

Origins of the Expanding Universe: 1912-1932
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Dismantling Hubble's Legacy?

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Abstract. Edwin Hubble is famous for a number of discoveries that are well known to amateur and professional astronomers, students and the general public. The origins of these discoveries are examined and it is demonstrated that, in each case, a great deal of supporting evidence was already in place. In some cases the discoveries had either already been made, or competing versions were not adopted for complex scientific and sociological reasons.

1. Introduction

Edwin Hubble is considered one of the titans of early 20th century observational cosmology. He is credited in most textbooks¹ and the popular literature for a series of important discoveries made between 1920 and 1930:

- The confirmation of the Island Universe hypothesis
- The classification of extragalactic nebulae
- The discovery of a linear relationship between distance and velocity for extragalactic nebulae, providing the first evidence for the expanding universe
- The brightness profile of galaxies

The discoveries above are well-known to the astronomy community and most astronomers would associate them solely with Edwin Hubble; yet this is a gross oversimplification. Astronomers and historians are beginning to revise that standard story and bring a more nuanced version to the public's attention. This paper is adding to this burgeoning reappraisal.²

As a (small) counter-narrative, William Hoyt (1980, p. 411), in his biographical memoir of V. M. Slipher exclaims that “[Slipher] probably made more fundamental discoveries than any other observational astronomer of the twentieth century.”³ Clearly some historians in the 1970s and 1980s thought that Slipher made more fundamental

¹See Smith (2009, p. 98).

²Some examples include Nussbaumer & Bieri (2009); Kragh & Smith (2003); Bartusiak (2010).

³Also see Hall (1970).

discoveries than Hubble.⁴ Yet how can that be true given all we *know* today? In this paper we re-examine Hubble’s discoveries in some detail in order to see if they are better understood in a broader context. Given the focus on V. M. Slipher at this conference we will also explicitly discuss his contributions in two of the cases above.

2. Discovery of the Island Universe

The hypothesis of Island Universes has a long history going back at least to the 18th Century with contributions by: Swedenborg (1734); Wright (1750); Kant (1755)⁵, and Lambert (1761).⁶ William Herschel (1785) initially believed that the spiral nebulae were external to the Milky Way, but later changed his mind. Knut Lundmark (1927e, Chapter 1) does an excellent job of explaining the origins of the Island Universe that I do not believe has been much bettered by time.⁷

To get from philosophical speculation to modern quantification one must fast-forward to the late 19th and early 20th century to find a large number of investigations of objects termed “Nebulae” with the new art of photography and ever larger telescopes.⁸ For example, Huggins & Miller (1864) were deeply interested in the spectra of nebulae, while astronomers such as Isaac Roberts (1903) built photographic catalogs.⁹ Using the catalogs of nebulae like that of Roberts’ and others Lundmark (1925, pg.869) claimed that nearly 1200 spiral nebulae proper motions had been measured at that time. We now know this claim was incorrect – most likely it was an incorrect assessment of observational errors.¹⁰ The field was clearly in its infancy, but progress on distance estimates to objects like globular clusters and spiral nebulae was moving rapidly forward.

Table 1 lists all of the main distance estimates to spiral nebulae (known to this author) from the late 1800s until 1930 when standard candles began to be found in spiral nebulae.

The ability to estimate *accurate* distances of objects beyond the reach of parallax only came into being with the publishing of the period–luminosity relationship for Cepheid Variable stars by Henrietta Leavitt & Edward Pickering (1912) and its later calibration by Ejnar Hertzsprung (1913); Henry Norris Russell (1913)¹¹ and later Har-

⁴See contributions in this book by John Peacock, Joseph S. Tenn, Robert Smith, Laird Thompson and Kevin Schindler for more on Slipher’s discoveries.

⁵Kant actually cited the work of Thomas Wright (1750).

⁶See contribution by Ayala in this book.

⁷The essence of the hypothesis, in an early 20th century context, was that the universe was populated by many Milky Way galaxies known then as spiral nebulae. This was opposed to the belief that the universe consisted of a single Milky Way object with satellites such as spiral nebulae, globular clusters and Magellanic cloud-like objects.

⁸See Gingerich (1987) for more on this early period.

⁹Only later was Roberts’ catalog compiled and completed by his wife Mrs. Isaac Roberts.

¹⁰Only in 2012 was the proper motion of M31 possibly measured by Sohn et al. (2012) using optical observations.

¹¹Using 13 Cepheids

Table 1. Early distance estimates to Spiral Nebulae

Reference	Object	Distance ^a	Method
Herschel (1786)	M31	<17,200 ^b	color/magnitude
Nichol (1850)	“cluster”	154,800 ^c	magnitude comparison
–	–	302,505	–
Clark (1890)	M31	564?	nova of 1885
Clark (1903)	M31	<1000	Size
Bohlin (1907)	M31	19	parallax
Very (1911)	M31	4,000	diameters
Very (1911)	M31	1,600	S Andromedae
Wolf (1912)	M31 ^d	32,000	diameters
Curtis (1915b)	spirals	10,000	astrometry/radial velocity
Pease (1916)	NGC 4594	25,000	astrometry/radial velocity
Curtis (1917)	M31	20,000,000	novae
–	–	100,000	novae ^e
Shapley (1917)	M31	1,000,000	“bright stars”
van Maanen (1918)	M31	250	parallax
Lundmark (1919)	M31	650,000	novae
Curtis (1920)	misc	4,000,000	novae
–	misc	1,000,000	novae
–	misc	500,000	novae
Lundmark (1921h)	M33	1,000,000	“bright stars”
Luplau-Janssen & Haarh (1922)	M31	326,000	novae ^f
Öpik (1922)	M31	1,500,000	luminosity/mass
Hubble (1922d)	M33	100,000	“stars”
Shapley (1923)	NGC 6822	1,000,000	diameters/“bright stars”
Hubble (1925a)	M31/33	930,000	Cepheids
Hubble (1925c)	NGC 6822	700,000	Cepheids, “bright-stars”
Lundmark (1925)	M31, M87	1,400,000	novae
–	–	8,000,000	novae
Lundmark (1925)	M104	56,000,000	Öpik (1922) method
Hubble (1926a)	M33	850,000	Cepheids, Blue-Giants
Hubble (1929c)	M31	900,000	Cepheids, novae
M31 value (Dec. 2012) ^g	M31	2,588,440	19 Methods

^aUnits of light years^bHerschel stated on page 262 that “...I believe to be an indication that its distance in this coloured part does not exceed 2000 times the distance of Sirius.” Using the modern value of the distance to Sirius of 8.6 light years yields an upper limit of 17,200. Note that no parallax measurement to a star had yet been achieved.^cEstimated the maximum distance a *cluster* could be resolved using Herschel’s telescope to be either 18,000 or 35,175 times the distance to Sirius (p. 51). The modern distance to Sirius was used as above.^dBelieved the Milky Way to be ~1000 light years in diameter, so this number is well outside the Milky Way by his own estimate. Wolf also measured distances to a number of other Spiral Nebulae, e.g. M33 (86,000 light years), M81 (170,000), M101 (270,000), M51 (310,000) – all well outside the Milky Way.^eCurtis claimed that the distances to the novae in Andromeda were 100 times farther away than the galactic ones. The galactic novae were estimated to be 1000 light years away.^fThey used two methods, one like that of Lundmark (1919) using comparable brightnesses of novae in M31 and our Galaxy, and the other involving the distances between novae in M31 and our Galaxy.^gFrom the NASA Extragalactic Database. The value presented here is an average (as of December 2012) from 19 different methods and 133 data points: <http://ned.ipac.caltech.edu/cgi-bin/nDistance?name=MESSIER+031>

low Shapley (1918)¹² all utilizing the Lewis Boss (1910) catalog of proper motions.¹³ Before Cepheids were discovered in spiral nebulae there were a number of attempts to use novae as standard candles to measure the distances to spiral nebulae (Curtis 1917; Shapley 1917; Lundmark 1919; Luplau-Janssen & Haarh 1922). For example, Heber Curtis (1917) calculated an average distance to the spiral nebulae of 20,000,000 light years in one case and found them to be around 100 times as distant as the galactic novae in another. Lundmark obtained a distance to Andromeda of 650,000 light years. Shapley (1918) attempted to compare the “brightest stars” in our own galaxy to that of Andromeda and stated:

... the minimum distance of the Andromeda Nebula must be of order a million light years. At that remote distance the diameter of this largest of spirals would be about 50,000 light years a value that now appears most probable as a minimum for our galactic system.¹⁴

Initially the novae studies in Andromeda were difficult to reconcile with a supernovae observed in Andromeda ~35 years previously (Krueger et al. 1885; Hartwig 1885a,b; de Vaucouleurs & Corwin 1985)¹⁵, but given the multiple observations of fainter novae observed in spiral nebulae Curtis (and later others) was persuaded to drop S Andromedae and Z Centauri¹⁶ as anomalies.

Unfortunately for the novae derived distance measurements, other observations at that time called their accuracy into question. In the mid-1910s Curtis (1915a) and then Lampland (1916) detected rotation in spiral nebulae, but the former did not believe his own detection while the latter’s results were not influential (Smith 1982, p. 31). However, additional observations of this sort by van Maanen et al. (1916) in Messier 101 (M101) and later in Messier 33 (M33) and other nebulae (van Maanen 1923) along with the support of James Jeans (1917) convinced many astronomers like Shapley (1919a) that novae derived distances to spiral nebulae were impossible to reconcile without superluminal speeds of spiral nebulae rotation.¹⁷

However, in spite of the confusing novae observations and (incorrectly) observed rotation of spirals the evidence continued to mount that the spiral nebulae were indeed very distant objects. The first evidence of this was a fascinating paper by Öpik (1922) where “an expression is derived for the absolute distance in terms of the linear speed v_o at an angular distance ρ from the center, the apparent luminosity i , and E , the energy radiated per unit mass.” He calculated a distance of 1.5 million light years to M31. One year later Shapley (1923) used diameters of galaxies and the brightness of super-giant stars in NGC 6822 to state:

¹²Using 11 Cepheids of Hertzsprung’s original 13.

