The past and future of American astronomy

Seventy-five years ago, astronomy was full of promise and confusion, with new instruments, ingenious ideas, "paradoxers," polemics—sometimes modern, but often sparse in physics.

Carl Sagan

The world has changed since 1899, but there are few fields that have changed more—in the development of fundamental insights and in the discovery of new phenomena—than astronomy. When we glance at the titles (see the box below) of some of the recent papers published, for example, in the *Astrophysical Journal* and *Icarus,* we can only conclude that our astronomical ancestors would have extracted a glimmer of meaning from them, but that the principal reaction would have been incredulity.

When I was asked to chair the 75th anniversary committee of the American Astronomical Society, I thought this would provide a pleasant opportunity to acquaint myself with the state of our subject at the end of the last century. I was interested to see where we have been, where we are today and, if possible, something of where we may be going. In 1897, the year of the formal dedication of the Yerkes Observatory, then the largest telescope in the world, a scientific meeting of astronomers and astrophysicists was held in connection with the commemoration. A second meeting was held at Harvard College Observatory in 1898 and a third at Yerkes Observatory in 1899. By this time what is now the American Astronomical Society had been officially founded; Richard Berendzen's accompanying article gives a full account.

A dedication and a "scandal"

A first-rate general review of astronomy during the 19th century was written by Sir Norman Lockyer and was reprinted in the 1901 *Annual Report* of the Smithsonian Institution. A laudatory article on American astronomy, published in *Nature,* which stressed the remarkable American innovation of practical instruction in observational astronomy was reprinted in the *Publications* of the Astronomical Society of the Pacific.[1] But we can obtain a better impression of the astronomy of 75 years ago by reading the scientific literature of the time: the six volumes of the *Astrophysical Journal* for 1897 through 1899, the *Publications* of the Astronomical Society of the Pacific, the *Monthly Notices* of the Royal Astronomical Society, *Observatory,* and so on. Then as now, astronomy was an international science. An interesting perspective emerges in seeing what West Coast astronomers had to say about East Coast astronomers; British astronomers about American astronomers, etc. Here are my impressions:

The astronomy of 1897 to 1899 seems to have been vigorous, combative, dominated by a few strong personalities, and aided by remarkably short publication times. The average publication time for papers in the *Astrophysical Journal* in this period seems to be better than in *Astrophysical Journal Letters* today. The fact that a great many papers were from the Yerkes Observatory where the journal was edited may have had something to do with this. The opening of Yerkes Observatory—which has the year 1895 imprinted upon it—was delayed more than a year because of the collapse of the floor, which narrowly missed killing E. E. Barnard. The accident is mentioned[2] in the *Astrophysical Journal,* but one finds no hint of negligence there. However, *Observatory* clearly implies[3] careless construction and a coverup to shield those responsible. We also discover on the same page of *Observatory* that the dedication ceremonies were postponed some weeks because of Mr Yerkes's own European travel. The *Astrophysical Journal* says that "the dedication ceremonies were necessarily postponed from October 1, 1897" but does not say why.

The Astrophysical Journal was edited by George Ellery Hale, the director of the Yerkes Observatory and by James E. Keeler, the director of Lick Observatory. However, there was a certain domination of *Ap. J.* by Williams Bay (the Wisconsin site of Yerkes Observatory)—perhaps because the Lick Observatory dominated the *Publications* of the Astronomical Society of the Pacific in the same period. Volume 5 of the *Astrophysical Journal* has no less than thirteen plates of Yerkes Observatory, including one of the power house, and the first fifty pages of volume 6 have a dozen more. The eastern dominance of the American Astronomi-

Can you imagine . . .

. . . what an astronomer of the 1890's would think if he came upon the scene, Rip Van Winkle style, to see journal titles such as these, from recent issues of the *Astrophysical Journal* and *Icarus?* "G240-72: A New Magnetic White Dwarf with Unusual Polarization"; "Relativistic Stellar Stability: Preferred Frame Effects"; "Detection of Interstellar Methylamine," "A New List of 52 Degenerate Stars"; "The Age of Alpha Centauri"; "Do OB Runaways have Collapsed Companions?"; "Finite Nuclear-size Effects on Neutrino-pair Bremsstrahlung in Neutron Stars"; "Gravitational Radiation from Stellar Collapse"; "A Search for a Cosmological Component of the Soft X-ray Background in the Direction of M31"; "The Photochemistry of Hydrocarbons in the Atmosphere of Titan"; "The Content of Uranium, Thorium and Potassium in the Rocks of Venus as Measured by Venera 8"; "HCN Radio Emission from Comet Kohoutek"; "A Radar Brightness and Altitude Image of a Portion of Venus"; and "A Mariner 9 Photographic Atlas of the Moons of Mars."

