorum*, Napier's table of logarithms from the *Descriptio*, but with the number of decimal places reduced by two. (DSB describes Ursinus's table as an 'excerpt' from that of Napier, but in fact both tables occupy the same 90 pages; the only difference is the number of decimal places.) The book concludes with several pages of errata.

Kepler became aware of the invention of logarithms a few months before the publication of Ursinus's book, as we learn from a letter dated 11 March 1618 to his friend Wilhelm Schickard (1592-1635). "After detailing the various difficulties and resources of trigonometry, he stated:

'A Scottish baron has appeared, whose name escapes me, but he has proposed some wonderful method by which all necessity of multiplications and divisions are commuted to mere additions and subtractions; nor does he make any use of a table of sines. However, he still requires a table of tangents and in some cases the variety, frequency and difficulty of additions and subtractions exceed the labour of multiplication and division' [Kepler, *Epistolae* (1718), p. 672].

"This statement by Kepler is rather obscure since it seems to indicate that although he had seen a demonstration of logarithms before writing this letter to Schickard, Kepler had not fully understood how they were used (and therefore probably had not used them himself) since ‘a table of tangents’ is not required. Nor can Kepler’s concluding sentence be considered an accurate assessment of logarithms.

“This was Kepler’s first impression of Napier’s work but later in the same year he happened to read *Cursus mathematici practici volumen primum*, published in 1618 and written by his former assistant Benjamin Ursinus, in which Napier’s tables were reproduced for the first time on the Continent. Kepler soon recognized the potential of logarithms and he wrote a long letter to Napier in 1619, publishing it as a generous Dedication to Napier in his *Ephemerides* for 1620, shortly before he published *Harmonices Mundi*. Despite Kepler’s wide correspondence throughout Europe, it is clear that he was not aware that Napier had died two years previously” (Rice *et al*, p. 40).

“[Kepler] exploited the new logarithms to solve two problems introduced for the first time by the novel form of the *Rudolphine Tables*. The first arises in the solution of what is now called Kepler’s equation. For a planet moving in an ellipse, under Kepler’s law of areas, there is no elementary way to find explicitly the position angle corresponding to a given time. However, the converse is easily calculated. Therefore he solved his equation for a set of uniformly spaced angles, which determine a set of non-uniformly spaced times. Kepler tabulated the logarithms of these intervals as a convenient means for interpolating to the desired times. The second important use of logarithms arises from the thoroughly heliocentric nature of the book. In previous planetary tables, the motions of the sun and planets were combined into a single procedure. In the Rudolphine Tables we must find separately the heliocentric positions of the earth and planet in question. To find the geocentric position of the planet, these two positions must be combined – essentially a problem of vector addition. Kepler facilitated this maneuver by tabulating the logarithms of the radius vectors of earth and planet, and by providing a convenient double-entry table for combining them” (Gingerich).

A second issue of the *Cursus Mathematici Practici* appeared in 1619, and five years later Ursinus published an expanded set of logarithmic-trigonometric tables to eight decimal places and calculated to every ten-seconds of arc (compared with Napier's seven decimal places, calculated to every minute of arc).
The Ursinus is here bound with two very rare philosophical works (Ursinus bound first).

KECKERMANN, Bartholomäus. Commentarius ad systema logicum majus posthumous. Berlin: Martin Guthius, 1619. 8vo, pp. [iv], 304. Not on OCLC.

Born in Danzig, Prussia, Keckermann (c. 1572-1608) was a German writer, Calvinist theologian, and philosopher. As a philosopher, Keckermann was the most prominent representative of an Aristotelian, anti-Ramist (Petrus Ramus) scholastic philosophy. As perhaps the most talented and independent Reformed theologian of orthodoxy, Keckermann with his 'analytical method' became the father of modern 'systematic theology.'His numerous works were published towards the end of his short life, most of them posthumously.

TIMPLER, Clemens. Opticae systema methodicum per theoremata et problemata selecta concinnatum et duobus libris comprehensum. Cui subjecta est Physiognomia humana, itidem duobus libris breviter et perspicue pertractata. Hanover: Petrum Antonium, 1617. 8vo, pp. [xxiv], [8], 240.

Clemens Timpler (1563/4–1624) was a Reformed Protestant and professor of philosophy at the Gymnasium illustre Arnoldinum in Steinfurt (Westphalia). He is considered, together with Jakob Degen (1511-87), to be the most important Protestant metaphysician, establishing the Protestant Reformed *Neuscholastik*,

and is sometimes considered the 'Godfather of Ontology'. His textbook on metaphysics, first published in 1604 and reprinted at least eight times by 1616, was his most influential work.

Macclesfield 2027 (second issue, incorrectly stating that the book was published at Cologne); not in Tomash & Williams. Gingerich, 'Johannes Kepler and the Rudolphine Tables,' *Sky and Telescope* 42 (1971), pp. 328-333. Meyer, The mystery of Johannes Kepler's place of residence in the Linzer Hofgasse (http://sternwarte.at/Kepler_Linz/Kepler_Linz_Hofgasse_7_EN.pdf). Rice, González-Velasco & Corrigan, *The Life and Works of John Napier*, 2017. Westman, *The Copernican Question*, 2011.