¹³Although Shapley’s distances were strongly contested by a number of people including Curtis (1921).

¹⁴This is ironic given his later disavowals of his larger distance estimates and those related to novae in spiral nebulae (Shapley 1919b).

¹⁵This supernova was later denoted ‘S Andromedae’. At that time supernovae were unknown so they were easily confused with normal novae.

¹⁶Z Centauri (in NGC 5253) was another *bright* nova observed by Pickering (1895).

¹⁷This was also the time when Shapley came up with his 300,000 light year diameter Milky Way galaxy, much larger (> 30 times) than any other estimate at that time.

The above considerations all indicate that the distance of N.G.C. 6822 is of the order of a million light years. It appears to be a great star cloud that is at least three or four times as far away as the most distant of known globular clusters and probably quite beyond the limits of the galactic system.

This quote is particularly interesting in light of Shapley's long standing opposition to the Island Universe hypothesis and his super-galaxy model (Shapley 1921), but he still would not let go of his super-galaxy model just yet.

The issue was effectively settled by two papers from Hubble in 1925 in which he derived distances from Cepheid variables found in M31 and M33 (Hubble 1925a) of 930,000 light years¹⁸ and in NGC 6822 (Hubble 1925c) of 700,000 light years. Note that there were no citations to previous distance estimates in the former paper and only a reference to Shapley (1918) for his calibration of the Cepheid variable light curves in the latter.

Still, there was some confusion about van Maanen's spiral nebulae observations among his contemporaries including Knut Lundmark. Lundmark (1922c) was at first dismissive of van Maanen's measurements because of a preponderance of conflicting data. However, by 1922 he had changed his mind after having measured some of van Maanen's plates himself (Lundmark 1927e).¹⁹ Upon re-measuring the motions in M33 a couple of years later, Lundmark concluded that van Maanen's measurements were flawed (Lundmark 1926c, 1927e). However, it would not be until 1935 that van Maanen (1935) would nearly admit that his measurements of the rotation of spiral nebulae were false. In the same issue of *The Astrophysical Journal* Hubble (1935) published his own measurements showing any measured rotation to be within the measurement errors. Clearly Hubble wanted to make sure that the persistent observations of van Maanen were dismissed by publishing his own measurements given his (now) elevated status as one of the more highly respected astronomers of his day.²⁰ Thus it took nearly a decade after Hubble's 1925 paper for van Maanen's measurements to be completely disposed of and the Island Universe theory to be confirmed. Of course many astronomers felt the matter had been settled all the way back in 1926.²¹

Still, as mentioned by Robert Smith (2008, p. 114) "...what was missing to settle the dispute on the spirals was a method of calculating their distances that a great majority of astronomers could agree was accurate." Clearly Hubble (1925a) provided that method with his observations of Cepheids in spirals, but a great many people before him made his observations possible. As Smith (2009, p. 74) points out "... it is appropriate to view Hubble as confirming rather than discovering the extragalactic nature of spirals. But, following the dictum of John Herschel that he who proves discovers, Hubble was given the credit." Many important contributions to this story have been forgotten and most textbooks in astronomy today, if they discuss the "Island Universe"

¹⁸The original Cepheid that Hubble discovered in M31 now has a modern ephemeris and light curve published by Templeton et al. (2011).

¹⁹See page 17 where he states that [in 1922?] "When remeasuring Messier 33 during my stay at Mount Wilson the situation seemed to be rather hopeless for the followers of the island-universe theory."

²⁰Christianson (1996, Chapter 11) explains what happened in more detail.

²¹As mentioned above (Lundmark 1926c) but also Luyten (1926): "It is now universally accepted that the spiral nebula are millions of light years distant."

confirmation at all, bestow 100% of the credit on Hubble with scant attention to the earlier observations that clearly supported his measurements.

At that time the use of Cepheids as standard candles was considered a very reliable method of estimating distance. However, by the mid-1950s it had become clear that Hubble's distances measurements contained significant systematic errors. Recalibration of the Cepheids by Walter Baade (1956) later helped to show that Andromeda was twice as far away, and was actually larger than our own Milky Way. This would have serious implications for the Big Bang theory in the 1930s and 1940s.

2.1. Slipher's Contribution to the Island Universe story

The anniversary date for this conference was intended to overlap with the published date of Slipher's first observation of a doppler shift in a spiral nebula (Andromeda) on 17 September 1912 (Slipher 1913). He obtained an astounding value of -384 km s^{-1} . This was surprising because it was nearly an order-of-magnitude higher than any other measured doppler shift in the heavens at that time. By 1917 Slipher had observed 25 spiral nebulae, the largest having a redshift of 1100 km s^{-1} (Slipher 1917). As we have seen above the debate on whether spiral nebulae were Island Universes went on until Hubble discovered Cepheids in Andromeda and other spiral nebulae. Given the 25 spiral nebulae with radial velocities discovered by Slipher in 1917 (21 of which were redshifts) why didn't the astronomical community realize these objects could not be bound to the Milky Way and must be Island Universes? In fact a number of people did reach this conclusion including Campbell (1917) and Hertzsprung (see Robert Smith's chapter in this book). Still, it took several years for Slipher to convince the community that what he was observing was real. As well the reticence to push this interpretation by Slipher himself was related to his modest personality.²² In reality what made it difficult (and would make it difficult for *all* proponents of an Island Universe theory) were the observations of internal motion in Spiral Nebulae by Adrian van Maanen that began with his first publication on the subject in July of 1916 (van Maanen 1916).²³ Had it not been for the erroneous observations of van Maanen it is likely that Slipher's observations would have provided strong support for the Island Universe theory.

3. Classification of Extra-galactic Nebulae

The first systematic classification of nebulae was probably attempted by Herschel (1786) in his paper titled "Catalogue of One Thousand New Nebulae and Clusters of Stars." Therein he described eight different classes of objects:

1. Bright nebulae [93 examples]
2. Faint nebulae [402]
3. Very faint nebulae [376]
4. Planetary nebulae [29]
5. Very large nebulae [24]
6. Very compressed and rich clusters of stars [19]

²²See Section 7 and Robert Smith's chapter in this book.

²³See David DeVorkin's contribution to this proceedings and the first chapter in Smith (1982).

7. Pretty much compressed clusters of large or small stars [17]
8. Coarsely scattered clusters of stars [40]

His descriptions of the nebulae were extremely detailed using terms with single letter abbreviations, for example: B. Bright, S. Small, v. very, e. extremely, R. Round, M. in the middle, l. a little, g. gradually, r. resolvable, m. milky. These were used in combinations, one of his own examples being vgmbM: (v)ery (g)radually (m)uch (b)righter in the (M)iddle.

Later Lord Rosse (1850) gave the term spiral to some of Herschel's nebulae by using his new 1.8m telescope "Leviathon of Parsonstown," but he described it first via a drawing of M51 presented to the 15th meeting of the British Association for the Advancement of Science (Rosse 1845; Hoskin 1982; Dewhirst & Hoskin 1991).

Table 2. Early Classification schemes for Extragalactic–Nebulae

Reference	Notes
Herschel (1786)	"first comprehensive scheme?"
Rosse (1850)	terminology "Spirals" used
Wolf (1908)	"widely cited scheme"
Bailey (1908)	–
Pahlen (1911)	–
Bigourdan (1914)	–
Shaw (1915)	–
Curtis et al. (1918)	"bars"
Curtis (1919)	–
Jeans (1919)	Theoretical
Reynolds (1920)	Classification of spirals like Hubble (1922a)
Hubble (1922a)	Preliminary scheme
Lundmark (1926a)	Preliminary scheme
Hubble (1926b)	More complete scheme
Lundmark (1927e)	Full scheme
Shapley (1927)	–
Jeans (1928)	Tuning-fork diagram suggestion
(Hubble 1936)	Tuning fork diagram added to create complete scheme

The classification scheme of Herschel (1786) (with later modifications by son John Herschel) was considered unwieldy and complicated, but was probably the only one referred to consistently until new schemes in the early 20th century such as that of Wolf (1908).²⁴ Wolf's classification scheme worked for all types of nebulae. He not only lists specific examples of each but also includes a table of images. He labeled them with letters 'a–w' (there is no letter 'j', but rather an 'h' and 'h_o'). Another interesting scheme was developed by Bigourdan (1914).

Today Hubble (1926b) is generally given credit for coming up with the first usable classification scheme of "Galaxies", or as they came to be known "Extra-galactic neb-

²⁴The classification scheme of Wolf (1908) is mentioned in a number of articles, but without a proper citation. The UGC catalog of Nilson (1973, p. 452) cites the year 1909: "The well-known classification system for nebulae, the Wolf code a-w with 23 standard objects, was presented for the first time in 1909." It was actually 1908.

ulae”. In fact the Table in his 1926 paper is titled “Classification of Nebulae” which included both “Galactic nebulae” and “Extra-galactic nebulae” which is an extension of his earlier work (Hubble 1922a). Using his 1922 work as a basis, he had tried to build his Extra-galactic nebulae classification scheme in-line with the nebular evolutionary model of Jeans (1919).

One of the great strengths of the Hubble (1926b) paper was his formula that described the spiral divisions:

$$m_t = C - 5\log(d) \quad (1)$$

where m_t =total magnitude, d =diameter of the nebulae, C =Constant describing each object in his sequence (1–3): Sa(1),Sb(2),Sc(3),SBa(1),SBb(2),SBc(3) where 1=Early, 2=Intermediate, 3=Late. Hence as a nebulae aged from “Early” to “Late” the diameter and luminosity would change accordingly and (again) in-line with the theoretical work of Jeans (1919). However, other models at that time (Lindblad 1927) contradicted some aspects of Jean’s evolutionary sequence (Jeans 1919).²⁵

David Block & Ken Freeman (2008) appear to most recently describe how Hubble’s entire “Extra-galactic nebulae” classification scheme was remarkably similar to one developed by John Reynolds (1920). In fact they present clear evidence that Hubble (at this time) was aware of the Reynolds (1920) paper via an unpublished memo written to Reynolds, which they reproduce in their book. However, Hubble (1922a, 1926b) does not cite Reynolds (1920) in these papers, although he does give credit to Curtis et al. (1918) for the recognition of bars in spiral nebulae.