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A group of astronomers and their families pose on the steps of Lick Observatory, Mount Hamilton, in Northern California, some 75 years ago. Although the British journal *Observatory* in 1898 reported a "civil war" at Lick, this scene appears to be a peaceful one.

cal Society is also reflected by the fact that the first president of the Astronomical and Astrophysical Society of America was Simon Newcomb, and the first vice-presidents, Charles Young and Hale. *PASP* complained[4] about the difficulties in traveling to the third conference of astronomers and astrophysicists at Yerkes and appears to have voiced some pleasure that the promised demonstrations with the 40-inch refractor for this ceremony had to be postponed because of cloudy weather.[5] This was about the most in the way of inter-observatory rancor that can be found in either journal.

But in the same period *Observatory* had a keen nose for American astronomical gossip.[6] From *Observatory* we find that there was a "civil war" at Lick Observatory and a "scandal" associated with Edward Holden, the director, who is said, for example, to have permitted rats in the drinking water at Mount Hamilton. It also published a story about a chemical explosion scheduled to go off in the San Francisco Bay area and to be monitored by a seismic device on Mount Hamilton. At the appointed moment no staff member could see any sign of needle deflection except for Holden, who promptly dispatched a messenger down the mountain to alert the world of the great sensitivity of the Lick seismometer. But soon up the mountain came another messenger with the news that the test had been postponed. A much faster messenger was then dispatched to overtake the first and an embarrassment to Lick Observatory was, *Observatory* notes, narrowly averted. (Incidentally, lists of California earthquakes were regularly published in *PASP*.[7])

The youth of American astronomy in this period is eloquently reflected in the proud announcement in 1900 that the Berkeley Astronomical Department would henceforth be independent of the Civil Engineering Department at the University of California, Berkeley, as a separate entity under Armin Leuschner's direction.[8] A survey by George Airy (later the British Astronomer Royal), regretted being unable to report on astronomy in American in 1832 because essentially there was none (quoted by Newcomb[9]). He would not have said that in 1899.

There is never much sign in these journals of the intrusion of external (as opposed to academic) politics except for an occasional notice such as that of the appointment[10] by President McKinley of T.J.J. See as professor of mathematics to the US Navy and a certain continuing chilliness in scientific debates between the personnel of the Lick and Potsdam Observatories.

Some signs of the prevailing attitudes of the 1890's do occasionally trickle through as, for example, in this description of an eclipse expedition[11] to Siloam, Georgia, 28 May 1900: "Even some of the whites were lacking in a very deep knowledge of things 'eclipse-wise.' Many thought it was a money-making scheme and what I intended to charge for admission was a very important question, frequently asked. Another idea was that the eclipse could be seen only from the inside of my observatory... Just here I wish to express my appreciation of the high moral tone of the community, for, with a population of only 100, including the immediate neighborhood, it sustains 2 white and 2 colored churches and during my stay I did not hear a single profane word... As an unsophisticated Yankee in the Southland, unused to Southern ways, I naturally made many little slips that were not considered 'just the thing.' The smiles at my prefixing 'Mr.' to the name of my colored helper caused me to change it to 'Colonel,' which was entirely satisfactory to everybody."

Low salaries and "volunteer assistants"

A board of visitors was appointed to resolve some (never publicly specified) problems at the US Naval Observatory. A report of this group, which consisted of two obscure US senators and professors E. C. Pickering, George Comstock, and Hale, is illuminating because it mentions dollar amounts. We find that the annual running costs of the major observatories in the world[12] were: Naval Observatory, \$85 000; Paris Observatory, \$53 000; Greenwich Observatory, \$49 000; Harvard Observatory, \$46 000, and Pulkowa Observatory, \$36 000. We find that the directors' salaries were \$4000 at the US Naval Observatory and \$5000 at the Harvard Observatory. The distinguished board of visitors recommended that in a "schedule of salaries which could be expected to attract astronomers of the class desired" the salary of directors of observatories should be \$6000. At the Naval Observatory, computers (exclusively human at the time) were paid \$1200 per annum, but those at the Harvard Observatory only \$500. In fact, all salaries at Harvard, except for the director's, were significantly lower than at the Naval Observatory. The committee states: "The great difference in salaries at Washington and Cambridge, THE

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The editorial page of volume VIII of "Ap. J.," for the second half of the year 1898, shows that George Ellery Hale of Yerkes and James E. Keeler of Lick Observatory were the two coeditors.

especially for the officers of lower grade, is probably unavoidable. This is partly due to Civil Service Rules." An additional sign of astronomical impecuniosity is the announcement[13] of the post of "volunteer research assistant" at Yerkes, which had no associated pay but which was said to provide good experience for students with higher degrees.

