KLAUS HENTSCHEL*

The discovery of the redshift of solar Fraunhofer lines by Rowland and Jewell in Baltimore around 1890

ONE OF THE most important optical discoveries in the 19th century was the detection of sharp dark lines in the sun's spectrum by William Hyde Wollaston in 1802. In 1814, Joseph Fraunhofer increased the resolution of spectral observations enormously by observing through a telescope; thus he was the first to give a detailed and scaled map showing about 350 dark lines, soon to be called Fraunhofer lines, distributed over the entire visible spectrum of the sun.[1] These lines were of particular interest not only to Fraunhofer, but also to David Brewster and John Herschel, since they always appeared at the same position in the spectrum. Fraunhofer was interested in the lines as markers to determine the dispersive powers of prisms made out of different types

*Department of History of Science, Göttingen University, Humboldtallee 11, 3400 Göttingen, Germany. For comments on earlier versions of this paper, I would like to thank: Gerd Grasshoff, Jost Lemmerich, the DPG history group, Dresden 1991, the editors, and my wife Ann. For archival help and permission to quote from the Rowland papers as well as the Johns Hopkins University Collection 137, both kept at the Eisenhower Library, Johns Hopkins University, my thanks go to Joan Grattan, Cynthia H. Requardt and Marion Suesil.

The following abbreviations are used: *AA, Astronomy and astrophysics*; *AJP, American journal of physics*; *AJS, American journal of science*; *APJ, Astrophysical journal*; *AP, Annales de physique*; *AS, Annals of science*; *BMNAS*, National Academy of Science, *Biographical memoirs*; *CRAS*, Académie des Sciences, Paris, *Comptes rendus*; *DAB, Dictionary of American biography*; *DSB, Dictionary of scientific biography*; *JHUC*, Johns Hopkins University, *Circulars*; JHUC, Johns Hopkins University Collection; *JP, Journal de physique théorique et appliquée*; *JRE, Jahrbuch der Radioaktivität und Elektronik*; *PA*, American Academy of Arts and Sciences, *Proceedings*; *PM, Philosophical magazine*; *PRSL*, Royal Society, London, *Proceedings*; *PTRS*, Royal Society, London, *Philosophical transactions*; *PZ, Physikalische Zeitschrift*; RP: Rowland papers, MS6, Special Collections, The Milton S. Eisenhower Library, Johns Hopkins University, Baltimore, MA; *TC*, Technology and culture; *ViA, Vistas in astronomy*; *ZP, Zeitschrift für Physik*; *ZwPh, Zeitschrift für wissenschaftliche Photographie.*

[1] J. Fraunhofer, "Bestimmung des Brechungs- und Farbenzerstreuungs-Vermögens verschiedener Glasarten, in Bezug auf die Vervollkommung achromatischer Fernröhre[!]," Akademie der Wissenschaften, Munich, *Denkschriften*, *5* (1814/15), 193–226, esp. plate II.

of glass in order to improve his achromatic telescopes; a precise determination of the angle of deflection for one of these dark lines would measure their dispersion. The merely useful phenomenon became a field of research of its own after Gustav Kirchhoff and Robert Bunsen showed that each element has a characteristic spectrum in the Bunsen flame. A further acceleration of instrument development followed the birth of the disciplines of spectral analysis and spectroscopy, the former looking specifically for any interdependency of the chemical nature of light emitting or absorbing bodies and their spectra,[2] the latter measuring and further analyzing these spectra, especially with respect to series characteristics.[3]

Progress in spectroscopy in the second half of the 19th century rested on the improvement of diffraction gratings.[4] David Rittenhouse's primitive grating of 1785 and Fraunhofer's first grating of 1821 were made of wires stretched parallel to each other in the notches of a long screw.[5] Later, Fraunhofer introduced the technique of scratching parallel lines with a diamond point onto a glass surface coated with a very thin layer of gold.[6] This technique was considerably improved and extended to the scratching of metal surfaces, which gave rise to diffraction spectra by reflection. The much higher regularity of ruled metal over ruled glass surfaces gave metal the advantage despite the low intensity of light reflected from them. Every introduction of technological improvements into the art of ruling gratings caused a jump in the spectroscopic resolution $\lambda/\delta\lambda$: converted into today's units, measurements made before 1850 had $\delta\lambda \simeq 1$ Å, Nobert's carefully ruled glass gratings led to an tenfold increase in resolution around 1860 ($\delta\lambda \simeq 0.1$ Å), Rutherfurd's gratings improved it once again by a factor of 2 to 5 in the late 1860s and 1870s, and Rowland's concave gratings allowed for $\delta\lambda < 0.01$ Å.

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[2] Frank A.J.L. James, "The establishment of spectro-chemical analysis as a practical method of qualitative analysis, 1854–1861," *Ambix, 30* (1983), 30–53.

[3] Cf. Heinrich Konen, entries "Spektralanalyse" and "Spektroskopie," in *Handwörterbuch der Naturwissenschaften*, 1st ed. (Jena, 1913) *9*, 205–214, 222–251, esp. 223, for a general characterization of the aims of both disciplines.

[4] For an overview of the development of technology for the ruling of gratings as well as scratching into surfaces for other purposes before Rowland, see Deborah Jean Warner, "Rowland's gratings: Contemporary technology," *ViA*, *29* (1986), 125–130, on 125f. and references therein.

[5] Cf. Th.D. Cope, "The Rittenhouse diffraction grating," Franklin Institute, *Journal*, *214* (1932), 99–104, resp. Fraunhofer (ref. 1). Fraunhofer's grating consisted of 260 parallel wires.

[6] Cf. David Brewster as quoted in Warner (ref. 4), on 126 for his acknowledgment of Fraunhofer's "superior powers and means of investigation."

Table 1
Development of spectroscopes in the 19th century[a]

Name	Year	Instrument	Size (cm)	No. of Lines	Line separation (cm)
Fraunhofer	1814	Prism and telescope	-	-	-
Fraunhofer	1821	First diffraction grating; parallel wires; later scratches in a gold-coated glass plate		3,600	0.0048
Nobert	1851	Diamond scratches in glass	ca 2.5	ca 1,000	0.0025
Steinheil	1860	Flint glass prisms, 4–6 in sequence			-
(Ditscheiner)	1864	Grating by Nobert	1.38	3,000	0.00046
(Angström)	1864	Grating by Nobert	ca 2	ca 4,500	0.00046
Rutherfurd	1868	Grating in glass and metal	ca 5	ca 20,000	0.00025
Rutherfurd	1881	Grating in metal for Mendenhall	4.4 4.4	ca 30,000	0.00015
Rowland	1882	Concave grating	ca 7.2	ca 45,000	0.00017
Rowland	1896	Concave grating	ca 14.5	ca 110,000	0.00015
Michelson	1907	Concave grating	22·11	ca 110,000	0.00010

a. Names in parentheses stand for observers who did not manufacture their own spectroscopes but obtained them from other instrument makers.

The table shows that with Rowland the total number of lines in the surface of the grating increased while the distance of neighboring lines was nearly constant, though its regularity improved. Because the highest resolution of a grating $\lambda/\delta\ \lambda$ is equal to the product of total number of lines and the order of interference m, and since Rowland's gratings allowed observation in the third and even fourth order of the spectrum, his gratings obtained resolutions of up to 400,000, which was then considered as the practical limit of resolution for precision spectroscopy.[7] Prism spectrographs and the earlier gratings could resolve at best two spectral lines distant by 1/40th the separation between two sodium D-lines; Rowland's gratings had a resolution 2.5 times as great. The instrument revolution in spectroscopy achieved by Rowland was immense, "a new departure in spectrum-analysis," "one of the greatest inventions ever made in spectroscopy."[8] In the 20th century, the continued use of refined gratings, for example, by Zeeman at Leiden and Kayser at Bonn, was supplemented increasingly by interferometric methods, as produced among others by Michelson, Fabry, Pérot, and Benoît.[9] Thus, *grosso modo*, we have three phases of measurement in spectroscopy:

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[7] Konen (ref. 3), 227.

[8] Quotes from, resp., "President's address" (p. 481) in H.A. Rowland, "Remarks on the award of the Rumford Medals," *PA*, *19* (1883/84), 482–483, and Edward Charles Cyrill Baly, *Spectroscopy*, 3rd ed. (London, 1929), *1*, 28.

[9] See A.A. Michelson, "On the application of interference methods to spectroscopic measurements," *PM*, *30* (1891), 338–346, and *34* (1892), 280–299; cf. Michelson, Benoît, Fabry and Pérot (ref. 155), Weber (ref. 169).

1. Prisms (–1823, later only for special applications such as stellar spectroscopy)
2. Gratings (1823–1906, and later as a supplement to other methods)
3. Interferometers (from 1895 on, more frequently from 1901 on, especially for the definition of a primary and a few secondary standard wave lengths) with resolutions of up to 800,000.

The subject of this paper, the discovery of the redshift of Fraunhofer lines in the sun's spectrum, took place at the height of precision spectroscopy dominated by Rowland's gratings.

1. ROWLAND'S REVOLUTIONS

Henry Augustus Rowland (1848–1901) graduated as a civil engineer from the Rensselaer Polytechnic Institute at Troy, New York, in 1870.[10] He became instructor in physics in 1872 and assistant professor of physics in 1874.[11] In 1875, he became the first professor of physics at the newly founded Johns Hopkins University, a position which he held until his premature death in 1901. For the planning of his laboratory, he visited James Clark Maxwell in England and Hermann von Helmholtz in Berlin, working in the latter's laboratories for four months.[12] With the instruments and equipment he bought for more than $6,000, his laboratory became the best-equipped in the United States, and attracted many students. Furthermore, a well-equipped workshop in which new apparatus could be produced was closely connected with his laboratory. Despite reports of some deficiencies as a pedagogue, between 1879 and 1901 Rowland had 165 graduate students and 45 Ph.D. students, thirty of whom received stars in *American men of science.*[13]

Rowland's widest influence probably came through his gratings, which were used by all the important spectroscopists of the late 19th

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[10] About Rowland cf. obituaries and biographical sketches by R.T.G. in *Nature, 64* (1901), 16–17; T.C. Mendenhall, *BMNAS, 5* (1905), 115–140; J.S. Ames, *Science*, N.S., *13* (1901), 681–684, and *DAB, 16* (1935), 198–199; H.F. Reid, *APJ, 28* (1941), 117–119; H. Crew, *AJP, 17* (1949), 576–577, D.J. Kevles, *DSB, 11/12* (1981), 577–579; A.D. Moore, *Scientific American, 246* (1982), 118–126; S. Rezneck, "The education of an American physicist—Henry August Rowland," *AJP, 28* (1960), 155–162.

[11] According to *Biographical Record of the Officers and Graduates of the Rensselaer Polytechnic Institute 1824–1886* (Troy, 1887), 115, 117, 164.

[12] S. Rezneck, "An American physicist's year in Europe, Henry Rowland," *AJP, 30* (1962), 877–886; John David Miller, "Rowland and the nature of electric currents," *Isis, 63* (1972), 5–27.

[13] Robert Hugh Kargon, "Henry Rowland and the physics discipline in America," *ViA, 29* (1986), 131–136; cf. "List of scientific apparatus," Harvard University, *Library Bulletin, 11–12*, 302–304, 350–353, esp. 351–353, for the Baltimore Physical Laboratory.

and early 20th centuries:[14] Henri Deslandres and Alfred Cornu (who later worked on band spectra using Rowland's gratings), Pieter Zeeman (who employed Rowland's gratings in the experiments that led to the discovery of the influence of magnetic fields on spectra in 1896),[15] Carl Runge, August Kundt, the Vogel brothers, Heinrich Kayser, and Friedrich Paschen,[16] Janne Rydberg (Lund),[17] Arthur Schuster (Manchester), George Higgs (Liverpool), several investigators at Cambridge,[18] and one at the Royal University of Ireland.[19] George E. Hale used a Rowland grating from his early days at the Kenwood Observatory.[20] Charles Edward St. John, Arthur Scott King and their colleagues used one later at the Mount Wilson Observatory, Frank Wadsworth and others had one specially ruled for the Yerkes and Allegheny Observatory,[21] W.F. Meggers, K. Burns and others had theirs at the National Bureau of Standards, Washington, and, of course, Albert A. Michelson got one for Chicago.

By 1895, Rowland had sold more than 100 of his gratings to spectroscopists all over the world at a price determined by production costs.[22] By January 1901, sales totaled more than $13,000, which represents between 250 and 300 gratings sold at cost to physical and chemical laboratories as well as to astronomical observatories all over the world, not counting those gratings given away for free.

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[14] William McGucken, *Nineteenth-century spectroscopy. Development of the understanding of spectra* (Baltimore, 1969), esp. 135.

[15] Pieter Zeeman, *Researches in magneto-optics* (London, 1913), on 9ff.

[16] Robert Bezler, "Zur Geschichte des grossen Rowland-Gitters am Physikalischen Institut der Universität Tübingen," *Bausteine zur Tübinger Universitätsgeschichte, 3* (1987), 141–178.

[17] See J.R. Rydberg, "On a certain asymmetry in Prof. Rowland's concave gratings," *PM*, (5) *35* (1893), 190–199, also in *AA, 12*, 439–448.

[18] Catalogue 5 of the *Whipple Museum of the History of Science* lists Rowland gratings in the former possession of the Cavendish laboratory, the Institute of Astronomy, the Department of Physical Chemistry and of R.S. Whipple.

[19] W.E. Adeney and J. Carson, "On the mounting of the large Rowland spectrometer in the Royal University of Ireland," *PM* (5), *46* (1898), 223–227.

[20] Horace W. Babcock, "Diffraction gratings at the Mount Wilson Observatory," *ViA, 29* (1986), 153–174, and *Physics today, 39* (1986), 34–42, on 34, reporting that Hale later transferred his plane Rowland grating to the Mount Wilson Observatory.

[21] Cf. Frank L.O. Wadsworth, "On the aberration of the concave grating, when viewed as an objective spectroscope," *PM, 6* (1903), 119–156, on 121.

[22] See Miller (ref. 12), n. 2, Kevles (ref. 10), 579, Warner (ref. 4), 129; the manufacture of the gratings and quality testing was mostly carried out by Lewis E. Jewell, while distribution was left in the hands of Brashear. Cf. *John Alfred Brashear: The autobiography of a man who loved the stars*, ed. W. Lucien Scaife (Boston, 1925), 76, for further customers of Rowland gratings.

On receiving the gold and silver Rumford medals from the American Academy of Arts and Sciences in 1883/4, he disclosed how his interest in ruling gratings arose:[23]

> My attention was first called to the construction of dividing-engines by an inspection of a dividing-engine constructed by Professor W.A. Rogers, at Waltham, in this State [Massachusetts]. On returning to Baltimore, I devoted much time to the general problem of such machines; and, through the liberality of the trustees of the Johns Hopkins University, I was enabled to construct an engine.

Ruling technology

Like his predecessors, Rowland employed a "ruling engine" (figure 1), which guided a sharp, carefully chosen and mounted diamond point over a coated glass plate or metal surface.[24] After one grating line was ruled, a mechanism raised the diamond point and shifted the grating surface a short distance, whereupon the diamond ruled the next line. The straightness of the lines was easily achieved by guiding the diamond along two parallel metallic rails; the positioning of the diamond point after each ruling by exactly the same distance, no more than 0.00015 cm in Rowland's machine, constituted a greater challenge.

From studies carried out by William August Rogers (1832–1898) at the Harvard College Observatory, Rowland knew about the main sources of error in the gratings of Friedrich Adolph Nobert (1806–1907) and Lewis Morris Rutherfurd (1816–1892).[25] According to Rogers, even the best diffraction gratings of his time were subject to three classes of errors:[26]

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[23] Rowland (ref. 8), 482.

[24] Hugo Schroeder, "Ueber die Verwendung des Diamanten in der Präzisions-Mechanik," *Zeitschrift für Instrumentenkunde*, *7* (1887), 261–269, 339–347; J.S. Ames, "Henry August Rowland," Johns Hopkins University, *Alumni magazine*, Jan 1916, 92–99, on 96. Rowland's assistant Jewell became his expert for the choosing and purchase of diamonds, usually bought at Tiffany's, New York.

[25] Cf. Edward W. Morley, "Memoir of William August Rogers," *BMNAS*, *4* (1899), 187–199; W. Rollmann, "Friedrich Adolph Nobert," Naturwissenschaftlicher Vereine van Nue-Vorpommern und Rügen im Greifswald, *Mittheilungen*, *15* (1884), 38–58; G.L.E. Turner, "The contributions to science of F.A. Nobert," Institute of Physics, *Bulletin*, *18* (1967), 338–348; B.A. Gould, "Memoir of Lewis Morris Rutherfurd," *BMNAS*, *3* (1895), 417–441.

[26] William A. Rogers, "On the first results from a new diffraction ruling engine," *AJS* *19* (1880), 54–59, on 54; Morley (ref. 25); D.J. Warner, "Lewis M. Rutherfurd: Pioneer astronomical photographer and spectroscopist," *TC*, *12* (1971), 190–216, on 214f.

1. accidental errors of single subdivisions, mainly due to the irregular motion of the ruling diamond upon a non-homogenous metal
2. systematic, more precisely periodic errors, being a function of one revolution of the ruling screw
3. errors dependent upon the position of the nut upon the screw, equivalent to a varying pitch

The most surprising error was the periodic error in the separation of the lines; Rogers proved its existence in Nobert's, Rutherfurd's, and his own gratings and showed it to be of the order of magnitude of 1/20,000 inch.[27] He attributed the unwanted periodicity not to the screw itself but to its mounting, but wherever it came from, periodic errors in diffraction gratings caused trouble. They caused the appearance of additional faint lines in the spectrum, the so-called "ghosts," which could easily be (and often were!) mistaken for true lines.

The versatile mathematician, scientist, and philosopher Charles Sanders Peirce (1839–1914) confirmed the suspicion that the periodic errors in the line separation ϵ caused ghosts through a rigorous mathematical analysis and subsequent experimental checks using various Rutherfurd gratings.[28] Rowland's first challenge was to eliminate this type of error. Another troublesome factor was *irregular* variation of ϵ, which, according to Rowland, resulted in a general loss in sharpness of the spectral image. But Rowland was quite sure that his gratings were too precise for ghosts haunting other experimenters: "The ghosts are very weak in most of my gratings."[29] In any case, he thought he could distinguish between artifacts of a badly manufactured grating and real lines. We learn more in an article about screws that Rowland wrote for the 9th edition of the *Encylopædia Britannica.*[30] According to this text, "ghost lines" tended to change their positions relative to other lines, while true lines only changed in their scale, but not in their relative positions, under a certain adjustment of his spectrometer. Apparently, this way of discriminating the "good"

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[27] W.A. Rogers, "On a possible explanation of the method employed by Nobert in ruling his test plates," *PA, 11* (1875), 237–255, on 243 for a description of the method employed to measure directly the magnitude of the periodic error. Rogers preferred the Nobert grating over Rutherfurd's.

[28] C.S. Peirce, "Note on the progress of experiments for comparing a wave length with a metre," *AJS, 18* (1879), 51; H.A. Rowland, "On concave gratings for optical purposes," *PM, 16* (1883), 197–210, on 198, citing Peirce's more detailed paper in the *American journal of mathematics*, 1879; A.A. Michelson, "On the spectra of imperfect gratings," *APJ, 18* (1903), 278–286.

[29] H.A. Rowland, "A few notes on the use of gratings," *JHUC, 8* (1889), 73–74.