A year after he introduced his 1926 classification scheme, Hubble (1927f) mentioned a paper from earlier in 1927 in which Reynolds (1927) criticizes Hubble’s published classification scheme of 1926. Hubble again omits mention of Reynolds (1920) while Reynolds not only mentions Hubble’s work, but an even earlier classification scheme by Shaw (1915).

What makes Hubble’s omission of Reynolds (1920) particularly troubling is that he accused Lundmark of plagiarism not only in personal correspondence²⁶, but also on page 3 of his 53-page classification scheme article (Hubble 1926b):²⁷

Meanwhile K. Lundmark, who was present at the Cambridge meeting and has since been appointed a member of the Commission, has recently published (Arkiv för Matematik, Astronomi och Fysik, Band 19B, No.8, 1926) a classification, which, except for nomenclature, is practically identical with that submitted by me. Dr. Lundmark makes no acknowledgments or references to the discussions of the Commission other than those for the use of the term ‘galactic’.

This is rather remarkable because in no paper published by Hubble between 1920 and 1930 is the classification scheme of Reynolds (1920) mentioned. Lundmark (1927e)

²⁵In particular Lindblad (1927) states, “We do not assume a general development from less flattened to more flattened system of higher angular speed of rotation [like that of Jeans].” Lindblad also believed that the centers of the the nebulae were simply unresolved faint stars – contrary to Jeans.

²⁶He asked Lundmark to explain himself and threatened to publish his accusation (Holmberg 1999, p. 103).

²⁷It was published a few months after (Lundmark 1926a).

strongly rebutted Hubble on page 24 of his 127-page paper titled “Studies of Anagalactic Nebulae.”²⁸ The latter denotes the classification scheme of Wolf in one of the columns²⁹ next to his own, but also mentions other work that preceded his (page 23):

Classifications of nebulae based on photographic material have been made by Bailey, Curtis, Mrs. Isaac Roberts, Max Wolf, Hubble and others.

We do not have the space here to delve deeply into the personalities of Hubble or Lundmark, yet we may get some feeling for what their contemporaries felt about them and how they felt about their contemporaries via the limited notes placed in papers and in their personal correspondence. Some of the latter can be found in Smith (1982), while some specific examples in the case of Hubble are described in Christianson (1996, Chapter 11).³⁰

On the other hand Lundmark has been called enigmatic by Smith (1982), but at least some of his contemporaries appreciated his general attitude. Take this quote from Ludwik Silberstein (1925):

... I should like to express my deep gratitude to Dr. Lundmark for having devoted so much attention to the discussion of this problem from a perfectly impartial attitude.

This was in reply to a paper by Lundmark (1924b) that criticized Silberstein (1924d) for his use of globular clusters (GCs) to determine the curvature radius of the Universe. Lundmark felt that GCs were not distant enough.³¹ Holmberg (1999) in his Chapter titled “Lundmark and the Lund Observatory” also paints a picture of a complex character, but whose bitterness towards some of his Swedish colleagues appeared to surface later in his career when his scientific productivity was waning.

Finally, it is clear that Lundmark had been thinking of a classification scheme for nebulae at least since 1922 (Teerikorpi 1989) and even discussed a simplified “class of objects” in Lundmark (1925). Lundmark’s scheme for “Anagalactic nebulae”³² was broken into 4 groups (see page 22 of Lundmark (1927e)): 1.) Anomalous nebulae (Aa), 2.) Globular, elliptical, elongated, ovate or lenticular nebulae (Ae), 3.) Magellanic (“irregular”) nebulae (Am), 4.) Spiral nebulae (As), where the degree of condensation toward the center was his main criteria. On the other hand Hubble (1926b) separated his ellipticals by eccentricity. Spirals were separated based on form and degree of arm development. In fact the Lundmark and Hubble schemes were *not* considered the same by their contemporaries.³³

²⁸See Appendix C for Lundmark’s full reply.

²⁹No citation is provided, but he must be referring to Wolf (1908).

³⁰A particular quote from Walter Adams, then the director at Mt. Wilson, is worth repeating in reference to the conflict between Hubble and van Maanen over the distances to the spirals: “This is not the first case in which Hubble has seriously injured himself in the opinion of scientific men by the intemperate and intolerant way in which he has expressed himself.” This was in reference to the conflict between Hubble and van Maanen that Adams had to negotiate as director.

³¹It may be amusing to note that six years later Lundmark (1930d) asked whether the GCs and Extragalactic (he used the word Anagalactic) nebulae were related.

³²His name for “Extra-galactic nebulae.”

³³See Smith (1982, p. 152).

A year later Shapley (1927) joined in the classification attempts with a model that incorporated aspects of the work of both Lundmark and Hubble, but his model was not adopted. Contrary to Hubble, Shapley carefully cited his predecessors including Bailey (1908); Reynolds (1920); Wolf (1908); Hubble (1922a); Lundmark (1926a); Hubble (1926b).

Hubble’s classification scheme is also noted for its later tuning fork design to separate the barred spirals from non–barred ones (Hubble 1936). Block et al. (2004) have pointed out that Hubble was not the first to describe the tuning fork diagram – that was originally proposed by Jeans (1928).

It is generally acknowledged that Hubble’s classification scheme became standard because it had an evolutionary component and mathematical description (see Equation 1) that previous schemes did not. But as should be clear from Table 2 there were a great many classification schemes leading up to that of Hubble’s which surely influenced him. Is it troubling that Hubble does not readily cite two of the most important and influential schemes (before his own was published) of Reynolds (1920) and Wolf (1908) and yet accuses a contemporary (Lundmark) of plagiarism on the basis of scant evidence? This lack of citation by Hubble will be further discussed in Section 6.

4. Discovery of the “Hubble Constant”

A great deal has been written in recent years on the topic of the discovery of the expanding universe (Nussbaumer & Bieri 2009; Shaviv 2011; Kragh & Smith 2003; Smith 1982). A number of accusations have been levelled against Hubble (Block 2011), some of which have been discredited (Livio 2011). Several chapters in this book contain discussions on the discovery of the expanding universe (see chapters by Cormac O’Raifeartaigh, Ari Belenkiy, Harry Nussbaumer, John Peacock, and Robert Smith). For that reason there is no need to go into a great deal of detail here, but suffice it to say that this “discovery” is even more complicated than the other stories described above. In Table 3 one can see a steady progression of three related measures: 1.) the solar motion with respect to the nebulae, 2.) the radius of curvature of the universe and 3.) the linear relation of velocity and distance for the spiral nebulae that lent support to an expanding universe model over a static one in the first half of the 20th century. Note that papers that do not explicitly discuss observational data are not included in the table. Some further details on selected publications from Table 3 are worth mentioning in detail:

- O.H. Truman (1916): was the first to measure the solar motion relative to the spiral nebulae like that of Campbell (1913) and Airy (1860):

$$V = X \cos\alpha \cos\delta + Y \sin\alpha \cos\delta + Z \sin\delta$$
³⁴
- George Paddock (1916): realized there may be a non-random spiral nebulae recession component (denoted as K). He didn’t believe it was real, but others quickly thought otherwise. This paper contains the first appearance of the “K

³⁴ α =Right Ascension, δ =Declination, X,Y,Z = velocity components of our sun through space.

Table 3. Early estimates of solar motion, curvature radius and H_0 via spiral nebulae

Reference	Date	Notes
Truman (1916) ^a	1915/12/30	RA=20h, Dec=-20°, V=-670km s ⁻¹ (14 spirals)
Young & Harper (1916) ^a	1916/02/01	RA=20h24m, Dec=-12° 10', V=-598±234km s ⁻¹ (15 spirals + MC)
Paddock (1916) ^b	1916/05/00	V=-295±202 km s ⁻¹ , K=+248±88 km s ⁻¹ Using Young & Harper (1916) data.
Wirtz (1916)	1916/08/00	Various values of RA, Dec, V, but no K term
Wirtz (1917)	1916/12/06	Various values of RA, Dec, V, but no K term
Slipher (1917)	1917/04/13	RA=22h, Dec=-22°, V=-700 km s ⁻¹
de Sitter (1917)	1917/07/00	First estimates of R for Models A and B
Wirtz (1918)	1917/12/00	K=+656 km s ⁻¹ , V=-830 km s ⁻¹
Shapley & Shapley (1919)	1918/11/00	Magnitude vs. Velocity
Lundmark (1920)	1920/01/26	K=+587 km s ⁻¹ (29 spirals, page 75)
Wirtz (1922)	1921/10/00	K=+656 km s ⁻¹ , V=-820 km s ⁻¹
Friedmann (1922)	1922/05/29	Set M=5×10 ²¹ M _⊙ , λ=0 and found "world period"=10 ¹⁰ years
Wirtz (1924)	1924/03/00	Distance vs. velocity: v(km)=2200-1200×log(Dm) ^c
Silberstein (1924d)	1924/03/00	Distance vs. velocity Relation and calculation of curvature radius (R)
Silberstein (1924e)	1924/03/08	Calculates R for 11 GC ^d
Silberstein (1924a)	1924/04/26	First distance vs. velocity plot for GC and LMC/SMC
Lundmark (1924b)	1924/06/00	First distance vs. velocity plot for spiral nebulae, K _{spirals} = +800 km s ⁻¹
Silberstein (1924b)	1924/06/07	Same method and data as before with new estimate of R
Silberstein (1924c)	1924/10/00	
Silberstein (1924f)	1924/Late	Distance vs. velocity plot
Silberstein (1925)	1925/01/00	R=7.2×10 ¹² A.U. and updated plot of distance vs. velocity for GC+MC+M33
Strömberg (1925)	1925/06/00	Estimates of K, but no relation found for distance vs. velocity
Lundmark (1925)	1925/06/00	Re-defines: K=k+lr+mr ² , First time for a variable K-term
Dose (1927)	1926/11/00	K=+765±111 km s ⁻¹ for spirals (no variable K-term)
Lemaître (1927)	1927/04/00	Discovers that K is linearly dependent on distance
Robertson (1928)	1928/00/00	Expects a correlation between velocity and distance
Hubble (1929b)	1929/01/17	Distance vs. Velocity plot using Cepheid distances yields a linear fit
de Sitter (1930)	1930/05/26	Using observational data calculates R and estimates slope of velocity vs. distance
Hubble & Humason (1931)	1931/03/00	Updated list of distances and velocities yields 558 km s ⁻¹ Mpc ⁻¹
Oort (1931)	1931/11/30	Finds H ₀ =290 km s ⁻¹ Mpc ⁻¹ after finding some distance inaccuracies