Then as now, astronomy was besieged by proponents of fringe or crackpot ideas, which however were then described by the gentler term "paradoxers." One American paradoxer[14] proposed a telescope with 91 lenses in series as an alternative to a telescope with a smaller number of lenses of larger aperture. The British in this period were similarly plagued, but in perhaps a gentler way. For example, an obituary notice in *Monthly Notices*[15] of Henry Perigal informs us that the deceased celebrated his 94th birthday by becoming a member of the Royal Institution, but was elected a fellow of the Royal Astronomical Society in 1850. However "our publications contain nothing from his pen." The obituary describes "the remarkable way in which the charm of Mr. Perigal's personality won him a place which might have seemed impossible of attainment for a man of his views; for there is no masking the fact that he was a paradoxer pure and simple, his main conviction being that the Moon did not rotate, and his main astronomical aim on life being to convince others, and especially young men not hardened in the opposite belief, of their grave error. To this end he made diagrams, constructed models, and wrote poems; bearing with heroic cheerfulness the continued disappointment of finding none of them of any avail. He has however, done excellent work apart from this unfortunate misunderstanding."

The number of American astronomers in this period was very small. The by-laws of the Astronomical and Astrophysical Society of America state that 20 members constitute a quorum. By the year 1900, only nine doctorates had been granted in astronomy in the US.[16] In that year, there were four astronomical doctorates, two from Columbia University for G. N. Bauer and Carolyn Furness; one from the University of Chicago for Forest Ray Moulton; and one from Princeton University for Henry Norris Russell.

Prize-winning research

Some idea of what was considered important scientific work in this period can be garnered from the prizes that were awarded. Barnard received the Gold Medal of the Royal Astronomical Society in part for his discovery of Jupiter 5 and his astronomical photography with a portrait lens. His steamer, however, was caught in an Atlantic storm and he did not arrive for the celebration ceremony. He is described as requiring several days to recover from the storm, whereafter the Society hospitably gave a second dinner for him.

Barnard's lecture seems to have been spectacular and made full use of that recently improved audio-visual aid, the lantern-slide projector. And it is easy to see the effect that Barnard's photographs had on his audiences. Consider, for example, the photograph (on page 26) by Barnard of the region of the Milky Way near Theta Ophiuchi. In his discussion[17] of this photograph, Barnard concludes that "the entire groundwork of the Milky Way ... has a substratum of nebulous matter." (Meanwhile H. K. Palmer[18] reported no nebulosity in photographs of the globular cluster M13). Barnard, a superb visual observer, expressed considerable doubts about the Lowellian view of Mars. In his thanks to Barnard for his lecture, the President of the Royal Astronomical Society, Sir Robert Ball, expressed[19] concern that henceforth he "should regard the canals in Mars with some suspicion, nay, even the seas had partly fallen under a ban. Perhaps the lecturer's recent experiences on the Atlantic might explain something of this mistrust." Percival Lowell's views were not then in favor in England, as another notice in *Observatory*[20] indicated. In response to an inquiry on what books had most pleased and interested him in 1896, Lockyer replied, "*Mars* by Percival Lowell, *Sentimental Tommy* by J. M. Barrie. (No Time for Reading Seriously)."

Prizes in astronomy for 1898 awarded by the Academie Française included one to Seth Chandler for the discovery of the variation in latitude; one to Aristarch A. Belopolsky, partly for studies of spectroscopic binaries; and one to Charles Schott for investigations of terrestrial magnetism.

There was also a prize competition for the best treatise on "the theory of perturbations of Hyperion." We are informed[21] that "the only essay presented was that by Dr. G. W. Hill of Washington to whom the prize was awarded." The Astronomical Society of the Pacific's Bruce Medal was awarded in 1899 to Arthur Auwers of Berlin. The dedicatory address[22] included the following quote: "Today Auwers stands at the head of German astronomy. In him is seen the highest type of investigator in our time, one perhaps better developed in Germany than in any other country. The work of men of this type is marked by minute and careful research, untiring industry in the accumulation of facts, caution in propounding new theories or explanations, and, above all, the absence of effort to gain recognition by being the first to make a discovery." In 1899 the Henry Draper Gold Medal of the National Academy of Sciences was presented for the first time in seven years. The recipient was Keeler. In 1898, William Brooks, whose observatory was not far from Rochester in Geneva, N. Y., announced[23] the discovery of his 21st comet—which Brooks described as "achieving his majority." Shortly thereafter he received[24] the Lalande prize of the Academie Française for his record in discovering comets. In 1897, in connection with an exhibition in Brussels, the Belgian government offered prizes for the solutions of certain problems in astronomy. These problems included[25] the value of the acceleration due to gravity, secular acceleration of the Moon, the net motion of the solar system, the variation of latitude, the photography of planetary surfaces, and the nature of the canals of Mars. A final topic was the invention of a method to observe the solar corona in the absence of an eclipse. (*Observatory*[26] commented "if this pecuniary reward does induce anyone to solve this last problem or in fact any of the others, we think the money will be well spent.")