[30] H.A. Rowland, "Screws," *Encyclopædia Britannica* (9th edn.), *21* (1884), 552–553, reprinted in *The physical papers of Henry Augustus Rowland* (Baltimore, 1902), 506–511.

from the "bad" lines worked better than the old method of distinguishing between them, namely changing the order of the lines observed. Ghosts revealed themselves by appearing in all orders of the spectrum:[31]

> They [the ghosts] never cause any trouble, as they are easily recognized and never appear in the solar spectrum. In some cases the higher orders of ghosts are quite as apparent as those of the first order....Hence, to avoid them, obtain magnification by increasing the focal distances instead of going to the higher orders.

These are rare hints. Observation routines were seldom mentioned in textbooks or scientific articles but only acquired through practical work in the laboratory under the supervision of a skillful teacher.[32]

In all ruling machines made after Nobert, the transport of the grating under the ruling diamond point was activated by the turning of a screw by an exact number of degrees.[33] Rowland realized that special care had to be invested in the design and production of the screw and in its proper installation (with the aid of many adjustment screws) to get the necessary degree of uniformity of the line separations.[34] The screws, up to 25 cm (10 inches) long, were made out of special flawless steel in a painstaking process that could take up to 14 days. The raw screw, held in a nut, was then continuously tightened in a process that could take up to another 14 days, again under conditions of constant temperature with continuous removal of friction heat through liquid grinding materials (such as emery powder and oil or optical rouge) and with a regular switch of the direction of the screw in the nut every ten minutes throughout the whole procedure to avoid any asymmetries in the screw driving. Rowland claimed that for a screw produced in this manner, "there was not an error of half a wave-length, although the screw was nine inches long," indeed a remarkable precision for a mechanically produced object.[35]

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[31] Rowland, *Papers* (ref. 30), on 519.

[32] Cf. Ames (ref. 24), on 97: "I have seen Rowland stand by the machine with a screw driver in his hand looking at the specimen of ruling and then say 'I think I'll try this.' Then he would poke his screw driver in, doing something which would be impossible for anyone else to understand clearly; and the chances were that after one or two such attacks on the machine it would work all right. When I would ask him what he had done, and why he had done it, he was never able to explain fully. The truth was that his knowledge of machines of all kinds was in part a process of instinct."

[33] Nobert's ruling engine was still based on a large circle divider.

[34] For a survey of the contemporary technology of screw production, see Charles and John Jacob Holtzapfel, *Turning and mechanical manipulation* (5 vols., London 1881), *4*, reprinted as J.J. Holtzapfel, *Hand or simple turning: Principles and practice* (New York, 1976), esp. chapt. 10.

[35] Rowland (ref. 8), on 482.

Rowland's screws typically had 20 threads to the inch, and were turned at a constant angle (1/720 of a full circle) by a toothed wheel; the grating thus moved about $1/20 \cdot 1/720 = 1/14400$ inch after each line was ruled.[36] A motor run by water power (because of its greater regularity and higher reliability than electric power), drove the process, which could take up to 14 days. Every effort was made to keep the room temperature constant, because even minute fluctuations might spoil the grating.[37] To Lord Rayleigh (as to many others), he wrote in March 1882 after realizing that his ruling engine really worked well and reliably:[38]

> I have just completed in our workshop a machine for ruling gratings and it is a great success, among gratings fully equal if not superior to Rutherfurd's and of larger size....Rutherfurd could only make one good grating out of many, but my machine makes them as good as his *best every time.*

Although he exaggerated—Jewell later reported the large number of wasted rulings eliminated after the quality test he conducted—Rowland rightly claimed the superiority of his gratings over those of Rutherfurd and other contemporaries. Their excellence was soon acknowledged by the scientific community.[39]

Rowland's gratings thus excelled because of this unprecedented care in the manufacture of the screw and in its careful mounting, which Rowland found "even more difficult to make without error than the screw itself."[40] His ruling engines were able to rule gratings of up to 110,000 lines into a metal surface with an accuracy of one millionth of a millimeter.[41] Although others soon tried to improve on Rowland,[42] the only person to better his performance was Rowland himself. He built three ruling engines altogether, the first one, completed the autumn of 1881, could rule 14,438 lines per inch; the

[36] This line distance was considered by him to be the practical limit "with the ordinary conditions of ruling;" see ibid.

[37] T.C. Mendenhall, "On the determination of the coefficient of expansion of a diffraction grating by means of the spectrum," *AJS, 21* (1881), 230–232.

[38] Rowland to Lord Rayleigh, 6 Mar 1882, as quoted in Strutt 136f. and in Warner (ref. 4), 128 (emphasis orig.); cf. Warner (ref. 26), 215.

[39] Lord Rayleigh, "President's address," British Association for the Advancement of Science, *Report* 1884, 17: "the magnificent gratings of Rowland are a new power in the hands of the spectroscopists, and as triumphs of mechanical art seem to be little short of perfection."

[40] H.A. Rowland, "Preliminary notice of the results accomplished in the manufacture and theory of gratings for optical purposes," *PM, 13* (1882), 469–474, on 471.

[41] Cf. H. Kayser, *Erinnerungen aus meinem Leben*, unpubl. typescript [1936], 187.

[42] E.g., Ertel, Fraunhofer's successor in Munich, Thomas Grubb in Dublin, and Adam Hilger in London; see Warner (ref. 4) for references.

second and third, of 1889 and 1894, ruled 20,000 and 15,020 lines per inch, respectively, over as much as 25 square inches, and each incorporated improvements in the adjustment mechanism of the main screw and the carriage of the ruling diamond.[43]

Rowland used to say that "No mechanism operates perfectly—its design must make up for imperfections."[44] An indication of the practical application of this dictum appears from a description and evaluation of the carriage system given by one of Rowland's direct descendants in the grating art, John Strong:[45]

> The grating grooves are ruled on the grating blank by repeated, straight-line strokes of a diamond point—a point guided by a carriage that spanned the blank. It is carried on divided cross-ways; guided by one sliding shoe on the right side of a rectangular-bar at one end of the carriage, together with a second shoe on the other end of the carriage, bearing on the left side of another rectangular-bar, aligned and parallel.

After Rowland's death, Professor Joseph Ames suggested to John Anderson that the shoes might better slide on the same side, right or left; Anderson never followed up the suggestion, because he realized the intricate compensation of imperfections granted by the symmetric arrangement. Strong again:[46]

> In Rowland's arrangement, using opposite sides, the motion of the diamond midway between the two shoes becomes immune to lateral shifts due to the lubricating oil thickness, as long as the variations of the oil film during the ruling stroke are equal. And the arrangement also makes the motion immune to wear.

This clever built-in stability under unavoidable variations of the oil film and wear enabled Rowland to claim that the diamond point repeated a straight line stroke to within the incredible precision of half a wavelength of visible light. A similar remark applies to Rowland's overcoming of the mechanical difficulties in ruling on concave surfaces by allowing for a judicious tipping of the spherical blank on its carriage resulting in a nearly invariant angle between the diamond and the ruled surface.[47]

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43. See Rowland, *Papers* (ref. 30), Appendix; Cf. the detailed report by J.S. Ames, "The present condition of Rowland's ruling machines," *JHUC, 4* (1906), 62–65. Kevles (ref. 10), 578 even reports on a machine capable of ruling up to 43,000 lines per inch but I could not find independent confirmation for this value.

44. Rowland, as quoted in John Strong, "Rowland's diffraction-grating art," *VtA, 29* (1986), 137–142, on 137.

45. Ibid.

46. Ibid. Cf., however, Joh. Adolf Repsold, *Zur Geschichte der Astronomischen Messwerkzeuge* (2 vols., Leipzig, 1908–14), *2,* 140f, for critical remarks about the prismatic form of the guiding shoes.

47. Cf. Strong (ref. 44), 141; Ames, "A description of the dividing engines designed by Professor Rowland," in Rowland, *Papers* (ref. 30), 691–697, and plates 1–5.

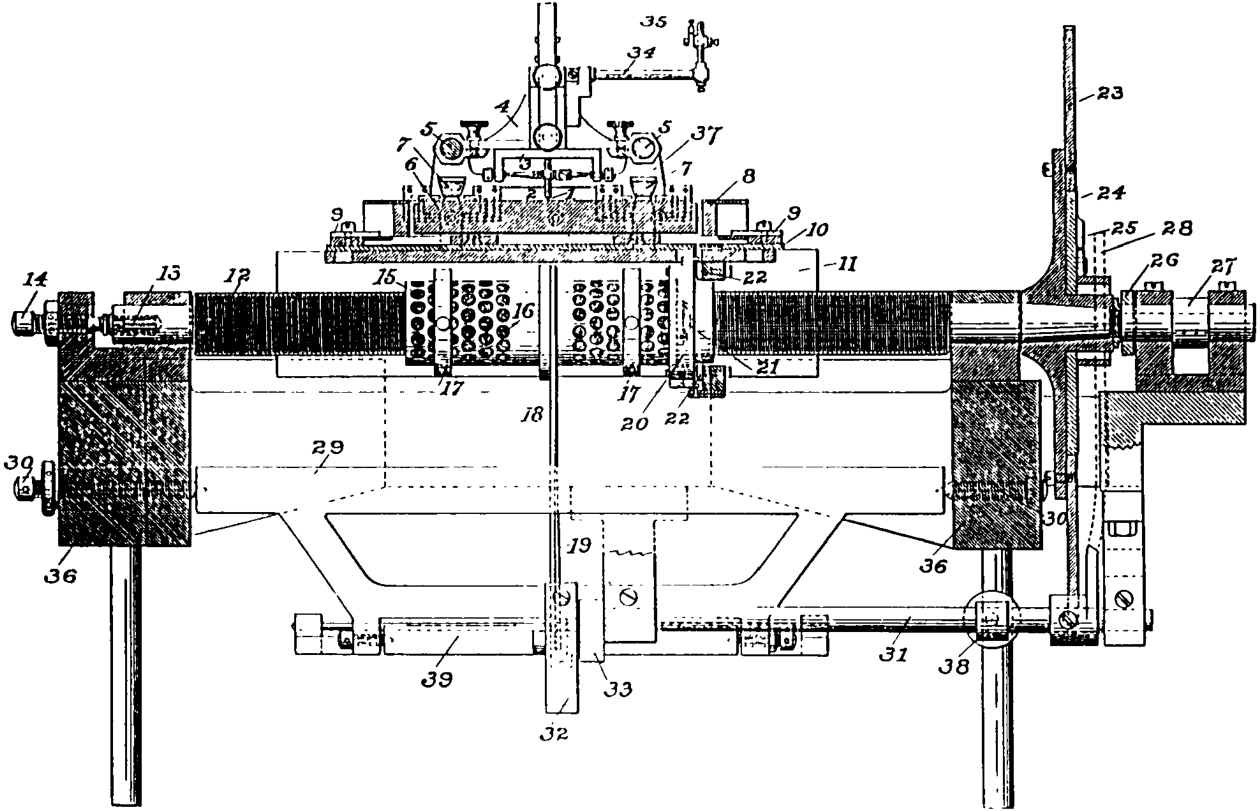

FIG. 1 Transverse sectional elevation view of Rowland's ruling engine, showing feed-screw (12), nut (15), adjustable diamond holder (2) with diamond point (1), plate to be ruled (6) and the intricate corrector frame mechanics. Ames (ref. 47), plate 5.

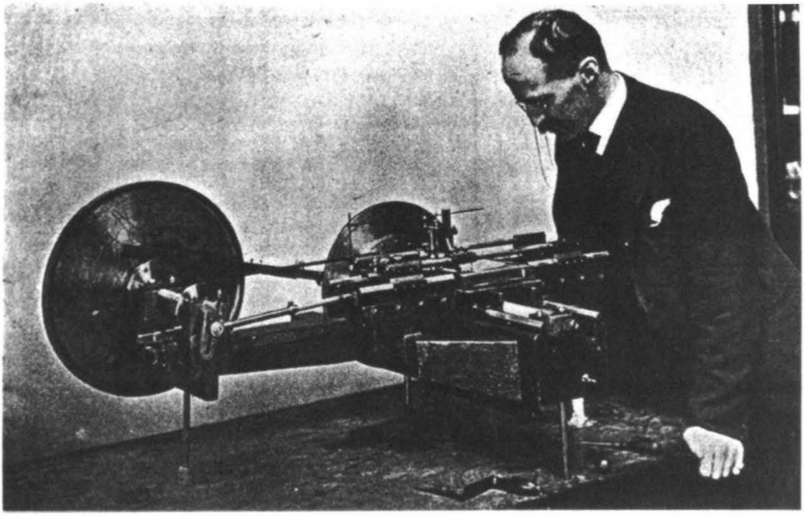

FIG. 2 Rowland in front of his first (and smallest) ruling engine. Rowland (ref. 30), 43, Appendix.

Rowland did not make public his procedures for testing his gratings and ruling engines. In this silence he copied what he believed was Nobert's policy of keeping testing a "trade secret."[48] In fact, Norbert had lifted the veil slightly in an obscure Prussian journal in 1845, but not enough to allow his contemporaries to reproduce his rulings easily.[49] The same applies to Rowland's direct predecessor in the manufacturing of diffraction gratings, Lewis Morris Rutherfurd;[50]

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[48] Rogers (ref. 26), 238: "Nobert has well kept the secret of his process;" John Mayall, "Nobert's ruling machine," Royal Society of Arts, *Journal*, *33* (1885), 707–715; Mendenhall (ref. 10), 124. For Rutherfurd's techniques, see L.M. Rutherfurd, "On the construction of the spectroscope," *AJS*, *39* (1865), 129–132, and A.M. Mayer, "Spectrum," *Appleton's Cyclopedia* [2nd edn.], *15* (1878), 238–254, 243f; for Rowland's, H.A. Rowland, "Preliminary notice on the results accomplished in the manufacture and theory of gratings for optical purposes," *JHUC*, *17* (1882), 248–249, also in *PM*, *13*, (1882), 469–474, and in *Nature*, *26*, (1882), 211–213, and J.S. Ames, "The concave grating in theory and practice," *JHUC*, *73* (1889), also in *PM*, *27*, (1889), 369–384.

[49] F.A. Nobert, "Ueber Kreistheilung im Allgemeinen und über einige, bei einer Kreistheilmaschine angewendete Verfahren zur Erziehung einer grossen Vollkommenheit der Theilung derselben," Verein zur Beförderung des Gewerbefleisses in Preussen, *Verhandlungen*, (1845), 202–212; W. Rollmann (ref. 25), on 53; Rogers (ref. 26), 237: "You properly ask me if I can reproduce these rulings. I frankly answer that I cannot."

[50] Cf. B.A. Gould (ref. 25); Warner (ref. 26).

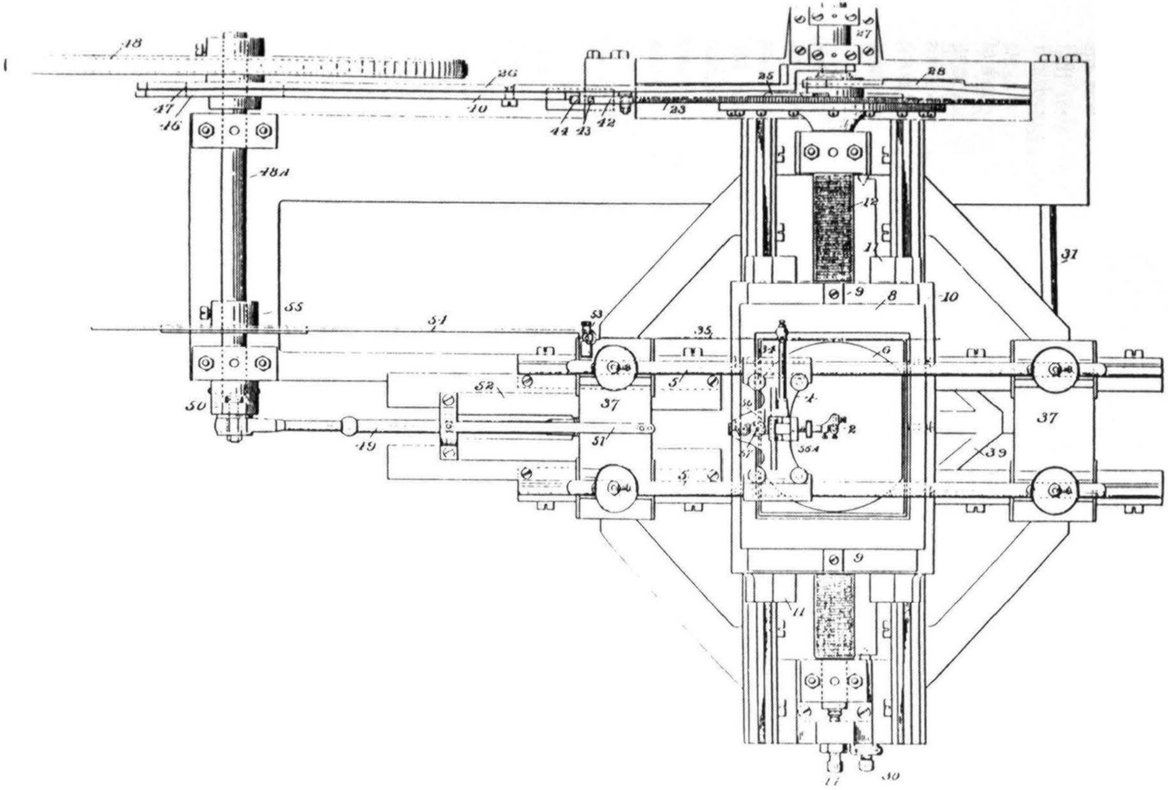

FIG. 3 View of Rowland's ruling engine, showing again the diamond point holder (2), its carriage (4), the divided crossway (5), and the grating blank (6). Ames (ref.47), plate II.

Rowland himself remarked that "many mechanics in [America] and in France and Germany have sought to equal Mr Rutherfurd's gratings, but without success."[51] Even if Rowland had wanted to transmit his methods, he could have done so only to someone with whom he was in daily practical collaboration. The only one who qualified was his mechanician Theodore C. Schneider, who died in the same year that Rowland did. *The Baltimore Sun* of April 17, 1901, commented:[52]

> A question which is being asked since the announcement of the death of Professor Rowland is, Will his art of making microscopically fine gratings on a concave surface for spectroscopes be lost with him, or was his work left in such conditions that other scientists will be able to take it up where he left off and continue to furnish the gratings which have practically revolutionized the art of spectroscopic analysis?
>
> Dr. Remsen said last night that he is sure the art died with Professor Rowland. About a month ago Mr. Theodore C. Schneider, the mechanic who was trained by Professor Rowland and who was the only man besides him who could construct the machine, died. Since Mr. Schneider's death Mr. Charles Childs, who had long been associated with Professor Rowland, had been learning the art. Mr. Childs has made rapid progress, but it is thought by those who are in a position to know that he has not yet gotten to the point where he can count upon successfully carrying on the work without having the master mind to direct him.
>
> Mr. Schneider, skilled man that he was and working in harmony with the ideas of the inventor, would occasionally strike obstacles which were entirely beyond the range of mechanical skill and required the closest application of pure theoretical reasoning before the way could be discovered for the resumption of operations.

It took about ten years, before Rowland's work on the ruling of gratings was taken up by John Anderson.[53]

Schneider did manage to transmit some information about Rowland's techniques to Heinrich Kayser, who tried to visit Rowland in Baltimore during his first trip to America. Rowland was out of

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[51] Rowland (ref. 40); cf. Henry Draper, "On diffraction spectrum photography," *AJS, 6* (1873), 401–409, also in *PM, 46*, 417–425, and C.A. Young, "Note on the use of a diffraction grating as a substitute for the train of prism in a solar spectroscope," *AJS, 5* (1873), 472–473, on 472: "the spectra furnished by these plates far exceed in brilliance and definition anything of the kind ever before obtained."