^aSolar motion relative to Spiral Nebulae, MC=Magellanic Clouds^bFirst appearance of K correction^cDm = Distance via diameter^dGC = Globular Clusters

correction”³⁵ in the formula for solar motion:
 $V = X \cos\alpha \cos\delta + Y \sin\alpha \cos\delta + Z \sin\delta + K.$ ³⁶

- Willem de Sitter (1917): attempted to measure the radius of curvature of the universe (R) for Einstein’s model A and his own model B in a number of ways. In model A he made an estimate of the mass and total volume of the universe to obtain $R \leq 5 \times 10^{13}$ Astronomical Units (A.U.)³⁷ (790×10^6 light years). In model B he made a number of estimates (all found in Section 6 of his paper), but perhaps the most relevant are:
 1. “If we accept the existence of a number of galactic systems whose average mutual distances are of the order 10^{10} all we can say is that πR must be several times 10^{10} or roughly $R > 10^{11}$ [A.U.]” (1.6×10^6 light years)
 2. “For the lesser Magellanic cloud Hertzsprung found the distance $r > 6 \times 10^9$. The radial velocity is about $+150 \text{ km sec}^{-1}$. This gives $R > 2 \times 10^{11}$ [A.U.]” (3×10^6 light years)
 3. By averaging the velocities of 3 spiral nebulae ($+600 \text{ km s}^{-1}$) and their distances (326,000 light years) he obtained $R = 3 \times 10^{11}$ A.U. (4.7×10^6 light years)
- Shapley & Shapley (1919): “The speed of spiral nebulae is dependent to some extent upon apparent brightness, *indicating a relation of speed to distance* or, possibly, to mass.” [our emphasis]
- Alexander Friedmann (1922): derived the first non-static solutions in addition to the solutions of Einstein (1917) (model A) and de Sitter (1917) (model B). He estimated the age of the universe, but we do not know where he obtained his numbers from. He set the mass of the universe $M = 5 \times 10^{21} M_{\odot}$, set $\lambda = 0$ and found a “world period” = 10^{10} years. However see Ari Belenkiy’s chapter in this book. Belenkiy believes this number should have been written 10^9 years.
- Silberstein (1924d): estimated a distance vs. velocity relation for 7 GC, LMC and SMC³⁸, $R = 94 \times 10^6$ light years ($R = 6 \times 10^{12}$ A.U.). His estimate was later criticized by Lundmark (1924b) (among others) for only using 7 of 16 known GC. Lundmark (1924b) states: “... there is no good reason for selecting such an arbitrary limit for excluding objects *which do not give a rather constant value of R.*” Lundmark (1924b) also complained that the low value of K ($+31$ for GCs vs. $+800 \text{ km s}^{-1}$ for spiral nebulae) “suggests that the former are comparatively near as compared with the latter”, implying that they were inappropriate for calculating R. [Published March, 1924]
- Silberstein (1924e): for 11 GC $R < 94 \times 10^6$ light years ($R < 6 \times 10^{12}$ A.U.) [Published March 8, 1924]

³⁵Not to be confused with the modern usage of “K correction” which refers to resetting the colors of a galaxy to the rest frame.

³⁶K is the average recessional velocity of spiral nebulae observed in km s^{-1} .

³⁷de Sitter (1917, p. 25)

³⁸GC=Globular Clusters, LMC=Large Magellanic Cloud, SMC=Small Magellanic Cloud)

- Silberstein (1924a): first distance vs. velocity plot with 2 fits for 11 *Globular Clusters* including the LMC and SMC. (one fit only used 8 GC). $R < 158 \times 10^6$ light years ($R < 1 \times 10^{13}$ A.U.). [Published April 26, 1924]
- Silberstein (1924b): used the same objects as in Silberstein (1924a) and found $R = 110 - 126 \times 10^6$ light years ($R = 7 - 8 \times 10^{12}$ a.u.). [Published June 7, 1924]
- Lundmark (1924b): first distance vs. velocity plot for *spiral nebulae*. Included separate plots for GC+LMC+SMC, and a variety of stellar types, but without any lines fit to any samples since he did not feel it was warranted. He used the novae distances to Andromeda and then used spiral nebulae diameters (assuming constant nebular diameters and luminosities) for the other spirals in his sample in comparison with Andromeda – his x-axis units were in ‘Distances of Andromeda Nebula.’ He also calculated $K = +800 \text{ km s}^{-1}$ (for spirals), $R = 19.7 \times 10^{12}$ A.U. (3×10^8 light years) for GC (but with a very large dispersion of 26.1×10^{12} A.U.)³⁹ and $2.4 - 6.6 \times 10^{12}$ A.U. ($3.8 \times 10^7 - 1 \times 10^8$ light years) for spirals.⁴⁰ Duerbeck & Seitter (1999) fit Lundmark’s data in the plot for spirals using Lundmark’s smaller value of the distance to Andromeda of 0.2 Mpc to yield a slope with the origin through zero of $90 \text{ km s}^{-1} \text{ Mpc}^{-1}$. If one uses an Andromeda distance of 0.5 Mpc (derived by Lundmark using novae) one obtains $36 \text{ km s}^{-1} \text{ Mpc}^{-1}$. Also see Peacock’s contribution in this volume. [Published June, 1924]
- Silberstein (1924c): $R = 7.2 \times 10^{12}$ A.U. (1.1×10^8 light years). [Published September 6, 1924]
- Silberstein (1924f): distance vs. velocity plot for 11 GC, plus the LMC, SMC and M33. Values for R were nearly the same as in Silberstein (1924d). [Published late 1924]
- Silberstein (1925): newly updated distance vs. velocity plot using 18 GC, the LMC, SMC, and M33. Obtained $R = 7.2 \times 10^{12}$ A.U. (1.1×10^8 light years). [Published January 1925]
- Strömberg (1925): after an extensive investigation into spiral nebulae (and separately GCs) he considered correlations between distance vs. velocity and positions on the sky ($\cos \lambda$) vs. velocity. He ended his article by stating: “In conclusion we may say that we have found no sufficient reason to believe that there exists any dependence of radial motion upon distance from the sun. The only dependence fairly well established is one that is a function of position in the sky.” He plotted *two* equivalent correlations for the globular clusters on the same plot with wildly different slopes and stated: “It is significant, however, that the regression-line for the clusters does not go through the origin as expected from the theory.” [Published June 1925]

³⁹Lundmark stated: “As the dispersion in R is 26.1×10^{12} km and thus considerably higher than what could be expected from the dispersions in V and r, it does not seem that the curvature of space-time, at least for the present, can be determined with any accuracy by using the displacements in the spectra of globular clusters.”

⁴⁰Note: Lundmark quoted R in units of kilometers, but it is clear in comparisons with Silberstein’s papers and with his own distance estimates to GCs and spiral nebulae that he must have meant to write A.U. rather than km.

- Lundmark (1925): initially believed the K-term was a constant for spirals, but decided it was given by $K = k + lr + mr^2$. Here k,l,m are constants, and the r is relative distance via the apparent diameter. Solving with 44 velocities gave $k=513, l=10.365, m=0.047$.
- Georges Lemaître (1927): derived a non-static solution to Einstein's equations and coupled it to observations to reveal a linear distance vs. redshift relation with a slope of 670 or 575 $\text{km s}^{-1} \text{Mpc}^{-1}$ (depending on how the data is grouped). Radial velocities were from Strömberg (1925), distances from apparent magnitudes given in Hubble (1926b) that were taken from Hopmann (1921) and Holetschek (1907).
- Howard Percy Robertson (1928): "... we should nevertheless expect a correlation $v \approx cl/R$ between assigned velocity v, distance l, and radius of the observable world R." [equation 17] Using the data of Hubble (1926b) for distances and Slipher (Eddington 1923) for velocities he obtained $R=2 \times 10^{27} \text{cm}$ ($1.3 \times 10^{14} \text{A.U.}$, 2.1×10^9 light years). Hilmar Duerbeck & Waltraut Seitter (1999) have estimated his distance vs. velocity slope as $460 \text{ km s}^{-1} \text{Mpc}^{-1}$. Robertson also says that a similar relation to that of equation 17 was deduced by Weyl (1923).
- Hubble (1929b): used Cepheids and bright stars for distances and spiral nebulae Doppler shifts mostly from Slipher (Eddington 1923). He found a linear relation between distance and velocity using the data available (grouping them two ways) and an updated solar motion equation: $V = X \cos \alpha \cos \delta + Y \sin \alpha \cos \delta + Z \sin \delta + kr$, where the old K is now a function linearly dependent upon distance ($K=kr$). He quoted a slope of $\sim 465 \pm 50 \text{ km s}^{-1} \text{Mpc}^{-1}$ for 24 objects, and $\sim 513 \pm 60 \text{ km s}^{-1} \text{Mpc}^{-1}$ for 9 groups. He stated: "The outstanding feature, however, is the possibility that the velocity-distance relation may represent the de Sitter effect, and hence that numerical data may be introduced into discussions of the general curvature of space."
- de Sitter (1930): used observational data from nebulae and calculated the slope of the velocity vs. distance linear fit: $V/cr = 0.5 \times 10^{-27} \text{ c.g.s}$ ($V/r \sim 450 \text{ km s}^{-1} \text{Mpc}^{-1}$) which for model A yields $R_A = 2.3 \times 10^{27} \text{cm} = 1.5 \times 10^{14} \text{A.U.}$ and for model B yields $R_B = 2 \times 10^{27} \text{cm} = 1.3 \times 10^{14} \text{A.U.}$

Perhaps the most notable name that readers will find missing from the table and individual descriptions is Arthur Eddington. Eddington took part in this project in important ways that did not include actual "discoveries":

1. He participated in a number of important discussions with most of the authors listed in Table 3.⁴¹
2. He was responsible for the re-publication and translation of Lemaître's 1927 paper in *Monthly Notices of the Royal Astronomical Society* (Lemaître 1931). He initially brought Lemaître's work to the attention of the world in his May 1930 paper (Eddington 1930).