However, in actually reading the scientific papers of this time, the focus seems clearly to have shifted to other topics than those for which prizes were being given.

Sir William and Lady Huggins performed laboratory experiments,[27] which showed that at low pressures the emission spectrum of calcium exhibited only the H and K lines. They concluded that the Sun was composed chiefly of hydrogen, helium, "coronium" and calcium. Huggins had earlier established a spectral sequence that he believed was evolutionary. The Darwinian influence in science was very strong in this period and, among American astronomers,

A region of the Milky Way as photographed by E. E. Barnard with his 6-inch portrait lens.

See's work was notably dominated by a Darwinian perspective. Huggins's spectral sequence[28] is compared with the present Morgan–Keenan spectral types in the Table on this page.

In it we can see the origin of the present terms "early" and "late" spectral type, which reflect the Darwinian spirit of late Victorian science, as well as the beginnings—through the later Hertzsprung–Russell diagram—of modern theories of stellar evolution. W. H. S. Monck,[29] using the Harvard classification, thought that the spectral types in order of brightness should be B, A, K, M, F, G. His main contribution seems to have been to suggest that B came before A. He also noted a possible connection of high proper motion with spectral type M. The bulk density of stellar members of close binaries was deduced[30] to be low, among others by Russell.

The only paper in the six volumes of the *Astrophysical Journal* for 1897–99 that in any way resembles modern theoretical attempts at understanding stellar evolution is an extract by the editors of past writings of Ritter.[31] This is early Lane-Emden physics. A ratio of specific heats of 4/3 was recognized for adiabatic equilibrium, but stars were thought to run off the Kelvin–Helmholtz contraction. Ritter concluded that, 5.5 million years ago, the radius of the Sun equalled the present semimajor axis of the Earth's orbit, and therefore that geologists who thought that the age of the Earth is hundreds of millions of years were mistaken. He does state that the temperature of a star may be much greater than that of the Sun only when the mass of the star is much greater than that of the Sun.

Input from physics

There were major developments in physics during this period and readers of the *Astrophysical Journal* were alerted of it by the reprinting of summaries of important papers, for example by Antoine Henri Becquerel, Johann Balmer, Pieter Zeeman, H. A. Lorentz, Charles Fabry and Alfred Perot, Lord Kelvin, and Friedrich Paschen. Experiments were still being performed on the basic radiation laws. We find Paschen[32] suggesting that the Wien displacement law is of the form $\lambda_{max}T^{0.95}$ = const, and that the observational form of the Stefan–Boltzmann law may involve an exponent of 4.7 instead of 4. On the other hand a *hohlraum* experiment[33] to duplicate a blackbody includes an observational confirmation of the Stefan–Boltzmann exponent of 4, showing numerous points lying on the theoretical curve. The author, however, then goes on to deduce an effective solar temperature of 11 500 K. There was also a paper[34] on the meaning and validity of Kirchhoff's law.

In some papers, the level of physical sophistication was not of the highest caliber—as for example, an article[35] in which the linear momentum of Mars is calculated as the single product of the mass of the planet and the linear velocity of the surface. It concluded "the planet, exclusive of the cap, has a momentum of 183 and 3/8 septillion foot pounds." Exponential notation for large numbers was evidently not in wide use.

In this time we have the publication of visual and photographic light curves, for example, of stars[36] in M5 and experiments[37] in filter photography of Orion by Keeler. An obviously exciting topic was time-variable astronomy—which then must have generated something of the excitement that pulsars, quasars and x-ray sources do today. There were many studies of variable velocities in the line of sight from which were derived the orbits of spectroscopic binaries[38] as well as periodic variations in the apparent velocity of Omicron Ceti from the doppler displacement of H gamma and other lines.[39]

Huggins's ordered sequence of spectral classification, with modern (Morgan-Keenan) spectral classification in parentheses.

	Sirius (A1 V)	Vega (A0 V)
	—	
Young	Altair (A7 IV-V)	
	Rigel (B8 Ia)	
	Deneb (A2 Ia)	
	—	
Old	—	
	Capella (G8, G0)	Sun (G0)
	Arcturus (K1 III)	
	Aldebaran (K5 III)	
	Betelgeuse (M2 I)	

PRELIMINARY TABLE OF SOLAR SPECTRUM WAVE-LENGTHS. XVI.

By HENRY A. ROWLAND.

