THE MYSTERIOUS NEBULAE, 1610–1924*

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ABSTRACT

The history of our knowledge of nebulae is traced from the earliest visual observations to the recognition that spiral nebulae were indeed “island universes.”

On the night of August 28, 1758, while observing the comet of that year in the constellation Taurus, a 28-year-old apprentice astronomer atop the Tour de Cluny in Paris glimpsed what appeared to be a second, equally bright comet about five degrees farther south. His pulse must have quickened, for he had not yet found a comet of his own, and such a discovery would have brought recognition and perhaps a promotion. It was already after midnight, but no doubt he waited until dawn, anxiously hoping for the telltale movement that would designate a comet. We can imagine the high anticipation with which the young Charles Messier waited for the next night, and the crushing disappointment when his first comet turned out to be a mere nebula.

A mere nebula indeed! Little could he know that his nebula would come to be far more fascinating than all of the comets he had ever discovered. “A whitish light, elongated like the flame of a candle, without any stars,” he described it; only later did it get the designation M1 from his catalogue, and much later the name “Crab Nebula.”

Within a year Messier had found his own comet; fame and promotions did follow, and eventually, in 1770, he gained the coveted election into the Académie Royale des Sciences in Paris. And what could be more fitting as his first major memoir for the Academy publications than a catalogue of his nebulae and clusters, starting with the one he had found that summer night in 1758? He rounded up about forty troublemakers, and then topped off his list with well-known objects such as the Orion Nebula and Praesepe for a total of 45. In the years following he added one and then a second supplement to his original list, bringing the total to 103.

In our century, Messier’s list is a guide to showpieces for professional astronomers and a delight to amateurs, and in fact, I began my own career as an amateur astronomer sweeping the sky for these sparkling star clusters and faint wispy nebulae. And so it was that one night in July of 1948 I stumbled on a nebula that moved! With quickened pulse I watched in my 8-inch reflector long enough to be

sure it was a comet, and then dashed off a telegram to Harvard College Observatory, the Western-hemisphere clearing house for comet discoveries. A week later I got back a postcard saying, "Thank you for your observation of Comet Honda."

In the long run, however, I would have to say that my investment in the telegram paid off, because one thing led to another and in 1949 I got a job as a summer assistant to Harlow Shapley, who was then director of Harvard College Observatory. In the dusty book stacks at H.C.O., I found all the original publications of Messier and his contemporaries, and there at H.C.O. also (a few years later) I first met Helen Sawyer Hogg. Indeed, it did not take too long to discover among those dusty almanacs and memoirs that I was close on her trail. Many of these long-forgotten antique reports of nebulae she had already brought to light in her wonderful series for this journal called "Out of Old Books," which ran from 1946 to 1966. I took courage in hand and wrote to ask for her Bibliography of Individual Globular Clusters, which was rich in citations of early literature. Distribution of the bibliography was rather restricted, she responded, but in special cases like this it was available, and with that vote of confidence she sped me on my way to becoming a professional astronomer, and ultimately, a professional historian of astronomy.

I was fascinated with the old books still found on open shelves at Harvard Observatory in those days, and I pursued Messier with a particular passion. My French was then singularly primitive, and Helen generously gave me a long translation that substantially aided my researches. One of the interesting games was to discover the patterns of nebular discovery in the 18th century. It was clear that Messier's colleague Pierre Méchain was the first to appreciate the richness of the Virgo region of galaxies, and that Méchain had found a number of faint nebulae that Messier never got around to adding to his own list. These latter discoveries were described in a letter by Méchain published in Bode's Astronomisches Jahrbuch for 1786. Dr. Hogg had identified three of them, and now they are sometimes listed as M 105, M 106 and M 107. I found that it was not too difficult to identify the other two, but I have always been a little uneasy by designations such as Messier 105 or Messier 109, since we have no specific evidence that Messier ever examined those particular nebulae. It would perhaps be more appropriate to call them Méchain 105 or Méchain 109, but even that would be rewriting history since Méchain never made any attempt to number them systematically.