⁴¹See Smith (1982)

3. He published the final list of Slipher's radial velocities (Eddington 1923).
4. By some he is even considered to be the *transition* figure who triggered the major paradigm change from the "static or stationary" model of the universe to an "evolving geometry" (Ellis 1990).⁴²

Overall we find that Lemaître was the first to seek and find a linear relation between distance and velocity *in the context of an expanding universe*, but that a number of other actors (e.g. Carl Wirtz, Ludwik Silberstein, Knut Lundmark, Edwin Hubble, Willem de Sitter) were looking for a relation that fit into the context of de Sitter's Model B world with its *spurious* radial velocities. This is discussed in a number of other papers in this book (see contributions by Harry Nussbaumer, Cormac O'Raiheartaigh, and Ari Belenkiy).

4.1. Slipher's Contribution to the Expanding Universe Story

Slipher's radial velocities played a critical role in *all* of the publications listed in Table 3 above. Lets look at Slipher's data in several of the most important papers in this table.

Lundmark (1925) used Slipher's radial velocity data of spirals to look for a relation between distance and velocity. While he did not cite Slipher's work, he did state on page 866, "Mainly on account of the enthusiastic and skilful work of V. M. Slipher we have now knowledge of 44 radial velocities of spiral nebulae." Why wouldn't Lundmark cite Slipher's work containing the 44 radial velocities he used? In fact Slipher never published his final list of radial velocities, the final list was found in Arthur Eddington's book of 1923 (Eddington 1923).⁴³

Lemaître (1927) also used the radial velocities of Slipher, but Slipher's name did not appear in this paper. Rather he cited the work of Strömberg (1925) as his source. Strömberg (1925) listed 56 velocities obtained from Slipher (it included some globular clusters in addition to spiral nebulae), but stated "Slipher's determinations are given without references. . . ." Strömberg otherwise praised Slipher in his Introduction stating ". . . but through the perseverance of Professor V. M. Slipher, a fairly large number of such velocities has been derived." Perhaps Lemaître could be forgiven as he was mainly a theorist, but it's troubling that he didn't take the time to cite the original sources of his data.

Hubble (1929b) used Slipher's radial velocities for 20 out of 24 objects⁴⁴ listed in his famous Figure 1 showing a "Velocity–Distance Relation among Extra-Galactic Nebulae". Hubble gave no attribution to Slipher in this paper, only stating that "Radial velocities of 46 extra-galactic nebulae are now available. . . ." However, he did give credit to a few people, mentioning that two of the distances listed in his Table 1 were those of Shapley (he gave no citation), three velocities were those of Humason, and with the exception of three measured by himself, the rest of the visual magnitudes listed were "Holetschek's visual magnitude as corrected by Hopmann."

⁴²See page 98, Table 6.1.

⁴³See the contribution by Robert Smith in this book, and Slipher's table of velocities in Eddington (1923) reproduced in Ari Belenkiy's contribution.

⁴⁴The numbers come from Peacock's chapter in this book.

4.2. Hubble Finds his Expanding Universe?

It is commonly believed that Hubble not only discovered an expanding universe, but that he was also looking for it. The former is credible, but the latter is not. Thus far historians have unearthed no evidence that Hubble was searching for the clues to an expanding universe when he published his 1929 paper (Hubble 1929b). Given the timing of events it is difficult to reconcile. There were only a few people with knowledge of a non-static solution to Einstein's equations in 1928-29:

1. Alexander Friedman: passed away in 1925.
2. Yuri Krutkov & Paul Eherenfest who worked to get Friedman's papers published and negotiated with Einstein over their validity (see Belenkiy in this book).
3. Georges Lemaître: his 1927 paper was published in French in an obscure Belgian journal. He sent his paper to at least Einstein, de Sitter and Eddington.
4. Einstein: discussed Lemaître's paper with him at the 1927 Solvay conference,⁴⁵ but told him he did not believe in his solution. For the first time Lemaître also learned from Einstein of the Friedman 1922 and 1924 papers.
5. De Sitter: it is not clear that he ever read Lemaître's 1927 paper prior to 1930.
6. Eddington: appears to have forgotten about Lemaître's 1927 paper until he was reminded of it in early 1930.⁴⁶

It was not until May 1930 when the papers of Eddington (1930) and de Sitter (1930) were published that the rest of the world became aware of the non-static solutions of Lemaître and later the earlier solutions of Friedman.

5. Brightness Profile of Galaxies

Reynolds (1913) was perhaps the first to attempt the measurement of the light profile of the Andromeda Galaxy, but only across the bulge, not out to the spiral arms. His careful measurements yielded:

$$Luminosity = \frac{Constant}{(x + D)^2} \quad (2)$$

where x = distance from the center of the nucleus/bulge along the major axis (out to a diameter of $7'$).

Seven years later Reynolds (1927) went after more nuclei in a number of spiral nebulae (M65, M99, M100, M94, and M64) but with mixed success at applying Eq. 2.

Three years later Hubble (1930c) describes a "Distribution of Luminosity in Elliptical Nebulae." In this particular case Hubble *does* cite the Reynolds (1913, 1927) papers, but perhaps since Reynolds was focused on the bulges of spiral nebulae rather

⁴⁵See Belenkiy's chapter in this book.

⁴⁶See Nussbaumer & Bieri (2009), Chapter 11.

than Ellipticals Hubble didn't feel the need to ignore his competitor. Regardless, Hubble later generalized the relation for Ellipticals as:

$$I = \frac{I_o}{\left(\frac{r}{a} + I\right)^2} \quad (3)$$

Hubble (1930c, p. 133) also gives credit where credit is due:

The pioneer investigations along this line are due to J. H. Reynolds, who, in 1913 found that the luminosity along the major axis of M 31, out to r' from the nucleus, could be represented by the formula $L = \text{Constant}/(x+I)^2$.

Here we have a (unique?) case where Hubble has properly cited and praised his predecessor. This relation is now referred to as the Hubble Luminosity Profile, but perhaps it would be more properly named the Reynolds–Hubble Luminosity Profile.

In the end it is perhaps not so relevant as there are a number of other far more popular profiles in use today including the de Vaucouleurs (1948), King (1962) and Sérsic (1963) profiles.

6. Combing Through the Literature

It may be possible to better quantify Hubble's unwillingness to cite his predecessors by examining the literature more closely. Most bibliographies in the 1920s were not compiled at the back of each article as it is done today. A work was cited by the person's name and the citation would be contained in a footnote at the bottom of the same page. For this reason the SAO/NASA Astrophysical Data Service (ADS) does not have complete bibliographies for the papers of the period of interest. This author took all of the scientific publications of Hubble from 1920 through 1930 and attempted to put together a pseudo-bibliography for each paper. Thankfully Hubble did not write that many papers in comparison with Lundmark (see Appendices A and B). A pseudo-bibliography here means that if someone's name was mentioned in the context of a previous publication but no bibliographic information was included it was included as a citation. Full citations are also included in the counting.

Knut Lundmark was chosen as a comparative figure to Hubble as he was probably considered by his peers to be a figure of equal stature during the 1920s⁴⁷. Lundmark wrote many more papers than Hubble, but ADS does not have a complete set of the papers he wrote during this period of time. A handwritten book was obtained⁴⁸ from the Uppsala University Library that contained a listing of all of Lundmark's papers in his career (even it is not complete, but more so than ADS). Some of the papers missing from ADS found in this book are included in Appendix B. Papers were not included that were considered popular science.⁴⁹ The recording of citations in Lundmark's papers were approached in the same manner as for Hubble.

⁴⁷This was more so in the beginning of the 1920s, less so in the latter after Hubble's many discoveries.

⁴⁸The author is unknown, hence there is no citation for it.

⁴⁹Lundmark wrote at least 35 articles for the Swedish magazine *Populär Astronomisk Tidskrift*, none of which are presently in ADS (There is an effort to make it so). Some of the articles are on historical figures such as Tycho Brahe, obituaries, and discoveries or reports from meetings. He also wrote several popular science books and encyclopedia articles that are not included.

To ensure these are relatively comparable figures ADS was used to see how often other authors mentioned Lundmark and Hubble in their articles. Using the ADS Labs Fulltext Service⁵⁰ Statistics were compiled on how many times authors mentioned Lundmark and Hubble by name from 1920 through 1930. Astronomers did not consistently include a full citation to other authors' works, but often only referenced the author's name. All papers have been eliminated where Lundmark or Hubble have cited/mentioned themselves.⁵¹ From Appendices A and B, it is apparent that Lundmark did not simply fill up his papers with citations for the sake of doing so, but because he had a broad knowledge of the scientific literature in his field of study. This is confirmed by Holmberg (1999, p. 130):

He had read widely and perhaps knew the astronomical literature better than most astronomers...