Wave-length	Substance	Intensity and Character	Wave-length	Substance	Intensity and Character
3259.720		0	3265.454	Co	0 N
3259.834		0	3265.678		2
3259.977		0000 N	3265.762	Fe	4
3260.110	Cr, Fe	4	3265.822		00
3260.266		000	3266.016	V	1
3260.386 s	Mn, Ti-Fe	5 d?	3266.102		000
3260.593		0000	3266.275		000
3260.673		00	3266.362		000
3260.813		000 N	3266.561		00 Nt?
3260.950	Co	1	3266.798		3 N
3261.079		0000 N	3267.072		1
3261.186		00 N d?	3267.184		1
3261		0000 N	3267.328		00
	Fe	?	391		000

A small fragment of Henry Rowland's 20 000-wavelength "preliminary" solar spectrum table, published by Ap. J. in the 1890's. His accurate ruling engine revolutionized spectroscopy.

E. F. Nichols performed the first infrared measurements of stars[40] at Yerkes Observatory. The study concludes: "We do not receive from Arcturus more heat than would reach us from a candle at a distance of 5 or 6 miles." No further calculations are given. The first experimental observations of the infrared opacity of carbon dioxide and water vapor were performed in this period by Rubens and Aschkinass,[41] who discovered the $\nu_{2\,\mathrm{fundamental}}$ of carbon dioxide at 15 microns and the pure rotation spectrum of water. In essence they also found the 10- and 20-micron windows in the Earth's atmosphere.

There is preliminary photographic spectroscopy of the Andromeda nebula by Chistoph Scheiner[42] at Potsdam who concludes "the previous suspicion that the spiral nebulae are star clusters is now raised to a certainty." An example of the level of personal vituperation tolerated at this time is the following extract from a paper[43] by Scheiner, in which W. W. Campbell is criticized: "Professor Campbell attacks, with much indignation, some remarks of mine criticizing his discoveries... Such sensitiveness is somewhat surprising on the part of one who is himself given to severely taking others to task. Further, an astronomer who frequently observes phenomena which others cannot see, and fails to see those which others can, must be prepared to have his opinions contested. If, as Professor Campbell complains, I have only supported my views by a single example, I was only withheld by courteous motives from adding another. Namely, the fact that Professor Campbell cannot perceive the lines of aqueous vapor in the spectrum of Mars which were seen by Huggins and Vogel in the first place, and, after Mr. Campbell had called their existence in question, were again seen and identified with certainty by Professor Wilsing and myself." The amount of water vapor in the Martian atmosphere that is now known to exist would have been entirely indetectable by the spectroscopic methods in use at the time.

Should red be on the left?

Spectroscopy was a dominant element in the science of the time. Henry Rowland's published solar spectrum (partly reproduced here) ran to 20 000 wavelengths, each to seven significant figures. Occasionally the astronomers of the time took note of the extraordinary nature of their discoveries: "It is simply amazing that the feeble twinkling light of a star can be made to produce such an autographic record of substance and condition of the inconceivably distant luminary" (Young, quoted by Keeler[44]). A major topic of debate for the *Astrophysical Journal* was whether spectra should be shown with red to the left or to the right. Those who favored red to the left, cited the analogy of the piano, but *Ap. J.* opted gamely for red to the right. Some room for compromise was available on whether, in lists of wavelengths, red should be to the top or to the bottom. Feelings ran high and Huggins wrote[45] to say "any change . . . would be little less than intolerable." But the *Ap. J.* won, as all astronomers know.

Another major discussion in this period was on the nature of sunspots. G. Johnstone Stoney proposed[46] that they were due to a layer of condensed clouds in the photosphere. But this was objected to by Wilson and Fitzgerald[47] on the grounds that no conceivable condensates could exist at these high temperatures with the possible exception of carbon. They suggested instead that sunspots are due to "reflection by convection streams of gas." John Evershed[48] had a more ingenious idea. He thought that sunspots were holes in the photosphere permitting us to see to much greater and hotter depths. But why are they dark? He proposed that all the radiation would be moved from the visible to the inaccessible ultraviolet. This of course is before the Planck distribution. It was not at this time thought impossible that the spectral distributions of blackbodies of different temperatures should cross; indeed, some experimental curves of this period showed such crossing—because of emissivity and absorptivity differences, as we now know.

A refractor for Pasadena?

Sir William Ramsay had recently discovered the element krypton, which was said to have, among fourteen detectable lines, one at 5570 Å, coincident with "the principal line of the aurora." E. B. Frost concluded[49] "Thus it seems that the true origin of that hitherto perplexing line has been discovered."

There were a great many papers on instrumental design, one of the more interesting being by Hale.[50] In January 1897, he suggested that both refractors and reflectors were needed, but there is a clear movement toward reflectors, especially equatorial coudé telescopes. In a historical memoir Hale[51] mentions that the 40-inch lens was available to Yerkes Observatory only because a previous plan to build a large refractor near Pasadena, California, had fallen through. What, I wonder, would the history of astronomy have been like if the plan had succeeded? Curiously enough, Pasadena seems to have made an offer to the University of Chicago to have the Yerkes Observatory situated there. It would have been a long commute for 1897.