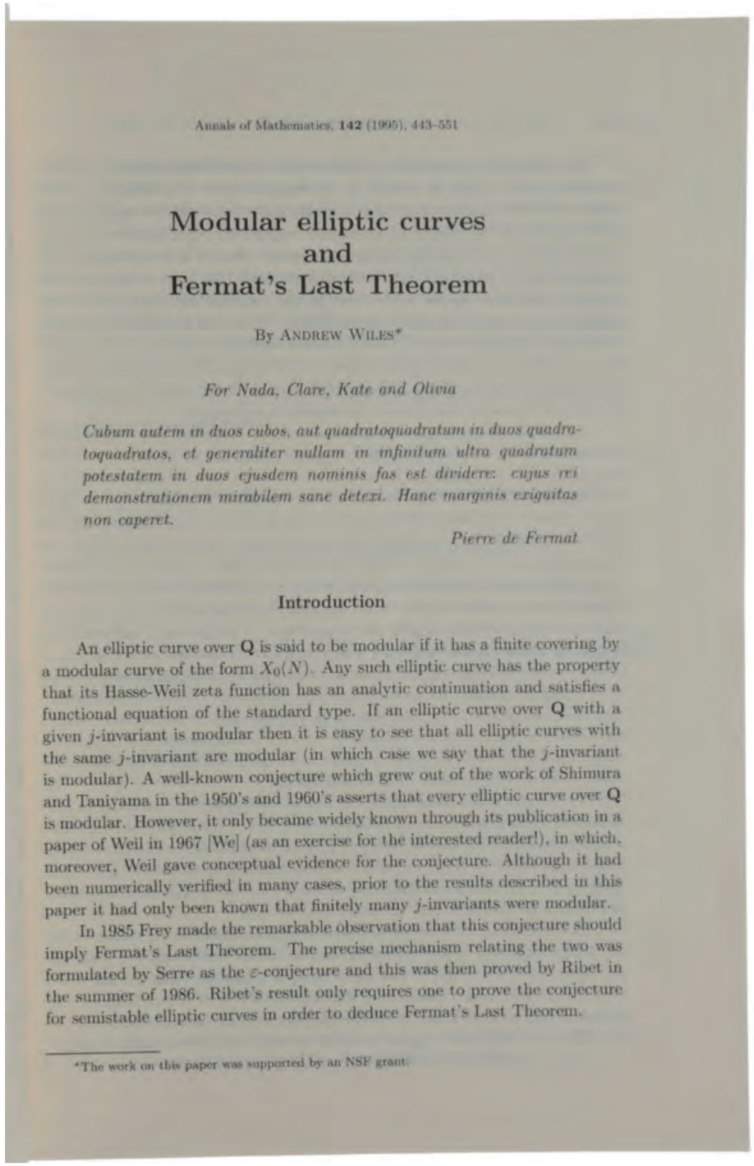
THE PROOF OF FERMAT'S LAST THEOREM

WILES, Andrew. & Richard TAYLOR. *'Modular elliptic curves and Fermat's Last Theorem' [with] 'Ring-theoretic properties of certain Hecke algebras', in: Annals of Mathematics, Vol. 141, No. 3, May 1995, pp. 443-551 & 553-572.* Princeton: Princeton University Press, 1995.

$4,500

8vo (254 x 178 mm), pp. 443-551 & pp. 553-572. Original printed wrappers, in virtually mint condition. Custom cloth box.

First edition, journal issue, of his proof of Fermat's Last Theorem, which was perhaps the most celebrated open problem in mathematics. In a marginal note in the section of his copy of Diophantus' *Arithmetica* (1621) dealing with Pythagorean triples (positive whole numbers x, y, z satisfying $x^2 + y^2 = z^2$ – of which an infinite number exist), Fermat stated that the equation $x^n + y^n = z^n$, where n is any whole number *greater* than 2, has *no* solution in which x, y, z are positive whole numbers. Fermat followed this assertion with what is probably the most tantalising comment in the history of mathematics: 'I have a truly marvellous demonstration of this proposition which this margin is too narrow to contain.' Fermat believed he could prove his theorem, but he never committed his proof to paper. After his death, mathematicians across Europe, from the enthusiastic amateur to the brilliant professional, tried to rediscover the proof of what became known as Fermat's Last Theorem, but for more than 350 years none succeeded, nor could anyone disprove the theorem by finding numbers x, y, z which did


Annals of Mathematics, **142** (1995), 443-551

Modular elliptic curves
and
Fermat's Last Theorem

By ANDREW WILES*

For Nada, Clare, Kate and Olivia

Cubum autem in duos cubos, aut quadratoquadratum in duos quadratoquadratos, et generaliter nullam in infinitum ultra quadratum potestatem in duos ejusdem nominis fas est dividere: cujus rei demonstrationem mirabilem sane detexi. Hanc marginis exiguitas non caperet.

Pierre de Fermat

Introduction

An elliptic curve over **Q** is said to be modular if it has a finite covering by a modular curve of the form $X_0(N)$. Any such elliptic curve has the property that its Hasse-Weil zeta function has an analytic continuation and satisfies a functional equation of the standard type. If an elliptic curve over **Q** with a given j-invariant is modular then it is easy to see that all elliptic curves with the same j-invariant are modular (in which case we say that the j-invariant is modular). A well-known conjecture which grew out of the work of Shimura and Taniyama in the 1950's and 1960's asserts that every elliptic curve over **Q** is modular. However, it only became widely known through its publication in a paper of Weil in 1967 [We] (as an exercise for the interested reader!), in which, moreover, Weil gave conceptual evidence for the conjecture. Although it had been numerically verified in many cases, prior to the results described in this paper it had only been known that finitely many j-invariants were modular.

In 1985 Frey made the remarkable observation that this conjecture should imply Fermat's Last Theorem. The precise mechanism relating the two was formulated by Serre as the ε-conjecture and this was then proved by Ribet in the summer of 1986. Ribet's result only requires one to prove the conjecture for semistable elliptic curves in order to deduce Fermat's Last Theorem.

*The work on this paper was supported by an NSF grant.

satisfy Fermat's equation. When the great German mathematician David Hilbert was asked why he never attempted a proof of Fermat's Last Theorem, he replied, "Before beginning I should have to put in three years of intensive study, and I haven't that much time to squander on a probable failure." Soon after the Second World War computers helped to prove the theorem for all values of n up to five hundred, then one thousand, and then ten thousand. In the 1980's Samuel S. Wagstaff of the University of Illinois raised the limit to 25,000 and more recently mathematicians could claim that Fermat's Last Theorem was true for all values of n up to four million. But no general proof was found until 1995.