Except for a large number of nebulae near the end of his catalogue, Messier was apparently the independent discoverer of the objects in it. However, he did report a certain number of earlier sightings. He found his first object, M 1, depicted on an atlas authored by John Bevis in England. Bevis had bad luck with his Uranographia Britannica, for after the plates were made but before even a title page had been set, around 1750, his printer went bankrupt. Apparently Bevis had a number
of proof copies that had been struck from the plates, one of which Messier eventually saw. (Complete sets are now quite rare – fewer than 20 are known to exist.) Messier does not mention Bevis in the 1783 supplement to his catalogue, but in the revised and extended edition of the following year he does include the information that Bevis had found the nebula in the horn of the Bull in 1731. Except for this printed note of Messier’s, the 1731 date is completely undocumented. Since Bevis had died in 1771, Messier clearly did not get the information directly from the discoverer, and the source of that date remains a mystery, although, in the absence of a standard title page, Messier might have for some reason believed (erroneously) that 1731 was the date of the atlas.

In the popular literature, Messier is always referred to as a comet hunter who catalogued the nebulae primarily as nuisances to his primary goal. His original memoir of 1771 contains nary a hint of this, and certainly gives the impression that he was examining the nebulae for their own intrinsic interest. However, I think his true colours are revealed by a passage that I found in another of the old almanacs at Harvard, the *Connaissance des Tems* for 1801. (Note the rationalized spelling of the revolutionary period.) There he wrote:

> What caused me to undertake the catalogue was the nebula I discovered above the southern horn of Taurus while examining the comet [of 1758] . . . This nebula had such a resemblance to the comet, in its form and brightness, that it caused me to search for others, so that astronomers would not confuse these same nebulae with faint comets. I observed more of them with telescopes suitable for comet hunting, and this was the intention I had in forming my catalogue. After me the celebrated Herschel published in the *Philosophical Transactions* for 1786 and 1789 a catalog of 2,000 that he has observed. This unveiling of the sky was made with instruments of great power, which are not useful for sweeping the sky in a search for comets just beginning to appear. Thus, my object was different from his.

In contrast, three decades earlier Messier remarked that each time he had observed a nebula he had made a careful drawing, “so that in the future it would be useful for observing them to see if they had been subject to any changes.” Such a motivation placed him onto the cutting edge of speculative 18th-century sidereal astronomy, for the possible evolution of nebulae was an emerging question, and one that remains of interest to this day. That Messier did nothing more with it undoubtedly reflects the observational difficulty of finding changes in the nebulae as well as his distaste for theoretical or mathematical astronomy.

Nevertheless, the idea of nebular evolution arches over the entire history of telescopic observations. Recently there has even been speculation concerning possible observed changes in the Orion Nebula since the time of Galileo; I would like to argue that the case for a marked increase in brightness early in the 17th century is built on quicksand. The grounds for these (spurious) claims go back to 1610, when Galileo was first examining the starry realms with his newly developed
CONNOISSANCE DES TEMPS,
Pour l'Année Bissextile 1788.

PUBLIÉE
Par l'ordre de l'Académie Royale des Sciences,
Et calculée
Par M. Méchain, de la même Académie.

PARIS,
DE L'IMPRIMERIE ROYALE.
M. DCCLXXXV.

ASSOCIÉS ORDINAIRES:
Pour la Géométrie.

1772. M. Cousin, Lecteur royal en Physique, au Collège royal.


1785. M. Charles, rue des Bourdonnais, Manufacture royale de Tours.

Pour l'Astronomie.


1777. M. le Comte de Cassini, de l'Institut de Bologne, à l'Observatoire.

1785. M. le Paute d'Agelet, Professeur royal de Mathématiques, à l'École royale militaire.

relle du Jardin du Roi; Associé de l'Académie royale des Sciences & Belles-lettres de Marseille, rue Massaré, N° 41.