At the end of the 19th century, solar-system studies displayed the same mixture of future promise and current confusion that the stellar work did. One of the most notable papers of the period is Russell's on "The Atmosphere of Venus."[52] This is a discussion of the extension of the cusps of crescent Venus, based in part on the author's observations with the 5-inch *finder* telescope of the "great equatorial" of Halsted Observatory at Princeton. The essence of the analysis is correct by present standards. Russell concluded that refraction was not responsible for the extension of the cusps and that the cause was to be found in scattering: ". . . the atmosphere of Venus, like our own, contains suspended particles of dust or fog of some sort, and . . . what we see is the upper part of this hazy atmosphere, illuminated by rays that have passed close to the planet's surface." He later says that the surface may be a dense cloud layer. The height of the haze is calculated as about 1 km above what we would now call the main cloud deck, a number which is just consistent with Mariner 10 limb photography. Although Russell thought, from the work of others, that there was some spectroscopic evidence for water vapor and oxygen in a thin Venus atmosphere, the essence of his argument has stood the test of time remarkably well.

William H. Pickering announced[53] his discovery of Phoebe, the outermost satellite of Saturn, and A. E. Douglass of Lowell Observatory published observations that led him to conclude[54] that Jupiter 3 rotates about 1 hour slower than its period of revolution.

Others who estimated periods of rotation in this period were not quite so successful. For example there was a Leo Brenner who observed from the Manora Observatory in a place called Lussinpiccolo. Brenner severely criticized[55] Lowell's estimate of the rotation period of Venus. Brenner himself compared two drawings of Venus in white light made by two different people four years apart—from which he deduced a rotation period of 23 h 57 m 36.377 28 s, which he said agreed well with the mean of his most reliable drawings. Considering this, Brenner found it incomprehensible that there could still be partisans of a 224.7-day rotation period and concluded "an inexperienced observer, an unsuitable telescope, an unhappily chosen eyepiece, a very small diameter of the planet, observed with an insufficient power, and a low declination, all together explained Mr. Lowell's peculiar drawings." The truth, of course, does not even lie between the extremes of Lowell and Brenner but rather at the other end of the scale with a minus sign, a retrograde period of 243 days.

In another communication, Brenner begins[56] "Gentlemen: I have the honor to inform you that Mrs. Manora has discovered a new division in the Saturnian ring system"—from which we discover that there is a *Mrs.* Manora at the Manora Observatory in Lussinpiccolo and that she performs observations along with Herr Brenner. Then follows a description of how the Encke, Cassini, Antoniadi, Struve, and Manora divisions are all to be kept straight.

The Moon is for troglodytes?

Not everyone believed the quoted rotation periods of the inner planets. For example, Holden criticized[57] Pickering's confirmation of Giovanni Schiaparelli's conclusion that Mercury was in synchronous rotation. Schiaparelli, incidentally, was chiefly celebrated in the late 1890's for his discoveries of the synchronous rotations of Mercury and Venus and the canals of Mars.

At the Second Conference of Astronomers and Astrophysicists at Cambridge—which, incidentally, Charles Abbott attended—there was a paper on the "suggestion" that asteroid rotation, if any, might be deduced from a light curve. But no variation of the brightness with time was found and Henry Parkhurst concluded[58] "I think it is safe to dismiss the theory."

Venus drawn here by Barnard. Scattering explains the extensions of the cusps.

In discussion of the thermal properties of the moon, made independently of the one-dimensional equation of heat conduction but based on laboratory emissivity measurements, Frank Very concluded that a typical lunar daytime temperature was about 100°C—exactly the right answer. His conclusion is worth quoting: "Only the most terrible of Earth's deserts where the burning sands blister the skin, and the men, beasts, and birds drop dead, can approach noontide on the cloudless surface of our satellite. Only the extreme polar latitudes of the Moon can have an endurable temperature by day, to say nothing of the night, when we should have to become troglodytes to preserve ourselves from such intense cold." The expository styles were often fine.

Earlier in the decade Loewy and Puiseux at the Paris Observatory had published an atlas of lunar photographs, and they discussed[59] its theoretical consequences. The Paris group proposed a modified volcanic theory for the origin of the lunar craters, rills and other topographic forms, which was later criticized by Barnard after he examined the planet with the 40-inch telescope. Barnard was then criticized by the Royal Astronomical Society for his criticism, and so on. One of the arguments in this debate had a deceptive simplicity: Volcanoes produce water; there is no water on the moon; therefore the lunar craters are not volcanic. While most of the lunar craters are not volcanic, this is not a convincing argument in that it neglected the problem of possible sinks for water. Very's conclusions on the temperature of the lunar poles and nighttime could have been read with some profit. The other possibility is that water might escape from the Moon.

