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The 1920 Shapley–Curtis Discussion: Background, Issues, and Aftermath

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ABSTRACT. No one now living attended the original lectures by Curtis and Shapley, and the scientific and other worlds in which they moved are connected to ours only by the written record and second-hand stories. Depending on which corners you choose to peer into, those worlds can seem remarkably modern or remarkably ancient. As is often the case for classic dichotomies, the wisdom of hindsight reveals that each of the speakers was right about some things and wrong about others, both in choosing which data to take most seriously and in drawing conclusions from those data. Modern (mostly casual) discussions of the 1920 event leave the impression that Shapley was, on the whole, the winner. But the two men's reactions to Hubble's discovery of Cepheids in the Andromeda galaxy make clear that both felt the issue of existence of external galaxies (on which Curtis had been more nearly correct) was of greater long-term importance than the size of the Milky Way (on which Shapley had been more nearly correct). Shapley is much the better known today and is generally credited in text books with the Copernican task of getting us out of the center of the galaxy. Under modern conditions, he would probably also have gotten most of the press notices. Curtis's repeated theme, "More data are needed," is remarkably difficult, then as now, to turn into a headline.

1. INTRODUCTION

The suggestion came originally from George Ellery Hale, whose father had endowed a lecture series for the National Academy of Sciences. After some initial hesitation, the NAS Home Secretary, C. G. Abbot,[1] agreed that the 1920 William Ellery Hale lectures would be a discussion on "The Distance Scale of the Universe," with Harlow Shapley of Mt. Wilson Solar Observatory and Heber Doust Curtis of Lick Observatory as the discussants. Both the published versions of their presentations (Curtis 1921; Shapley 1921) and the notes from which they spoke (Hoskin 1976) are now available, as is a good deal of information on the lead-up to what much later came to be called "the great debate" and on its scientific aftermath.

We first examine the cultural and scientific environments in which the 1920 event occurred, then the event and its participants, ending with an examination of the scientific issues as then perceived and as now understood. It is not clear whether any very useful lessons for the case of gamma-ray bursters can be drawn. As is frequently (but not always!) the case in scientific disputes, Shapley and Curtis each had hold of portions of the correct elephant.

[1]Solar astronomer Charles Greeley Abbot, dying on December 17, 1973 at the age of 101, became the last surviving of the major participants in the original Curtis–Shapley lectures. He was middle-named for newspaperman Horace Greeley, the (arguably unfairly) defeated Democratic presidential candidate of 1872. The name was shared by Hjalmar Horace Greeley Schacht, the practical German economist who stabilized the mark in the wake of post-World-War-I runaway inflation and survived the rigors of both World War II and post-war imprisonment to die at home. The name seems to have been luckier for its later holders than for the original one.

2. THE WORLD IN 1920

At the time of their Academy encounter, Heber Doust Curtis and Harlow Shapley were employed, respectively, at Lick and Mt. Wilson Observatories. A born Californian, I thought first of probing their world by comparing the road maps they would have used with the ones that now guide us to the observatory sites. At first glance, the differences seem small. The main north–south route into the Oakland–San Francisco area, then as now, split to go both ways around the Bay. And a motorist striving to get over the mountains surrounding Los Angeles had a choice of two routes, one now called the Hollywood Freeway and one the Golden State Freeway, which follow the routes then called Cahuenga pass and San Fernando Road, the latter nearly the old Spanish El Camino Real from Mission San Gabriel to Mission San Fernando.

The speed limit on the open road was, however, 35 mph (30 mph in Oregon), and the driving instructions rejoiced in stretches that were "paved all the way" and presented "no grades steeper than 12%." Alum Rock Road, where one begins the modern climb out of San Jose to Lick was on the maps, but petered out within a few miles into randomly placed images of hillocks and mountains that might almost have been labeled "here be dragons." The Mt. Wilson road was both better marked and more often traveled by casual visitors, but the site of Palomar Observatory was simply a random part of northern San Diego County, between the Pala Indian Reservation and "Nellie Warner's Hot Springs." The modern access road, from the south, was built by San Diego County much later. According to a contemporary hand-drawn map, the site could, however, be reached from the west, via a route later called Harrison Grade (and then carrying a name so politically incorrect that I dare not mention it) past landmarks like "Doane's old cabin" and "Elbow Creek Telephone Line."

The auditorium in which we meet had existed for about seven years and contained seats made of materials suitable for the pre-microphonic age. Curtis and Shapley necessarily filled the room with their own voices.

2.1 Politics, History, and Demographics

The 9:30 pm Conversatione following the 1920 William Ellery Hale lectures took place without the customary glasses of wine, for the 19th amendment to the US Constitution took effect on January 16th, ushering in "the great experiment" of prohibition (which, though it had the desired effect of considerably decreasing ethanol consumption, is nevertheless generally held to have failed).

That year, also, American women went to the polls nationwide for the first time, increasing voter turn out nearly 25% over the previous two electrons and helping to elect Warren Gamliel Harding and Calvin Coolidge over James M. Cox and Franklin Delano Roosevelt by 16.1 to 9.1 million votes (by modern standards an overwhelming majority). Eugene V. Debs, running for the Socialists, also lost, for the fifth and last time. Only Norman Thomas, his successor, with six defeats, ever equalled or beat his record. Levi P. Morton (vice president under Benjamin Harrison) died at the age of 96, and I mention it because he was born in 1824 and so overlapped by two years the lives of Thomas Jefferson and John Adams. We are a young country! (My grandmother, dying at 98 in 1984, had lived through more than half the life of our Constitution.)

Outside the US, the League of Nations was established (fatally, without the US); Austria held her first elections; and the Communist party completed taking control over the newly-named USSR. Two Georges ruled England (David Lloyd as prime minister and "V" as king); Poland retook Wilno/Vilna from Lithuania (with long-term implications for the universities and demographics of the region); and Benedict XI was pope, in succession to Pius X, the last occupant of the chair of St. Peter so far elevated to sainthood (we hope in spite of, not because of, his abolition of solos in liturgical music).

World population was roughly 2 Gigapersons, with 108 million of them resident in the US. Within the US, only 4.7% of the population was over 65, and the male:female ratio was 1.04 (and greater than unity even for the over-65's, the last census for which this was true). The foreign-born fraction was about 13%, higher than at any time since. Women made up 22% of the labor force, and unemployment was 5.2%, quite close to the current level.

Our national debt, left from the first world war (and the first one never significantly repaid) stood at $24.3 million, or $228.32 per person. This was something like 10 weeks' salary for a semi-skilled craftsman and so also not so very different from the current level.

Among the 300 000 people who graduated from high school, women outnumbered men by 50%, but men outnumbered women nearly 2:1 among the 48,000 college graduates. Another legacy of the "great war," Spanish influenza, wound down after killing roughly 20 million people in three years, compared to about 8.5 million in World War I itself (and insert your own best estimate for AIDS fatalities to date).

2.2 Sports and Culture

Somehow these items seem to present the most striking contrast of ancient and modern. The Cleveland Indians (their name not yet threatened by the forces of political correctness) defeated the Brooklyn Dodgers (long gone) in the 1920 World Series. Harvard edged out Oregon 7–6 in the Rose Bowl, in striking contrast to the "Fight Fiercely, Hahvard" image we inherit from Tom Lehrer. Jack Dempsey was seventh world heavyweight champion, while Emanuel Lasker of Germany, the first man ever declared world chess champion, still held the title. At the 7th Olympiad, Pavlo Nurmi won his first gold medals (one of a large number of Finnish track and field winners). The American men raking in gold for swimming events carried Hawaiian surnames, a reminder of the time when swimming was a survival skill rather than a recreation.