[52] "Death of Prof. Rowland," *The Sun* [Baltimore], 17 Apr 1901, p. 4, col. 2, "None to fill place. Science suffers irreparable loss in death of Prof. Rowland. How great physicist died," ibid., p. 12, col. 1–3; and "Great men mourn," ibid., 4–5.

[53] See H.D. and H.W. Babcock, "The ruling of diffraction gratings at the Mount Wilson Observatory," Optical Society of America, *Journal, 41* (1951), 776–786; Strong (ref. 44), Babcock (ref. 20); R.F. Jarrell, "Gratings, production of," *Encyclopedia of spectroscopy* (New York, 1960), 174.

town. Schneider, however, was at the laboratory. Inspired by their common language (the mechanician was of German descent) and an immediate sympathy, Schneider told Kayser some of the tricks of the trade, at which he had been working since 1876 (Rowland knew Schneider from his student years at Rensselaer). Schneider had built the ruling engine and oversaw the manufacture of the gratings.[54] John Brashear produced and polished the spherically curved grating surfaces onto which Schneider ruled the lines.[55]

Among the details that Kayser learned from Schneider was the method of adjusting the ruling screw:[56]

> Schneider showed me all the installations necessary to manufacture the screw, the critical part, and he explained with admiration, how Rowland adjusted the setting of the screw. Its end must be exactly positioned to a millionth of a millimeter, which is achieved by a number of fine adjustment screws. The procedure is as follows: on a test plate, a certain number of lines is ruled with the ruling engine, say one thousand, then the grating surface is turned a bit, and now again about one thousand lines are ruled across the first set of lines. If the main screw is correctly adjusted, and thus the distances between the lines are everywhere the same insofar as they depend upon the position of the screw, then the points of intersection of both systems of parallels are on straight lines too. But if the distances slightly vary, then the points of intersection follow a curve, one sees some form of moiré. Anyone can see this and then knows that the adjustment of the ruling screw is not yet perfect. Rowland, however, looks at this moiré for a few minutes, then he says: "Turn adjustment screw A by about one twentieth of a full turn to the right, screw B by about one tenth to the left. You might also turn screw D by about one fiftieth to the right." And then often the adjustment is complete. How he is able to decipher this from the moiré is hard to grasp.

Rowland and his collaborators had already reached such a level of refinement, that spectroscopists who tried to reproduce his results and did not have the privilege of having been one of his pupils, occasionally had serious trouble. Even the "master of light," Albert Abraham Michelson, who was attracted to the technological challenge of the ruling engine around 1904, later regretted "ever having got this bear by the tail."[57] Here is what Johannes Hartmann of the Potsdam Observatory wrote in 1903 about Rowland:[58]

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[54] Kayser (ref. 41), 190; Ames (ref. 43), 62.

[55] Mendenhall (ref. 10), 125; Warner (ref. 4), 129; J.A. Brashear (ref. 22).

[56] Kayser (ref. 41), 190f.

[57] See Babcock (ref. 29), 154; cf. A.A. Michelson, "The ruling and performance of a ten inch diffraction grating," American Philosophical Society, *Proceedings, 54* (1915), 137–142.

[58] J. Hartmann, "A revision of Rowland's system of wave-lengths," *APJ, 18* (1903),

> How Rowland obtained the screw value [in his readings of wavelengths] with sufficient accuracy for such long distances is not to be readily ascertained from his publications, which, indeed, contain so few data as to the measurements themselves that a test of them is impossible.

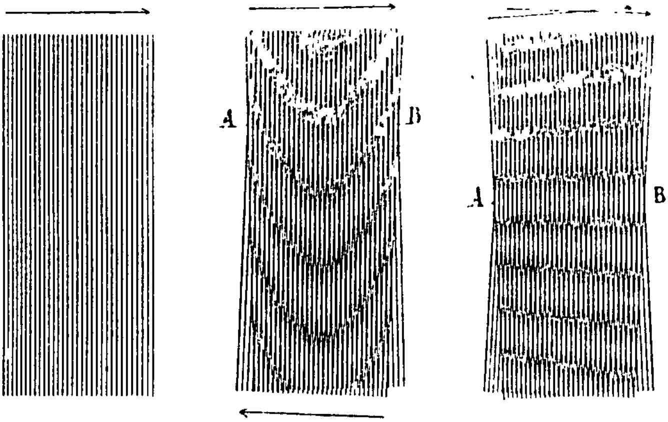
Fig. 3. Fig. 4. Fig. 5.

FIG. 4 "Moirés" produced by the superposition of two rulings with a slight systematic error of one-sided increase of intervals amounting to 1/240 mm. A. Cornu, "Sur les diverses méthodes relatives à l'observation des propriétés appelées anomalies focales des réseaux diffringentes," *CRAS*, *116* (1893), 1421–1428, on 1426.

Concave gratings

During the winter of 1882/3, Rowland spent much time trying to account for the influence of irregular variation of the line distance in his gratings. Surprisingly, many gratings proved much more reliable than the theoretical estimates given by Peirce and himself had led him to expect, and the quality of the gratings often differed for different sections of the spectrum. Thomas Young, in a comment Rowland knew of, had observed that often the quality and sharpness of the image of a grating can be improved considerably by slightly bending its surface.[59] Perhaps the unavoidable distortions in the images of plane gratings might have been compensated by a slight curvature? If

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167–190, on 169; cf. Konen (ref. 76), 792.

[59] Rowland (ref. 29), 199.

so, the flaw could be made a virtue by ruling gratings on curved surfaces.[60]

A further consideration may have strengthened Rowland's interest in following this direction. Whether spectroscopists used prisms or plane gratings, they had to employ at least two lenses to get sharp images: the so-called collimator, essentially to guide the light directly onto the prism or grating surface, and a second lens to project the diffracted light for observation or photography. Both lenses had to be corrected; the collimator for achromatism, the camera lens with its large focal plane for spherical aberration.[61] Even in the setup devised by Littrow,[62] which employed the same lens as collimator and projector, there were serious disadvantages. The lenses *absorbed* parts of the spectrum, in particular in the infrared and ultraviolet regions; the *dispersion* of the glass varied too much to produce a well-scaled image; and other imperfections of the lenses gave rise to further *distortions*. These blemishes menaced high precision spectrometry.

Accordingly, around 1882 Rowland broke with the practice of several generations of spectroscopists and decided to eliminate all lenses. A spectroscopic grating ruled on a concave surface works like a burning mirror, thus eliminating the problem of projection of the image of the spectrum. The collimator too became superfluous for a concave grating, since a simple slit, wide enough to allow the light from the source to illuminate the whole surface of the grating, was sufficient. Slit, concave grating, photographic plate, and a stable mounting were all Rowland needed for his new spectroscope. Rowland soon realized the optimal geometrical configuration of the three basic components of his apparatus: the slit (functioning as the source of light in the geometric analysis of the apparatus), the grating, and the photographic plate should all be installed on a large circle of diameter equal to the radius of curvature of the spherical grating.[63] This insight was based on a detailed mathematical analysis of the optical characteristics of his new instrument, reported to the London Physical Society in 1882, which proved that automatic focusing occurred

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[60] Anon (ref. 52); Ames (ref. 24), 95; Strong (ref. 44), 138ff.

[61] In spectrographs, the difference in the focal length for the different parts of the spectrum was compensated for by slightly bending the photo plates, hence the use of such thin glass plates (Jost Lemmerich, personal communication).

[62] Cf., C.F. Brackett, "Note on the Littrow form of spectroscope," *AJS*, *24* (1882), 60–61; Konen (ref. 3), 223, 226.

[63] Rowland (ref. 28); W. Baily, "On the spectra formed by curved diffraction gratings," *PM 15* (1883), 183–187; R.T. Glazebrook, "On curved diffraction-gratings," *PM 15* (1883), 414–423; E. Mascart, "Sur les résaux métalliques de M. H.-A. Rowland," *Journal de physique*, 2 (1883), 5–11; and Wadsworth, "The modern spectroscope," *APJ*, *3* (1896), 47–62, esp. 54–60.

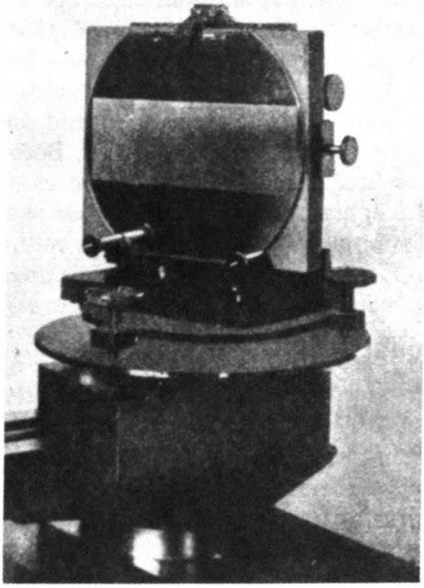

FIG. 5 Photograph of a six-inch concave grating, especially manufactured for Zeeman by Rowland (central part of the upper circle) including adjustment screws and a turning lathe support. Zeeman (ref. 15), 10.

whenever the slit, the grating with spherical curvature radius R, and the photographic plate resided on a circle of radius $R/2$, and that the best optical image would be obtained if grating and photographic plate were nearly opposite one another.

Rowland chose the following *proportions* for his spectroscopes:

- a spherical radius of curvature of 21.6 feet (10.8 feet for the early gratings), which determined the size of the whole apparatus
- a diameter of the spherically concave grating surface of up to 6 inches
- a line density on the gratings of 7,200, later (around 1887) of 14,400 lines per inch and finally (1896) of 20,000 lines per inch
- a micrometer run of 5 inches with a precision of 1/20,000 inch.

With these dimensions Rowland could arrive at dispersions of one second of arc (=0.0012 inch) so that the sodium doublet had a width of about 4 mm (figure 6). Figure 6 shows a Rowland grating set up for business; notice the solid supports for all pieces of instrumentation.

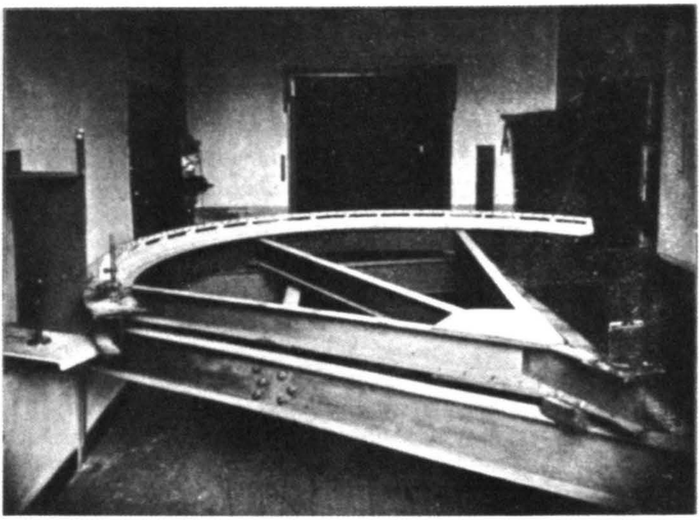

FIG. 6 A spectrograph in Zeeman's Amsterdam laboratory, with a radius of curvature of 3m and a Rowland concave grating of 4 inches and 52,000 lines. The grating is in the lower right corner of the picture; the slit at the left edge; and, in the center, the segment of the circle with the angle indicator and mounted camera (in front of the second dark surface from the left). The whole apparatus rests upon two steel supporting beams anchored to the walls of the laboratory building for maximum stability. Zeeman (ref. 15), 12.

Rowland's geometrical analysis of 1883 had shown that he could photograph the whole spectrum by moving camera M and the grating GA along the circle with the diameter of the radius of curvature AM of the concave grating (see figure 7). The direct reflection of the light coming from the slit S would then show up at the point O symmetrically mirrored to S across the symmetry axis AM of his apparatus. Diffraction patterns of higher orders would appear in sharp focus in both directions from this point O on the circle SGAM.

Opposite to the grating GA, near the point M of the circle, the distances between the spectral lines are directly proportional to the wavelengths. Because of the very large diameter of the circle (21.6 feet!), this region of linearity, best suited for spectrometric high precision measurements, was about 10 cm long. For a region of about 6 inches, Rowland estimated the obtainable accuracy to be as good as 1/1,000,000, and for about 18 inches still as high as 1/350,000. Further away from M, proportionality of distances of spectral lines to

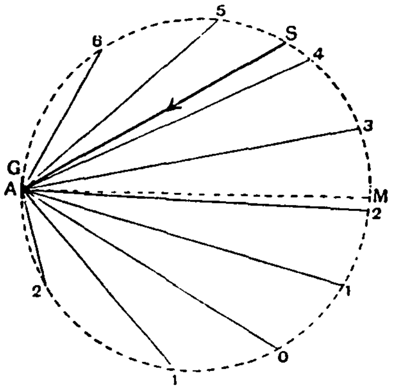

FIG. 7 Observations of different orders of diffraction using a Rowland grating GA with radius of spherical curvature AM, light coming from the slit S and registered in camera M moving along the circle SGAM. The numbers refer to the order of diffraction in the spectrum. Rowland (ref. 28).

their wavelength no longer exists, because of the effects of the curvature of the circle SGAM, which distorts the images of the spectrum.[64] Because measurements in the second or third order are most reliable (they are still bright enough for clear observation of the spectral lines with a telescope, best suited for fine grained photography, and free of reflections and ghosts that vex in the first order), the slit typically stood somewhere between the grating and the camera (in figure 7, the camera M is in the second order of the spectrum). To cover the whole range of the spectrum, Rowland preferred to move the grating GA *together with* the camera M along the circle SGAM, keeping the slit S fixed, so that the camera and the grating always opposed each other and thus remained in the region of best approximation to linearity.

Rowland's spectrograph and its simple geometry made the routines much easier for him than for his forerunners, who had to repeat the focussing of their images for each exposure anew. No wonder it received his enthusiastic praise: "nothing can exceed the beauty and simplicity of the concave grating when mounted on a movable bar....Thus the work of days with any other apparatus becomes the work of hours with this."[65] Later, some experimenters slightly varied Rowland's original procedure by keeping both the slit S and the grating fixed and moving the camera along the circle. To do so, they had

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[64] Rowland (ref. 28), 203–204.

[65] Ibid., 205.

to use photographic plates bent with a radius of curvature of AM/2. This idea seems to go back to Sir William de Wiveleslie Abney (1844–1920); the first to practice it was Carl Runge (1856–1927); and later Kayser and his pupils employed it in their measurements of wavelength normals in the spectrum of iron.[66]

Much craftsmanship was required in the actual installation as well as the manufacture of Rowland gratings. The whole apparatus had to be anchored firmly. Kayser reported that the practical utility of the precious Rowland grating in his possession was largely diminished throughout his time in Berlin because of nearby traffic on the street just outside his laboratory:[67]

> So I worked many a night; since it soon became apparent that during the daytime the vibrations in the building were much too strong; but even at night, not even one fifth of the exposures [taken with the Rowland grating] were usable. Because of this, a lot of material and time was wasted, and only when I made a new installation [of the Rowland grating] in my own institute at Hannover a couple of years later, could I earn the fruits of my labor....
>
> [For the installation] a [steel] rail curved in the form of a semicircle with a diameter of 6 m was needed, which had to be manufactured with very high precision, that is, on a turning lathe. In Germany, a turning lathe of this size only existed at Krupp in Essen, where it was needed for armored turrets of tanks. Prof. Fuchs...had contacts at Krupp, the library of which he supervised, and through him came the request to Krupp to supply such a rail. Krupp not only did this, he also funded the whole installation, which includes a massive cement foundation and other metal pieces, and sent us workers from his factory who installed the whole thing....Ever since, it was possible to work with the large grating without wasting time, and to obtain accurate measurements.

This remarkable statement again demonstrates the close interdependence of measurement technique within science and industrial production processes. The accurate manufacture of the steel support was crucial for making effective use of the precision of Rowland's concave grating, because the slit, camera, and grating had to be on a very accurate circle, to guarantee the automatic focussing. Apart from vibrations, temperature variation also threatened high precision spectroscopy. Zeeman's new laboratory (built in 1921–23), had a room with a Rowland grating and a special control that held temperature constant to 0.01 degrees Celsius.[68]

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[66] Zeeman (ref. 15), 12; Kayser (ref. 41), 235f; H. Konen, "Uber die Kruppsche Gitteraufstellung im physikalischen Institut der Universität Bonn," *ZwPh, 1* (1903), 325–342.

[67] Kayser (ref. 41), 120, 235f; see also H. Konen (ref. 66).

[68] G.C. Gerrits, "Zeeman," in *Grote Nederlanders* (Leiden, 1948), "Zeeman," 473–501, on 480.

Kayser's unpublished autobiography makes clear the impact of Rowland's new concave gratings:[69]

> I myself had begun to work spectroscopically, namely I wanted to determine and measure spark spectra photographically. Only, I did not make much progress; there were no suitable instruments available. I only had some lenses and prisms at my disposal, and that it is not quite so easy to build a spectroscope with these everyone today knows. Then, in the year 1883 a paper appeared, by Rowland in Baltimore, in which he reported on the production and performance of his concave gratings, which had brought about the rapid developments within spectroscopic measurement. I showed this paper to Helmholtz and asked him whether he could not obtain such a grating through Rowland for the Physical Institute. Helmholtz said that I myself should write to Rowland in his name. Rowland, who had formerly worked under Helmholtz, responded in the friendliest manner to the request, and sent us a grating as a gift.

We see here that Rowland was not only central as the *inventor* of the new type of grating, but also as its actual *producer*; through Brashear, his gratings were sent to institutes for chemistry, physics, astronomy, and astrophysics all over the world.[70] Owing to the small output of Rowland's shop of about 10 gratings per year, the possession of an authentic Rowland grating became one of the hallmarks of the quality of an institute.[71] Even among these gratings, a hierarchy of quality existed, based on comparisons made by Rowland himself. Kayser again:[72]

> I received a letter from Rowland, in which he told me that he was sending one of his gratings, which was for sale, to an exhibition of instruments at Berlin. He informed me of this, because he would be very happy if this grating would end up in my hands, since I had given sufficient proof that I know how to use it. This was [according to Rowland] the second best grating ever made in his workshop; he was keeping the very best, of course, for his own use.—Since I was not in the position to buy the grating myself, I sent the letter to Helmholtz with the request that he might ask the [Prussian] Academy [of Science at Berlin] to buy it and then to lend it to me. And so it happened, and the

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[69] Kayser (ref. 41), on 119–120. Cf. Kayser's letters to Rowland, 31 Jul and 19 Nov 1882, 11 May and 4 Aug 1883, preserved in RP.

[70] The use of at least one, often several Rowland gratings is acknowledged, e.g., by Kundt, H.W. Vogel, H.C. Vogel, Gieseler, Kayser, Konen, Runge, Paschen, Back, Higgs, Lockyer, Meggers, Hale, and many others.

[71] Adeney and Carson (ref. 19), 223, quote from a letter by Brashear: "You are very fortunate in getting this grating, for no one knows when we will get another." Cf. note 96.

[72] Kayser (ref. 41), 146.

> marvellous grating was put to extensive use, first by me in Hannover, then, once I got the permission to take it with me upon my move to Bonn, by me and my students [there]. Over a third of the approximately 150 spectroscopic studies carried out under my supervision at Bonn were made using this grating.