This was recognized by Stoney in a remarkable paper[60] called "Of Atmospheres upon Planets and Satellites." He deduced that there should be no lunar atmosphere because of what today we call Jeans escape, nor any large buildup of hydrogen or helium on the Earth. He believed there was no water vapor in the Martian atmosphere and that the atmosphere and caps of that planet were probably carbon dioxide. He implied that hydrogen and helium were to be expected on Jupiter, and that Triton might have an atmosphere. He also concluded that Titan should be airless, a prediction with which some modern theorists agree—although Titan seems to have another view of the matter.

Eyeball spectroscopy

In this period there are also a few breathtaking speculations, such as one[61] by the Rev. J. M. Bacon that it would be a good idea to perform astronomical observations from high altitudes—for example, from a free balloon. He suggested that there would be at least two advantages, better seeing and ultraviolet spectroscopy.

Hermann Vogel had previously found, by eyeball spectroscopy, an absorption band at 6183 Å in the body of Saturn. Subsequently the International Color Photo Company of Chicago made photographic plates which were so good that wavelengths as long as H alpha could be detected for a 5th magnitude star. This new emulsion was used at Yerkes, and Hale reported[62] that there was no sign of the 6183 Å band for the rings of Saturn. The band is now known to be at 6190 Å and is $6\nu_3$ of methane.

Another consequence of Lowell's writings can be gleaned from Keeler's address[63] at the dedication of the Yerkes Observatory: "It is to be regretted that the habitability of the planets, a subject of which astronomers profess to know little, has been chosen as a theme for exploitation by the romancer, to whom the step from habitability to inhabitants is a very short one. The result of his ingenuity is that fact and fancy become inextricably tangled in the mind of the layman, who learns to regard communication with the inhabitants of Mars as a project deserving serious consideration (for which he may even wish to give money to scientific societies), and who does not known that it is condemned as a vagary by the very men whose labors have excited the imagination of the novelist. When he is made to understand the true state of our knowledge of these subjects, he is much disappointed and feels a certain resentment towards science, as if it had

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imposed upon him. Science is not responsible for these erroneous ideas, which, having no solid basis, gradually die out and are forgotten."

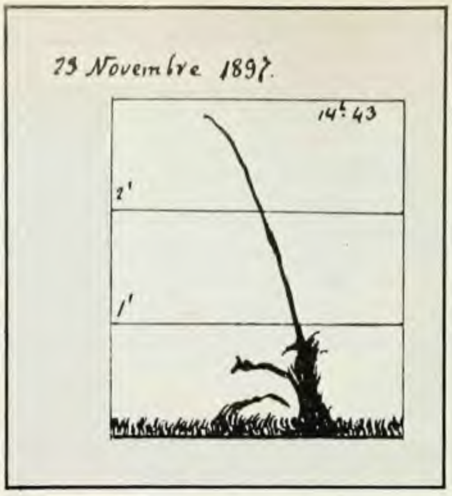
The address[64] of Simon Newcomb at the dedication of Yerkes Observatory contains some remarks with a remarkably modern ring: "Is the man thus moved into the exploration of nature by an unconquerable passion more to be envied or pitied? In no other pursuit does such certainty come to him who deserves it. No life is so enjoyable as that whose energies are devoted to following out the inborn impulses of one's nature. The investigator of truth is little subject to the disappointments which await the ambitious man in other fields of activity. It is pleasant to be one of a brotherhood extending over the world in which no rivalry exists except that which comes out of trying to do better work than anyone else, while mutual admiration stifles jealousy... As the great captain of industry is moved by the love of wealth and the politician by the love of power, so the astronomer is moved by the love of knowledge for its own sake and not for the sake of its application. Yet he is proud to know that his science has been worth more to mankind than it has cost... He feels that man does not live by bread alone. If it is not more than bread to know the place we occupy in the universe, it is certainly something that we should place not far behind the means of subsistence."

Waiting for Bohr

After reading through the publications of astronomers three quarters of a century ago, it is an irresistible temptation to imagine the 150th anniversary meeting of the American Astronomical Society—or whatever name it will have metamorphosed into by then—and imagine how our present endeavors will be viewed.

In examining the late 19th century literature, we are amused at some of the debates on sunspots, and impressed that the Zeeman effect was not considered a laboratory curiosity but something which astronomers should devote considerable attention to. These two threads intertwined, as if prefigured, a few years later in Hale's discovery of large magnetic field strengths in sunspots.

Likewise we find innumerable papers in which the existence of a stellar evolution is assumed but its nature remains deeply hidden; in which the Kelvin–Helmholtz contraction was considered the only possible stellar energy source. But at the same time, and sometimes in the same volume of the *Astrophysical Journal*, there is acknowledgment of curious work being done by Becquerel in France on radioactivity. Here again we see the two apparently unrelated



Solar prominences such as this, captured in Ap. J., stimulated speculation about sunspots.

threads moving through our few-year snapshot of late 19th-century astronomy and destined to intertwine 40 years later.