The winner of the Kentucky Derby (Paul Jones—horse, not rider) had a winning time only marginally longer than current records, though the purse at $30,375 sounds small until you inflate it. But the winner of the Indianapolis 500 (Gaston Chevrolet, driving a Monroe) had an average speed of 88.62 mph, slower than many of us have driven our production models. Bill Tilden (from the US) won Wimbledon, and the Ottawa Senators carried away the Stanley Cup.

The Academy Awards had yet to be invented, but Eugene O'Neill won a 1920 Pulitzer for *Beyond the Horizon*. It was not a great year for the Nobel Prizes, several of the winners inviting a "hoo hee" response from non-experts. Physics went to Charles Guillaume, Peace to Leon Bourgeois, Literature to Knut Hamsun, Physiology or Medicine to August Krogh, and Chemistry to Walter Nernst (who illustrates the advantages of having a theorem named after you).

Enrico Caruso sang his last performance (La Juive)—which feels infinitely long ago, and Agatha Christie published her first murder mystery (*The Mysterious Affair at Styles*—which was obviously only yesterday, since she brought out new volumes long enough to see many of us through high school and beyond. Sinclair Lewis published *Main Street*, which remains a classic (something everybody wants to have read, but nobody wants to read).

The first regular transcontinental airmail opened between Boston and San Francisco. Deaths during the year included artist Modigliani and explorer Admiral Robert E. Peary—both controversial figures down to the present. An incomplete list of those born in 1920 includes Ravi Shankar, Issac Stern, Nat "King" Cole, Alex Hailey, Isaac Asimov and Ray Bradbury, Mickey Rooney, Frederico Fellini, Yul Brynner, Eileen Farrell, Lana Turner, Tony Randall, David Brinkley, Dave Brubeck, Jack Webb, Stuart Udall, Walter Matthau, and Patti Andrews. Christmas came on a Saturday, and the April 26 debate on a Monday.

Finally, what would prove to be the trial of the year or even the decade began with the arrests of Nicola Sacco and Bartolomeo Vanzetti, the good shoemaker and the poor fish peddlar, for a murder most now think they never committed (though they died for it in 1927), but really for the crime of not being upper-middle-class WASPs. The drawing of modern analogies is left to the reader.

2.3 Astronomy in 1920

The Astrophysical Journal was already a quarter of a century old and under the joint editorship of Hale, Frost, and Gale. They had just added abstracts to the standard paper format and admitted that page charges were here to stay, (owing to the numbers of overseas subscribers not having recovered after the War), at least for authors or institutions who incurred more than $200 of production expenses in any one volume (of which there were two per year, with fewer than 30 papers each).

Very few of our current "best-buy" theories were yet in place (Russell, Dugan, and Stewart 1926; Eddington 1926). The Chamberlin–Moulton (dynamic-encounter) hypothesis for the origin of the solar system was in favor, largely because the Sun seemed to have too little angular momentum to have come from a "nebular hypothesis." The solar wind eventually resolved that issue. Elements common in the earth (silicon, iron, oxygen) were supposed also to dominate the stars, giving them (with ionization) a mean molecular weight close to 2.1. It took Cecilia Payne's 1925 Harvard thesis on K giants and H. N. Russell's later work on the Sun to sort this one out.

Not surprisingly, the source of stellar energy was unknown. The 2 Gyr age of some earth rocks (found by Rutherford and his colleagues) and the stability of Cepheid pulsation periods had already demonstrated that neither gravitational potential energy nor radioactivity was sufficient. New ideas in the air were "subatomic energy" that might power the Sun for 10^{10} years without much changing its mass (advocated by Eddington) and some form of total annihilation of electrons and protons (the only known particles) that would suffice for 10^{12} years (favored by James Jeans because he thought that much time was needed to allow star clusters to relax). The only picture of stellar evolution sufficiently developed for comparison with observations was Russell's giant and dwarf theory, whose imprint lingers today in the use of "early" and "late" for spectral types. The idea was that stars begin bright and red, contracting toward the main sequence until they have used up all their "giant stuff," whatever it was, and then move diagonally down the main sequence, living on their "dwarf stuff" for a much longer time, fading out as red or white dwarfs. The debaters were both more or less subscribers to this point of view, and Shapley invokes it as part of the theoretical argument for his distance scale.

Events of 1920 within the astronomical community included the deaths of Lockyer (discoverer of helium and founder of *Nature*), Brashear (of the process), and Hermann Struve. The Royal Astronomical Society marked its centenary, with Frank Dyson (whose successor is our moderator) as Astronomer Royal. Warner and Swazey Observatory was dedicated, and installments of the *Henry Draper Catalogue* (spectral types) and the Wolf Catalog of proper motions were published. The International Astronomical Union, the first of the international scientific unions established under the Treaty of Versailles, which specifically abolished all international organizations of the pre-war period, came into being. The losers in the recently ended conflict were specifically barred from membership, and Germany did not adhere to the Union until another war had come and gone.

Publications during the year relevant to "the scale of the Universe" included Shapley on globular clusters, Haber claiming that Cepheids were eclipsing binaries (a well-known crank in his day, now nearly forgotten), Kapteyn and van Rhijn arguing for a small, nearly Sun-centered galaxy on the basis of star counts, and H. N. Russell demonstrating that the large positive velocities of the spiral nebulae could not be caused by radiation pressure from the Milky Way. Shapley apparently thought this a possible mechanism while he was preparing his manuscript. That anyone could have entertained the idea for more than five minutes suggests a painful shortage of envelope backs. The Thompson cross section and the momentum carried by light were already old ideas.

Some of the less relevant papers were remarkably prescient. Albert Michelson was advocating use of the 60-inch and 100-inch telescopes (the latter only 3 years old) at Mt. Wilson for interferometry. Eleanor Seiler suggested the use of photoelectric cells as photon detectors for astronomy. And Walter S. Adams and Cora Burwell pointed out that novae must really be ejecting material. Other 1920 authors who are part of our folklore include Joel Stebbins (who, with a 64-year history of publications in ApJ, 1901–64, may be the longest-productive astronomer ever; Abt 1995), E. O. Hulbert, Francis G. Pease, Karl T. Compton, F. H. Seares, Edwin Hubble, Robert Millikan, Leigh Page, R. S. Dugan, Gustave Strömberg, and Seth Nicholson. Among those who lived long enough that I (and undoubtedly many of you) had a chance to meet them were Alfred H. Joy, Ira S. Bowen, Harold D. Babcock, Bancroft W. Sitterly, and Paul Merrill. The proportion of women authors was not so very different from the current mix. In addition to Burall and Seiler just mentioned, I spotted Mary Fowler (on eclipsing binaries), Mary Ritchie and Helen David (both Shapley co-authors in the Harvard tradition of measuring project lengths in woman-years).

Shapley and Curtis were not the only well-known scientists to speak at the 1920 Academy meeting, though the usual difficulties of travel were compounded by, as secretary Abbot described it, "Washington [being] still somewhat congested" in the aftermath of the war. What would he think of the place now, when a change in power structure means that Republicans arrive but Democrats don't leave (or conversely)? There were no parallel sessions, but a good many speakers were allotted only 5, 10, or 15 minutes.