Fig. 1—The title page of the Harvard College Observatory copy of the Connaissance des Temps for 1788 bearing the circular bookplate inscribed with Messier's name, and a page showing where he has updated his own list of honours.
The Mysterious Nebulae, 1610–1924

refractor. Early that year he mapped the sword of Orion, without recording any nebulosity, at least as far as the published diagram in his *Sidereus nuncius* is concerned. Does this mean that there was no milky nebulosity to be seen?

Here we must look carefully at the context of his published chart. The title page of Galileo’s report proclaims (in part),

THE STARRY MESSENGER, revealing great, unusual, and marvellous sights, observed by Galileo Galilei, with the aid of a spyglass newly invented by him, on the face of the moon, the innumerable fixed stars, the Milky Way, the nebulous stars, and especially the four planets revolving at different distances around Jupiter…

These discoveries had fallen very quickly into Galileo’s hands after his first recorded observation of the moon on November 30, 1609 and the sequence of Jovian observations beginning January 7, 1610. He readied his small booklet for the printer in February and early March, and apparently decided as an afterthought to expand the section on the stars, for these observations come on two unnumbered leaves inserted between 16 and 17.

In this section Galileo writes,

I have observed the essence or substance of the Milky Way, with an ocular certainty to resolve all the disputes that have vexed philosophers throughout so many ages: the Galaxy is nothing but a congeries of innumerable stars packed together in clusters. And not only in the Milky Way are these whitish clouds seen, but several patches shine here and there in the aether with similar faint colour, and if a telescope is turned on any of them, we are confronted with a closely packed mass of stars. And what is even more surprising, the stars which astronomers have up till now called nebulous turn out to be groups of small stars wonderfully arranged.

This, then, is another discovery worthy of note on the book’s title page.

To clinch his point more specifically, Galileo draws in detail two regions that have been called “nebulous” both by Ptolemy and by Copernicus in their star catalogues. One is *Praesepe* in Cancer, the other is the head of Orion, and both are well resolved, into 38 and 21 stars respectively.

But Galileo also diagrams in rich detail the belt and sword regions of Orion, without the slightest indication of any milky nebulosity in the sword. Why not? Had Galileo noticed the nebulosity (and it would be remarkable if he had not), he surely would have supposed that this milkiness, too, would be resolved with some improved telescopic power. It would have diminished his important discovery to mention so quickly a counterexample, and in his mind it would surely have been unwarranted to conclude that a genuine nebulosity existed after the previously known examples had broken into stars under his telescopic scrutiny.

Recently an old and exceedingly rare printed account of nebulae by the Sicilian astronomer Hodierna has come to light. Hodierna had no axe to grind about the nature of nebulae, and in 1653 he depicts (for the first time) the Orion Nebula. It seems to me to be an outright false deduction to suppose that the Orion Nebula came into brilliance in the short interval between 1610 and 1653. In detective
Fig. 2—The belt and sword region of Orion from Galileo's *Sidereus Nuncius* (1610), showing numerous additional stars but omitting the nebula.
Fig. 3—Lord Rosse’s greatly metamorphosed drawing of the Crab Nebula, made with his 36-inch reflector around 1844, and the later drawing made by Secchi with a 9-inch refractor and published in 1856, showing obvious influence from Lord Rosse’s erroneous depiction.

work, we must always remember that absence of evidence is not evidence of absence.

Nevertheless, the question of possible changes in nebulae tantalized astronomers increasingly in the 19th century. Because the Orion Nebula had been so comparatively well observed, it became the inevitable focus for examination. Edward Holden, who had not yet become director of Lick Observatory, published in 1882 an entire monograph on this object. Lavishly illustrated, it included what was then the earliest known depiction, by Christiaan Huygens, as well as 18th-century drawings by Messier and Herschel. The role of 19th-century observers was extensive, and Holden included drawings by Struve, Bond, Lassell, Rosse, Secchi, d’Arrest and Trouvelot. These images proved little about any evolutionary changes in the Orion Nebula, but they do illuminate interesting points about both the psychology and physiology of observation.