The impact of Rowland's gratings was amplified by improvement in photography, especially dry plate processing, to which Rowland contributed an emulsion that enabled him "to photograph from the violet down to D line."[73] The enlargement permitted by photography also helped by revealing many lines, hitherto supposed to be single, as double lines; Rowland published his results in two *Photographic maps* of the solar spectrum.[74] As an obituarist summed up:[75]

> A new weapon was placed in the hands of spectroscopists; it became possible to photograph spectra directly without the use of prisms or lenses, and with a greatly increased dispersion and resolving power; the beautiful maps issued at a later date by Rowland himself and by Higgs of Liverpool are striking evidences of the value of the grating; the additions to our knowledge arising from this one discovery are already enormous; much has been achieved which, without it, would have been impossible.

This is also true of the minute shifts of spectral lines in the sun's spectrum discovered around 1890 in Rowland's laboratory at Baltimore.

New standards

Rowland and his collaborators put together extensive tables of spectral lines, some 20,000 in all running from 2100 to 7100 Å. No one then could put forth a plausible explanation of their origin and magnitude. Rowland's research program was a huge Baconian exercise in data taking. He referred the wavelengths of all the 20,000 lines in the solar spectrum he recorded to the absolute value of the primary reference line, the D_1-component of the sodium D-doublet. The wavelength of this line had been measured independently by many different observers; notably, Ångstrom and Thalén in Uppsala, Muller and Kempf in Potsdam, Kurlbaum in Berlin, Peirce at the U.S. Coast Survey, and, in 1887/8, Louis Bell in Rowland's laboratory, in a very careful determination. In 1887, Rowland took all these published

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[73] Rowland (ref. 8), 483; Jon Darius, *Beyond vision* (Oxford, 1984), for the history of scientific photography.

[74] H.A. Rowland, *Photographic map of the normal solar spectrum* (Baltimore, 1888), and *Photographic map of the B and D lines and carbon bands of the solar spectrum* (Baltimore, 1889).

[75] R.T.G. (ref. 10), 16–17.

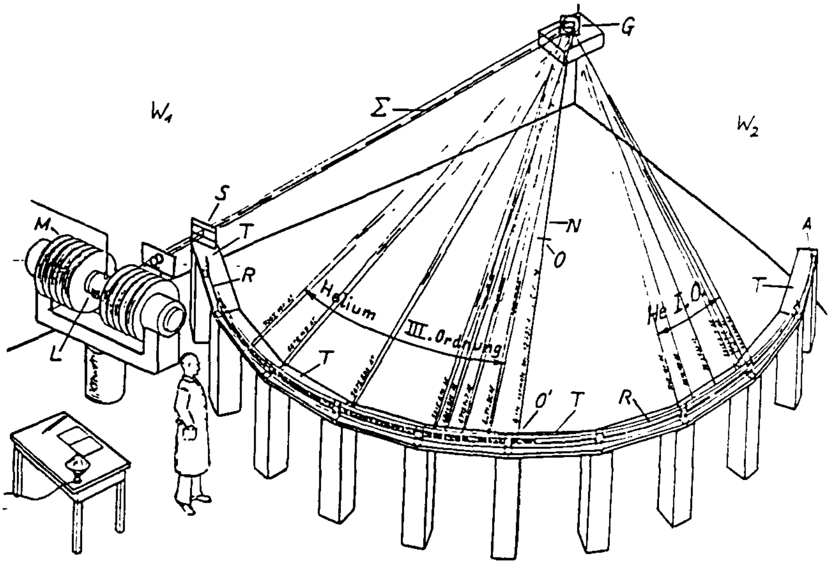

FIG. 8 Sketch of the "Rowland Room" at the Physical Institute, University of Tübingen, with Rowland's concave grating G installed at the upper right and the camera movable on the semi-circle AS covering several orders. Here the spectrum of helium is being tested under the influence of a large magnet. Bezler (ref. 16), 144.

values for the D_1 component of the sodium doublet, attributed relative weights to them, and put forward the weighted average as the *definitive absolute* value to serve as the *conventional* unit for all further precision measurements he would undertake. Rowland employed the

Table 2[a]

Rowland's averaging procedure to define an absolute value for λ[Na-D_1]

Observer	Year	$\lambda[D_1]$	weight
Ångström and Thalén	1868/84	5895.81	1
Müller and Kempf	1886	5896.25	2
Kurlbaum	1888	5895.90	2
Peirce	1879	5896.20	5
Bell	1887/88	5896.20	10
Rowland's average	1887	5896.156	-

a. From Rowland, "A new table of standard wave-lengths," *PM, 36* (1893), 49–50.

"method of coincidences" using selected lines at regular intervals as a secondary standard; between these lines, he determined the wavelengths of all the others by interpolation, relying on the superb quality of his custom-made screws to secure accuracy of his micrometers. The coincidence method rests on the supposition that a coincidence of two spectral lines λ_1 and λ_2 belonging to orders m_1 and m_2 occurs if

$$m_1\lambda_1 = m_2\lambda_2.$$

He started with the value of prominent lines in the spectrum relative to his absolute standard NaD_1; he then obtained some fifteen reliable reference lines throughout the spectrum and interpolated between them within one order of the spectrum only.[76]

Rowland claimed an improvement of a factor of ten over the previous efforts of precision spectroscopy, especially the tables of Ångström,[77] and he estimated the accuracy of his own values one in a million for the visible part of the spectrum.[78] Sometimes, dropping the cloak of Anglosaxon understatement, he said that no greater perfection was possible:[79]

> Thus I have constructed a table of about one thousand lines, more or less, which are intertwined with each other in an immense number of ways. They have been tested in every way I can think of during eight or nine years, and have stood all the tests; and I think I can present the results to the world with confidence that the results of the relative measures will never be altered very much. I believe that no systematic error in the relative wavelengths of more than about ±0.01 [Å] exists anywhere except in the red end as we approach [the Fraunhofer line] A.

Rowland always stressed the intricate interweaving of all his measured wavelengths through many built-in checks, most notably the method of coincidence, which made use of the fact that all orders of the spectrum were focussed simultaneously in his apparatus.[80] He did not merely compile interpolated wavelengths, but established a "system of

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[76] H. Kochen, "Der rote Teil des Eisenbogenspektrums," *ZwPh*, *5* (1907), 285–299, on 290, and Konen, "Wellenlängenmessang," *Handbuch der Physik*, *19* (1928), 777–801, 792, for the coincidence method.

[77] H.A. Rowland, "On the relative wave-lengths of the lines of the solar spectrum," *AJS*, *33* (1887), 182–190, on 183, also in *PM*, *22*, 257–265.

[78] Rowland (ref. 28), 204f.

[79] H.A. Rowland, "A new table of standard wave-lengths," American Academy of Arts and Sciences, *Memoirs*, *12* (1896), 101–186, on 105; see also Rowland (ref. 28), 209.

[80] Rowland (ref. 79), 102f.

wave-length standards." This system remained essentially undisputed in the 19th century:[81]

> Through the use of his concave gratings, Rowland succeeded in establishing a system of wavelength normals [for the arc and the sun's spectrum], the relative accuracy of which was estimated to be some thousandths of a ten millionth of a millimeter, and which in fact formed the basis for all wavelength measurements until the year 1906 within the fields of physics and astrophysics.

Even many decades after Rowland's publications, updates, or as they were called, revisions, of Rowland's tables were published in 1928 and then again in 1966.[82] It was within this then recently established standard system of solar and laboratory wavelengths that, in 1890, the red shifts of the Fraunhofer lines in the sun's spectrum were discovered.

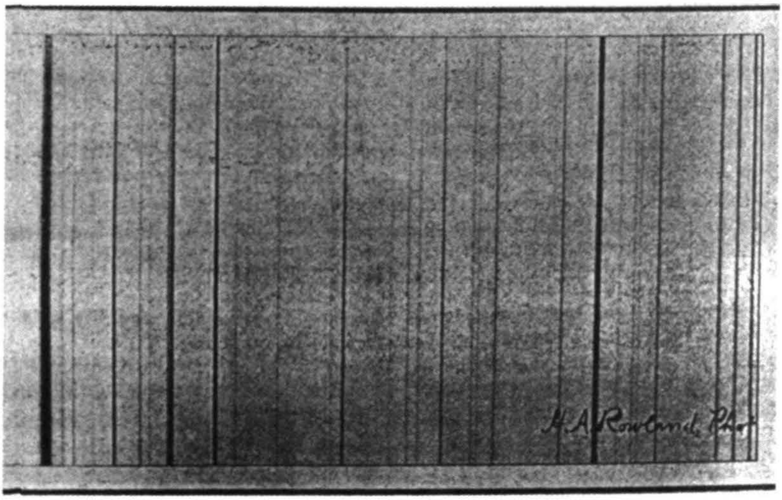

FIG. 9 Short section of a photograph of the solar Fraunhofer spectrum made by (and signed) "H.A. Rowland, Phot[ographer];" the two darkest lines are the sodium D lines. Reproduced from Moore (ref. 10), 124.

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[81] Konen (ref. 67), 780.

[82] Charles Edward St. John et al., *Revision of Rowland's tables of solar spectrum wave lengths with an extension to the present limit of the infra-red* (Washington, 1928); Charlotte Emma Moore, M.G.J. Minnaert, and J. Houtgast, *The solar spectrum 2935 Å to 8770 Å; second revision of Rowland's preliminary table of solar spectrum wavelengths* (Washington, 1966).

2. REDSHIFTS

A key assumption of Rowland and the spectroscopists of his day was the *invariability of the position of the lines in the spectrum*, or, the *precise coincidence* of the emission lines in their laboratory spectra with the corresponding absorption lines in the Fraunhofer spectra from sunlight (figure 10).

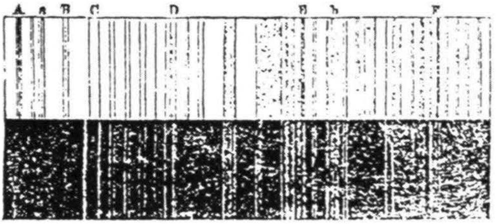

FIG. 10 Coincidence of some of the bright lines of iron with some Fraunhofer lines. Lockyer (ref. 121), 268.

How far was the assumption of absolute coincidence justified? How much did it predetermine the outcome of measurements? No standard independent of Rowland's measurements existed; in a sense, he did not *measure* but rather *defined* the wavelengths. The foregoing diagram shows that, with his concave gratings, Rowland had just reached the point where the red and violet shifts in the sun's spectrum became detectable; before him observers could not have seen the effect.

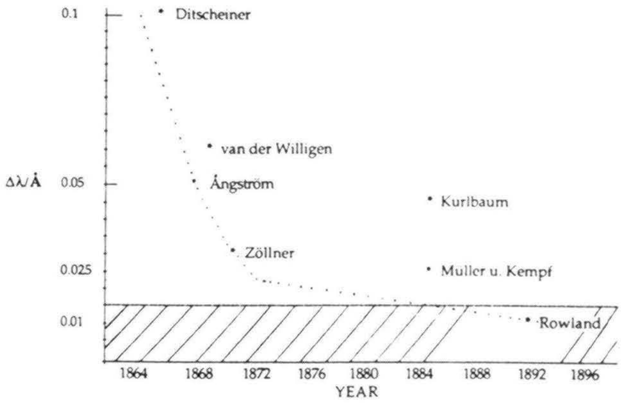

FIG. 11 Accuracy obtained in spectrometry, 1864–1896; the order of magitude for red and violet shifts in the sun's Fraunhofer spectrum is indicated by the shaded area. Author's drawing.

3. THE DISCOVERY OF REDSHIFTS IN THE SOLAR SPECTRUM

While working on the *Preliminary table of solar spectrum wavelengths*, Rowland encountered a problem whenever he obtained photographic plates showing both the Fraunhofer lines in the sun's spectrum and a comparison spectrum of emission lines of glowing gases from the source in his laboratory. Instead of finding the expected precise coincidences of the sort as illustrated in figure 10, but on a much more detailed scale, he nearly always found minute shifts:[83]

> In every plate having a solar and metallic spectrum upon it, there is often—indeed always—a slight displacement. This is due either to some slight displacement of the apparatus in changing from one spectrum to the other, or to the fact that the solar and the electric light pass through the slit and fall on the grating differently. In all cases an attempt was made to eliminate it by exposing on the solar spectrum, both before and after the arc, but there still remained a displacement of 1/100 to 1/200 division of Ångström, which was determined and corrected for by measuring the difference between the metallic and coinciding solar lines. selecting a great number of them, if possible.

The effect proved to be too persistent to be simply ignored. Therefore he tried to eliminate it. He exposed the lower third of his plate to the sun's spectrum, then the middle third to light from a laboratory arc, then the upper third to sunlight. Since this procedure was designed to exclude accidental shifts owing to minute changes in the relative position of slit, grating, and camera between the exposures of solar and arc spectra, he expected perfect coincidences. The effect nonetheless persisted. Now he had no choice but to mention it in his major publication and to invent a notation for his tables to indicate those lines where these shifts had occurred most conspicuously:[84]

> However, it is not always possible to correctly assign the exact position. and consequently there are probably many errors in the positions assigned. Where the solar line is too strong to be due entirely to the element with which it is identified, it is represented thus: -Fe, and indicates that the iron line is coincident with the red side of the solar line. the origin of the rest of the line being unknown.

In keeping with this convention, he denoted a slight shift of the Fraunhofer line in the solar spectrum relative to his laboratory spectra toward the red by a minus sign to the left of the element symbol. In the case of a shift to the violet, the minus sign was placed at the right

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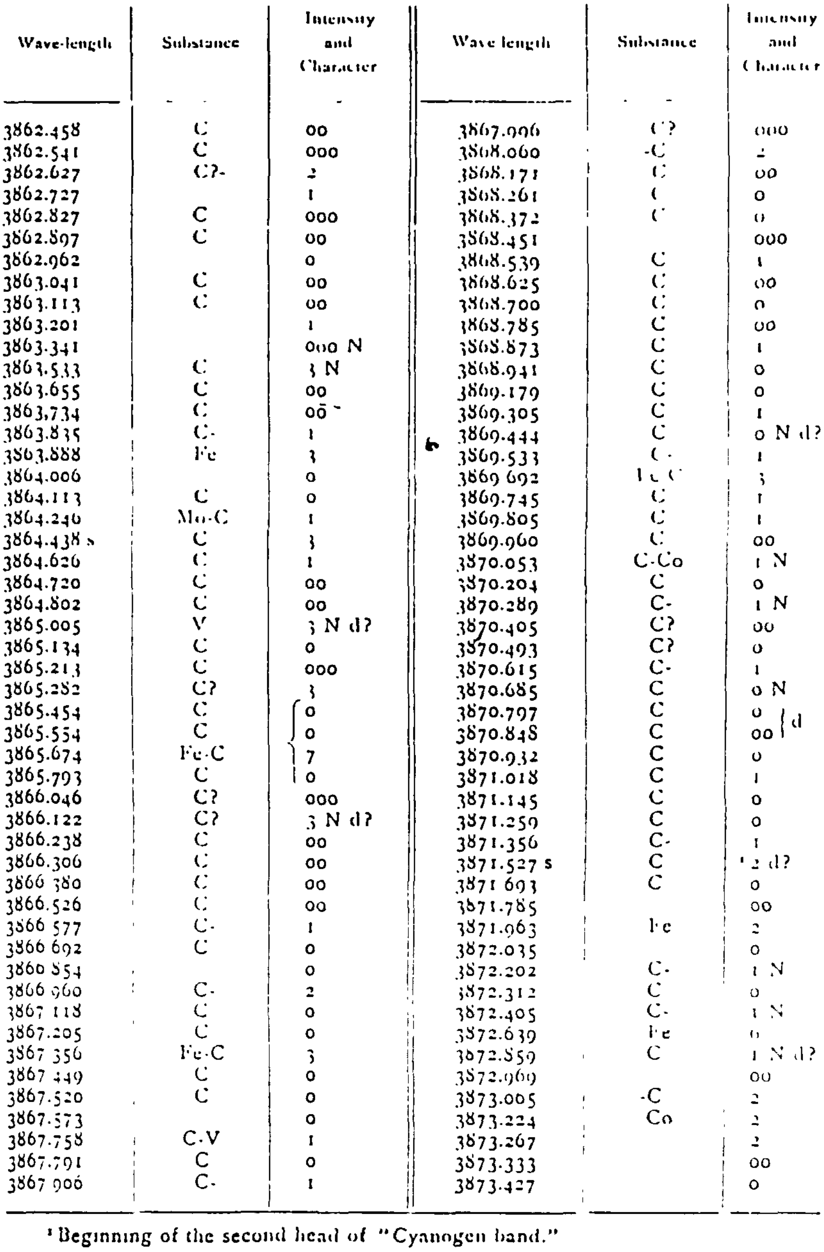
[83] Rowland (ref. 79), 116; cf. Rowland (ref. 77), 186.

[84] Rowland, *Preliminary table of solar spectrum wave-lengths* (Chicago, 1896) signed Dec 1894, on 6.

side of the element symbol of each line in his tables of solar spectrum wavelengths. Both cases occurred, as figure 12 illustrates.



Wave-length	Substance	Intensity and Character	Wave length	Substance	Intensity and Character
3862.458	C	00	3867.996	C?	000
3862.541	C	000	3868.060	-C	2
3862.627	C?-	2	3868.171	C	00
3862.727		1	3868.261	C	0
3862.827	C	000	3868.372	C	0
3862.897	C	00	3868.451		000
3862.962		0	3868.539	C	1
3863.041	C	00	3868.625	C	00
3863.113	C	00	3868.700	C	0
3863.201		1	3868.785	C	00
3863.341		000 N	3868.873	C	1
3863.533	C	3 N	3868.941	C	0
3863.655	C	00	3869.179	C	0
3863.734	C	00	3869.305	C	1
3863.835	C-	1	3869.444	C	0 N d?
3863.888	Fe	3	3869.533	C-	1
3864.006		0	3869.692	Fe-C	3
3864.113	C	0	3869.745	C	1
3864.246	Mo-C	1	3869.805	C	1
3864.438 s	C	3	3869.960	C	00
3864.626	C	1	3870.053	C-Co	1 N
3864.720	C	00	3870.204	C	0
3864.802	C	00	3870.289	C-	1 N
3865.005	V	3 N d?	3870.405	C?	00
3865.134	C	0	3870.493	C?	0
3865.213	C	000	3870.615	C-	1
3865.282	C?	3	3870.685	C	0 N
3865.454	C	0	3870.797	C	0 } d
3865.554	C	0	3870.848	C	00
3865.674	Fe-C	7	3870.932	C	0
3865.793	C	0	3871.018	C	1
3866.046	C?	000	3871.145	C	0
3866.122	C?	3 N d?	3871.259	C	0
3866.238	C	00	3871.356	C-	1
3866.306	C	00	3871.527 s	C	[1] 2 d?
3866.380	C	00	3871.693	C	0
3866.526	C	00	3871.785		00
3866.577	C-	1	3871.963	Fe	2
3866.692	C	0	3872.035		0
3866.854		0	3872.202	C-	1 N
3866.960	C-	2	3872.312	C	0
3867.118	C	0	3872.405	C-	1 N
3867.205	C	0	3872.639	Fe	0
3867.356	Fe-C	3	3872.859	C	1 N d?
3867.449	C	0	3872.969		00
3867.520	C	0	3873.005	-C	2
3867.573		0	3873.224	Co	2
3867.758	C-V	1	3873.267		2
3867.791	C	0	3873.333		00
3867.906	C-	1	3873.427		0

[1] Beginning of the second head of "Cyanogen band."

FIG. 12 Sample page from Rowland (ref. 84), 15.