In any case, Frank Boas spoke on "growth and development as determined by environmental issues." He meant of people, and the issue is still (or again) a burning one. Robert Yerkes presented the results of a psychological study of Army doctors. Robert H. Goddard proposed "possibilities of the rocket in weather forecasting." Hale described recent results from the 100-inch telescope (as old then as Keck is now), Edward Kasner discussed "geodesics and relativity," Millikan "reflection of molecules from surfaces," Michael Pupin "wave balance," whatever that is, and Arthur Noyes (brother of the poet Alfred) the direct combustion of nitrogen and chlorine. Some of these would be perfectly possible titles or subjects for this year's academy meeting. Some definitely would not.

Topics whose presenters are less familiar to our selective memories were a similar mix of ancient and modern—"conservation of nature resources," "rate of growth of the population," "Indian tribes of the Klamath River region," "common foods as sources of vitamines" (but note the spelling; they were all still thought to be true amines), "specific heat of powder gases," "alternating current for submarine transmission," "improvements in telegraphy," and two presentations on the properties of Springfield rifles! Yes, American militia units really carried the black powder, smokey "trapdoor" right up to, and occasionally beyond, the moment we went "over there" (Sweeney 1995; Pinckney 1995).

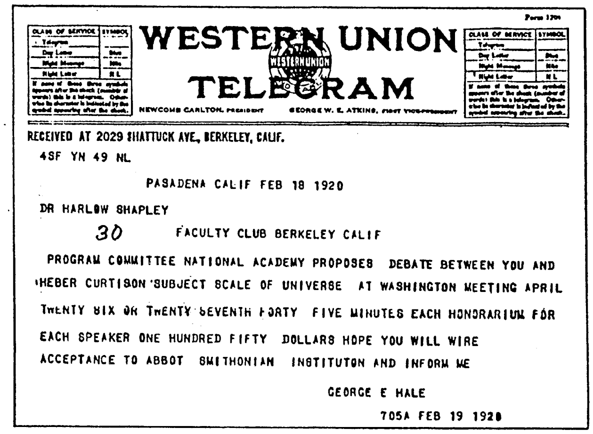
3. HISTORY OF THE DEBATE AND DRAMATIS PERSONAE

The background and circumstances of the 1920 lectures have been described by Struve and Zebergs (1962), Whitney (1971), Jaki (1972), and Berendzen et al. (1976), among others, on the assumption that the printed versions of the talks (Curtis 1921; Shapley 1921) were a close approximation to the material presented orally. Hoskin (1976) has shown that this is not the case, and his discussion therefore takes precedence.[2]

William Ellery Hale I, having presciently moved his family out of the center of Chicago shortly before the 1871 fire, made his fortune by constructing elevators for the buildings that grew up afterwards, as well as for the Eiffel tower and the other structures (Wright 1966; Osterbrock 1993). Some of the profits of these ventures bought his elder son, George Ellery, his first microscopes and telescopes, and, eventually, much of the Mt. Wilson 60 inch. In addition, he endowed a fund for the National Academy of Sciences to be used, among other purposes, for invited lectures at annual meetings. Shapley and Curtis each received a $150 honorarium (out of which, however, they had to pay their travel expenses to Washington; Gingerich 1995).

Not surprisingly, G. E. Hale (elected to the Academy in 1902) had some considerable say in how these funds were expended. In late 1919, he spoke to Charles G. Abbot, Home Secretary of the NAS, proposing that there be a William Ellery Hale Lecture at the 1920 April meeting in the form of a debate or discussion on either general relativity or the distance scale of the Universe. Abbot's reaction was that it might be difficult to stir up interest in so specialized a topic as the existence of island universes, and that everyone would

[2]The mistake of placing the debate in 1921 is curiously common. Bok (1974) does it in his obituary of Shapley, as do several of the secondary accounts of the debate. And Florence (1994) manages to make several chapters out of the events of "April 1921." The cause is, presumably, the date of the publications and the fond belief that refereeing didn't take so long in those days!


WESTERN UNION TELEGRAM

NEWCOMB CARLTON, PRESIDENT GEORGE W. E. ATKINS, FIRST VICE-PRESIDENT

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DR HARLOW SHAPLEY

30 FACULTY CLUB BERKELEY CALIF

PROGRAM COMMITTEE NATIONAL ACADEMY PROPOSES DEBATE BETWEEN YOU AND HEBER CURTISON SUBJECT SCALE OF UNIVERSE AT WASHINGTON MEETING APRIL TWENTY SIX OR TWENTY SEVENTH FORTY FIVE MINUTES EACH HONORARIUM FOR EACH SPEAKER ONE HUNDRED FIFTY DOLLARS HOPE YOU WILL WIRE ACCEPTANCE TO ABBOT SMITHONIAN INSTITUTON AND INFORM ME

GEORGE E HALE

705A FEB 19 1920

FIG. 1—Telegram inviting Shapley to participate in the April NAS debate. He was apparently away from Pasadena at the time. Sharp-eyed fans of the old Western Union will note that the message was sent as a night letter, dispatched on February 18th and delivered on the 19th. The original is in the Harvard College archives, and the copy was provided by Owen Gingerich and Vera Rubin.

be heartily sick of relativity by then. He counterproposed causes of the ice ages or some topic in zoology or biology. Hale had originally suggested that the discussants on island universes and the distance scale should be William Wallace Campbell (1862–1938, then director of Lick Observatory), presenting the conventional view, and Harlow Shapley (Hale's junior associate at Mt. Wilson), putting forward his new, larger distance scale, based on variable stars in globular clusters and other considerations.

When the dust settled, they had agreed on two talks, by Harlow Shapley and Heber D. Curtis (of Lick) on "the distance scale of the Universe," and Hale sent out telegrams of invitation on 18 February. Shapley's invitation still exists (Fig. 1).

After some discussion, the lecturers agreed to exchange their ideas in advance and each to give a single talk, with Shapley going first, and to include responses to each other's viewpoints therein, rather than to adopt a debate format, with rebuttals. The participants in the 1995 commemorative event similarly considered several possible formats, but made a different choice, opting for a formal debate structure.

Table I presents some aspects of the lives and works of the four people most closely associated with the 1920 debate: Hale who suggested it, Shapley and Curtis who carried it out, and Edwin Hubble who, a few years later, collected the data that settled the issue of island universes. All were born in the midwest, within 21 years of each other, and all had doctorates of some sort, though Hale's were all honorary. I mention their activities during World War I because at least part of the source of the life-long coolness between Hubble and Shapley was that Shapley, remaining at Mt. Wilson, carried out some project that Hubble had intended to pursue as soon as he could take up his proffered position there after returning from active duty overseas (Hoffleit 1995). Hubble had volunteered immediately after defending his thesis and apologizing to Hale for not being able to accept the Mt. Wilson