"Between 1954 and 1986 a chain of events of occurred which brought Fermat's Last Theorem back into the mainstream. The incident which began everything happened in post-war Japan, when Yutaka Taniyama and Goro Shimura, two young academics, decided to collaborate on the study of elliptic curves and modular forms. These entities are from opposite ends of the mathematical spectrum, and had previously been studied in isolation.

"Elliptic curves, which have been studied since the time of Diophantus, concern cubic equations of the form:

$y^2 = (x + a).(x + b).(x + c)$, where a, b and c can be any whole number, except zero.

The challenge is to identify and quantify the whole solutions to the equations, the solutions differing according to the values of a, b, and c.

"Modular forms are a much more modern mathematical entity, born in the nineteenth century. They are functions, not so different to functions such as sine and cosine, but modular forms are exceptional because they exhibit a high degree of symmetry. For example, the sine function is slightly symmetrical because $2p$ can be added to any number, x, and yet the result of the function remains unchanged, i.e., $\sin x = \sin (x + 2p)$. However, for modular forms the number x can be transformed in an infinite number of ways and yet the outcome of the function remains unchanged, hence they are said to be extraordinarily symmetric …

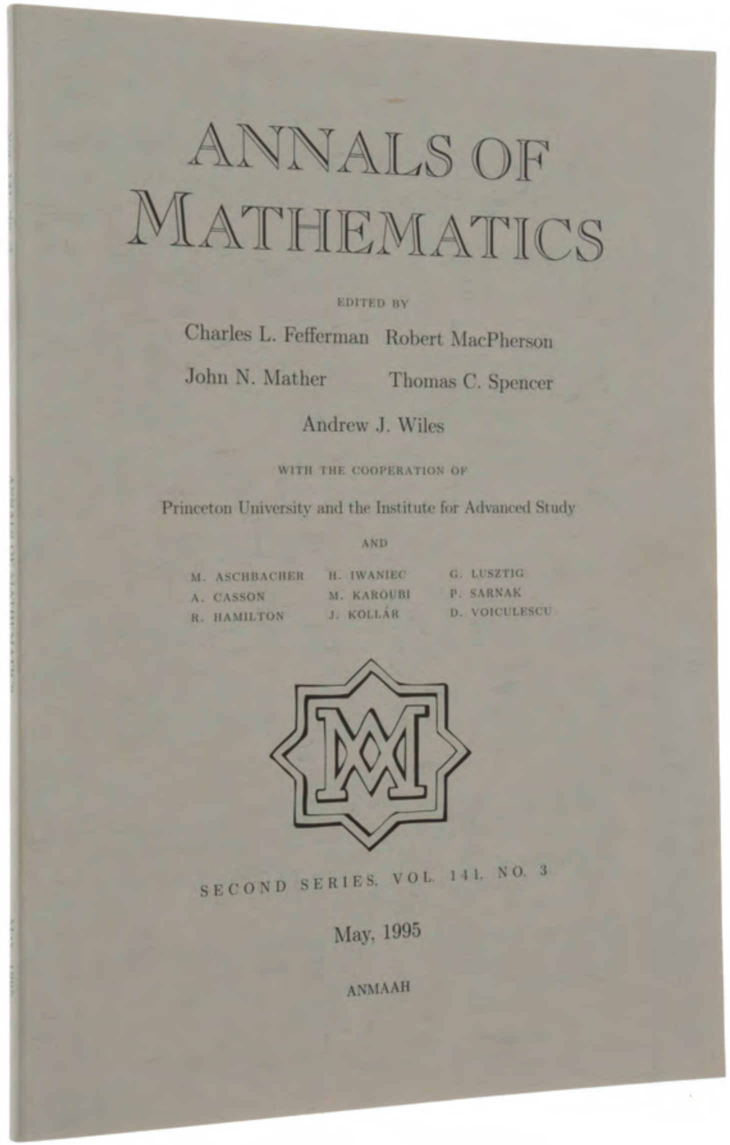
"Despite belonging to a completely different area of the mathematics, Shimura and Taniyama began to suspect that the elliptic curves might be related to modular forms in a fundamental way. It seemed that the solutions for any one of the infinite number of elliptic curves could be derived from one of the infinite number of modular forms. Each elliptic curve seemed to be a modular form in disguise. This apparent unification became known as the Shimura-Taniyama conjecture, reflecting the fact that mathematicians were confident that it was true, but as yet were unable to prove it. The conjecture was considered important because if it were true problems about elliptic curves, which hitherto had been insoluble, could potentially be solved by using techniques developed for modular forms, and vice versa …

"Even though the Shimura-Taniyama conjecture could not be proved, as the decades passed it gradually became increasingly influential, and by the 1970s mathematicians would begin papers by assuming the Shimura-Taniyama conjecture and then derive some new result. In due course many major results came to rely on the conjecture being proved, but these results could themselves only be classified as conjectures, because they were conditional on the proof of the Shimura-Taniyama conjecture. Despite its pivotal role, few believed it would be proved this century.

"Then, in 1986, Kenneth A Ribet of the University of California at Berkeley, building on the work of Gerhard Frey of the University of Saarlands, made an astonishing breakthrough. He was unable to prove the Shimura-Taniyama conjecture, but he was able to link it with Fermat's Last Theorem. The link occurred by contemplating the unthinkable – what would happen if Fermat's Last Theorem was not true? This would mean that there existed a set of solutions to Fermat's equation, and therefore this hypothetical combination of numbers could be used as the basis for constructing a hypothetical elliptic curve. Ribet demonstrated that this elliptic curve could not possibly be related to a modular form, and as such it would defy the Shimura-Taniyama conjecture. Running the argument backwards, if somebody could prove the Shimura-Taniyama conjecture then every elliptic curve must be related to a modular form, hence any solution to Fermat's equation is forbidden to exist, and hence Fermat's Theorem must be true. If somebody could prove the Shimura-Taniyama conjecture, then this would immediately imply the proof of Fermat's Last Theorem. By proving one of the most important conjectures of the twentieth century, mathematicians could solve a riddle from the seventeenth century.