Beginning in the 1860s the drawings by G.P. Bond, William Lassell, and Lord Rosse depicted the nebula in increasingly cubistic terms. When observers with lesser telescopes followed this style, one is almost obliged to conclude that it was fashionable to sketch the Orion Nebula this way, rather than to show what the nebula was really like.

The effect of a prior image influencing a depiction is even more striking with respect to M 1, the Crab Nebula. In 1844 Lord Rosse published in the *Philosophical*
Fig. 4—The cubistic representation of the central region of the Orion Nebula made by Lord Rosse with the 72-inch reflector in 1865-7 should be compared with the detail on p. 109 of David Malin and Paul Murdin’s *Colours of the Stars* (Cambridge, 1984).

*Transactions* a figure of M 1 made with his 36-inch reflector that showed the nebula with remarkable appendages. Not long afterward Angelo Secchi, using a much smaller refractor at the Vatican Observatory, produced a notoriously similar view. All later depictions, including Rosse’s, were totally different, and in 1880 he remarked “would have figured it different from drawing in *Phil. Trans.* 1844.” A
recent examination by David Dewhirst of the archival drawings from Birr Castle shows how the original telescopic drawing (perhaps made from memory the next morning) metamorphosed through a series of intermediate images to the 1844 published drawing, which no longer bore much resemblance to the celestial object.

But Father Secchi was not the only observer who brought to the eyepiece unwitting preconceptions. After Lord Rosse found the spiral structure in M 51, calling it the “Whirlpool Nebula,” he began to see spirals everywhere in the sky – not just in genuine spiral galaxies, but in planetary nebulae, and even in the irregular companion to M 51 itself. (See Lord Rosse’s drawing, which I have found since this lecture was originally given in 1985.)

After the 1880s, drawing nebulae became obsolete. The introduction of dry plates made possible the photography of faint objects such as the Orion Nebula – first in 1882 by Henry Draper in New York state, and then much more dramatically in 1883 by Andrew Ainslie Common in England. These early photographs in fact showed some of the blocky structure of the visual drawings, but soon a very different, more homogeneous view of the Orion Nebula imprinted itself on astronomers’ minds. Not until the recent unsharp mask photographs by David Malin have we truly been able to appreciate the wonderful skills of the visual observers of a century ago. Perhaps in part they were psychologically influenced by the fashions of the day, but the fashions had a real physiological basis: they exaggerated contrast differences at the same time that they suppressed large intensity differences in order to produce their remarkable drawings, so astonishingly similar to the unsharp mask images.

The introduction of photography into astronomy gradually transformed the way nebulae were discovered and studied. When James Keeler became director of Lick Observatory in 1898, he took over the previously scorned 36-inch Crossley Reflector and made it into a powerful tool for discovery of nebulae. By the turn of the century he could estimate that 120,000 nebulae were bright enough to be photographed by the Crossley, and a very large fraction of them appeared to be spirals.

Photography eventually showed that the sought-for large-scale evolutionary changes were virtually absent in the nebulae. However, it began to reveal more subtle alterations: the changing brightness of stars within the spirals, first novae, and then other variables. Keeler’s success in photographing the spiral nebulae with the Crossley Reflector at Lick Observatory paved the way for the building of the large reflectors at Mt. Wilson Observatory under the astronomical entrepreneurship of George Ellery Hale. Soon these two mountain-top observatories were locked into intense rivalry. The first novae were found in the spirals in March, 1917 by Heber D. Curtis with the Crossley Reflector, but before he published his discovery, George Ritchey at Mt. Wilson announced by telegram that he had found one in NGC 6946. Curtis, although scooped by the competing establishment to the south, at least had a clear idea of what to do with the discoveries, which he
promptly used to infer that "the novae in spirals furnish weighty evidence in favour of the well known 'island universe' theory of the spiral nebulae."