TABLE 1
Some Highlights of the Lives of Hale, Curtis, Shapley, and Hubble

	HALE	CURTIS	SHAPLEY	HUBBLE
	George Ellery	Heber Doust	Harlow	Edwin Powell
born	1868 Chicago	1872 Muskegon Mich.	1885 rural Missouri	1889 rural Missouri
1st degree	1890, BS MIT	1893, classics U. Michigan	1911, astronomy U. Missouri	1910, BS Chicago + Oxford
PhD	12 honorary	1902 U. Virginia	1913, Princeton	1917, Chicago
WWI	National Research Council	Taught navigation, Berkeley & San Diego; NBS optical section	...	Active duty, France 1917–19, infantry battalion CO
Career	Harvard & private observatory 1886–96 Yerkes/Chicago 1887–1904 Mt. Wilson 1904–23 honorary director & private observatory 1923–38	Taught Latin & Greek, later math, 1893–1900 Lick 1902–20 Director, Allegheny 1920–30 Director, U. Michigan Observatories 1931–42	Mt. Wilson 1914–20 Director HCO 1921–52 Emeritus to 1972	Mt. Wilson 1919–53
WWII	d.	d.	refugee resettlement	Aberdeen ballistic missile laboratory
died	1938, Pasadena	1942, Ann Arbor, MI	1972, Boulder, CO	1953 Pasadena
obituaries	NAS (W.S. Adams) and at least 8 others	PASP 54 (McMath)	QJRAS (1974, B. Bok) Nature (1972, Z. Kopal)	PASP 66 (H.P. Robertson)

position at once. He was wounded in France and rose to the rank of major. Correspondingly, during the second world war, while Shapley remained at Harvard (helping to resettle refugees), Hubble moved to Aberdeen Proving Ground to direct its ballistics laboratory.

Of our four protagonists, Hale was far the most wide-ranging in his activities (Wright 1966; Osterbrock 1993). Astronomers know him as the founder and initial fund raiser for Yerkes, Mt. Wilson, and Palomar Observatories. A strong believer in international cooperation, he was among the prime movers in establishing the International Union for Co-operation in Solar Research in the years before World War I. Not easily discouraged in those days, he reacted to its abolishment by the Treaty of Versailles (which dissolved all pre-war scientific and cultural international organizations) by starting over with a still larger vision and persuading into existence the entity now called the International Council of Scientific Unions, as well as the International Astronomical Union under it.

During the war years, Hale was the first pure scientist to try seriously to persuade President Woodrow Wilson (awkwardly stuck with the slogan "He kept us out of war") that the services of his colleagues would be needed to win the war and the peace that followed. The organization he founded with that goal in mind is now the National Research Council. The Yerkes Primate Lab at Chicago is another of his inspirations. Curiously, the Robert Yerkes for whom it is named was not a close relative of the industrial magnate whose name the observatory bears. Hale early encouraged psychologist Yerkes to turn his attentions from people to other primates. Under the circumstances, one can only be astounded that Hale also made fundamental contributions to our understanding of the solar spectrum, magnetic field, and activity cycle, though he failed in a life-long ambition to photograph the solar corona outside of eclipse.

Curtis, too, was interested in the Sun and participated in 11 eclipse expeditions between 1900 and 1932 (McMath 1942). His years at Lick were, however, devoted primarily to photographing spiral nebulae with the Crossley telescope, the work that resulted in his being asked to face off with Shapley in 1942. Curtis moved later the same year to the directorship of Allegheny Observatory (having already served as president of the Astronomical Society of the Pacific in 1912) and later to the corresponding position at the University of Michigan. He was an important force in the transformation of McMath-Hulbert Observatory from a private endeavor to a serious research facility. His own research days essentially ended when he left Lick, though his name continued to grace the astronomical journals with papers on subjects as unlikely as "A Voyage to the Moon." Curtis guided to their PhDs Helen Dodson Prince (1934), Ralph B. Baldwin (1937), and K. O. Wright (1940) among others. The University of Michigan had, incidentally, been admitting women students to its astronomy graduate program since before 1920, when Julia May Hawkes received her PhD for work on the positions of stars and nebulous knots in the Great Nebula of Andromeda (Sears 1995). Curtis died in the observatory director's house in Ann Arbor, with his directing, if not his observing, boots on.

Ralph Baldwin (1995, whose thesis was on the spectrum of Nova Cygni 1920 and its relationship to that of Nova Herculis 1934) remembers Curtis as "a small, quiet man with a remarkable sneeze." Curtis was "not full of wild enthusiasm for Einstein's theory," to which he had a long list of objections, and he once ended a graduate course by throwing out the final exams of the five or so students on the grounds that if he hadn't given them enough work over the whole semester to get to know them, "the three hours here isn't going to tell me anything new." The grades were all A's. He described the 37 1/2-inch telescope at Michigan is "focusing like a dish pan," and had great expectations for the 98 1/2-inch mirror he had cast at Corning in 1936 (while Corning was in the process of learning to produce the 200-inch blank for Hale and Palomar). Unfortunately, the money to turn it into a telescope never materialized, and the 98 1/2-inch sat next to the observatory parking area for many years until it became the primary of the Isaac Newton Telescope, and so sat next to Herstmonceux Castle for an additional number of years (contributing at least slightly more to astronomy in the latter location).

Curtis, like Hubble, was a confirmed pipe smoker, who sporadically set his wastebasket on fire. It was a search for the correct pronunciation of his middle name that eventually led to my making contact with Baldwin. The correct answer is "to rhyme with soused." And if you think you have heard of Baldwin in some other context, you probably have. He was one of the very first and most vocal proponents of impact cratering as the explanation for *The Face of the Moon* (Baldwin 1949).

Shapley, too, spent more of his life as an observatory director than as a research astronomer, taking up the reins of Harvard College Observatory shortly after the 1920 debate as successor to Pickering (and handing over to Donald H. Menzel more than 30 years later). He brought Harvard firmly into the 20th century, though he retained always a preference for relatively small telescopes with wide fields of view (Kopal 1972). In the post-war years he served as president of the American Astronomical Society, the American Association for the Advancement of Science, and the honorary scientific fraternity Sigma Xi, and was a firm opponent both of the communist witch hunts in the US and of the nonsense propounded by Velikovski.

Cecilia Payne Gaposchkin (at Harvard from 1923 to her death in 1979) described Shapley's style of leadership as "divide and rule" (Haramundanis 1984, p. 224). His decision that she must switch from spectroscopy after her thesis work (which was the first clear demonstration that stars consist mostly of hydrogen) to variable stars, leaving the spectroscopy to Menzel, hurt her deeply without in the least making Menzel dislike Shapley less (Hoffleit 1995). On the other hand, it was Shapley who persuaded Hoffleit to go on for her PhD (on spectroscopic parallaxes), though it would take her away from the work she was doing for him, and he welcomed her back at Harvard from war work at Aberdeen, though it had been done under the supervision of Hubble. Shapley's commitment to international cooperation rivaled that of Hale, and he is generally credited as the man who put the S in UNESCO.

Hubble, in contrast, was primarily a research astronomer all his life. He never directed an observatory or held an AAS office, though he served two 3-year terms as President of the IAU Commission now called Galaxies. While we remember him here for the discovery of Cepheids in NGC 6822, M33, and M31, which settled the issue of the existence of external galaxies, he is at least as well known for helping to draw the distinction between emission and reflection nebulae, discovering the linear redshift–distance relation that bears his name, classifying galaxies into their "Hubble types," and demonstrating that virtually all spiral galaxies rotate in the same direction, with their arms trailing. That Hubble was not personally known to more of us is a consequence of his having been the shortest-lived of our protagonists. I have not attempted to assemble any personal impressions of him, but suggest that readers should take the one presented by Florence (1994) with some reservations, based on his treatment of Hale (Osterbrock 1993, 1995).