"The Shimura-Taniyama conjecture had remained unproven since the 1950s and so there was little optimism that it was a realistic route to a proof of Fermat's Last Theorem. Some mathematicians joked that, if anything, the Shimura-Taniyama conjecture was even further out of reach, because, by definition, anything that led to a proof of the Last Theorem must be impossible. But for Wiles, anything that would lead to the Last Theorem was worth pursuing. He knew that this might be his only chance to realise his childhood dream and he had the audacity to attack the Shimura-Taniyama conjecture. As a graduate student at Cambridge University, he had concentrated on studying elliptic curves, and then as a professor at Princeton University he had continued his research, putting him in an ideal position for attempting a proof.

"As he embarked on his proof, Wiles made the extraordinary decision to conduct



his research in complete secrecy. He did not want the pressure of public attention, nor did he want to risk others copying his ideas and stealing the prize. In order not to arouse suspicion Wiles devised a cunning ploy that would throw his colleagues off the scent. During the early 1980s he had been working on a major piece of research on a particular type of elliptic curve, which he was about to publish in its entirety until the discoveries of Ribet and Frey made him change his mind. Wiles decided to publish his research bit by bit, releasing another minor paper every six months or so. This apparent productivity would convince his colleagues that Wiles was still continuing with his usual research. For as long as he could maintain this charade Wiles could continue working on his true obsession without revealing any of his breakthroughs. For the next seven years he worked in isolation, and his colleagues were oblivious to what he was doing. The only person who knew of his secret project was his wife – he told her during their honeymoon.

"The number of elliptic curves and modular forms is infinite, and the Shimura-Taniyama conjecture claimed each elliptic curve could be matched with a modular. However, to succeed Wiles did not have to prove the full Shimura-Taniyama conjecture. Instead he only had to show that a particular subset of elliptic curves (one which would include the hypothetical Fermat elliptic curve) is modular. However, this subset is still infinite in size and it includes the majority of interesting curves.

"To prove that something is true for an infinite number of cases required Wiles to pull together some of the most recent breakthroughs in number theory, and in addition invent new techniques of his own. He adopted a strategy loosely based on a method known as induction. Proof by induction can prove something for an infinite number of cases by invoking a domino toppling approach, i.e., to knock down an infinite number of dominoes, one merely has to ensure that knocking down any domino will always topple the next domino. In other words, Wiles had to develop an argument in which he could prove the first case, and then be sure that proving any one case would implicitly prove the next one.

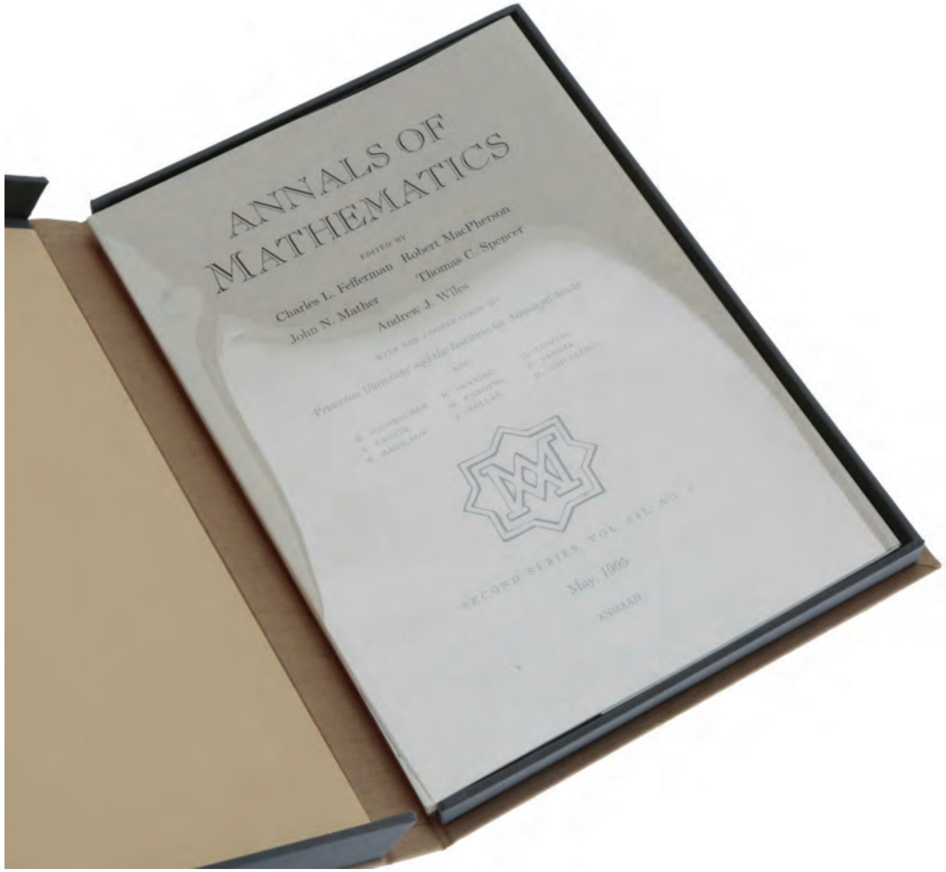
"At each stage Wiles could never be sure that he could complete his proof. He realised that even if he did have the correct strategy, the mathematical techniques required might not yet exist – he might be on the right track, but living in the wrong century. Eventually, in 1993, Wiles felt confident that his proof was reaching completion. The opportunity arose to announce his proof of a major section of the Shimura-Taniyama conjecture, and hence Fermat's Last Theorem, at a special conference to be held at the Isaac Newton Institute in Cambridge, England. Because this was his home town, where he had encountered the Last Theorem as a child, he decided to make a concerted effort to complete the proof in time for the conference. On June 23rd he announced his seven-year calculation to a stunned audience.