While Hubble was inconsistent in his citations, this inconsistency was not necessarily reflected by the status of the individual he did or did not cite. To attempt to properly quantify these kinds of tendencies would require a much larger and sophisticated effort than that provided herein; nonetheless, it is clear from Figure 1 that in the early half of the 1920s it is Lundmark who is "cited" more frequently, while in the latter half it is Hubble. This is not surprising given not only the facilities with which Hubble was able to conduct his research (including the largest aperture telescope in the world at Mt. Wilson), but also Hubble's success at promoting himself and Mt. Wilson as described above.

Figure 2 shows how often Lundmark and Hubble cited other authors. Unfortunately there is no way to accomplish this within ADS at present.⁵² To obtain these numbers was difficult and required reading through every paper and counting the number of actual citations, not just names, to authors.⁵³ Figure 2 shows a clear trend in that Lundmark cites authors almost twice as frequently as Hubble. If one allows all of Lundmark's publications (Hubble's longest was 65 pages, while Lundmark's has 3 over 100 including one over 250) Lundmark is still over a factor of two higher in citation rate per page.

A more specific comparison can be made by examining two papers by Hubble and one by Lundmark (Figure 3). The first one of Hubble was 51 pages in length (Hubble 1926a); not including pages with tables or photos taking up a full page it is only 41. A second Hubble paper (Hubble 1929c) is 63 pages in length; without full page tables/photos only 41. For Lundmark a paper 195 pages in length was chosen (Lundmark 1927e) (91 eliminating full pages with tables/photos). In both Hubble and Lundmark's papers the number of author names per paper was counted (not including

⁵⁰Currently residing at <http://labs.adsabs.harvard.edu/fulltext>

⁵¹There are certain to be a number of complaints about this methodology. For example, perhaps Lundmark simply likes to include lots of citations, or Hubble only cites "big shots", etc. Those and others are certainly valid complaints, but they should not distract one from making an attempt.

⁵²The author is currently working with leading text data mining researchers to make this possible in the future.

⁵³By citation we are being flexible in that a citation can simply refer to an author's work by name (without a journal reference), but they can only be "cited" once per paper (this is sometimes difficult to discern).

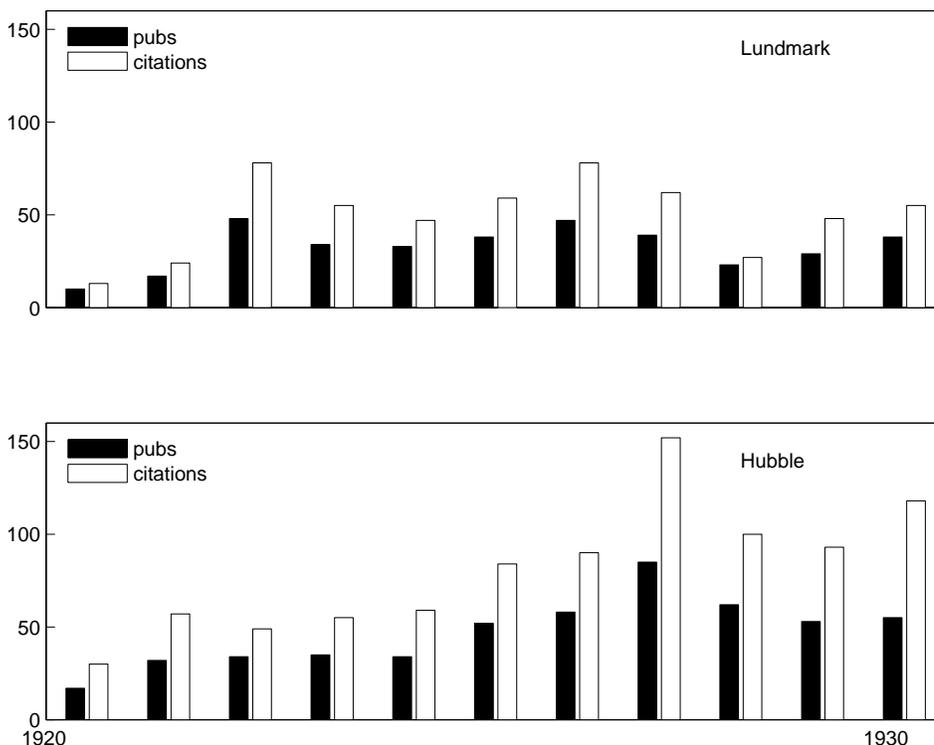


Figure 1. Top: Black are the number of publications 1920 through 1930 that cite Knut Lundmark per year, white are the total number of citations per year. Bottom: The same for Hubble.

their own names).⁵⁴ In the two Hubble papers there were a total of 66 and 75 names mentioned (many repeated of course), which comes to 1.61 and 1.83 names per page. For Lundmark's paper there were 423 names mentioned which comes to 4.65 names per page.

Of course the raw numbers as presented in Figure 3 should be renormalized to the number of pages, but one can see that even dividing Lundmark's numbers by a little over a factor of two would reveal that he still mentioned his colleagues much more frequently than Hubble. It should also be obvious to the knowledgeable reader that all of the more highly-cited names shown are the expected authors in this particular domain.

One could also extend this type of study in numerous directions to better quantify these effects. For example, it could be interesting to compare how often authors cite others who their work explicitly relies upon. Do the works on classification of Herschel, Reynolds, or Wolf show up in the classification works of Lundmark or Hubble? We know from above that Reynolds (1920) was never found in any of Hubble's work, but what about the others who would have influenced Hubble?

⁵⁴This is a looser criterion from that above where only *citation* related names were compiled.

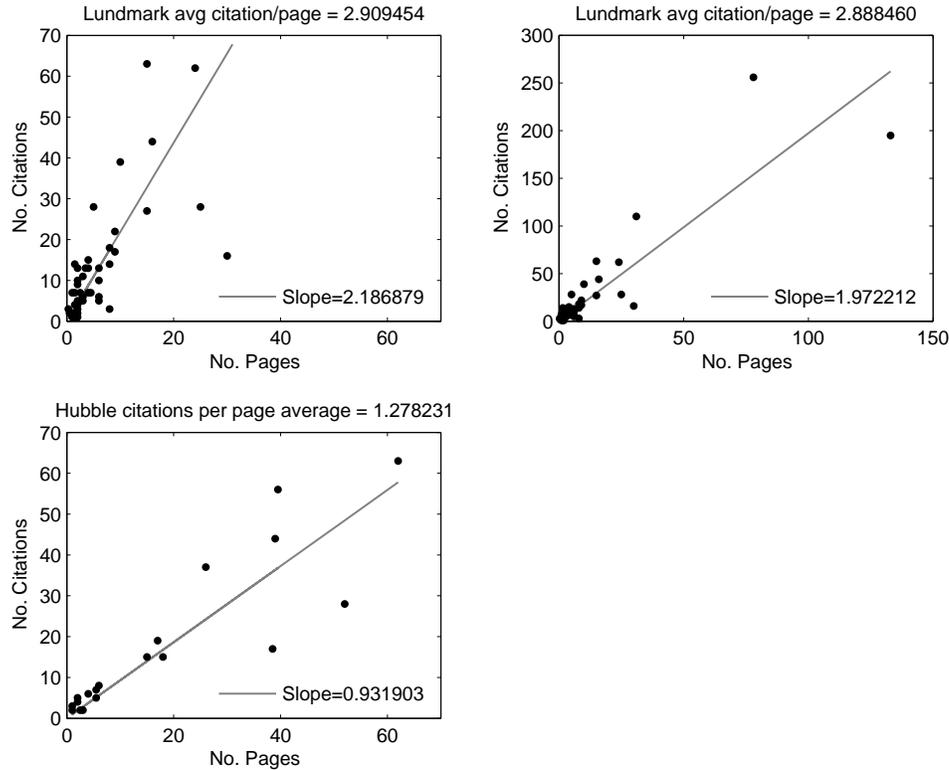


Figure 2. Top Left: Citations per page in Lundmark 1920–1930 publications of less than 70 pages. Top right: Citation per page in all Lundmark 1920–1930 publications. Bottom left: Hubble citations per page in all publications 1920–1930.

7. The Making of Mythic Heroes

It would be inappropriate to suggest that Hubble was irrelevant to the history astronomy, but it is clear that many of the advances discussed above would have happened within a short period of time of their original discovery even if Hubble had never worked as an astronomer.⁵⁵ This may be contrary to the assertion by Smith (2009, p. 72) that technological determinism alone does not explain Hubble’s discoveries. Indeed, technological determinism, the belief that such discoveries were inevitable given facilities such as the large telescopes at Mt. Wilson, is not needed. Instead one may consider historical determinism “lite.” Clearly there were many astronomers working in these topics and steady progress was being made in each field. If Hubble had not found Cepheids in spiral nebulae in 1924/25, then someone else surely would have within a few years.

⁵⁵ As a (distant) analogy let’s consider the accomplishments of Alexander the Great. Were his accomplishments inevitable because the Macedonian state was ready for a strong ruler after the death of his father Philip? Wasn’t the Macedonian version of the Greek phalanx with its longer spear and the high level of training required to master this technology in place well before Alexander? If one answers yes to these things, then perhaps another “strong man” could have come to the helm of the state and accomplished much the same as Alexander. Or if Philip had lived he surely would have attempted many of the same things that Alexander attempted.