Of course, a very large number of other astronomers contributed relevant data and ideas before, during, and after the epoch of the "great debate." Vesto Melvin Slipher (1875–1969) measured the first wavelength shifts of spiral nebulae. Johannes C. Kapteyn (1851–1922) was a life-long proponent of a small Milky Way, centered nearly on the Sun, and his "Kapteyn universe" continued to bedevil attempts to picture the large-scale distribution of stars for decades after his death (both Trumpler and Shapley trying to picture Kapteyn's star cloud as part of the disk of some larger structure traced out by the globular clusters).

Adriaan van Maanen (1884–1946) was responsible for most of the measurements of apparent rotation of spiral galaxies that prevented Shapley from considering the possibility of their being at large distances until very late. Van Maanen's plates and equipment were not at fault. Although the instrument at Mt. Wilson bore the legend "Do not use this stereo-comparator without consulting A. van Maanen," Knut Lundmark (1889–1958), visiting from Sweden, actually used it a few years after the debate to remeasure van Maanen's plates. He found no rotation, and, while the non-existence of the rotation is no longer in question, nobody has ever been quite sure what van Maanen did wrong. Lundmark was also the first to write, in 1920, that some novae might be so bright as to be detectable even at millions of light years from us. He advocated a quadratic relationship between redshift and distance (as expected in a de Sitter universe) before Hubble promulgated his law. Though van Maanen's sign remained on the blink comparator through my own graduate days (1964–68) and down to the time Berendzen photographed it (1972), I and others did eventually use it without consulting him. A minor point of possible confusion: "Mt. Wilson" was long used to mean, indifferently, the Mountain site and the administrative offices on Santa Barbara Street in Pasadena (the blink comparator was in the basement at Santa Barbara Street). Both places still exist, though the latter has undergone name changes to "Mt. Wilson and Palomar Observatories," "Hale Observatories," "Mt. Wilson and Las Campanas Observatories," and "Las Campanas Observatory," and "Observatories of the Carnegie Institution of Washington." And I may have forgotten one or two.

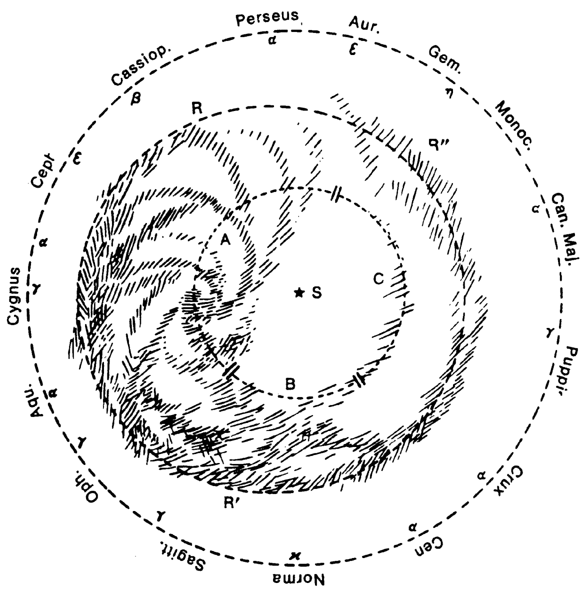

FIG. 2—Cornelius Easton's model of the Galaxy in 1900. He was the first to give the Milky Way spiral arms.

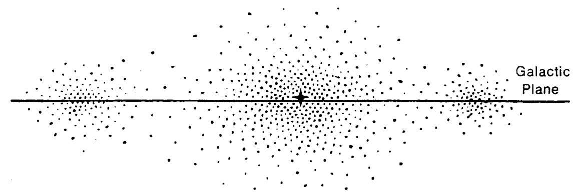

FIG. 3—Arthur Eddington's (1912) galaxy placed the Sun's position 60 LY above the center of the galactic plane.

4. IMAGES OF THE MILKY WAY AND OUR DISTANCE TO THE GALACTIC CENTER

For more than a century after Herschel (1785), astronomers lived essentially at the center of a galaxy not much more than 6000 LY across (illustration 18 in Jaki 1972). Herschel arrived at his result by counting stars as a function of apparent magnitude in various directions ("star gauging") and, according to Kopal (1971) increased the diameter to 20,000 LY in 1806. The issue of whether the spiral nebulae might constitute other island universes was discussed sporadically through the 19th century, but was not the focus of anyone's research. Simon Newcomb (1882; illustration 19 in Jaki 1972), for instance, put "the region of the nebulae" immediately above and below a Herschel-like disk. It is widely believed that Newcomb was Walt Whitman's "Learned Astronomer," but this should probably not be held against either of them.

Cornelius Easton's (1900; Fig. 2) galaxy was also small and Sun-centered, but he was the first to give the Milky Way spiral arms. An honest examination of the sky forced him to displace the center of the spiral pattern away from us by more than half the galactic radius in the direction of Cygnus, and his drawing gives the impression of a man struggling with the truth and losing. Parsecs gradually replaced Light Years as the unit of choice between 1900 and 1920. Karl Schwarzschild's (1910) galaxy was 10 kpc across, 2 kpc thick, and Sun centered, while Arthur S. Eddington (1912, Fig. 3) put us 60 LY above the center of the galactic plane. Hugo von Seeliger, the most thorough counter of stars since Herschel, and many others, concurred (Seeliger 1911).

Shapley (1918, 1919 and earlier references therein) shows a certain youthful exuberance in his distances—67 kpc for NGC 7006 and 13.9 kpc even for M3. The centroid of his distribution slid from 13 to 25 kpc, with the 1919 paper settling on 20 kpc and a total diameter at least three times that. Shapley's universe had precious little room for anything outside this enormous galaxy, and he attempted at one point (Shapley 1930) to describe the Milky Way as more like the Coma–Virgo cloud of galaxies than like a single spiral or disk system. This is also the purport of his remark, quoted in Russell, et al. (1926) that, if the spiral galaxies are islands, the Galaxy is a continent. Anton Pannekoek (1919) agreed with Shapley in placing the Sun far off-center but in a smaller galaxy (R_0=40–60,000 LY, d=80–120,000 LY).

At the time of the debate, Curtis's Milky Way was only 10 kpc across, with the Sun at R_0=3 kpc. Meanwhile, Kapteyn and van Rhijn (1920; Kapteyn 1922) were counting stars more precisely than they had ever been counted before, but with no allowance for absorption by dust. Their first result was R_0=0 and d=24 kpc; the second R_0=3 kpc, d=17 kpc (Fig. 4). But Shapley's numbers dominated people's thinking very quickly. Sir Harold Spencer Jones (1923, General Astronomy), Sir James Jeans (1927, Astronomy and Cosmology), as well as Russell, Dugan, and Stewart (1926, vol. 2) place the galactic center 20 kpc away. Jeans describes the Milky Way and other spirals as having the relationship of a cake to a bunch of bisquits. All attempt to fit Kapteyn's "universe" in somewhere as a local stellar subsystem.