"His secret research programme had apparently been a success, and the mathematical community and the world's press rejoiced. The front page of the New York Times exclaimed "At Last, Shout of 'Eureka!' in Age-Old Math Mystery", and Wiles appeared on television stations around the world … While the media circus continued, the official peer review process began. Over the summer the 200-page proof was examined line by line by a team of referees. The manuscript was split into seven chapters, and each chapter was sent to a pair of expert examiners. Wiles checked and double-checked the proof before releasing it to the referees, so he was expecting little more than the mathematical equivalent of grammatical and typographic errors, trivial mistakes that he could fix immediately. However, gradually it emerged that there was a fundamental and devastating flaw in one stage of the argument.

"Essentially, the inductive argument used by Wiles could not guarantee that if one domino toppled, then so would the next. Over the course of the next year his childhood dream turned into a nightmare. Each attempt to fix the error ended in failure, each attempt to by-pass the error ended in a dead-end. And throughout this period the manuscript had only been seen by the small team of referees and Wiles himself. There were calls from the mathematics community to publish the flawed proof, which would allow others to try and fix it, but Wiles steadfastly refused. He believed that he deserved the first chance to correct a piece of work that had already taken him seven years.

"After months of failure Wiles did take into his confidence Richard Taylor, a former student of his, hoping that this would give him someone to bounce ideas off, someone who could inspire him to consider alternative strategies. By September 1994 they were at the point of admitting defeat, ready to release the flawed proof so that others could try and fix it. Then on September 19th they made the vital breakthrough. Many years earlier, when he was working in secrecy, Wiles had considered using an alternative approach, but it floundered and so he had abandoned it. Now they realised that what was causing the more recent method to fail was exactly what would make the abandoned approach succeed.

Wiles recalls his reaction to the discovery: "It was so indescribably beautiful, it was so simple and so elegant. The first night I went back home and slept on it. I checked through it again the next morning and, and I went down and told me wife, 'I've got it! I think I've found it!'. And it was so unexpected that she thought I was talking about a children's toy or something, and she said, 'Got what?' I said, 'I've fixed my proof. I've got it'" (Simon Singh, 'The Whole Story,' simonsingh.net/books/fermats-last-theorem/the-whole-story/).



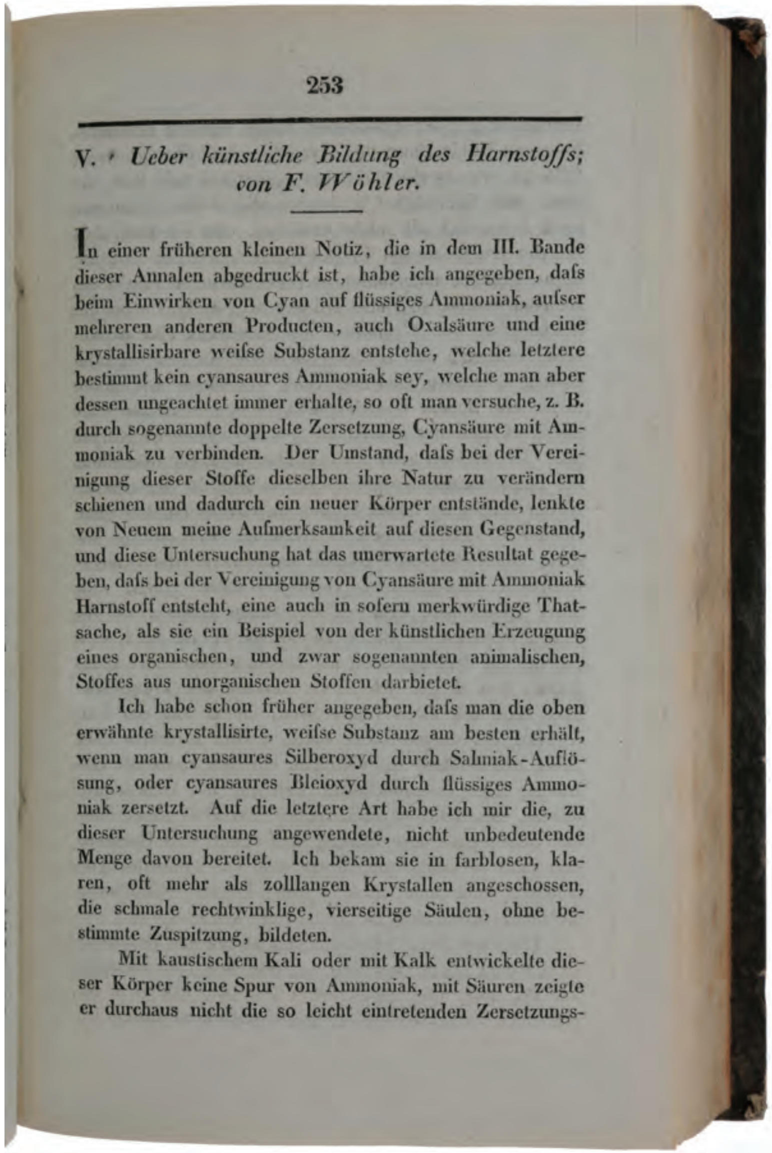
DIBNER 45 - THE BEGINNING OF ORGANIC CHEMISTRY

WÖHLER, Friedrich. *'Ueber die künstliche Bildung des Harnstoffs,' pp. 253-256 in: Annalen der Physik und Chemie, Folge 2, Band 12.* Leipzig: Johann Ambrosius Barth, 1828.

$2,800

8vo (199 x 122 mm), pp. viii, 632. Contemporary speckled boards, paper spine label lettered in manuscript (binding a little rubbed). Stamp of Carl Alexander Bibliothek in Eisenach. Preserved in a custom leather box. A very good copy.