Meanwhile, Harlow Shapley, a young Missourian with a Princeton Ph.D. who had been hired by Hale to study star clusters, used the 60-inch reflector at Mt. Wilson to find his own nova, in M 31. Shapley promptly published a discussion in which he stated that "the minimum distance of the Andromeda Nebula must be of the order of a million light-years." This was not the entire story, however, for photography was revealing (or so it seemed) yet another change in the spirals: a steady rotation. In his paper Shapley mentioned the work of his Pasadena colleague, Adriaan van Maanen, on the apparent rotations of the spirals, which if correct, and if the spirals were at extragalactic distances, would have led to rotational velocities approaching the speed of light. "Measurable internal proper motions, therefore, can not well be harmonized with 'island universes' of whatever size," Shapley concluded, but in October, 1917 he was prepared to dismiss van Maanen's findings on the evidence of the novae.
Within a few months, however, Shapley flip-flopped on his weighting of the conflicting evidence. At Princeton he had worked on the orbits of eclipsing binary stars, and among other things, he convinced himself that the Cepheids could not be interpreted (as was then commonly thought) as eclipsing binaries. Hence he had strong reasons to believe that the periodic variation arose not by geometric accident but by the intrinsic physical nature of each Cepheid. Thus Henrietta Leavitt's period-luminosity connection was more than just a "remarkable relation" but a physical law, something that could be assumed to be the same everywhere and therefore suitable for distance measurements. Shapley calibrated the intrinsic brightnesses of the Cepheids statistically (on the rather shaky basis of proper motions of eleven galactic Cepheids), and, applying this result to his globular clusters, concluded that they lay at tens of thousands of light years from the sun.

By the beginning of 1918 Shapley took a mighty conceptual leap: the globular clusters must define a distant nucleus of our own extended Milky Way system. Writing to Eddington on January 8, Shapley said:

I have had in mind from the first that results more important to the problem of the galactic system than to any other question might be contributed by the cluster studies. Now, with startling suddenness and definiteness, they seem to have elucidated the whole sidereal structure …

The equatorial diameter of the system is in the order of 300,000 light-years; the center is some 60,000 light-years distant. Our local cluster, very loose and perhaps ill-defined, is about half way out to the edge … . So far as I have gone everything seems to fit together beautifully. The above is sketchy and arrogant, I know; and I haven't much excuse for it. You will be able to see all the consequences I have, and probably many more, so there is no pressing need to summarize further. [Harvard University Archives.]

Among the other consequences of this 300,000-light-year diameter was that the Milky Way seemed to be so much larger than M 31 (at a million-light-year distance) that Shapley abandoned the idea of island universes, and van Maanen's measurements of the internal motions in the spirals seemed to corroborate this judgement. The story of how Shapley debated with Curtis over the scale of the universe is now part of the standard lore of astronomy. The two men faced off at an evening "converzatione" at the National Academy of Sciences on April 26, 1920. Shapley, not an experienced debater, was apprehensive. He devoted his talk primarily to an elementary exposition of the distance scale, coming eventually to the large size of the Milky Way, and he ended with a glowing account of an optical "image intensifier" that he had invented. This account must have been intended for the benefit of a pair of Harvard representatives, who were there scouting for a new observatory director and who probably needed something like an image intensifier at the Cambridge observatory to overcome the handicaps of East Coast weather. Curtis, in a momentary quandary, decided to carry on with his more technical rebuttal of Shapley's use of Cepheids and his considerable doubts concerning van Maanen's spiral motions. Speaking from the floor, Henry Norris Russell defended
Fig. 6—Dr. Harlow Shapley at his famous circular, revolving desk. Photo by Frank Hogg, Spring, 1929.
Shapley’s variable star techniques so brilliantly that the Harvard representatives decided straightaway that the vacant directorship should be offered to Russell!