Trumpler (1930, Fig. 5) made a valiant attempt to declare all particles correct. His drawing shows a coordinate system centered at the Sun in the middle of a slightly titled 10 kpc

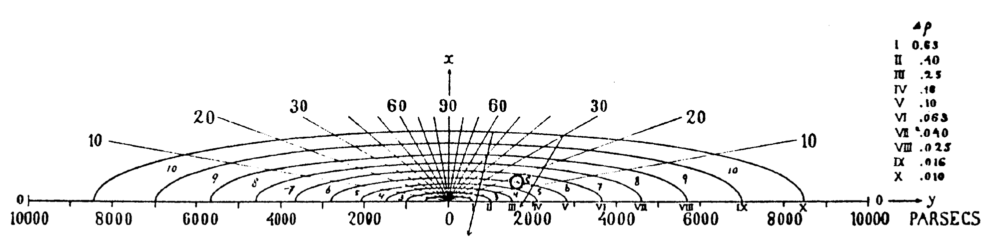

FIG. 4—Kapteyn and van Rhijn (1920) and Kapteyn (1922) deduced galactic dimensions based on star counts with no allowance for absorption by dust.

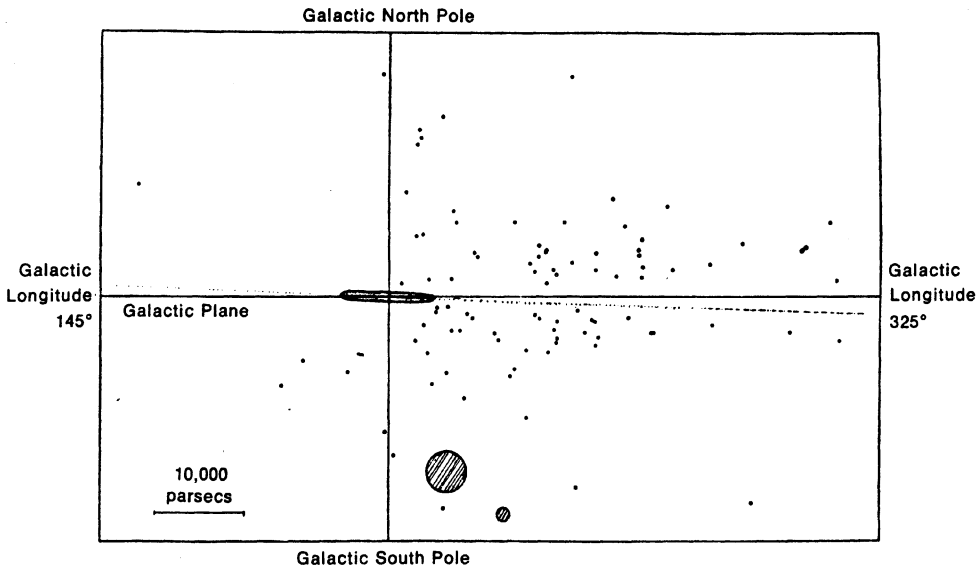

FIG. 5—Trumpler's (1930) drawing of the galactic environs shows a coordinate system centered at the Sun in the middle of a slightly tilted 10 kpc Kapteyn universe, but with globular clusters scattered over an 80 kpc spheroid centered about 18 kpc away.

Kapteyn universe, but globular clusters scattered over an 80 kpc spheroid, centered about 18 kpc away from us.

Jan Oort's discovery of galactic rotation quickly led to a new calibration of distance scales. His first version (shown in Oort 1927) reported R_0=6300±2000 kpc, soon revised upward to 10 kpc (shown in Oort 1932). This value was widely used over the next 20 years and incorporated in many images (see, e.g., Bok 1937).

Walter Baade (1953), however, looked again at the globular clusters and their RR Lyrae variables and settled on R_0=8.16 kpc. This value, rounded off to 8.2 kpc, was generally accepted as the standard for reducing galactic rotation curves over the next decade (as shown by Westerhout 1956 and Kerr 1962). Nancy Grace Roman (private communication 1992), who attended the symposium where Baade shrank the galaxy, describes herself as having gone to college at 10 kpc and to graduate school at 8.2 kpc.

The present author did precisely the opposite; for in 1963, Oort (1964, cf. Schmidt 1965) moved us back out to 10 kpc. And there the official IAU set of galactic rotation constants kept us until the 1985 General Assembly in Bangalore, where the Commission on Galactic Structure (cf. Kerr and Lynden-Bell 1986) voted to reduce R_0 to 8.5 kpc. This number is the average of a long table that includes numbers between 6 and 11 kpc. Subsequent trends have perhaps been toward the small end of the range. Thus our present distance from the galactic center is quite close to the geometric mean of the numbers advocated by Shapley and Curtis in 1920.

5. SCIENTIFIC ISSUES IN 1920 AND THEIR RESOLUTION

Shapley and Curtis disagreed to some extent on at least 14 astronomical issues. These are presented in the following paragraphs in roughly the order in which they occur in the printed texts (Shapley 1921; Curtis 1921), which is neither in order of importance nor in any other pattern a modern reviewer would be likely to choose. According to the actual texts reproduced by Hoskin (1976), no other additional scientific points were made during the main talks, though some may have arisen during Russell's rebuttal or other parts of the discussion, no record of which has been preserved. Each paragraph indicates an issue, what each disputant thought (or anyhow wrote or said), what we think now and sometimes why, and who should be counted the winner on each issue.

(1) *Resolved* F, G, *and* K *stars in globular clusters*. Shapley believed they were giants like local F–K giants, with absolute magnitudes near −3, placing average globular clusters 10–30 kpc from us. Curtis said they were like the commonest sorts of stars around us, F–K dwarfs, with average visual magnitudes of about +7, putting the clusters at a kpc or two. As became unambiguously clear when the first 200-inch color–magnitude diagrams of globulars reached the main-sequence turnoff (e.g., Sandage 1953), Shapley was essentially right on this one.

(2) B *stars in globular clusters*. Shapley said they should have absolute magnitudes near 0, like nearby main-sequence late-B and early-A stars. Curtis responded that something very strange must be going on, since the brightest blue stars in the solar neighborhood are brighter than the brightest red stars, while the opposite is true in the clusters. It took the insight of Walter Baade and his data gathered during the blackouts of World War II to sort this one out, with the concept of two stellar populations. Each of the speakers was right about the particular point he emphasized.

(3) *Cepheids as distance indicators*. Shapley used the relative period–luminosity relation found in the Large Magellanic Cloud with its zero point calibrated on a handful of Milky Way disk examples using statistical parallax. He noted that the nearby Cepheids of the cluster type (that is, RR Lyrae stars) are high-velocity objects and must not be used

for the calibration. Curtis responded that there was no evidence for a period–luminosity relation in the Milky Way, and that a larger sample, including some stars with geometric parallax measurements, even ruled it out. This was the point on which he said most firmly "more data are needed." When they came, Milky Way Cepheids did display a P–L relation, based both on secular parallaxes (or statistical) and on open-cluster members. But the zero point was offset from the globular cluster one by more than a magnitude. This also was the work of Baade, who knew something was wrong the day (or rather night) he turned the 200-inch toward Andromeda and saw no RR Lyrae stars. Curtis was right about "more data" but wrong about what they would show—he had placed too much faith in tiny geometric parallaxes, though he had more sense (paragraph 14) than to be misled by tiny proper motions. Shapley was right that Cepheids are generally good distance indicators.