First edition, complete journal volume, of this foundation work of organic chemistry. "In 1828, Wöhler succeeded in effecting an artificial synthesis of urea by heating ammonium cyanate. This was the first time that an organic substance had ever been built up artificially from the constituents of an inorganic substance without any intervention of vital processes, and it soon became clear that there is no essential difference between the structural chemistry of life and that of inanimate nature" (GM). "This was the first synthesis of an organic compound, and this accomplishment is generally regarded as the beginning of organic chemistry" (Sparrow, p. 37). "The discovery destroyed the vitalistic theory which held that organic compounds could be produced only by living organisms, and led eventually to the brilliant results that have been achieved in attempts to synthesize other organic compounds" (Dibner). "In 1828, Friedrich Wöhler (1800-1882) published a short article in which he described the unexpected formation of urea from ammonium cyanate. The appearance of urea as a product was entirely



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V. *Ueber künstliche Bildung des Harnstoffs; von F. Wöhler.*

In einer früheren kleinen Notiz, die in dem III. Bande dieser Annalen abgedruckt ist, habe ich angegeben, dafs beim Einwirken von Cyan auf flüssiges Ammoniak, aufser mehreren anderen Producten, auch Oxalsäure und eine krystallisirbare weifse Substanz entstehe, welche letztere bestimmt kein cyansaures Ammoniak sey, welche man aber dessen ungeachtet immer erhalte, so oft man versuche, z. B. durch sogenannte doppelte Zersetzung, Cyansäure mit Ammoniak zu verbinden. Der Umstand, dafs bei der Vereinigung dieser Stoffe dieselben ihre Natur zu verändern schienen und dadurch ein neuer Körper entstände, lenkte von Neuem meine Aufmerksamkeit auf diesen Gegenstand, und diese Untersuchung hat das unerwartete Resultat gegeben, dafs bei der Vereinigung von Cyansäure mit Ammoniak Harnstoff entsteht, eine auch in sofern merkwürdige Thatsache, als sie ein Beispiel von der künstlichen Erzeugung eines organischen, und zwar sogenannten animalischen, Stoffes aus unorganischen Stoffen darbietet.

Ich habe schon früher angegeben, dafs man die oben erwähnte krystallisirte, weifse Substanz am besten erhält, wenn man cyansaures Silberoxyd durch Salmiak-Auflösung, oder cyansaures Bleioxyd durch flüssiges Ammoniak zersetzt. Auf die letztere Art habe ich mir die, zu dieser Untersuchung angewendete, nicht unbedeutende Menge davon bereitet. Ich bekam sie in farblosen, klaren, oft mehr als zolllangen Krystallen angeschossen, die schmale rechtwinklige, vierseitige Säulen, ohne bestimmte Zuspitzung, bildeten.

Mit kaustischem Kali oder mit Kalk entwickelte dieser Körper keine Spur von Ammoniak, mit Säuren zeigte er durchaus nicht die so leicht eintretenden Zersetzungsunexpected, because theory predicted that cyanic acid and ammonia should produce a compound with the properties of a salt. Urea was not a salt, and it did not possess any of the properties expected for cyanates. In the article, Wöhler repeatedly noted the novelty of the artificial synthesis, but he and his mentor, the well-known Swedish chemist Jöns Jacob Berzelius (1779-1848), were most intrigued by the formation of a nonsalt from a salt, and that ammonium cyanate and urea had the same elemental composition. Neither Wöhler nor Berzelius commented, as the epigraphs might suggest, on how the synthesis influenced the doctrine of vitalism, but within a few decades, chemists came to regard Wöhler's experiment as an 'epochal' discovery that would mark both the death of vitalism and the birth of organic chemistry as a sub-discipline of chemistry. The myth has proved remarkably enduring – a survey of modern organic chemistry textbooks has revealed that 90 per cent of them mention some version of the Wöhler myth" (Ramberg, p. 60).

"Wöhler, the son of an agronomist and veterinarian, attended the University of Marburg and then the University of Heidelberg, from which he received a medical degree with a specialty in obstetrics (1823). However, his passion always was chemistry. The eminent professor of chemistry at Heidelberg, Leopold Gmelin, judged Wöhler to be already too advanced to profit from his courses, so he sent him to study with the world-famous Swedish chemist Jöns Jacob Berzelius. A year of mineral analysis in Stockholm not only provided Wöhler with the best chemical training then available but also cemented a close lifelong bond between the two men. Wöhler quickly mastered the Swedish language and subsequently served as Berzelius's translator and advocate in Germany" (Britannica).

"Wöhler became acquainted with his lifelong friend Liebig as a result of what seemed in 1825 to be a minor squabble over the interpretation of analytical results but became a classic example of a new phenomenon that Berzelius in 1830 called isomerism: both silver cyanate and silver fulminate correspond to the empirical formula AgCNO. Liebig had studied the explosive fulminate and at first rejected Wöhler's 1824 results for the stable cyanate. In the 1820's most chemists assumed that only one chemical compound corresponded to one set of analytical percentages. Wöhler and Liebig exchanged letters, often visited each other, and sometimes took vacations together from 1829 until Liebig's death in 1873.

"Wöhler's interest in cyanates led to a historic preparation of 'artificial' urea, the circumstances of which are best described in his letter to Berzelius of 22 February 1828:

'I can no longer, as it were, hold back my chemical urine; and I have to let out that I can make urea without needing a kidney, whether of man or dog: the ammonium salt of cyanic acid is urea.

'Perhaps you can remember the experiments that I Performed in those happy days when I was still working with you, when I found that whenever one tried to combine cyanic acid with ammonia a white crystalline solid appeared that behaved like neither cyanic acid nor ammonia. . . . I took this up again as a subject that would fit into a short time interval, a small undertaking that would quickly be completed and – thank God – would not require a single weighing.