There are many related examples, for example in the interpretation of series spectra of non-hydrogenic elements obtained at the telescope and pursued in the laboratory. New physics and new astronomy were the complimentary sides of the emerging science of astrophysics.

Accordingly, it is difficult not to wonder how many of the deepest present debates—for example on the nature of quasars or the properties of black holes or the emission geometry of pulsars—must await an intertwining with new developments in physics. If the experience of 75 years ago is any guide, there will already be people today who dimly guess which physics will join with which astronomy. And, a few years later, the connection will be considered obvious.

We also see in the 19th-century material a number of cases in which the observational methods or their interpretations are clearly in default by present standards. In one of the worst examples, planetary periods are deduced to ten significant figures by the comparison of two drawings, made by different people, of features which we now know to be unreal to begin with. But there are many others, including a plethora of "double-star measurements" of widely separated objects; a fascination with pressure and other effects on the frequencies of spectral lines when no one is paying any attention to curve-of-growth analysis; and acrimonious debates on the presence or absence of some substance based solely upon naked-eye spectroscopy.

Also curious is the sparseness of the physics in late Victorian astrophysics. Reasonably sophisticated physics is almost exclusively the province of geometrical and physical optics, the photographic process, and celestial mechanics. To make theories of stellar evolution based on stellar spectra without wondering much about the dependence of excitation and ionization on temperature, or attempting to calculate the subsurface temperature of the Moon without ever solving Fourier's equation of heat conduction seem to me to be less than quaint. In seeing elaborate empirical representations of laboratory spectra, the modern reader becomes impatient for Niels Bohr and Erwin Schrödinger and their successors to come along.

How will the future judge our work?

I wonder how many of our present debates and most celebrated theories will appear, from the vantage point of the year 2049, marked by shoddy observations, indifferent intellectual powers or inadequate physical insight. I sense that we are today more self-critical than in 1897; that, because of the larger population of astronomers, we check each other's results more often; and that, in part because of the existence of organizations such as the American Astronomical Society, the standards of exchange and discussion of results have risen significantly. I hope our colleagues of 2049 will agree.

The major advances between 1899 and 1974 must be considered technological. But in 1899 the world's largest refractor had been built. It is still the world's largest refractor. A reflector of 100-inch aperture was beginning to be considered. We have improved on that aperture only by a factor of two in the intervening years. But what would our colleagues of 1899—after the discovery of electromagnetic radiation but before its widespread application—have made of the Arecibo Observatory or the Very Large Array or Long Baseline Interferometry? (Michelson would have appreciated that.) Or checking out the debate on the period of rotation of Mercury by radar doppler spectroscopy? Or testing the nature of the lunar surface by returning some of it to Earth? Or pursuing the problem of the nature and habitability of Mars by orbiting it for a year and returning 7200 photographs of it, each of higher quality than the best 1899 photographs of the Moon? Or landing on the planet with imaging systems, seismometers and gas chromatograph–mass spectrometers, which did not even vaguely exist in 1899? Or testing cosmological models by orbital ultraviolet spectroscopy of interstellar deuterium—when neither the models to be tested or the existence of the atom which tests it were known in 1899, much less the technique of observation?

Spacecraft to all planets

In the last 75 years it is clear that American and world astronomy have moved enormously beyond even the most romantic speculations of the late Victorian astronomers. And in the next 75 years? It is possible to make pedestrian predictions. We will have completely examined the electromagnetic spectrum from rather short gamma rays to rather long radio waves. We will have sent unmanned spacecraft to all of the planets and most of the satellites in the solar system. We will have launched spacecraft into the Sun to do experimental stellar structure, beginning perhaps—because of the low temperatures—with the sunspots. (Hale would have appreciated that.) I think it likely that, 75 years from now, we will have launched subrelativistic spacecraft—traveling at about one-tenth the speed of light—to the nearby stars. Among other benefits such missions permit direct examination of the interstellar medium and give us a longer baseline for VLBI than many are thinking of today. We will have to invent some new superlative to succeed "very"—perhaps "ultra." I cannot but think that the nature of pulsars and quasars will be by then well in hand. It is even possible that we will have opened up a regular communications channel with civilizations on planets of other stars, and that the cutting edge of astronomy as well as many other sciences will come from a kind of *Encyclopaedia Galactica* transmitted at very high bit rates to some immense array of radio telescopes.

But in reading the astronomy of 75 years ago, I think it likely that, except for interstellar contact, these achievements, while interesting, will be considered rather old-fashioned astronomy; that the real frontiers and the fundamental excitement of the science will be in areas depending on new physics and new technology that we can today at best dimly glimpse.

* * *

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