(4) *Spectroscopic parallaxes in general.* Shapley believed these could be trusted as long as you could see any of the line ratios indicative of giant surface gravities in nearby stars. Curtis believed they should be trusted only in the region of less than 100 pc where they had been calibrated. Errors and omissions excepted (like some high-latitude B stars), Shapley was right on this, though one shudders to think of the faith of eye required to see luminosity indicators like the ratio of 4215 (Sr II) to 4454 (Ca I) in spectra of individual globular-cluster giants taken before 1920.

(5) *Interpretation of star counts.* Curtis said, correctly, that star counts, straightforwardly interpreted, require a small Milky Way. His idea that spiral nebula dust existed as a ring *around* the stellar disk prevented him from suggesting absorption as relevant to the problem. Shapley did not address the issue, presumably because his use of globular clusters had already committed him to the "negligible absorption" camp, and he could, therefore, say nothing to rebut the point. Robert Trumpler (1930), by correlating apparent diameters of open star clusters with their apparent brightnesses revealed the importance of dust *inside* the disk (though Jesse Greenstein and others had come very close to discovering it earlier).

(6) *Stellar evolution theory.* Shapley claimed that if, and only if, the globular clusters were put at large distances would their stars fit the Russell giant and dwarf theory and Eddington's models of gaseous giants. Curtis opined that spiral nebulae as a phase of stellar evolution didn't fit anywhere in any reasonable theory. (Remember protostellar nebulae were Out for solar system formation and encounters were In that year, and Jeans' idea that they were places where new stuff was pouring into the galaxy from Elsewhere had yet to be espoused and modified by Victor Ambartsumyan and others.) While both points were true enough, we have to count Curtis the winner on this one, since we no longer adhere to the giant and dwarf theory!

(7) *Distribution of spiral nebulae on the sky.* Shapley doesn't really mention this, but for a "single-system" man, it was no more unreasonable for spirals to avoid the galactic plane than for OB stars to favor it. Curtis was forced to deal with the problem and concluded that it was "neither impossible nor implausible" for the Milky Way to have an occulting ring around it, as many edge-on spirals seem to, so that we would not be able to see nebulae in the plane. Curtis was closer to the truth than Shapley, but missed the critical point that stars and absorbing material are mixed together.

(8) *Nova brightness at maximum light.* Both speakers agreed that "new stars" had been seen in the Milky Way and in several spiral nebulae. Shapley felt strongly that the implied real brightnesses would be totally ridiculous if the spirals were separate galaxies. Curtis said that, for four events with estimated distances in the Milky Way and a handful of novae in spirals, peak luminosity would be the same, provided the Milky Way had his preferred small size and the spirals were separate systems of similar physical diameter. He agreed that S Andromedae in 1885 was much brighter than this general run of events, said that Tycho's nova probably had been too, and concluded "a division into two classes is not impossible." One of the participants in our modern debate has previously suggested there might be two classes of γ ray bursters. Notice that Curtis was willing to trust a calibration based on four examples when he liked the answer, but not for the Cepheids, where he didn't. Two classes was, of course, the solution. Lundmark (1920) hinted at it, and Baade and Zwicky (1934) said it firmly from December 1933 onward, dubbing the brighter class super-novae (the hyphen disappeared the year Hale died; not causal). Curtis gets the points for this topic.

(9) *Nova mechanisms.* Shapley suggested, seemingly with a straight face, that both the star and the nebulosity had existed to begin with, and that nebulae (with their large velocities) overtook and enveloped stars, producing nova events. He claimed to get the right rate of a few per year in the Milky Way from the numbers of stars and nebulae in his model universe. Curtis countered that the proposed mechanism would yield a rate of 1 per 500 years in Andromeda, where several had already been caught in the last 20 years. Once again, Curtis 1, Shapley 0.

(10) *The large, positive average velocities of the spiral nebulae.* Shapley suggested the cause might be repulsion by radiation pressure from the Milky Way (a mechanism Russell showed to fail by many orders of magnitude the same year). Curtis simply proposed that large (mostly) positive wavelength shifts might somehow be intrinsic to the nebulae, and a large velocity also characteristic of the Milky Way. There are cases where "I haven't a clue" is the correct answer. It took the combined force of observations by Hubble, Milton Humason, and others and theoretical advances by Einstein, Alexander Friedmann, and others to come up with expansion of the Universe as the explanation. Curtis over Shapley again, though perhaps not full marks. Incidentally, in case I forget to mention it elsewhere, Einstein did not attend the 1920 debate, pace Florence (1994) and could not have, being still in Europe.

(11) *Properties of Galaxies. I.* Shapley pointed out that the observed central surface brightnesses of spiral nebulae are much larger than anything seen in the Milky Way and the radial distributions of colors and surface brightnesses are different. Curtis remained silent on the issue. The answer, of course, is absorption and reddening, so Shapley was right

about the data, but wrong about the interpretation. Love–love.

(12) *Properties of Galaxies. II.* According to Curtis, spiral nebulae have colors and line spectra a lot like those of star clusters, implying that the nebulae are mostly large assemblages of stars. Shapley did not mention this, and Curtis was right.

(13) *Central location of the Sun.* Shapley claimed this was an illusion, caused by the local star cloud now called Gould's belt. Curtis said it was God's own truth, and that our location kept us from readily seeing our own spiral arms. Once again, dust is an important part of the picture, but Shapley was nearly right.

(14) *Rotational proper motions of spirals as measured by van Maanen.* Shapley said these were "fatal to the comparable galaxy theory." Curtis fully agreed, but said that you should never trust a proper motion of less than 0.″1/yr for fuzzy things measured from a baseline of 25 years or less. A round of applause for Curtis and sympathy for Shapley, who said later than van Maanen was his friend, so of course he believed him.

6. AFTERMATH OF THE DEBATE

Immediate reaction to the two lectures was undoubtedly driven by the two men's styles of public speaking. Comments have come down to us indicating that Curtis was by far the more experienced lecturer and expounder to the public. He had, at any rate, taught at Detroit High School, Napa College (California), and College of the Pacific (first Latin and Greek, later mathematics and astronomy) for about five years before seeking his PhD (McMath 1942; Stebbins 1951). Russell's private reaction (Hoskin 1976) was that Shapley ought to be persuaded to offer a lecture course to hone his skills in this direction. From our modern vantage point, it is hard to see things this way. Shapley springs to mind as the man for whom, rightly, the AAS Shapley Lectures are named, while Curtis is the man with the rimless glasses (and without the hair) who was prone to describe astronomical hypotheses as "not impossible" and "neither implausible nor impossible," while intoning a refrain of "more data are needed." Shapley, however, appears literally to have read his paper (from a typewritten text with long- and short-hand corrections), while Curtis had his lecture notes on slides. He might even have used overhead plastics, like the 1995 debaters, if they had existed in his time.

Although the participants continued to speak and write among themselves about the "famous debate," "memorable set-to," and "memorable discussion" for several years after 1920 (Hoskin 1976), the event seems to have attracted very little attention in the popular or scientific press. Berendzen et al. (1976) were able to locate only one contemporary report (given by a historian of science, Peter Doig, at the December 1921 meeting of the British Astronomical Association). Several contemporary reviews of distance scales refer to the work of one or both debaters, but not to the debate, and conclusions drawn in the reviews are essentially those held by the writers before April 1920. A splashy headline on a May, 1921 issue of the Boston Sunday Advertizer refers only to Shapley's work on the distance scale and seems to have been featured primarily because he was, by then, a "Harvard astronomer." As noted in Sec. 4, Shapley's structure for the Milky Way was rapidly adopted by writers of astronomical textbooks, but without any reference to the 1920 event or 1921 publications.