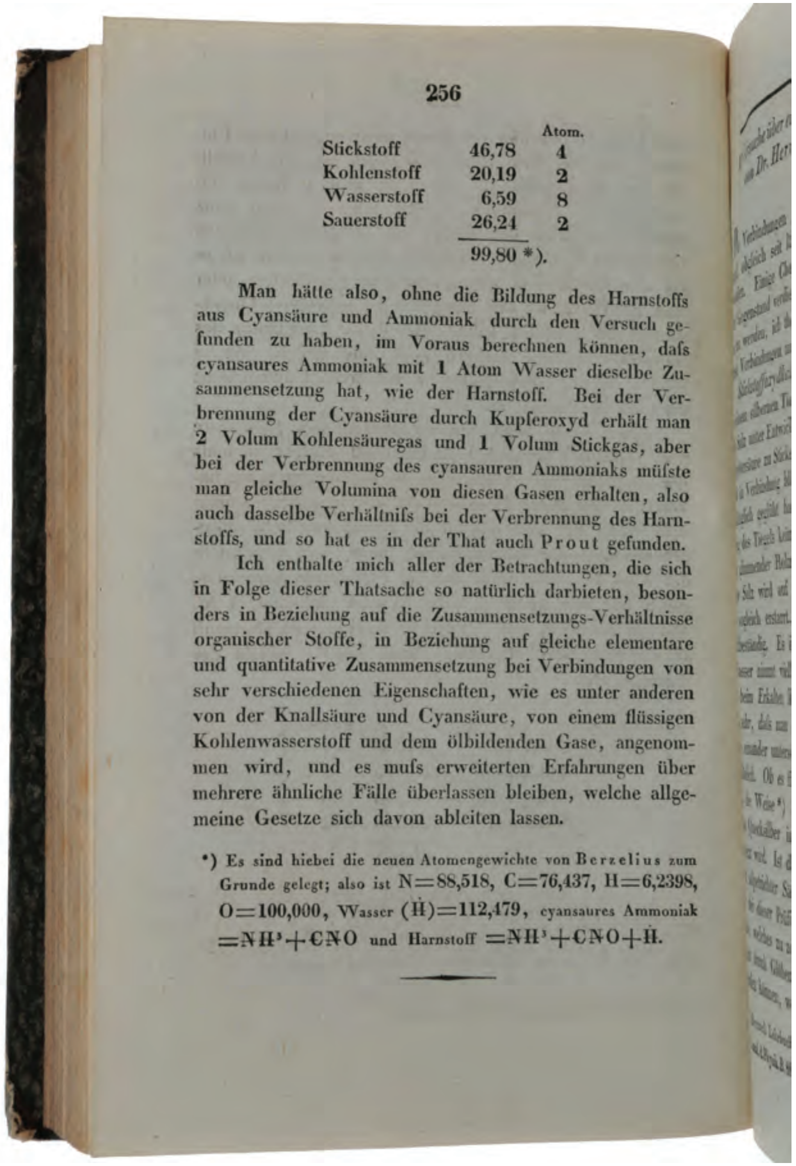
'The supposed ammonium cyanate was easily obtained by reacting lead cyanate with ammonia solution . . . Four-sided right-angled prisms, beautifully crystalline, were obtained. When these were treated with acids, no cyanic acid was liberated, and with alkali, no trace of ammonia. But with nitric acid lustrous flakes of an easily crystallized compound, strongly acid in character, were formed; I was disposed to accept this as a new acid because when it was heated, neither nitric nor nitrous acid was evolved, but a great deal of ammonia. Then I found that if it were saturated with alkali, the so-called ammonium cyanate reappeared; and this could be extracted with alcohol, Now, quite suddenly I had it! All that was needed was to compare urea from urine with this urea from a cyanate.'

"The letter goes on to describe how the discovery adds to the pairs of substances of similar composition but of different properties already known. 'It is noticeable that in making cyanates (and in making ammonia) we always have to start with an organic substance …'

"In his published paper [offered here] Wöhler referred to his work of 1823, in which he had shown that cyanogen and aqueous ammonia yielded oxalic acid and a white crystalline solid that he now realized was urea. This, and his new method, he considered to be remarkable examples of the preparation 'by art' of a substance of animal origin from inorganic materials" (DSB).

"As early as the 1840s, Wöhler's supporters began to tout his discovery as the 'death knell' of *vitalism*—and it is still usually described that way—but recent historical inquiry has shown that the situation was more complex; Wöhler's own antivitalist claims were necessarily muted and qualified. His discovery was at least as important for the history of isomerism as for vitalism, since very few cases were then known of two distinct compounds having identical compositions. Two years after Wöhler's synthesis of urea, Berzelius defined the concept and introduced the new word *isomerism*" (Britannica).

"The urea myth can be conveniently condensed into three components: (1) that Wöhler synthesized urea from the elements; (2) that the synthesis unified organic and inorganic chemistry under the same laws; (3) that the synthesis destroyed, or at least weakened, the idea of a 'vital force' in living organisms. As historians have extensively documented, however, each of these three parts is highly problematic.



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		Atom.
Stickstoff	46,78	4
Kohlenstoff	20,19	2
Wasserstoff	6,59	8
Sauerstoff	26,24	2
	99,80 *).	

Man hätte also, ohne die Bildung des Harnstoffs aus Cyansäure und Ammoniak durch den Versuch gefunden zu haben, im Voraus berechnen können, dafs cyansaures Ammoniak mit 1 Atom Wasser dieselbe Zusammensetzung hat, wie der Harnstoff. Bei der Verbrennung der Cyansäure durch Kupferoxyd erhält man 2 Volum Kohlensäuregas und 1 Volum Stickgas, aber bei der Verbrennung des cyansauren Ammoniaks müfste man gleiche Volumina von diesen Gasen erhalten, also auch dasselbe Verhältnifs bei der Verbrennung des Harnstoffs, und so hat es in der That auch Prout gefunden.

Ich enthalte mich aller der Betrachtungen, die sich in Folge dieser Thatsache so natürlich darbieten, besonders in Beziehung auf die Zusammensetzungs-Verhältnisse organischer Stoffe, in Beziehung auf gleiche elementare und quantitative Zusammensetzung bei Verbindungen von sehr verschiedenen Eigenschaften, wie es unter anderen von der Knallsäure und Cyansäure, von einem flüssigen Kohlenwasserstoff und dem ölbildenden Gase, angenommen wird, und es mufs erweiterten Erfahrungen über mehrere ähnliche Fälle überlassen bleiben, welche allgemeine Gesetze sich davon ableiten lassen.