A closer look at Rowland's "Preliminary table of solar spectrum wave-lengths" of 1896 reveals that about four dozen of the 400 solar lines that he could correlate with terrestrial elements were shifted to the red end of the spectrum, so that the earth line appeared to be at the violet end of the broader solar line, while about three dozen were shifted to the violet. As the previous quote shows, he attributed these deviations to accidental "errors in the positions assigned" or to the overlap between different lines unresolved by his apparatus, or to the "accidental movement of the apparatus, when changing from the spectrum of the sun to that of the arc."[85]

> As Professor Rowland was not convinced that the displacement was due to any other cause...the displacement was treated as due to this cause, and the wave-lengths of all metallic lines corrected for the average displacement of the stronger "impurity line" (generally iron) upon the plate, thus reducing them to an approximate agreement with the corresponding solar lines.

Since he adhered rigidly to his belief in the uniqueness of spectral line positions, Rowland multiplied possible instrumental causes of the apparent violation of the uniqueness principle:[86]

- accidental errors of the exposures or the micrometer readings
- systematic errors caused by the irregular ruling of the first few lines of the gratings, which were known to produce slight asymmetries in arc lines for long exposures
- a systematic error caused by unequal illumination of different parts of the grating by sun and laboratory light
- residual Doppler effects induced by light from the sun's limbs, which could produce either red or blue shifts, depending on the position of the slit, or by unknown turbulences in the sun's atmosphere, which might cause all sorts of minute shifts.

Since nearly nothing was known about the state of the gases in the sun's outer layers, and the contemporary solar models vastly differed in their assumptions about the conditions in the sun's interior, unknown turbulences could not be deemed implausible.[87]

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85. L.E. Jewell, "The coincidence of solar and metallic lines. A study of the appearance of lines in the spectra of the electric arc and the sun," *APJ, 3* (1896), 89–113, on 89; Johannes Hartmann, "A revision of Rowland's system of wave-lengths," *APJ, 18* (1903), 167–190, on 168ff; Heinrich Kayser, "On standards of wave-lengths," *APJ, 19* (1904), 157–161, on 158f.

86. C.E. St. John, "The displacement of solar spectrum lines," *Observatory, 43* (1920), 260–262.

87. Cf. Charles Augustus Young, *The sun and the phenomena in its atmosphere* (New Haven, 1872), and Robert Emden, *Gaskugeln* (Leipzig, 1907).

Although Rowland did not accept the shift of spectral lines as a real effect, one of his pupils and assistants, Lewis E. Jewell, persisted in studying it. Jewell had worked at The Johns Hopkins University at least since 1887.[88] Jewell was entrusted with the selection of diamonds suitable for ruling gratings as well as with the testing of gratings. He also participated in putting together the voluminous tables of standard wavelengths published by Rowland from 1887 on.[89]

During the winter of 1890, Jewell concluded that there existed systematic deviations between the wavelengths of metallic lines in the arc and those in the sun's spectrum that could not be explained on the basis of accidental errors:[90]

> Knowing that the plates measured [by Rowland and his collaborators] had been taken with the greatest care, I investigated the subject more carefully, and found not only was the displacement nearly the same for the same lines, taken upon different plates, but that there was a distinct difference in the displacement, not only for the lines of different elements, but also for the lines of different character belonging to the same element.

If these shifts were caused by motions of the apparatus between the recordings of the arc and sun spectra, the shifts should not be independent of the particular exposure, but to a large extent they appeared to be so; on the other hand, lines on the same photographs widely differed in their shifts, typically ranging from −0.025 Å to +0.01 Å. This ruled out the explanation favored by Rowland, that the apparatus moved between exposures. In February 1896, Jewell published a list of about 150 spectral lines from the sun, of which 127 were shifted towards the red, and 20 towards the violet; only 4 showed no shift at all when compared with the precise position of the corresponding emission lines in arc spectra. In addition, he found that the shifts tended to be proportional to the wavelengths and also that the strongest lines showing the greatest likelihood of reversal in the arc were displaced the most.

Contrary to his teacher, Jewell attributed shifts to differences in the physical conditions of the substances in the arc and in the solar atmosphere. Without being able to justify his claims very well, Jewell proposed Doppler shifts induced by motion in the line of sight of solar gases as a possible explanation; soon, he added influences of "pressure

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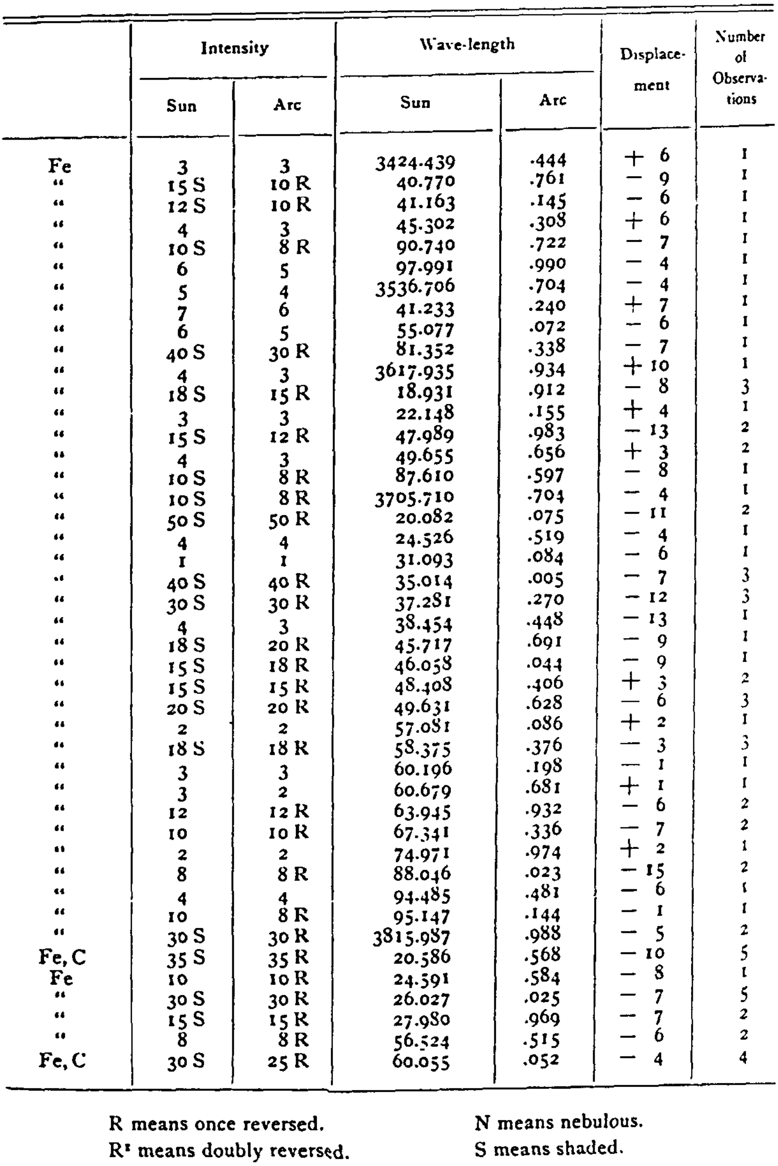
88. A bill for 30 hours of calculations at an hourly rate of 50 cents, dated 27 May 1887, exists at RP. According to the Maryland Census, Jewell was age 47 in 1910, single, and born in Ohio (National Archives, Washington).

89. Schroeder (ref. 24); Richard Tousey, "Solar spectroscopy from Rowland to SOT," *ViA*, *29* (1986), 175–200, on 176.

90. Ibid.; Jewell (ref. 85), 89f.



	Intensity		Wave-length		Displacement	Number of Observations
	Sun	Arc	Sun	Arc		
Fe	3	3	3424.439	.444	+ 6	1
"	15 S	10 R	40.770	.761	− 9	1
"	12 S	10 R	41.163	.145	− 6	1
"	4	3	45.302	.308	+ 6	1
"	10 S	8 R	90.740	.722	− 7	1
"	6	5	97.991	.990	− 4	1
"	5	4	3536.706	.704	− 4	1
"	7	6	41.233	.240	+ 7	1
"	6	5	55.077	.072	− 6	1
"	40 S	30 R	81.352	.338	− 7	1
"	4	3	3617.935	.934	+ 10	1
"	18 S	15 R	18.931	.912	− 8	3
"	3	3	22.148	.155	+ 4	1
"	15 S	12 R	47.989	.983	− 13	2
"	4	3	49.655	.656	+ 3	2
"	10 S	8 R	87.610	.597	− 8	1
"	10 S	8 R	3705.710	.704	− 4	1
"	50 S	50 R	20.082	.075	− 11	2
"	4	4	24.526	.519	− 4	1
"	1	1	31.093	.084	− 6	1
"	40 S	40 R	35.014	.005	− 7	3
"	30 S	30 R	37.281	.270	− 12	3
"	4	3	38.454	.448	− 13	1
"	18 S	20 R	45.717	.691	− 9	1
"	15 S	18 R	46.058	.044	− 9	1
"	15 S	15 R	48.408	.406	+ 3	2
"	20 S	20 R	49.631	.628	− 6	3
"	2	2	57.081	.086	+ 2	1
"	18 S	18 R	58.375	.376	− 3	3
"	3	3	60.196	.198	− 1	1
"	3	2	60.679	.681	+ 1	1
"	12	12 R	63.945	.932	− 6	2
"	10	10 R	67.341	.336	− 7	2
"	2	2	74.971	.974	+ 2	1
"	8	8 R	88.046	.023	− 15	2
"	4	4	94.485	.481	− 6	1
"	10	8 R	95.147	.144	− 1	1
"	30 S	30 R	3815.987	.988	− 5	2
Fe, C	35 S	35 R	20.586	.568	− 10	5
Fe	10	10 R	24.591	.584	− 8	1
"	30 S	30 R	26.027	.025	− 7	5
"	15 S	15 R	27.980	.969	− 7	2
"	8	8 R	56.524	.515	− 6	2
Fe, C	30 S	25 R	60.055	.052	− 4	4

R means once reversed.
R^1 means doubly reversed.

N means nebulous.
S means shaded.

FIG. 13 Sample page of the table of displacements between solar and arc lines of metals drawn up by Jewell. Jewell (ref. 85), 109.

or density of material and temperature, or both" as the only feasible reasons for these shifts.[91] Although far from an explanation, these remarks encompassed the main areas of further research in solar spectroscopy over the next twenty years.[92]

Kayser visited Baltimore again shortly after Jewell had reached conclusions about the shifts contrary to Rowland's. Kayser recalled:[93]

> [Jewell] is the man who in fact had done all[!] measurements and calculations for Rowland; in so doing he came upon difficulties, and disagreed with Rowland's way of passing over them. Some of these difficulties I had met, too, without being able to solve them; and [during my visit to Baltimore], we often discussed these matters back and forth, in the course of which I had to side with Jewell against Rowland. Today, these questions are solved—they are mainly due to a phenomenon called the pole effect, which causes the wavelengths of some spectral lines to appear variable. There was at that time no one else who knew more about these things than Jewell, and that's why the discussions with him were so valuable to me.

During the time he worked on the shifts, Jewell was not in Baltimore but in Marietta, Ohio. He continued to work for Rowland on calculations and corrections for the solar spectrum tables, and on measurements of the dispersion of air for ultraviolet and infrared light.[94] But his circumstances were not promising. He wrote Rowland in December 1893:[95]

> I wrote to you two weeks ago in regard to your note concerning the continuation of work upon the solar spectrum but have heard nothing whatever....I received a reply from Langley stating that the position he was inquiring about of you last spring was filled at present but he had filed my letter and would remember me and your recommendation should a vacancy occur etc., etc. which of course means that there is no use in my looking further in that direction....If there is no immediate prospect of getting an appropriation for the completion of the spectrum work, I *must* go at something else and that right away and when you do get as [illegible] you will have to get someone else to do the work.
>
> I do not like to write to you this way but in justice to myself I am obliged to do so for I cannot afford to wait indefinitely to find out

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[91] Ibid.

[92] See Eric Gray Forbes, "A history of the solar red shift problem," *AS*, *17* (1961), 129–164.

[93] Kayser (ref. 41), 205f.

[94] See, e.g., his bill to Rowland amounting to $250 "for work done upon the solar spectrum during the year 1893," 1 Dec 1893, and his letters to Rowland in 1896 (RP).

[95] Jewell to Rowland, 10 Dec 1893 (RP). Samuel Pierpont Langley (1834–1906) worked at the Smithsonian Institution and made important measurements of the total solar radiation; C.D. Walcott, *BMNAS*, *7* (1917), 247–268.

> whether I am to continue the work or not, for I must get something to do and there is no use in my trying to obtain work elsewhere if there is an immediate prospect of going ahead with the spectrum work, and also I can not possibly afford to wait here any longer waiting to possibly learn in the end that the spectrum work is to stop.
>
> You can see the position I am left in by this uncertainty....I think you ought to be able to let me know something definite about the matter by this time and I hope I shall not be subject to any more delay before finding out what I am to do.

Nothing satisfactory to Jewell resulted. He spent some time in Seneca Falls, New York, in 1896, and in military service in 1898.[96]

But very soon after Rowland's death in 1901, Jewell returned to the Physical Laboratory of The Johns Hopkins University, where he continued to work on the "revision of Rowland's system of standard wave lengths" together with Rowland's successor, Joseph Ames.[97]

Unfortunately, he was on no better terms with Rowland's successor than he appears to have been with Rowland himself, quarrels among Ames, Jewell, and Robert Wood led to a serious delay in the production of well-ruled gratings which were in great demand by the scientific community.[98] As Jewell later put it in a letter to George F Kunz of Tiffany's, New York, still the main supplier of diamond points for ruling gratings:[99]

> After Prof. Rowland's death I took up his work with the ruling engines for making diffraction gratings and got one of the engines in such good shape that I turned out the finest gratings that have ever been ruled here, and was doing this when Ames compelled me to stop the work we were doing so successfully to experiment with bigger work so that he could have something big to show. I protested and told him of the danger of doing so, but it was of no use and it resulted in the ruining of the ruling nut.

The repair and reconstruction of the ruling nut was delayed because Ames had turned over the lathe with which the work had been done formerly to the "students and negro janitor." "Ames has managed everything but would not redeem his promises until finally a new lathe was obtained, but it was months before he would allow it to be set up

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96. Jewell to Rowland, 3 Oct 1898 (RP).

97. Ames (ref. 48), 63; L.E. Jewell, "The revision of Rowland's system of wave-lengths," *APJ, 21* (1905), 23–34.

98. Cf. Donald E. Osterbrook, "Failure and success: Two early experiments with concave gratings in stellar spectroscopy," *Journal for the history of astronomy, 17* (1986), 119–129, esp. on 124, for Hale's efforts to get a concave grating from Jewell after 1901

99. Jewell to Kunz, May 1910, JHC no. 137, Eisenhower Library, Baltimore Special thanks to the archivists for having traced these letters.

and months more before he would allow necessary attachments to be obtained."[100] And then everything stopped:[101]

> If Prof. Ames had let me go ahead after we got the new lathe there is no doubt that I would before this have had both engines in working order and turning out work, but knowing nothing, or having no real understanding of the mechanical practical work necessary, he through (what I am obliged to call) his stupidity, tied my hands so that I could not do what was necessary, and while the scientific world was greatly in need of the gratings, he stopped the work.

In 1905, Jewell participated in the solar eclipse expedition to Algeria; around 1908/9 he was assistant in astrophysics at Johns Hopkins; but that position did not work out either, apparently owing to a clash with Wood.[102] There was some tension between him and Ames, who did not admire him as an astrophysicist, but praised him highly as a diamond expert:[103]

> I know of no one in America who has his knowledge of gems so far as the scientific theory of their proper cutting, etc. is concerned; nor do I know of anyone who has such wonderful eyes for detecting differences in color, etc. It has occurred to me that it might be possible for Tiffany & Co. to make use of his services in some way. It seems almost a crime that his wonderful scientific ability cannot be used in some way.

Jewell's complaints about his former colleagues, including the director of the Physical Laboratory who had bothered to write a recommendation for him, may have warned off Tiffany's; equally negative was the outcome of his efforts to "break into the Geological Survey on the strength of being an expert photographer and having some experience in drawing and a little in pointing."[104] His many talents did not counterbalance the influence of those he had made his enemies by his undiplomatic "frankness and honesty" and his uncompromising insistence in scientific matters. He wrote:[105]

> The circumstances (which of course are not understood) have made it very difficult for me to get a position in my line of work (astrophysics) as the chances in that line are limited at the best, and I have as yet been

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[100] Ibid.

[101] Jewell to Kunz, 3 June 1910, ibid.

[102] Ames to George F. Kunz of Tiffany's, 1910 (ibid.); Jewell to Kunz, May 1910, JHC no. 137; "the loss [of my job] being due to frankness and honesty on my part, and having told the truth to and regarding one of the most unprincipled scoundrels alive, without any sense of honor at all (which I have had occasion to tell him) viz., Prof. R.W. Wood."

[103] Ames to Kunz, 26 Mar 1910, JHC no. 137.

[104] Jewell to Kunz, May 1910, ibid.

[105] Ibid.

> unable to get placed, and the long delay together with what I had paid out for the work on the ruling engines have put me into very bad shape financially.

The collapse of Jewell's career seriously hampered the production of gratings at Hopkins and prompted a shift of precision spectroscopy to Mount Wilson, where the first ruling engine came into being that could make gratings better than Rowland's in both size and quality.[106]

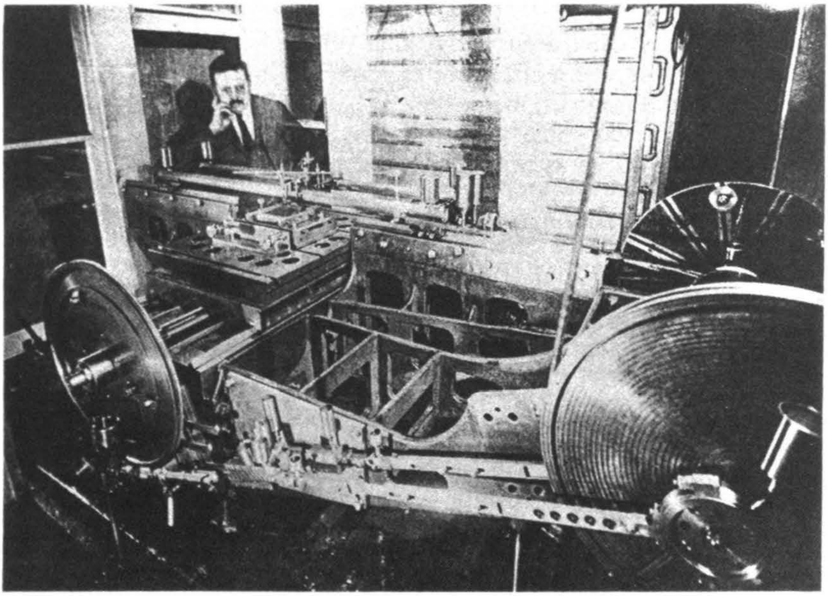

FIG. 14 Jacomini with the "A" ruling machine of Mount Wilson. The screw has a lead of 2 mm and the worm gear has 1,200 teeth. Babcock (ref. 20), 155.

Shifts before 1896

Around 1860, both Ångström and Kirchhoff independently advanced the thesis that the dark Fraunhofer lines of the sun's spectrum and the bright emission lines observed in the laboratory precisely coincided, and they each made inferences about the process of emission and absorption.[107] Below is Anders Jonas Ångström's version, intricately woven into a priority claim against Kirchhoff:[108]

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[106] Osterbrock (ref. 98), 127; Babcock (ref. 20), 155.