At the time of Curtis's death, the discussion at the NAS was sufficiently forgotten that McMath's (1942) obituary makes no mention of it. Shapley, in his 1969 autobiography, similarly averred that it had for long escaped his memory. The first commemorative account I have seen is Stebbins' (1951) talk at the dedication of the Curtis Memorial Telescope in 1950. Otto Struve (1960), writing on the 40th anniversary, spoke of "a historic debate," and described (William) Albert Whitford (PhD, University Michigan 1942) and his students at Wisconsin as having restaged the event several times around 1950. Following Struve's account, others (based largely on the published texts) appear in many books and articles. Struve could not have been at the original debate. Stebbins (who was there to welcome Curtis to Lick in 1902) could have been, but apparently was not.

Each debater at some point expressed the opinion that he had won, perhaps not surprising, given the near-equality of their scores (Sec. 5). The question of correct distance scales within (and without) the Milky Way has been iterated on many times since 1920. According to recent rounds, Shapley's galaxy was too big and Curtis's too small, but, more seriously, centered far too close to the Sun. The sort of sketch map most of us would draw has not changed much since that of Plaskett (1939, Fig. 6).

The question of the existence of separate, external galaxies, island universes, or whatever you want to call them, was resolved much more cleanly. A contribution by Edwin Hubble to the December/January 1924–25 meeting of the AAAS (read by H. N. Russell) announced the presence of Cepheids in several nebulae at apparent brightnesses that put them firmly outside even Shapley's bloated Milky Way. The result had actually been published in the 23 November 1924 New York Times, without attracting much attention. But Stebbins, Russell, and others who were at the AAAS meeting felt that the issue had been fully resolved.

The debaters apparently agreed. Curtis (quoted in Berendzen et al. 1976, p. 138) wrote in April, 1925, "I have always held this view [that spirals are separate galaxies], and the recent results by Hubble on variables in spirals seems to make the theory doubly certain." This sounds like a calm, reasoned reaction, appropriate to a scientist who had distrusted earlier conclusions based on Cepheids (and I cannot say whether the disagreement in number between subject and verb was Curtis's or accidentally introduced by Berendzen et al.).

Shapley's predictably much more flamboyant reaction was recalled long after by Cecilia H. Payne-Gaposchkin, who had come as Harvard's first PhD student in astronomy in 1923 (Haramundanis 1984, p. 209). She was in Shapley's office when a letter arrived from Hubble, describing the period–luminosity relation for Cepheids in M31. "Here is the letter that destroyed my universe," said Shapley, holding it out. She also recalled him saying, "I believed in van

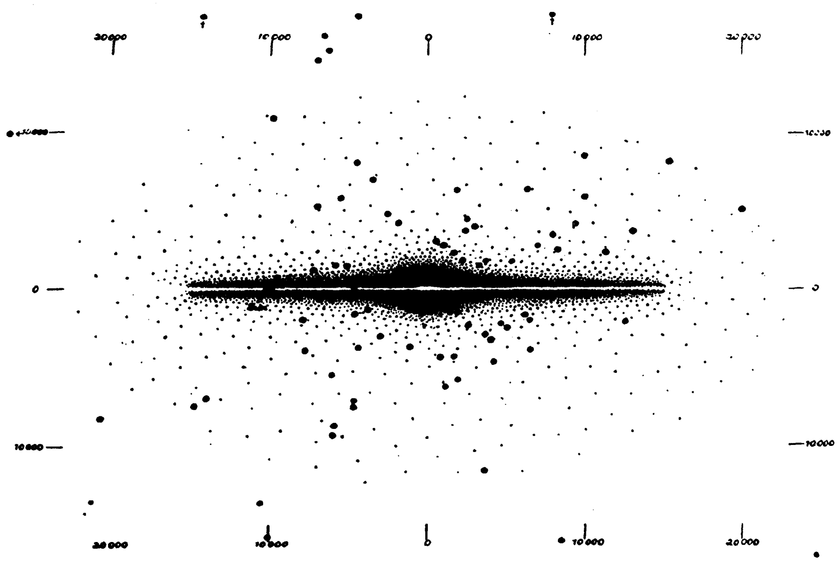

FIG. 6—Plaskett's 1939 sketch map of the galaxy.

Maanen's results ... after all, he was my friend." And she herself resolved (Haramundanis 1984, p. 227) that she would "not accept the conclusions of another astronomer simply because I am fond of him, or reject them because I dislike him (though I admit there is a temptation here)."

7. LESSONS FOR TODAY AND TOMORROW (AND POSSIBLY UP TO NEXT TUESDAY OR THEREABOUTS)

It is possible to discern, or perhaps imagine, several patterns in the 1920 debate and long-term repercussions. First, Curtis and Shapley each seem to have got things more or less right when they relied on data they had collected for themselves (Shapley's photometry of stars in globular clusters; Curtis's images of spiral nebulae), and to have gone astray when they attempted to make use of data assembled by others. This is not a happy omen for the 1995 debate.

Second, conclusions that they drew by attempting to rely on astrophysical theory did not have a very high batting average. You can argue about just what belongs in this paragraph, but Shapley invoking the giant–dwarf theory of stellar evolution, radiation pressure for the large redshifts of spiral nebulae, and an encounter hypothesis for nova events, and Curtis attempting a sort of generalized Copernican approach to stellar populations strike me as good examples. Most of us would, of course, say that correct theories do not get you into this kind of trouble, but rather add strength to observational conclusions by enabling you to understand them. This is what Eddington had in mind when he said he refused to believe an observation until it was confirmed by theory. But then, how much confidence should we have that any of the astrophysical theory brought to bear on the bursters so far is of this correct type?

Third, each of the 1920 protagonists had hold of part of the truth and so could claim partial victory. Other famous scientific disputes have ended this way, for instance that between the 18th century neptunists (who believed only in sedimentary rocks, laid down under Neptune's oceans) and the plutonists (who believed only in igneous rocks, rising up from Pluto's underworld). Both, of course, exist. I am inclined to suspect that the claims and counterclaims of star bursts versus monster central engines to model active galaxies will prove to be like this.

Not all scientific disputes can end in such mergers or compromises. There is no middle ground between a planetary system co-forming with the Sun and one dragged out of an already-established star by an intruder. Nor have various attempts to combine the virtues of standard hot big-bang cosmology with those of steady state succeeded.

What about the gamma-ray bursters? Could both galactic and very distant (and perhaps even "other") sources lurk among the classical events, with perhaps very different physical mechanisms at work in each? Since I have always felt that the popular image of the Curtis–Shapley debate gave the elder astronomer rather short shrift, I would like him to have the last word, taken from his comments on novae, "A division into two magnitude classes is not impossible."

First and foremost we all thank Robert Nemiroff for the enormous amount of work he has put into organizing this event, compromising among the wishes of people at least as discordant as their scenarios. I am personally deeply grateful to Ralph Belknap Baldwin, Owen Gingerich, Dorrit Hoffleit, Vera Cooper Rubin, and Richard Langley Sears for sharing memories and records that still connect us, tenuously, to the era of the original Curtis–Shapley debate, and to Katherine Gaposchkin Haramundanis for an invitation to write the introduction to the second edition of her mother's biographical volume, which led to my rereading it at just the right time.

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