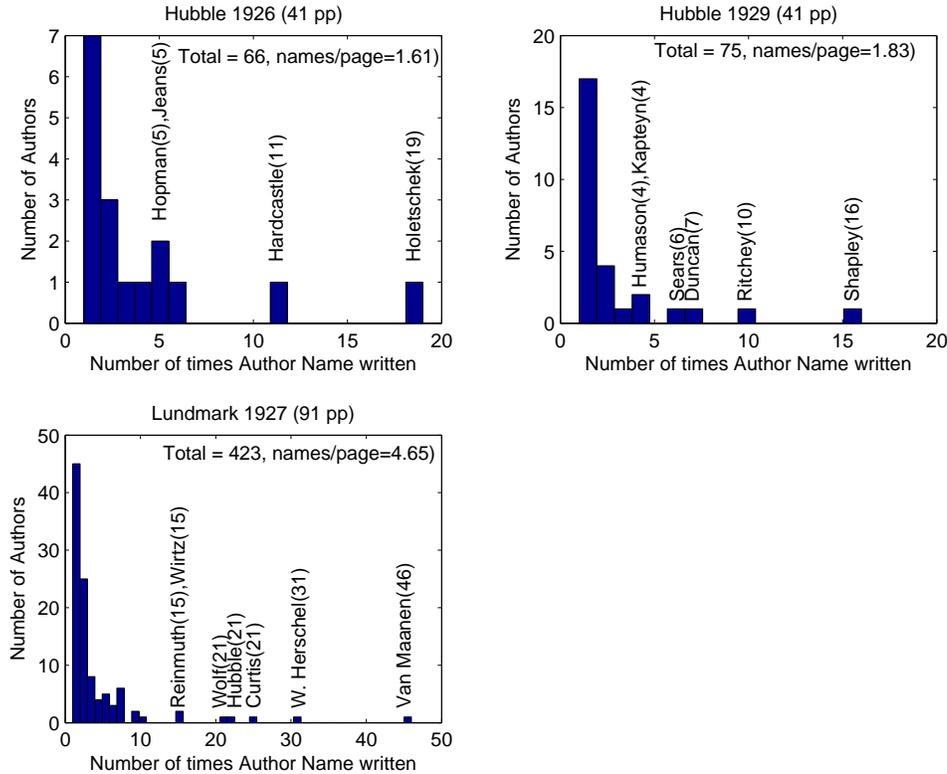


Figure 3. Top Left: Number of author names (including duplicates) found in (Hubble 1926a). Top Right: same for (Hubble 1929c). Lower Left: Same for (Lundmark 1927e). Note that all of the scales on the y-axes are distinct, as are the x-axes between the top two and bottom plot.

Lundmark's classification scheme, or the earlier scheme of Reynolds for spirals, could have easily replaced Hubble's at some level and the linear distance-velocity relation had already been postulated by Lemaître in 1927. Without Hubble it is clear that within a short period of time someone else would have been given credit for each of these initial discoveries, Stigler's Law of Eponymy notwithstanding (Merton 1965; Stigler 1980).⁵⁶ Hubble's success in gaining credit for his classification scheme and linear distance-velocity relation may be related to his verification of the Island Universe hypothesis – after the latter, his prominence as a major player in astronomy was affirmed. As pointed out by Merton (1968) credit for simultaneous (or nearly so) discoveries is usually given to eminent scientists over lesser-known ones.⁵⁷

One may also consider the competition between the Lick, Lowell and Mt. Wilson Observatories in early observational cosmology. Because Lowell Observatory did not

⁵⁶The most famous quote from Stigler's paper is: "No scientific discovery is named after its discoverer."

⁵⁷This has been termed "The Matthew Effect."

enjoy a high level of esteem,⁵⁸ it may have taken Slipher more effort than other professional astronomers to convince the community of the validity of his initial discoveries (see contributions by Joseph S. Tenn, Robert Smith and others in this book). In addition, Slipher had a modest personality and was not given to boasting or promoting his accomplishments in public. On the other hand, consider Lundmark or Wirtz who did not have regular access to telescopes, instrumentation and support facilities like that of Lick (Clark 36-in [1888] and Crossley 36-in [1905]), Mt. Wilson (60-in [1908] and Hooker 100-in [1917]), or Yerkes (40-in [1895]).⁵⁹ Did Lundmark and Wirtz have to write more cogent and highly interesting/readable papers to get the community to follow their research in a “competition for attention” (Collins 1975, p. 480)? If so, then we know that Lundmark succeeded at some level because his 1920 thesis (Lundmark 1920) was read by astronomers such as Shapley and van Maanen before he had even arrived at Mt. Wilson and Lick Observatories (Holmberg 1999, p. 94). In fact, Lundmark’s work was so highly regarded that a “Memorial Volume” was edited by Martin Johnson (1961) and published three years after Lundmark’s death. It included contributions by a number of highly regarded astronomers such as Milton Humason (p. 26), Harlow Shapley (p. 37), Boris Vorontsov-Velyaminov (p. 43), Fritz Zwicky (p. 55) and Gustaf Strömberg (p. 95).

Of course language may have also played a role. None of Wirtz’s major works were published in English,⁶⁰ whereas Lundmark wrote mainly in English language journals from around 1920.⁶¹ Is it possible that an astronomer like Hubble coming from the premier observatory didn’t need to be as explicit in his citing of previous work because people *had* to read and utilize his results regardless of the quality of the background scholarship?

There is also the issue of the relative decline of research activities at smaller institutions that took place in the U.S.A. (and Europe) around the turn of the century given that the latest observational astronomy research required large amounts of capital (Lankford 1997, Chapter 7). As well, the First World War did not do anything positive for the facilities at European institutions in this sense. Simply consider Shapley’s move from Mt. Wilson to Harvard around 1925. Clearly he knew he was giving up access to one of the finest observatories in the world to work at an institution with inferior equipment, although as director of Harvard Observatory he was to obtain sufficient research funding for otherwise large-scale projects.

One should take account of these changes in order to better quantify how the attitudes of scientists working at the premier institutions could have changed and how

⁵⁸The poor reputation was initially due to Percival Lowell’s claim to have discovered canals engineered by intelligent beings on Mars.

⁵⁹Lundmark did visit and observe at Mt. Wilson before his falling out with Hubble over their classification schemes. He also spent time at Lick and Mt. Wilson over a period of two years (1921-22) thanks to the Sweden-America foundation (Holmberg 1999, p. 94). At that time he was able to borrow plates from Lick Observatory. In 1929 and in 1932 he again visited several American observatories (including Mt. Wilson) to utilize their plate collections for his Lund General Catalogue project (Ibid., pp. 109-116).

⁶⁰Much of observational cosmology research was being produced at American observatories, while much of the theory was being promoted by Eddington, de Sitter and others in English language publications in Europe during the trying economic times after World War I.

⁶¹Lundmark also published in Swedish in popular science publications and in English and Swedish in journals like *Arkiv för Matematik, Astronomi och Fysik*, and *Lund or Uppsala Observatory Reports*.

credit for discoveries was subsequently awarded. While it may be a stretch to quote from William Pitt that “Unlimited power is apt to corrupt the minds of those who possess it.” One should not rule out such effects on the minds of successful scientists. Aggression and competition surely play some kind of role – a role which sociologists have already explored in a number of scientific contexts, including the “necessity” to defend one’s position and ideas (Lankford 1997, p. 187). Collins (1975, p. 482) has also suggested that as scientists move into higher status positions they may take on more aggressive roles. Certainly we have seen aspects of this behavior in Hubble in his footnote dispute with Lundmark (also see Chapter 11 in Christianson (1996)), and his persistent tendency to defend his discoveries as sole achievements of himself and Mt. Wilson.⁶²

It has been argued (see Kragh & Smith 2003) that much of Hubble’s fame at-large came after his death in 1953.⁶³ This retrospective view of Hubble’s accomplishments would certainly fit in with the well-known hypothesis of Thomas Kuhn (1962) that:

There is a persistent tendency to make the history of science look linear or cumulative, a tendency that even affects scientists looking back at their own research.

Of course, a similar analysis could apply to many scientific discoveries of importance in the early 20th century. An outstanding example is Henrietta Levitt’s discovery of a period–luminosity relation for Cepheid variable stars and the number of scientists it took to place it on a reliable and accurate footing.

8. Conclusion

Can one say anything definitive about the credit that Hubble has received for the seminal discoveries discussed herein, and his lack of acknowledgement of the work of others? At the present time it does not seem possible to quantify these observations. The line of research presented in this work is only an initial attempt. With new text data mining technologies growing in strength one should be able to better quantify some of the assertions above in the near future. Given what is known today it would not be fair to suggest that Hubble was a Lavoisier-like figure who regularly claimed the discoveries of others as his own (Butterfield 1959, pp. 206-9), but that he was inconsistent in awarding credit.

Future researchers are certain to mine the literature in detail to examine how major scientists cited (or not) their colleagues, but will this influence the writers of today? Perhaps this could be accomplished by demonstrating that over the long-term one may be discredited for neglecting to cite relevant work that one relied upon.

Take two recent books from the 2000s that were highly read in the scientific community: Stephen Wolfram’s book (Wolfram 2002) and that of Roger Penrose (Penrose 2005). Wolfram’s book contains almost no citations to other work, while Penrose makes

⁶²Hubble wrote to de Sitter in 1930 (Hubble 1930b) in response to de Sitter’s recent publication (de Sitter 1930) of a velocity-distance relation and his lack of sufficient credit to Hubble: “I consider the velocity-distance relation, its formulation, testing and confirmation, as a Mount Wilson contribution and I am deeply concerned in its recognition as such.”

⁶³Although in 1934 Lundmark was already lamenting a wave of “Hubbleianism” (Holmberg 1999, p. 100)

a valiant attempt to cite others for a book even broader in scope than Wolfram's. Needless to say Wolfram was pilloried in the scientific and popular press for this lack of attribution and general belief that his ideas have broader application than possible (e.g. Casti 2002; Hayes 2002; Economist 2002). Perhaps the general public agrees as well: Wolfram has received 3 stars out of 5 (from 344 reviews) on Amazon.com as of 2012/12/05 (with a large number of 1 stars (102) and 2 stars (61). Penrose received 4 out of 5 (from 204 reviews) with very few 1 (12) or 2 (12) stars. Penrose's bibliography is 30 pages in length (pp. 1050–1080) and has received rather more favorable reviews (e.g. Johnson 2005; Blank 2006).