[107] Cf. McGucken (ref. 14), chapt. 1, or James (ref. 2).

[108] Anders Jonas Ångström, "On the Fraunhofer lines visible in the solar spectrum," *PM, 24* (1862), 1–11. Euler's *Theoria lucis* mentioned in the quote was first published in 1746.

> In a former memoir... I have, for the purpose of illustrating the absorption of light, made use of a principle already propounded by Euler in his "Theoria lucis et caloris," viz that the particles of a body, in consequence of resonance, absorb principally those ethereal undulatory motions which have previously been impressed upon them, and I extend the validity of this principle not only to the case in which the absorbed light displays itself sensibly as light and heat, but also to that in which its effect is evidenced by chemical decomposition. Conversely, I endeavour also to show *that a body in a state of glowing heat emits just the same kind of light and heat which it absorbs under the same circumstances*....I had already, in my former paper, remarked that the Fraunhofer-lines in the solar spectrum were, so to speak, an inversion of the bright lines in the electrical spectrum, and that an explanation of the lines in the one system would in all probability also furnish an explanation of those of the other.

Gustav Robert Kirchhoff had arrived at a similar result by thermodynamic considerations. Soon after he realized the striking coincidence of so many lines of both types of spectra, he investigated the subject carefully and checked the lines of many elements separately, thus pioneering a chemical analysis of the composition of the sun's gaseous atmosphere by means of spectroscopy. Henry Roscoe, visiting Kirchhoff in Heidelberg in the autumn of 1860, remarked "the splendid spectacle of the coincidences of the bright lines of the iron spectrum with the dark solar lines."[109] The precision of the coincidences impressed him:[110]

> In the lower half of the field of the telescope were at least seventy brilliant iron lines of various colours, and of all degrees of intensity and of breadth; whilst in the upper half of the field, the solar spectrum, cut up, as it were, by hundreds of dark lines, exhibited its steady light. Situated *exactly* above each of the seventy bright iron lines was a dark solar line. These lines did not only coincide with a degree of sharpness and precision perfectly marvellous, but the intensity and breadth of each bright line was so accurately preserved in its dark representative, that the truth of the assertion that iron was contained in the sun, flashed upon my mind.

Kirchhoff had announced his discovery of the reversion of bright lines into dark lines on viewing a strong continuous source through a Bunsen flame in a talk about the sun's spectrum delivered on October 28, 1859, at the Naturhistorisch-medicinischer Verein zu Heidelberg.

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[109] Henry Enfield Roscoe, "On Bunsen and Kirchhoff's spectrum observations," Royal Institution of Great Britain, *Notice of the proceedings*, *3* (1861), 323–328, on 328.

[110] Ibid.

It contains the following statement:[111]

> The *precise* coincidence of dark lines of the sun's spectrum with the bright [lines of the spectra] of colored elements cannot be an accidental one; but so far no good reason could be given for it and no conclusions could be drawn from it either. This gap has now been filled by my observations....We are now permitted to say with great reliability that each of the Fr[aunhofer] lines that does not originate from [absorption in] our atmosphere is based upon the presence of a particular chemical constituent in the sun's atmosphere, and that it is the spectrum of precisely this element when put to a flame that shows a bright line in the corresponding region.

Because Ångström's paper was delivered more than one year after Kirchhoff's, the scientific community has awarded priority to Kirchhoff. John Tyndall, however, who had first-hand knowledge of Ångström's earlier work, gave it high marks for originality:[112]

> The man to whom we owe this beautiful generalization is Kirchhoff.... but like every other great discovery, it is compounded of various elements....The man who came nearest to the philosophy of the subject was Ångström. In a paper translated from Pogg[endorff's] Annalen by myself, and published in the *Philosophical magazine* for 1855, he indicates that the rays which a body absorbs are precisely those which it can emit when luminous. In another place he speaks of one of his spectra giving the general impression of *reversal* of the solar spectrum. Foucault, Stokes and Thomson have all been very close to the discovery....But Kirchhoff's claims are unaffected by these circumstances.

In 1862, Ångström himself acknowledged that Kirchhoff was the first to demonstrate the correctness of his [Ångström's] hypothesis, by making experiments with sodium and lithium.[113] Whether one prefers Ångström or Kirchhoff, their work brought the conviction that the coincidence of bright and dark lines opened the way to the chemical constitution of bodies inaccessible to other forms of chemical analysis. At least, as Ångström put the matter, the discovery allowed the determination of chemical composition with a "considerable probability." He wrote:[114]

> From the circumstance of two lines coinciding in both the spectra of the sun and of a given metal, it by no means follows as a necessary conse-

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[111] Gustav Robert Kirchhoff, "Ueber das Sonnenspectrum," Naturhistorisch-Medicinischer Verein zu Heidelberg, *Verhandlungen, 1* (1857–59), 251–255, on 254f.

[112] John Tyndall, "On the physical basis of solar chemistry," *PM, 22* (1861), 147–156, on 155; cf. Ångström (ref. 108), 3f.

[113] Cf. Ångström (ref. 108), on 4.

[114] Ibid.

> quence that this substance is to be found in the sun, because, on account of the enormous number of dark lines in the sun's spectrum, such coincidence may be accidental; nevertheless the probability of such an assumption increases in proportion to the number of such coincident lines and their phenomenal peculiarities.

The assumption of the precise coincidence of solar and terrestrial spectral lines soon became the mainstay of the then rapidly evolving science of spectroscopy. The more fruitful it proved to be, the more the initial reservations disintegrated: the hypothesis of the precise coincidence became a central tenet, if not to say a dogma, of spectroscopy. Especially in the English-speaking world, the dogma was reinforced by the development of models of the processes of emission and absorption described vividly by Tyndall:[115]

> What is the meaning of absorption? What is the meaning of radiation?....Radiation, then, as regards both light and heat, is the transference of motion from the vibrating body to the æther in which it swings; and, as in the case of sound, the motion imparted to the air is soon transferred to the surrounding objects, against which the aërial undulations strike, the sound being, in technical language, absorbed, so also with regard to light and heat, absorption consists in the transference of motion from the agitated æther to the particles of the absorbing body.

During the next decade, whenever the coincidence of a dark and bright spectral lines came into doubt in a chemical analysis, the doubt referred to the chemical identity of the substances causing the absorption and emission line, *not* to the coincidence hypothesis for identical elements. For example:[116]

> Whether those lines, which appear to coincide on the enclosed plates (aside from the atmospheric spectral lines) do in fact always precisely coincide is questionable; perhaps a stronger dispersion of a prism will still reveal minute deviances in the position of the lines—once at least it happened that I took a silver line to be coincident with a mercury line, which separated into two lines again with a very narrow slit.

One of the first spectroscopists to challenge the common assumption of the coincidence of solar and terrestrial spectral lines was Joseph Norman Lockyer (1836–1920), who had pioneered in virtually all fields of spectroscopy and solar observations from the 1860s on.[117]

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[115] Tyndall (ref. 112), on 149; original emphasis omitted.

[116] Friedrich Brasack, "Das Luftspectrum," Naturforschende Gesellschaft Halle, *Abhandlungen, 10* (1866), 1–42, on 7.

[117] On Lockyer, see the notice by Alfred Fowler in *PRSL, 104A* (1923), i–xiv, and by Herbert Dingle in *DSB, 8* (1973), 440–443; William McGucken (ref. 14), 83ff; A.J. Meadows, *Science and controversy A biography of Sir Norman Lockyer* (Cambridge, MA, 1972).

Here is what he wrote about the tacit knowledge in spectroscopy in 1879:[118]

> We are accustomed to say that the sun is surrounded by an enormous atmosphere, and that this atmosphere has in it the vapours of metals. such as iron, magnesium, & c., with which metals we are familiar on this planet. This statement has been based on the near agreement presented by the places of the lines in the spectrum of the substances as studied in our laboratories and the Fraunhofer lines themselves. The matching of these spectra is nothing like so perfect, and the conclusion drawn, therefore, is nothing like so firmly based, as is generally imagined.

He and other spectroscopists before him had indeed found changes in the precise location of spectral lines in laboratory spectra due to other substances and density variation. These early findings of changes in the wavelengths of emission lines prompted the coining of the term "shift":[119]

> Many observers have shown that in the case of various substances there is evidence to suggest that the refrangibility of lines is slightly changed by a change of chemical composition or physical condition....Much more evidence of the same kind has in fact necessitated the introduction of the word "shift" to define these slight changes of refrangibility. or want of coincidence of apparently the same line under different chemical and physical conditions.

While endeavoring to check Lockyer's hypothesis that chemical elements might dissociate into "finer constitutents" in the sun, H.W. Vogel had worked with spectra of substances dissolved in other media. and, as Kundt had already found, confirmed that "often... in strongly dispersive media the absorption-bands of a substance are displaced towards the red."[120]

In 1881, Lockyer declared an agnostic position about the "absoluteness" of coincidences between solar and terrestrial lines:[121]

> Here we must confess both our imperfect instrumental and mental means. We cannot talk of *absolute* coincidences because the next application of greater instrumental appliances may show a want of coincidence. On the other hand there may be reasons about which we know

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[118] J.N. Lockyer, "On the necessity for a new departure in spectrum analysis," *Nature, 21* (1879), 5–8, on 6.

[119] J.N. Lockyer, *Chemistry of the sun* (London, 1887), 369; cf. Konen (ref. 3), 235

[120] McGucken (ref. 14), on 83ff; H.W. Vogel, "Lockyer's dissociation theory," *Nature, 27* (1883), 233.

[121] Lockyer (ref. 119), 370; cf. Lockyer (ref. 118), and "Solar physics—chemistry of the sun," *Nature, 24* (1881), 267–274, 315–324, 365–370, 391–399.

> at present absolutely nothing which should make absolute coincidence impossible under the circumstances stated. The lines of the finer constituents of matter may be liable to the same process of shifting as that at work in compound bodies when the associated molecules are changed.

How right he was became apparent a decade later when Rowland introduced new and refined instrumental techniques that increased the resolution of spectrographs enough to detect the red and occasional violet shifts of spectral lines. But in 1881, Lockyer's warnings were not taken seriously by many of his colleagues.

Lockyer found interesting variations of intensity and minor variations of wavelengths in his comparison of chromospheric lines from the sun with earth spectra; indeed, he overdramatized the minute differences in favor of his dissociation theory.[122]

> The upshot of this inquiry even already is as follows: the discrepancy which I pointed out six years ago, between the solar and terrestrial spectra of calcium is not an exceptional, but truly a typical case. Variations of the same stare us in the face when the minute anatomy of the spectrum of almost every one of the so-called elements is studied. If, therefore, the argument for the existence of our terrestrial elements in extraterrestrial bodies, including the sun, is to depend upon the perfect matching of the wave-lengths and intensities of the metallic and Fraunhofer lines, then we are driven to the conclusion that the elements with which we are acquainted here do not exist in the sun.

However, Lockyer's was a minority view, and he was nearly the only one to point to the very minute differences of the Fraunhofer lines compared with laboratory spectra. Insofar as the community opposed his views on the dissociation hypothesis, it also opposed the hints towards possible shifts and other changes of the spectral lines.

Pressure shifts

A year before Jewell published his results on the spectral shifts, that is in February 1895, two of Rowland's graduate students, William Jackson Humphreys (1862–1949) and John Frederick Mohler (1864–1930) started to work on the "effect of pressure on the wave-lengths of lines in the arc-spectra of certain elements."[123] They confirmed that

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[122] J.N. Lockyer, "Discussion of 'Young's list of chromospheric lines,'" *PRSL*, *28* (1879), 432–444, on 444 (original emphasis omitted).

[123] Humphreys graduated from Hopkins in 1889, earned his Ph.D. under Rowland in 1897, and became director of the Research Station of the Mt. Weather Observatory and professor of meteorological physics at George Washington University in 1911; Mohler obtained his Ph.D. under Rowland in 1897 and became professor of physics at Dickinson College, Carlisle, Pennsylvania.

changes in pressure can have an influence on the position of some spectral lines. They motivated their inquiries as follows:[124]

> The purpose of the investigation described in this paper was to examine minutely the effects of pressure on the arc-spectra of the elements, and especially to note the effect, if any, on the wave-length. The idea was suggested by the fact that in the course of careful measurements of the arc and solar spectra made in this laboratory by Mr. L.E. Jewell, he detected certain discrepancies which showed a difference in the wave-lengths of the same line in the two spectra, and also that this difference varied with different elements.
>
> A further reason for taking up this work was the fact that the wave-lengths of the red, green and blue lines of the spark spectrum of cadmium vapor at low pressure as determined by Professor Michelson for the purpose of accurately comparing with the standard meter, are less than those of the same lines of the arc-spectrum at atmospheric pressure as determined by Professor Rowland.

These differences between the arc- and spark spectrum could not be accounted for at the time. Two puzzles therefore remained to be solved, in both of which, differences in the pressure under which the sun's gases absorb and the laboratory gases emit their spectra seemed to play a part.

By employing apparatus that Rowland had developed in 1889 for the examination of the arc spectrum under pressure, Humphreys and Mohler showed that the precise position of spectral lines in emission spectra of gases depends on the pressure of the air in the container in which an electric arc and the substance under examination were contained.[125] Although there had been earlier experiments with gases under low and high pressure by Frankland, Wüllner, and others,[126] the work of Humphreys, Jewell, and Mohler was the first to result in phenomenological laws describing the dependency of the shifts upon the increase or decrease of pressure.[127]

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124. W.J. Humphreys and J.F. Mohler, "Effect of pressure on the wavelengths in the arc-spectra of certain elements," *APJ, 3* (1896), 114–137, on 114.

125. See Louis Duncan, Rowland and R.I. Todd, *Electrical world, 22* (1893), 101, cf Humphreys and Mohler (ref. 124), 115f.

126. See J. Plücker and W. Hittorf, "On the spectra of ignited gases and vapours," *PTRS, 155* (1865), 1–30; E. Frankland, "On the combustion of hydrogen and carbonic oxide under great pressure," *PRSL, 16* (1868), 419–422; E. Frankland and J.N. Lockyer, "Preliminary note on researches on gaseous spectra in relation to the physical condition of the sun," *PRSL, 17* (1869), 288–291, and *18* (1869), 79–80; A. Wüllner, "Ueber die Spectra einiger Gase bei hohem Drucke," *Annalen der Physik, 137* (1869), 337–361, as well as later papers by Lockyer, Zöllner, Cailletet, Stearn and Lee, Cazin, Ciamician Schuster, Wilson, FitzGerald and Galitzin.

127. Cf. Humphreys and Mohler (ref. 124), 115: "nowhere have we found stated as a result of theoretical considerations or experiment that the wave-frequency itself may change and thus lead to a shift of the line as a whole."

Humphreys and Mohler distinguished the old effect of *broadening* from the "new" one of *shift*. They inferred that an increase in pressure could lead to shifts and not simply to asymmetric broadening from their finding that fine and sharp lines could be obtained regardless of the application of pressure, "nor was it a case of one line disappearing and another appearing in a slightly different position since it was often easy, while the pressure was being let off, to observe a line gradually change its position without alteration in width or other appearances."[128] They also ruled out the possibility that the effect did not depend on change of temperature coupled to a change in pressure by observing the shifts produced by a heavy current arc at the positive and at the negative poles of the arc; the temperature of the latter is much lower than that of the former, but no change in the shifts occurred.

As a side product of their research, they furnished an argument against Rowland's favorite explanation of the shifts. Motions of the apparatus would not do because all of Rowland's plates showed the carbon (cyanogen) bands unmoved, but many other lines displaced toward the red or occasionally toward the violet side on the same exposure. This strange fact fell into place when Humphreys and Mohler realized that carbon did not show any dependency upon pressure within the range of the pressures at their disposal (1–14 atm). They interpreted this finding as an indication of a definite correlation between the shifts in Rowland's tables and the pressure shifts observed in the laboratory.

Humphreys and Mohler found some functional dependencies of the shifts in arc spectra, most of which were corroborated by later research:[129]

- The displacements were approximately proportional to the wavelengths at high pressures
- The shifts invariably occurred toward the red end of the spectrum
- The shifts were also directly proportional to the excess of pressure above one atmosphere (see figure 15)
- The shifts for individual elements came out close to the product of the cube root of the atomic weight and the coefficient of linear expansion.

In calcium, and later also magnesium, they detected two groups of lines with different coefficients of proportionality of pressure excess and shift: the H and K lines, often found in the outer layers of the

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[128] Ibid., 117.

[129] Humphreys and Mohler (ref. 124), 119.

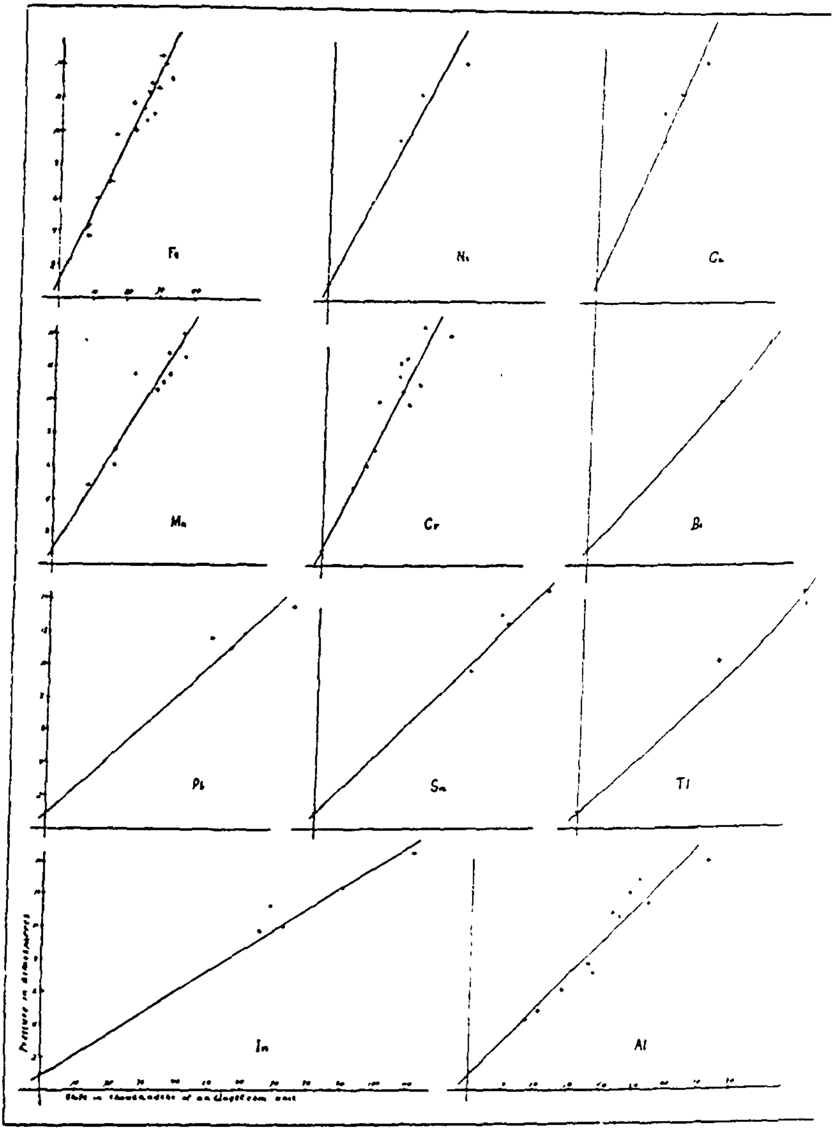

FIG. 15 Proportionality of displacements of spectral lines and pressure excess. Humphreys and Mohler (ref. 124), plate XII.

sun's atmosphere and its protuberances.[130] For a wavelength of 480μμ (4800 Å) and a pressure decrease of one atmosphere, the coefficient of proportionality between pressure excess and shifts had the following values:

Table 3
Dependency of pressure shifts of atomic weights[a]

Element	Shift Δλ	Element	Shift$_1$	Shift$_2$
Sodium	108	Calcium	54	27
Lithium	85	Strontium	65	37
Potassium	132	Barium	58	34
Chromium	26	Magnesium	44	30
Iron	25	Aluminum	55	—
Nickel	28	Titanium	22	—
Cobalt	24	Bismuth	49	—
Cyanogen	0	Uranium	9	—

a. Shifts in units of 10^{-5} μμ = 10^{-6} Å. The right half of the table refers to elements whose lines could be classified into two distinct pressure dependency groups.