*) Es sind hiebei die neuen Atomengewichte von Berzelius zum Grunde gelegt; also ist $N=88,518$, $C=76,437$, $H=6,2398$, $O=100,000$, Wasser ($\dot{H}$)$=112,479$, cyansaures Ammoniak $=NH^3+CNO$ und Harnstoff $=NH^3+CNO+\dot{H}$.

First, Wöhler's synthesis could be, and was, rejected as artificial, because there may have been a residual 'vital force' in his starting materials [Wöhler had started with organic matter, derived inorganic elements from it, and used those to produce urea]. Second, well before the urea synthesis, chemists had operated under the assumption, promoted by Berzelius, that organic and inorganic chemistry should follow the same laws of chemical combination. Third, 'vitalism' was not a single theory but a variety of related ideas about the nature of life that continued well after Wöhler's synthesis in both chemical and biological contexts …

"In most versions of the myth, vitalism is assumed to be a theory that supposes the existence of a mystical, nonmaterial entity that is present in living things but absent in inorganic systems – a 'rational soul' that is responsible for maintaining the complex systems found in living organisms … [However,] vitalism was not a single comprehensive theory but a variety of theories about *biological systems*. Berzelius himself had developed a version of vital materialism as early as 1806, which he incorporated into the section on organic chemistry in the 1827 edition of his textbook; he never substantially revised the entry in subsequent editions. Similarly, Justus von Liebig (1803-1873) described vital force in *Animal Chemistry* (1842) as 'a peculiar property, which is possessed by certain material bodies, and becomes sensible when their elementary particles are combined in a certain arrangement or form.' This force, analogous to gravity or electricity, arose from the complexity of the system. Because the synthesis of a single compound could have had little effect on vitalistic theories about an organized system, it should not be surprising that Wöhler and Berzelius failed to discuss the impact of the synthesis of urea on vitalism in their correspondence and that early textbooks on organic chemistry did not mention Wöhler or the urea synthesis …

"The Wöhler myth shows no signs of fading away, because it serves several specific purposes. For organic chemists, it provides a hero who accomplished a specific datable task that assumed great significance. The myth became widespread after Wöhler's death in 1882, in part to validate the theoretical autonomy of organic chemistry as a discipline that no longer required concepts from either biology or physics, and in part because German chemists wished to place the origins of the powerful German chemical community, in which synthesis played a central role, squarely in their own country. For biologists, the myth's simplistic image of vitalism provides a convenient foil for depicting how physiologists adopted the rigorous mechanistic and quantitative methods of chemistry and physics in the process of making biology more 'scientific' by ridding it of 'pseudoscientific' entities such as vital forces" (Ramberg, pp. 60-66).

"All that said, with respect to the insignificance claim for Wöhler's urea, there is a danger in proving too much. Wöhler obviously thought that he had done something rather dramatic in his excited letter of 22 February 1828 to his mentor Berzelius, stressing that he could make urea without a kidney, or even a living creature. Berzelius' reply two weeks later is just as enthusiastic and just as focused on the issue of organic synthesis; Wöhler had produced a 'jewel' for his 'laurel-wreath' that would 'immortalize' his name. Wöhler clearly noted in his letter that his accomplishment would fail to convert a committed sceptic, and his published article did not assert that vitalism had been refuted. Moreover, both Wöhler and Berzelius recognized that the reaction was very relevant to a separate issue, the emergent study of isomerism, for Wöhler had simultaneously discovered that urea and ammonium cyanate had identical compositions. However, these qualifications do not negate the fact that both men regarded the new reaction to be important for an evaluation of vitalist beliefs … The 'myth' of Wöhler's overthrow of vitalism in 1828 (and myth it surely was) was not created, as has been argued, in the spate of celebratory articles published during the centennial year, nor even in Hofmann's 1882 obituary of Wöhler. It was created in the immediate aftermath of the event itself. As early as 1843, Hermann Kopp, writing as a historian, urged that this was the deed that destroyed vitalist belief, and ignored the reaction's relevance for isomerism; Kopp's portrayal became an important source for later writers … Relevant it was; a refutation of vitalism it was not. However, by the early 1850s, if not before, the myth of a definitive refutation had become ensconced in the German textbook literature" (Rocke, pp. 240-241).

Ironically, the modern understanding of the mechanism of production of urea in the mammalian liver seems to retain a certain 'vitalistic' aspect, although it would certainly not be described in that way today. "… the chemical synthesis observed by Wöhler does not represent the reaction which is employed in the mammalian liver for urea synthesis. The mechanism of this process was elucidated by the German physician Hans A. Krebs and his medical student Kurt Henseleit in 1932 and was shown to include the ornithine cycle. This 'urea cycle' is only observed in living cells; this apparently vitalistic phenomenon is caused by the compartmentalization of the various enzymatic reactions in mitochondria and cytosol, respectively" (Kinne-Safran & Kinne).

Dibner, *Heralds of Science* 45; Garrison-Morton 671; Sparrow, *Milestones of Science* 197; DSB XIV, pp. 474 *et seq*. Kinne-Safran & Kinne, 'Vitalism and synthesis of urea. From Friedrich Wohler to Hans A. Krebs,' *American Journal of Nephrology* 19 (1999), pp. 290-294. Ramberg, 'Myth 7. That Friedrich Wöhler's synthesis of urea in 1828 destroyed vitalism and gave rise to organic chemistry,' pp. 59-66 in: *Newton's apple and other myths about science,* Numbers & Kampourakis (eds.), Harvard University Press, 2015. Rocke, *The quiet revolution: Hermann Kolbe and the science of organic chemistry*, 1993.