How credit is awarded for a discovery is often a complex issue and should not be oversimplified – yet this happens time and again. Another well-known example in this field is the discovery of the Cosmic Microwave Background (see Alpher & Herman (1988); Gribbin (1998); Kragh (1999)).

The problem is larger than awarding credit within a given field as outsiders pick anecdotal stories from astronomy, and not only get them wrong, but oversimplify them.⁶⁴ This can also happen in the case of professional astronomers/physicists (see Greene 2011).⁶⁵ One only needs to read a smattering of the contributions in this book to understand how misguided Greene was in his assumptions. Perhaps, as a community, astronomers can learn to do better and this book could be the beginning, at least in this particular domain.

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This work has also benefited from comments by Bengt Edvardsson, John Peacock, and Jeffrey Scargle and my excellent co-Editor Deidre Hunter.

⁶⁴A recent example would be Taleb (2010) in his *Inadvertent Discoveries* subsection of Chapter 11 (on page 168 of the paperback edition) where he discussed the discovery of the Cosmic Microwave Background Radiation.

⁶⁵To take one of many mis-statements in this piece: "In 1929, the American astronomer Edwin Hubble discovered that distant galaxies are all rushing away from us."

A. Hubble's published papers 1 Jan. 1920 through 31 Dec. 1930

No.	Reference	Pages	Citations	Notes ^a
1	Seares & Hubble (1920a)	15.0	15	+
2	Hubble (1920)	18.0	15	+
3	Seares & Hubble (1920b)	–	–	See ref. 1
4	Hubble (1921)	–	–	N/A not Hubble's work?
5	Hubble (1922b)	39.0	44	+
6	Hubble (1922c)	38.5	17	+
7	Hubble (1922a)	–	–	See ref. 5
8	Hubble & Lundmark (1922a)	2.0	4	+
9	Hubble (1922d)	–	–	See ref. 6
10	Hubble (1923a)	1.0	3	*
11	Hubble (1923b)	2.5	2	–
12	Hubble (1925d)	26.0	37	!
13	Hubble (1925a)	3.0	2	–
14	Hubble (1925b)	–	–	See ref. 13
15	Hubble (1925c)	–	–	See ref. 12
16	Hubble (1926c)	39.5	56	+
17	Hubble (1926b)	–	–	See ref. 17
18	Hubble (1926d)	52.0	28	–
19	Hubble (1926a)	–	–	See ref. 16
20	Hubble (1926e)	2.0	5	*
21	Hubble (1927e)	–	–	N/A Popular Article
22	Hubble & Duncan (1927a)	5.5	7	–
23	Hubble (1927a)	–	–	See ref. 34
24	Hubble & Humason (1927)	–	–	N/A written in 1934?
25	Hubble (1927d)	–	–	N/A written in 1936?
26	Hubble (1927c)	–	–	See ref. 10
27	Hubble (1927b)	3.0	2	–
28	Hubble & Duncan (1927b)	–	–	See ref. 22
29	Hubble (1927f)	5.5	5	+
30	Hubble (1928)	4.0	6	*
31	Hubble (1929a)	–	–	N/A Article "excerpt"
32	Hubble (1929d)	–	–	See ref. 33
33	Hubble (1929c)	62.0	63	!
34	Hubble (1929b)	6.0	8	+
35	Hubble (1930c)	–	–	See ref. 37
36	Hubble & Duncan (1930)	1.0	2	+
37	Hubble (1930a)	17.0	19	+

^a* No journal/book citations found, only mentions names.

+ At least one "reference" but without any citation, otherwise one or more normal citations.

! Includes +/* and/or reference to list of coordinates.

? Cannot obtain this reference.

B. Lundmark's published papers 1 Jan. 1920 through 31 Dec. 1930

No.	Reference	Pages	Citations	Notes ^a
1	Lundmark (1920)	78.0	256	–
2	Lundmark (1921a)	2.0	10	–
3	Lundmark (1921b)	2.0	10	–
4	Lundmark (1921e)	1.5	1	–
5	Lundmark (1921d)	15.0	27	!
6	Lundmark (1921g)	1.5	7	*
7	Lundmark (1921c)	3.0	11	!
8	Lundmark (1921f)	3.0	5	*
9	Lundmark (1921h)	4.0	15	!
10	Lundmark (1922i)	3.5	13	–
11	Lundmark & Luyten (1922d)	4.0	13	+
12	Lundmark (1922h)	2.0	1	*
13	Lundmark (1922l)	1.0	2	*
14	Lundmark (1922b)	1.0	7	*
15	Lundmark (1922j)	10.0	39	!
16	Lundmark (1922f)	0.5	2	*
17	Lundmark (1922c)	8.0	18	+!
18	Lundmark & Luyten (1922b)	2.0	13	+!
19	Lundmark & Luyten (1922a)	16.0	44	–
20	Lundmark (1922k)	9.0	22	+
21	Lundmark (1922a)	9.0	17	+
22	Lundmark (1922g)	5.0	28	+
23	Lundmark (1922d)	1.5	14	*+!
24	Lundmark & Luyten (1922c)	0.3	3	–
25	Hubble & Lundmark (1922b)	2.0	4	+
26	Lundmark (1922e)	8.0	3	–
27	Lundmark & Luyten (1923b)	2.0	2	–
28	Lundmark & Luyten (1923e)	2.0	9	+!
29	Lundmark & Luyten (1923d)	3.0	6	+!
30	Lundmark (1923b)	25.0	28	–
31	Lundmark & Luyten (1923a)	4.5	7	+
32	Lundmark & Luyten (1923c)	2.5	5	+
33	Lundmark (1923a)	2.0	4	+
34	Lundmark (1924b)	24.0	62	+
35	Lundmark (1924c)	4.0	7	+
36	Lundmark (1924a)	2.5	7	+
37	Lundmark (1925)	31.0	110	+!
38	Lundmark (1926d)	6.0	6	+
39	Lundmark (1926e)	–	–	?
40	Lundmark (1926c)	–	–	See ref. 38
41	Lundmark (1926a)	2.0	5	*
42	Lundmark (1926b)	6.0	13	+
43	Lundmark (1927a)	6.0	5	–
44	Lundmark (1927d)	–	–	?
45	Lundmark (1927b)	–	–	?

No.	Reference	Pages	Citations	Notes ^a
46	Lundmark & Luyten (1927b)	–	–	?
47	Lundmark (1927c)	15.0	63	+
48	Lundmark & Luyten (1927a)	2.0	3	+
49	Lundmark & Luyten (1927c)	2.0	5	+
50	Lundmark (1927e)	133.0	195	!
51	Lundmark (1928c)	–	–	?
52	Lundmark (1928a)	6.0	10	+
53	Lundmark & Ark (1928)	8.0	14	+
54	Lundmark (1928b)	–	–	?
55	Lundmark (1930d)	1.5	4	*
56	Lundmark (1930b)	1.0	1	*
57	Lundmark (1930c)	30.0	16	*
58	Lundmark (1930a)	3.0	5	*

^a* No journal/book citations found, only mentions names.

+ At least one "reference" but without any citation, otherwise one or more normal citations.

! Includes +/* and/or reference to list of coordinates.

? Could not obtain this reference.

C. Lundmark's reply to Hubble May 1927

Lundmark's reply to Hubble's accusation of plagiarism was presented as a paper to the Royal Society of Science of Upsala on May 6, 1927 and later published in the *New Proceedings of the Royal Society of Science of Upsala* ⁶⁶ and provides a unique look at how at least one of Hubble's peers viewed him at that time. I produce this quote (verbatim) here since this volume was, until recently, very difficult to obtain.

"In his paper, Extragalactic nebulae, *Aph. J.* 64:321, 1926, E. P. Hubble makes an attack on me which is written in such a tone that I hesitate to give any answer at all. Still, I may take the occasion to state a few facts.

I was present at the Cambridge meeting of the Astronomical Union.

I was not then a member of the Commission of Nebulae.

I did not have any, access whatsoever to the memorandum or to other writings of E. P. Hubble, neither did I have access to the report of nebulae (which does not give details of Hubble's classification) until at the end of the meeting, neither did I recognize until I obtained a letter from Hubble at the end of 1926 that he had made another classification of nebulae than the one published in his paper, *A general study of the Diffuse Galactic Nebulae*, *Mt Wils. Contr. No. 241*, 1922.

As much as I heard of the discussion in the committee of nebulae the only question was if the terms »galactic» and »extragalactic nebulae» should be accepted, From the discussion I got the impression that the intention of Hubble was to force through his nomenclature, One of the members

⁶⁶Nova Acta Regiae Societatis Scientiarum Upsaliensis, Volumen Extra Ordinem Editum 1927

told me outside the discussion that Hubble had suggested the subdivision »logarithmic spirals» but I did not understand that this suggestion was given in any memorandum to the Union, Now when reading Hubble's paper I am glad to note that he seems not to have carried out the unhappy idea introducing the term »logarithmic spirals» Slight changes in his classification might have been introduced since the Cambridge meeting.

Hubble's statement that my classification except for nomenclature is practically identical with the one submitted by him is *not correct*. Hubble classifies his subgroups according to eccentricity or form of the spirals or degree of development while I use the degree of concentration towards the centre. As to the three main groups, elliptical, spiral and magellanic nebulae it may be of interest to note that the two first are slightly older than Hubble and myself. The term elliptical nebulae thus is used by Alexander in 1852 and the term spiral by Rosse in 1845; The importance of the magellanic group has been pointed out by myself Observatory 47, 277, 1924 earlier than by Hubble. *As to Hubble's way of acknowledging his predecessors I have no reason to enter upon this question here.*" (the latter is our emphasis).

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