The table indicates that under a pressure diminution of one atmosphere, sodium lines changed their wavelengths by the relative amount of $108 \cdot 10^{-5}:480 = 2.25 \cdot 10^{-6}$, the equivalent of a Doppler shift induced by a velocity of 0.67 km/sec toward the observer. Of course, for sodium, which has a very strong anomalous dispersion, the dependency was especially strong, about five times as much as for iron. A new and unexpected experimental effect, the *variability of the relative positions of spectral lines*, thus developed out of a persistent anomaly that could not be explained away.[131] Note that no explicit theory was needed to inspire experiments to test the hunch that pressure might influence the position and shape of spectral lines: they needed only their technical knowledge of spectroscopy and other background knowledge to arrive at empirical claims on the dependence of shifts on pressure.[132]

[130] L.E. Jewell, J. Mohler, and W. Humphreys, "Note on the pressure of the 'reversing layer' of the solar atmosphere," *APJ, 3* (1896), 138–140, on 139f.

[131] Cf. Peter Galison, *How experiments end* (Chicago, 1987), § 4.17.

[132] Humphreys and Mohler (ref. 122); Jewell, Mohler and Humphreys (ref. 128); W.G. Duffield, "The effect of pressure upon arc spectra," *PRSL A, 79* (1907), 597–599, and *PTRS A, 208* (1908), 111–162, *209* (1908), 205–226, *211* (1910), 33–50, 51–73, and *PRSL A, 84* (1911), 118–123; R. Rossi, "The effect of pressure on the band spectra," *PRSL, A, 82* (1909), 518–523; "The effect of pressure upon arc spectra," *PRSL A, 83* (1910), 414–420; "The widening of the hydrogen lines by high pressures," *APJ, 34*

The proportionality between shift and pressure suggested that a decrease in pressure should shift lines towards the violet. Checking this inference required constructing an arc for low pressures. In October 1896, Mohler was able to demonstrate that "the effect of decreasing the pressure on the arc producing the lines is to shift them slightly towards the violet end of the spectrum," again with orders of magnitude of about 0.01 Å.[133] With these results, at least part of the astonishingly large differences (of around 0.2 Å) between the precision measurements of the three cadmium lines by Rowland and Michelson, referred to above, could be accounted for, because one of them had been measured with a vacuum arc while the other had the standard pressure of one atmosphere.

A natural next step was to check the approximately linear dependency of shifts on pressure beyond the 14 atmospheres available to the group in Baltimore. Experimenters in Baltimore and in Manchester took the step, first to about 20 atm, then to 100 atm, and finally to 200 atm, confirming the result at low variations of pressure, and increasing the precision with which pressure dependency coefficients could be estimated.[134] Each increase in pressure was proudly announced until the 1920s, when Eugen Paul Wigner, Victor Weisskopf, Heinrich Kuhn, Henry Margenau, and others gave the quantum mechanical explanation of the shifts.[135]

Theory had entered the game much earlier. The pressure dependencies observed by Humphreys and Mohler seemed relevant to a quantitative understanding of the sun's atmosphere and its spectrum, since the shifts were of the same magnitude as those in Rowland's tables of solar wavelengths. It suddenly seemed feasible to give estimates of the pressure in the sun's atmosphere:[136]

(1911), 299-302; "The widening of the hydrogen lines in the spark spectrum," *APJ, 40* (1914), 232-235; Arthur Scott King, "The effect of pressure upon furnace spectra," *APJ, 34* (1911), 37-56, and *35* (1912), 37-56; G. Gouy, "Sur la pression existant à la surface du soleil," *CRAS, 155* (1912), 115-118.

[133] J.F. Mohler, "The effect of pressure on wave-length," *APJ, 4* (1896), 175-181, on 177; see also August Hagenbach, "Spektroskopische Untersuchungen des Bogens unter vermindertem Druck," *PZ, 10* (1909), 649-657.

[134] Duffield (ref. 132); W.J. Humphreys, "Arc spectra under heavy pressure," *APJ, 26* (1907), 18-35, and "Apparatus for obtaining electric arcs under heavy pressure," ibid., 36-40.

[135] V. Weisskopf, "Die Breite der Spektrallinien in Gasen," *PZ, 34* (1930), 1-24; Weisskopf and E.P. Wigner, "Berechnung der natürlichen Linienbreite auf Grund der Diracschen Lichttheorie," *ZP, 63* (1930), 54-73; H. Kuhn, "Pressure shift and broadening of spectral lines," *PM, 18* (1934), 987-1003; H. Margenau and William W. Watson, "Pressure effects on spectral lines," *Reviews of modern physics, 8* (1936), 22-53.

[136] Jewell, Mohler, and Humphreys (ref. 130), 138 (quote); cf. G.E. Hale, "Note on the application of Messrs Jewell, Humphreys and Mohler's results to certain problems of astrophysics," *APJ, 3* (1896), 156-161, on 156; and the comment by Svante August

> The investigation of the effect of pressure on the wavelengths of the lines of arc-spectra, together with the observation that many lines of the solar spectrum do not coincide with the corresponding lines of arc-spectra at atmospheric pressure, seems to furnish a method of determining the pressures of the solar atmosphere where the Fraunhofer lines are produced.

Despite the very different pressure coefficients for various elements, Jewell could narrow his estimate for the pressure in the sun's reversion layer to within 3–7 atm, as the following table indicates.

Table 4
Pressure estimates (in atmospheres) for the sun's reversion layer

Element	Pressure	a.w.	Element	Pressure	a.w.
Aluminium	2	27	Manganese	5	55
Silicon	4	28	Iron	6	56
Calcium (a)	6	40	Nickel	7	59
Calcium (b)	3	40	Copper	7	63
Chromium	5	52	Cobalt	4	59

For elements with low atomic weights, the pressure turned out to be somewhat lower than the average of 5 atm, indicating that they occupied mostly the outer layers of the sun's atmosphere. The possibility of reasoning from laboratory measurements to physical conditions in the atmospheres of the sun (and eventually even of remote stars) excited astrophysicists. George Ellery Hale, the editor of the journal in which the papers by Jewell, Humphreys, and Mohler had appeared, wrote:[137]

> It is impossible in a limited space to touch upon the numerous applications of the new results to astrophysical problems. That the effect of pressure must receive attention in future investigations of the motions of stars and nebulæ in the line of sight cannot be doubted.

Hale pointed to the need for a closer analysis of the available spectroscopic data on shifts, taking into consideration the possible

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Arrhenius in his *Lehrbuch der kosmischen Physik* (Leipzig, 1903), 32. (Humphrey's observations allow us to hope that with more refined measuring instruments it will be possible not only to measure the pressure in the light-emitting or [light-]absorbing sections of stellar atmospheres, but also to measure the movement of stars in the line of sight. In this respect the fact that the lines which belong to different elements will all shift to the same degree as a result of motion, but will experience very different shifts as a result of pressure, will be significant).

[137] Hale (ref. 136), 161.

superposition of Doppler and pressure effects and hinting at the possibility that they might be compensated by pressure redshifts and violetshifts induced by motion towards the observer.[138] The results consequently did not allow easy interpretation: the shifts not only varied from element to element, but also for one element at least three different groups of spectral lines could usually be found, all with approximately linear dependence on the pressure, but different proportionality coefficients. Some of the lines did not show any position dependence on pressure, for instance, the cyanogen band, and some experimenters claimed that a few perverse lines moved not to the red but to the violet end of the spectrum when put under pressure. However, the early researches did arrive at consensus on two points:

- The approximately linear dependence of the shifts of variable lines on the pressure for a range of 1—100 atms,
- The unexpected appearance of three groups of spectral lines for each chemical element, differing in the quantitative dependence on pressure.

The first theoretical papers were published in 1896, the very year in which the pressure effect was established beyond doubt. Arthur Schuster discussed two alternative mechanisms:[139]

> The question which forces itself upon me is this: Is the displacement of the lines due to pressure only, i.e., to molecular impact, or is it due to the proximity of molecules vibrating in equal periods? The latter seems the more probable explanation—but if it is the true one it would follow that the displacement would not take place—or not to the same extent if the pressure is produced by molecules of a different kind to those under examination.

W.J. Humphreys immediately rejected Schuster's suggestion:[140]

> The suggestions of so eminent an authority should always receive most careful attention, but that the shifts of the spectral lines under the given conditions, are due to the cause mentioned seems impossible, since they are practically independent of the amount of material in the arc; a fact

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[138] Hale (ref. 136), 159, thereby anticipated work by C.E. St. John, E.F. Freundlich, and others around 1915. K. Hentschel, "The conversion of St. John," to appear in *Science in context, 6* (1993), and *Der Einstein-Turm, Erwin F. Freundlich und die Relativitätstheorie* (Heidelberg and Berlin, 1992).

[139] A. Schuster, "Note on the results of Messrs. Jewell, Humphreys and Mohler," *APJ, 3* (1896), 292; cf. George F. FitzGerald, "Note on the cause for the shift of spectral lines," *APJ, 5* (1897), 210–211, on 210.

[140] W.J. Humphreys, "A further study of the effect of pressure on the wave-lengths of lines in the arc-spectra of certain elements," *APJ, 4* (1896), 249-262, on 251; see also his "Changes in the wave-frequencies of the lines of emission spectra of elements, their dependence upon the elements themselves and upon the physical conditions under which they are produced," *APJ, 6* (1897), 169-232, on 183.

> well established before the publication of Schuster's suggestion, and established, too, by the very process he proposed, that is, by varying between wide limits the quantity of material in the arc.

Humphreys argued instead that:

- The observed shifts are roughly proportional to the cubic square of the atomic weights of the elements producing the spectral lines
- *If* all atoms and molecules have roughly the same density, *then* the square root of their atomic (molecular) weights is proportional to their linear dimensions
- Changes in pressure or temperature usually effect changes in the linear dimensions of bodies
- The frequency of elastic vibrations of a body is proportional to its linear dimensions
- Hence a linear increase in pressure should lead to a corresponding linear decrease in the resonance frequencies of the vibrators. Or, as Lockyer put it; "An increase of the density of the material, and presumably an increase of pressure, seemed to produce a damping effect upon the vibration period."[141]

George F. FitzGerald suggested another qualitative explanation: that an increase in pressure must increase the specific inductive capacity of the gases under pressure, and therefore must alter the vibrational frequencies of the molecules in it. "We can consequently conclude that here is certainly a *vera causa* for some shift towards the red in molecules causing light, for in them there can be no doubt that electric forces are at least a part of the forces affecting the periods of vibration." FitzGerald hoped, here presciently, that the study of spectroscopic details would throw light on atomic structure: "Everybody must feel the very greatest interest in this work. It is bringing us measurably nearer a knowledge of atomic movements and interactions, the great goal of modern physical research."[142]

Humphreys replied to FitzGerald:[143]

> The correctness of this suggestion [by FitzGerald] has not been submitted to actual experimental tests, nor does it seem very easy to do so, at least not directly, since the differences in the specific inductive capacities of gases are not sufficient to produce changes in the shifts greater than the errors of observation, even if the shifts are due entirely to the cause suggested. No matter what theory or suggestion is advanced, it must be remembered that it is imperfect if it does not account in some way for the important fact that at least many elements produce two or more groups of lines, differing greatly from each other in the magnitude of their shifts.

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141. J.N. Lockyer, "The shifting of spectral lines," *Nature*, *53* (1896), 415–417, 415.
142. FitzGerald (ref. 139), 210–211.
143. Humphreys (ref. 140), 184.

Humphreys criterion was met by no theory of the pressure effect prior to Babcock's, published thirty years later, in 1928, and making full use of quantum mechanics, spectral line classification, and quantum number assignments.

In 1898, Johannes Wilsing of the Potsdam Astrophysical Observatory tried to connect experimental results with theories about emission and absorption of lines proposed earlier by Eugen Lommel (1878) and Gustav Jaumann (1895). According to him, pressure should widen spectral lines; displacement towards the red would be a second-order effect, only perceivable in stellar spectra. Charles Godfrey at Trinity College, Cambridge, flatly contradicted Wilsing's conclusions.[144] Humphreys took up Wilsing's proposal, and tied it to J.J. Thomson's atomic model of negative electrons within a sphere of positive electricity (1904). Sir Joseph Larmor followed FitzGerald in attributing the shifts to electric properties of the surrounding gas.[145]

A more detailed and influential theory of the displacement of spectral lines produced by pressure was proposed by Owen Williams Richardson in 1907. Richardson attributed the observed shifts to the "effect of sympathetic vibrations occurring in the surrounding atoms:" increased pressure called additional forces into play analogous to dielectric polarization.[146] Richardson could not only derive the experimentally well-known linear dependence of shifts from pressure increases, but also predicted a cubic dependence of the shifts on the wavelengths of the spectral lines. Richardson's $\Delta\lambda \sim \lambda^3$-dependence was soon tested in experiments by Duffield and Rossi, both of whom claimed to confirm it, although their measurements scarcely favored the dependence on λ^3 over one on λ^2.

Once discovered, the pressure dependence of spectral lines became the object of a research industry. The more hands at work, the more complicated the results. Franz Exner, Eduard Haschek, and Heinrich Mache claimed that the wavelengths of spark spectra depended strongly upon the density of metal vapors and also upon the unknown

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[144] J. Wilsing, "Theoretical considerations respecting the dependencies of wave length on pressure which Messrs Humphreys and Mohler have observed in the arc-spectra of certain elements," *APJ*, *7* (1898), 317–329; C. Godfrey, "Note on Professor Wilsing's article on the effect of pressure on wave-length," *APJ*, *8* (1898), 114.

[145] W.J. Humphreys, "An attempt to find the cause of the width and of the pressure shift of spectrum lines," *APJ*, *23* (1906), 233–247; J. Larmor, "Note on displacement of spectral lines," *APJ*, *26* (1907), 120–122.

[146] O.W. Richardson, "A theory of the displacement of spectral lines produced by pressure," *PM*, *14* (1907), 557–578, on 558f.; cf. W.J. Humphreys, "Bericht über die Verschiebung von Spktrakkinien durch Druck," *JRE*, *5* (1908), 324–374.

conditions of discharge in the production of the spark,[147] while others insisted on the independence of these parameters, "as long as measurements are carried out correctly."[148] More and more factors appeared to influence the spectrum lines: density and temperature of the gases, electric and magnetic fields at the emitter, capacity of the electric devices used in producing the sparks, and so on.

Earlier measurements that had not taken into account these parameters or had not fixed their values were suddenly rendered unreliable or useless. The investigators, usually immunized against questions about their certified data, had to admit the limits of their measurements, took to criticizing one another's accuracy and to hunting down the errors in instrument design and experimental procedures of their rivals.[149]

4. PROBLEMS OF STANDARDIZATION

> It will only be noted here that due to individual errors and the unevenly distributed differences between Rowland and International λ, a reduction with an accuracy of better than 0.01 Å cannot be obtained. This deficiency is also strengthened by the fact that there are too few Rowland wavelength normals for iron, and most observers have therefore supplemented them by using the system of Fraunhofer lines. These lines in turn are loaded with other types of errors, such as pressure shifts, gravitational redshifts, etc., so that to most observers, the "Rowland system" does not at all constitute an identifiable system of measurements.[150]

With only slight exaggeration it might be said that each research school had its own system.

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[147] E. Haschek and H. Mache, "On the pressure in the spark," *APJ, 9* (1899), 347–357, and *12* (1900), 50–51; F. Exner and E. Haschek, "Uber die Verschiebung der Spektrallinien," Akademie der Wissenschaften, Vienna, Math.- Phys. Klasse, *Sitzungsberichte, 116:2a* (1907), 323–341.

[148] J.M. Eder and E. Valenta, "Unveränderlichkeit der Wellenlänge im Funken- und Bogenspektrum des Zinks," ibid., *112:2a* (1903), 1291–1304, and G.W. Middlekauf, "The effect of capacity and self-induction upon wave-length in the spark spectrum," *APJ, 21* (1905), 116–123; H. Kayser, "Die Veränderlichkeit der Wellenlängen im Funkenspektrum," *ZwPh, 3* (1905), 308–310, and "Die Konstanz von Wellenlängen von Spektrallinien," *ZwPh, 5* (1907), 304–308, briefly defended the view that the shifts in wavelengths were fictitious.

[149] J.M. Eder and E. Valenta, "The invariability of the the wave-lengths in the spark and arc spectrum of zinc," *APJ, 19* (1903), 251–262; Haschek and Mache (ref. 147); J.F. Mohler, "Pressure in the electric spark," *APJ, 10* (1899), 202–206.

[150] Konen (ref. 76), 797.

The realization of the potential influences of many parameters hitherto regarded as irrelevant led to the general demand for new standards. William Marshall Watts (at Manchester), Heinrich Kaiser (at Bonn), Eder and Valenta, and Exner and Haschek (both groups in Vienna) and Fabry and Pérot (later together with Buisson, at Marseilles) drew up their own spectrum tables, all expensively published, and all assigning slightly different values to the wavelengths of spectral lines.[151] A new type of research arose, consisting in the systematic comparison of the various standard tables and in the search for formulas for the conversion of their values.[152] Fabry and Buisson called a line 4427.313 Å, Rowland called it 4427.482 Å, and Kayser called it 4427.314 Å:[153]

> The conviction which had steadily been gaining ground for a long time past, that Rowland's wavelength system, otherwise quite accurate, which has been in use for the last twenty years as the exclusive basis of all spectroscopic research, is with respect to their absolute values subject to quite considerable errors, has thus received full confirmation; it has thus become apparent that a thoroughgoing reassessment of these values is necessary, using either Michelson's or some other similar interference method.

Michelson's application of interferometric methods to the definition of the unit of length led to a radically new way of defining the meter and thus to an absolute basis for precision spectroscopy. Michelson's measurements of 1895, and Benoît, Fabry, and Pérot's of

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[151] W.M. Watts, *Index of spectra* (London, 1872), as well as its Appendix M (Manchester, 1902) and Appendix A.A. (Wescliff, 1931); H. Kayser, "Standard lines in the arc spectrum of iron," *APJ, 13* (1900), 329ff, and *Handbuch der Spektroskopie, 5* (1910), 446ff., *7:1* (1924), 405ff.; Ch. Fabry and A. Pérot, "Measures of absolute wave-lengths in the solar spectrum and in the spectrum of iron," *APJ, 15* (1902), 73-96, 261-273; F. Exner and E. Haschek, *Wellenlängen-Tabellen für Spektralanalytische Untersuchungen* (2 vols., Vienna, 1902–04), and *Die Spektren der Elemente bei normalem Druck* (2 vols., Vienna, 1911-12); J.M. Eder and E. Valenta (ref. 149), *Atlas typischer Spektren* (Vienna, 1911), and *Wellenlängenmessungen des Lichtes* (Braunschweig, 1926).

[152] Jewell (ref. 97); Gustav Eberhard, "Systematic errors in the wave-lengths of the lines of Rowland's solar spectrum," *APJ, 17* (1903), 141-144; H. Kayser (refs. 148 and 152); "Bericht über den gegenwärtigen Stand der Wellenlängenmessungen," *ZwPh, 12* (1913), 296-308; Kuno Behner, "Über das Bogenspektrum des Titans von $\lambda = 7496$ bis $\lambda = 2273$," *ZwPh, 23* (1925), 323-342, Hartmann (ref. 104), "The correction of the standards of wave-lengths," *APJ, 20* (1904), 41–48; and "Tabellen für das Rowlandsche und das internationale Wellenlängensystem," Gesellschaft der Wissenschaften, Göttingen, *Abhandlungen, 10:2*, 1-78; Konen (ref. 76), 781.

[153] Values from H. Kayser, "Standards of third order of wave-lengths on the international system," *APJ, 32* (1910), 217-225; quote from Bernhard. Hasselberg, speech at the presentation of the Nobel prize to A.A. Michelson in 1907, in *Nobel Lectures 1901–1925* (Amsterdam, 1967), 162.

1907 and 1913, which gave selected wavelengths widely distributed in the solar spectrum to a precision of about 0.001 Å, forced a drastic revision of Rowland's earlier work. Rowland had believed that the *absolute* values he had given in 1889 were correct to one part in one hundred thousand, and that the *relative* errors, that is, the ratios of any two wavelengths, should not contain errors exceeding one part in a million.[154] But in 1893, Michelson's interferometric determination of the standard meter, found Rowland in error by about one part in 30,000.[155] Although this result drastically reduced the accuracy of Rowland's determinations, astrophysicists easily adapted themselves to it by multiplying his numbers by a factor close to unity. In any case, they cared more about the *relative* values of spectral lines, which would not alter by a rescaling of the *absolute* values.

The situation changed again, when Charles Fabry and Alfred Pérot made interferential measurements upon approximately thirty solar lines between 4643 Å and 6471 Å in 1901.[156] They compared their wavelengths against the standard of the red cadmium line: relying on their high-precision absolute measurement of one line, they calculated the others relative to it. If Rowland's numbers were relatively correct, their ratios for all lines should be the same, or very nearly so, as those of Fabry and Pérot. In fact, the ratio not only varied for each line, but also showed systematic tendencies (figures 16 and 17): The ratios have nearly the same value for lines close together, but they differ for lines from widely different parts of the sun's spectrum, reaching eight parts in a million, more than eight times the limit of error for relative

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[154] H.A. Rowland, "Table of standard wave-lengths," *PM, 27* (1889), 479–484; cf. Ch. Fabry and H. Buisson, "Wave-length measurements for the establishment of a system of spectroscopic standards," *APJ, 28* (1908), 169–196, on 170; William Frederick Meggers, "Standard wave-lengths," Optical Society of America, *Journal, 5* (1921), 308–322, on 309.

[155] A.A. Michelson, "Comparaison du mètre international avec la longueur d'onde de la lumière du cadmium," *CRAS, 116* (1893), 790–794, "Les méthodes interférentielles en métrologie," *JP, 3* (1894), 5–22, and "Détermination expérimentale de la valeur du mètre en longueurs d'ondes lumineuses," Bureau International des Poids et des Mesures, *Traveaux et mémoires, 11* (1895), 1–85; J.-R. Benoît, "Application des phénomènes d'interférences à des déterminations métrologiques," *JP, 7* (1898), 57–68, and "De la précision dans la détermination des longueurs en métrologie," Congrès International de Physique, *Rapports, 1* (1900), 30–77; J.R. Benoît, Ch. Fabry and A. Pérot, "A redetermination of the lengths of the red cadmium line," *APJ, 26* (1907), 378–380, "Nouvelle détermination du rapport des longueurs d'onde fondamentales avec l'unité métrique," Bureau International des Poids et des Mesures, *Traveaux et mémoires*, 15 (1913), 1–134, and "Observations," ibid., i–cxlvi.

[156] Ch. Fabry and A. Pérot, "Longueurs d'onde de quelques raies du fer," *CRAS, 132* (1901), 1264–1266, and "Mesures de longueurs d'onde en valeur absolu," *AP, 25* (1902), 98–139.

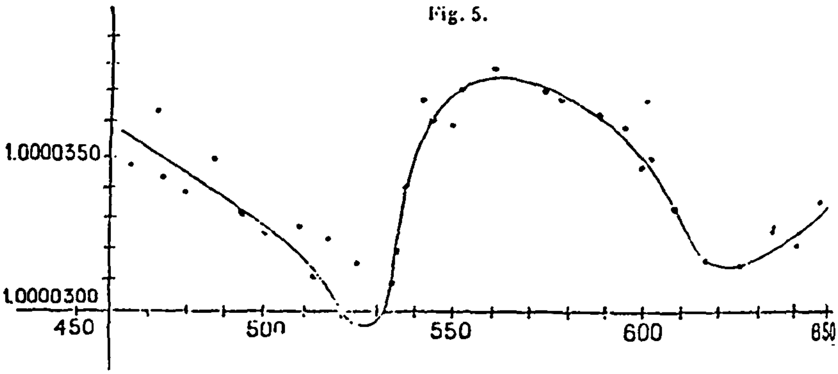

FIG. 16 Variation of Rowland's relative wavelengths as measured by Fabry and Pérot in 1902 (ref. 156), 136.

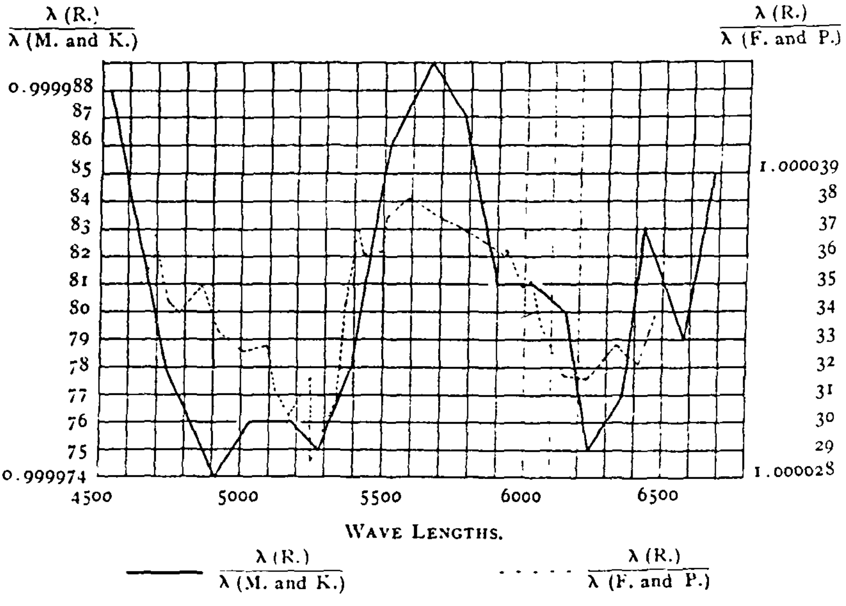

FIG. 17 Correction curve for Rowland's wavelengths, 1903. Eberhard (ref. 152), 143.

values that Rowland assigned to his values. The relative error of Rowland's determinations appeared to be ten times larger than he had claimed.

To save Rowland's results, spectroscopists devised an elaborate set of conversion tables.[157] Around 1904, Kayser claimed to have found that the errors in Rowland's determinations arose from systematic errors of the ruling of the gratings.[158] Others preferred other causes, such as disturbances in the apparatus or the failure to correct for possible Doppler shifts and temperature effects.[159] Slowly, a consensus developed that the grating showed systematic deficiencies for the determination of wavelengths over large intervals. Consequently, no reduction of Rowland's values would be possible with an absolute accuracy of more than a few percent of one Å.[160] This limitation of Rowland's system was officially acknowledged by the commission for wavelength standards of the International Union for Cooperation in Solar Research at its first meeting in 1904.

Spectroscopists needed a new reliable *primary* standard linking their measurements with metrology, and a better, different method of measuring major reference lines at regular distances not too far apart from each other, the so-called *secondary standards*. Further reference lines between these secondary standards would be required for routine measurements in the laboratories all over the world (*tertiary standards*). Gratings could be used for the determination of lines between the reference lines, because the errors induced by gratings could be neglected over intervals less than 50 Å. "The grating, which is an excellent dispersive piece, is well adapted for measurements made by interpolation within a narrow interval, but is unsuitable either for absolute measurements or for the comparison of widely separated lines."[161]

The community of astrophysicists demanded a new system of standards of wavelengths, if possible derived from artificial sources to avoid problems of the sun's physics.[162] The problem became so urgent that it prompted formation of the International Union for Co-Operation in Solar Research.[163] But only at the second meeting of the

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157. Konen (ref. 76), 797f.

158. Kayser (ref. 148), and (ref. 151f); cf. Kochen (ref. 76).

159. Janet Tucker Howell, "The fundamental law of the grating," *APJ, 39* (1914), 230–242; Jewell (ref. 97); F. Goos, "Standard wave-lengths in the arc spectrum of iron," *APJ, 35* (1912), 221–232, and "A further contribution towards the establishment of a normal system of wave-lengths in the arc spectrum of iron," *APJ, 38* (1913), 141–157.

160. Konen (ref. 76), 779ff.

161. Fabry and Buisson (ref. 154), 171.

162. Cf. G.E. Hale, "Co-operation in solar research," *APJ, 20* (1904), 306–312.

163. See the proceedings of the first meeting reported in *APJ, 20* (1904), 301ff.

International Union in Oxford in 1905 did the body take decisions toward achieving its aim practically:[164]

1. A line suitable for high-precision interferometry should be selected as a so-called *primary standard of wavelength*, defining once and for all the unit in which all other wavelengths are to be measured, differing as little as possible from 10^{-10} meters and to be called the Ångström in honor of Anders Jonas Ångström.
2. Additional lines should be selected from throughout the solar spectrum as further reference lines, the so-called *secondary standards*, to be measured by interferometric methods relative to the primary standard.[165]
3. For everyday use, further easily reproducible lines should be selected in intervals of about 50 Å, the *tertiary standards*, by careful interpolation between the secondary standards. Because nearly 4000 Å were to be covered in the visible and UV-range of the spectrum, about one hundred secondary standards, chosen mostly from the Fe-spectrum, were needed.[166]
4. A good spectral source in the laboratory would be an electric arc operating at about 6 to 10 ampères.

The community agreed on the red cadmium line as the primary standard, as the narrowest line then known, upon Michelson's recommendation based on his research on its fine structure by means of

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[164] Reported in the *Transactions of the International Union, 1* (1906), 230ff., and Fabry and Buisson (ref. 154), 172.

[165] Fabry and Buisson (ref. 154); Paul Eversheim, "Determination of wave-lengths of light for the establishment of a standard system," *APJ, 26* (1907), 172–190, "Measurement of wave-lengths of standard iron lines," *APJ, 31* (1910), 76–77, and "Wellenlängennormale im Eisenspektrum," *AP, 36* (1911), 1071–1076, *45* (1914), 454–456; A.H. Pfund, "A redetermination of the wave-lengths of standard iron lines," *APJ, 27* (1908), 197–211.

[166] Kayser, *Handbuch, 7:1* (ref. 151); Kochen (ref. 76); E.J. Evans, "The arc spectrum of iron λ 6855 to λ 7412," *APJ, 29* (1909), 157–163; Franz Papenfus, "Die Brauchbarkeit der Koinzidenzmethode zur Messung von Wellenlängen," *ZwPh, 9* (1911), 332–346, 349–360; F. Goos (ref. 159); C.E. St. John and L.W. Ware, "Tertiary standards with the plane grating: The testing and selection of standards," *APJ, 36* (1912), 14–53, *39* (1914), 5–28; Keivin Burns, "The arc spectrum of iron," Lick Observatory, *Bulletin, 8* (1913), no. 247, 27–42; Ludwig Janicki, "Wellenlängennormalen dritter Ordnung aus dem Bogenspektrum des Eisens," *ZwPh, 13* (1914), 173–185; Heinrich Viefhaus, "Ein Beitrag zur Bestimmung tertiärer Normalen," *ZwPh, 13* (1914), 209–234, 245–264; Sophie Hoeltzenbein, "Messungen im Bogenspektrum des Eisens zwecks Bestimmung tertiärer Normalen," *ZwPh, 16* (1916), 225–253; H. Werner, "Messung von Wellenlängennormalen im internationalen System für den roten Spektralbereich," *AP, 44* (1914), 289–296; F. Goos, "Wellenlängen aus dem Bogenspektrum des Eisens im internationalen System," *Astronomische Nachrichten, 199* (1914), 33–44; H. Pickhan, *Untersuchungen des Systems der Eisennormalen* (Ph.D. thesis; University of Münster, 1918), Friedrich Müller, "Beitrag zur Aufstellung des Systems internationaler Wellenlängen," *ZwPh, 22* (1922), 1–20.

interferometry.[167] At its third meeting, the International Union for Co-Operation in Solar Physics (IUCSP) attributed to this line the value 6438.4696 Å in dry air under a normal pressure of 760 mm mercury at 15°C on the basis of two nearly concordant measurements made by Michelson in 1895 and by Benoît, Fabry and Pérot in 1907.[168] The accuracy, about one part in ten million, bettered Rowland's determinations by a factor of 100. Later research into the fine structure of the red Cd-line showed that further progress in high precision spectroscopy would depend on the choice of yet another, sharper spectral line, such as λ 5649 or λ 5570 of the inert gas krypton, which have a width of only 0.006 Å and a limiting order of interference of about 600,000 at ordinary temperatures.[169]

Several observers then started to measure a group of about 80 lines using interferometric methods and a pairwise comparison with the primary standard. The independent measurements of A.H. Pfund, Paul Eversheim, and Fabry and Buisson were submitted to the fourth meeting of the International Union in 1910, which decided to average their very close values and to adopt the averages as the secondary standards.[170] Furthermore, in 1922 the International Astronomical Union adopted a supplementary system of 20 neon lines as secondary standards.[171]

Several groups of spectroscopists measured the tertiary standards during World War I, especially in Bonn (by doctoral students of Kayser) and at the U.S. National Bureau of Standards (Meggers, Burns, Kiess). In 1922 the International Astronomical Union adopted a system of 302 iron arc lines carefully interpolated between eighty secondary standards previously adopted.[172]

During his work on tertiary standards in the iron arc spectra, Fritz Goos discovered the so-called pole effect in 1913: Depending on where the slit of a spectrometer is focussed between the two poles of the electric arc, the laboratory emission wavelengths change their shape and shift by as much as 0.1A˙. Certain iron lines, particularly

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[167] International Union for Co-Operation in Solar Research, *Transactions, 1* (Manchester, 1906), 80ff., and *2* (1908), 109ff.

[168] The two values differed by less than 1 part in 16 million (= 0.0003 Å); cf. IUCSP, *Transactions, 2* (1908), 109ff.; Konen (ref. 76), 790ff.

[169] Anton Peter Weber, "Eine neue Methode höchster Genauigkeit zur interferometrischen Wellenlängenmessung und ihre erstmalige Anwendung zur Vorbestimmung der für den deutschen Anschluss des Meters an Lichtwellen vorgeschlagenen Kryptonlinien," *PZ, 29* (1928), 233–239; Konen (ref. 76), 780.

[170] *APJ, 32* (1910), 215ff., and *33* (1911), 85ff.; H. Kayser, *Handuch der Spektroskopic, 6* (Leipzig, 1912).

[171] IUCSP, *Transactions, 1*, 35ff.; Meggers (ref. 154), 311f.

[172] IUCSP, *Transactions, 1*, 35ff.

sensitive to pressure, had slightly different wavelengths in the center of the arc than near the negative pole. It took some time to establish the existence of this effect beyond doubt.[173] In the end, however, there was no choice but to amend the recommendations of the International Union with further details of the electric circuits involved, the precise distance between the two poles of the electric arc, and the point on which to focus the slit of the spectrometer; temperature and pressure varied too much over the length of the arc to allow spectroscopists to measure where they pleased.[174] All earlier measurements in which these parameters had not been specified clearly enough to recalibrate had become more or less worthless. Frustration prevailed.[175]

In connection with the testing of Einstein's prediction of a gravitational redshift, the American astrophysicist Charles Edward St. John (1857–1935) realized the need for a revised table of wavelengths of solar spectral lines. Without such a revision, Einstein's prediction could not be tested, since the test required undisputed and sharp values for both solar and laboratory wavelengths free from any other effects.[176] In 1920, at the very beginning of the work that resulted in the Mount Wilson Tables of 1928, which covered the whole range from $\lambda = 2975$ Å to 10200 Å, St. John wrote about the endeavors of Rowland more than 20 years earlier:[177]

> It has long been recognized that the wave-lengths of Rowland's Preliminary Table of Solar Spectrum Wave-Lengths, owing to an error in his primary standard, do not represent absolute values in the C.G.S. system and that the errors in the relative wave-lengths due to the method of coincidence used in passing from his primary standard are roughly periodic. It was the opinion of the solar physicists at the Brussels meeting of the International Astronomical Union in 1919 that the time had arrived when consideration should be given to the preparation of a table of solar wave-lengths based upon the international system.

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[173] Goos (ref. 159); Thomas Royds, "An investigation of the displacement of unsymmetrical lines under different conditions of the electric arc," Kodaikanal Observatory, *Bulletin, 40* (1914), 83–94; C.E. St. John and H.D. Babcock, "A study of the pole effect in the iron arc," *APJ, 42* (1915), 251; "The elimination of the pole-effect from the source for secondary standards of wave-length," *APJ, 46* (1917), 138–166.

[174] See IUCSP, *Transactions, 4* (1914), 58f.; Meggers (ref. 154), 312: 6mm arc, 6 Amp for wavelengths greater than 4000 Å, for others 4 Amp or less, a potential of 220 Volt, iron rods of 7mm diameter and the choice of the axial part in the center of the light source plus the restriction to iron lines of pressure dependency class a-d (Mt. Wilson classification).

[175] Kayser (ref. 41), 248f, 267f.

[176] Hentschel (ref. 138); cf. John Earman and Clark Glymour, "The gravitational redshift as a test of general relativity," *Studies in history and philosophy of science, 11* (1980), 251–278.

[177] Carnegie Institution of Washington, *Yearbook, 19* (1920), 228; cf. St. John, Moore, Ware, Adams, and Babcock (ref. 82).

Despite the need to improve Rowland's tables of solar wavelengths, the astrophysical community did not lose its respect for his life's work. When the second revision of Rowland's tables was to be published, Marcel Gilles Jozef Minnaert, himself involved in a photometric *Atlas* of the sun's spectrum as another complement to Rowland's *Tables*, wrote:[178]

> What we have felt ever and ever again in the course of these years, that is the deepest admiration for Henry Rowland, who accomplished a similar enterprise 70 years ago, with so much less technical means, and whose work is still now a marvel of perfection.

That an unanticipated effect—solar redshift—was discovered during Rowland's decade-long efforts to establish solar wavelength measurements to eight digits, seemed at first to constitute a major challenge to precision spectroscopy; instead, this search for the next decimal turned into a virtue for high-precision physics.

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[178] M.G.J. Minnaert, "Forty years of solar spectroscopy," in C. de Jager, ed., *Solar spectrum symposium* (Dordrecht, 1963), 3–25. Minnaert and Houtgast (ref. 82) used a Michelson grating of 12 cm width.