The following day the Kansas City Star carried a 17-inch account of the debate. Since it is unlikely that they had a reporter present, and since Shapley himself had once been a Kansas reporter and was always a fanatical baseball enthusiast, this ephemeral account (here considerably abridged) from his vast archive of newspaper clippings probably gives Shapley’s view of the debate (although he can hardly be held accountable for the headlines):

**A LEAGUE OF UNIVERSES?**

**OUR UNIVERSE, INCLUDING THE MILKY WAY, IS A COMPARATIVELY SMALL AFFAIR, A TAILENDER IN A BUSH LEAGUE, ONE FACTION HOLDS**

Washington, April 27. – Is there one great universe or a million? Is the great unmeasured nebulae [sic] of which the world is but one atom, one big unit or is there a league of universes, each independent of each other?

Undisturbed by the problems of reconstruction, heedless of the League of Nations war between the President of the United States and the senate, whether overalls will accomplish the undoing of high prices, and like pregnant issues of the earthly hour, the National Academy of Sciences in session here listened to a learned debate on the dimensions of the universe . . .

On the one side ranged the supporters of Dr. Heber D. Curtis of the Lick Observatory, who defended the old theory that our universe is one of many universes, a theory until recently generally accepted by astronomers.

On the other side appeared the following of Dr. Harlow Shapley, who, in an exhaustive discourse, replete with terms of the higher and highest calculus, sought to demonstrate that there is but one universe, but a universe ten times larger than that hitherto conceived by the wildest astronomical calculation . . .

In the confines of his comparatively cramped universe, Dr. Curtis touched off a series of pyrotechnic displays of cosines and tangents demonstrating to the satisfaction of his partisans his conception of a league of universes, leaving the question open whether our universe belongs to a major, minor or bush league in the cosmic game . . .

Then Dr. Shapley came to bat with his theory of one big universe, which he did not attempt to bound. He began by postulating a galaxy of stars ten times greater than any conceived by Dr. Curtis. He descanted upon star spirals “as presumably inter-galactic objects of nebular construction – that is, a part of the grand system and not individual galaxies or other great universes . . .”.

In the weeks immediately following the “Great Debate” Henry Norris Russell turned down the offer of the Harvard College Observatory directorship, and eventually Harlow Shapley received the position. By April of 1921 he had left Mt. Wilson, and therefore he witnessed from afar the resolution of the island-universe controversy.

The resolution came through the researches of Edwin Hubble, another Missourian brought to Mt. Wilson by Hale. Hubble exploited the 100-inch reflector with great effect in his studies of spiral nebulae, and on a plate of the outer regions of M 31 taken October 5, 1923 he recognized as a Cepheid a star that he had at first mistaken for yet another nova. After he had obtained a full light curve, he broke the
news in a letter to Shapley. In response, Shapley admitted, “Your letter telling of
the crop of novae and of the two variable stars in the direction of the Andromeda
nebula is the most entertaining piece of literature I have seen for a long time . . . .
That second, fainter variable (the Cepheid) is, under the circumstances, a highly
important object.” (February 27, 1924.)

An opportunity to present these results came at the 1924 joint meeting of the
American Astronomical Society and American Association for the Advancement
of Science held in Washington, D.C., where a substantial prize was being offered
for the best scientific paper. Joel Stebbins, then the secretary of the AAS, later
described to Hubble an amusing and memorable anecdote of the occasion:

On the first evening of the meeting, I happened to take dinner with Russell who arrived rather late,
and one of the first things he inquired about was whether you had sent in any contribution. On my
answering no, he then said, “Well, he is an ass. With a perfectly good thousand dollars available
he refuses to take it.” These remarks led to some discussion, and afterwards in a group in the hotel
lobby we drafted a telegram urging you to send by night letter the principal results which Russell
and Shapley could make up into a paper. After this message was drafted, Russell and I started to go
over to the telegraph office to send it, but on the way we stopped at the desk and put it on a regular
blank. Just as we were leaving, Russell’s eye caught beyond him on the floor a large envelope
addressed to himself, and at the same time I spied your name on the upper left corner. The clerk
gave us the material, and we walked back to the group in the lobby saying that we had got quick
service, and that the paper was in hand. At the time, the coincidence seemed a miracle. [Stebbins
to Hubble, February 16, 1925, AAS Archives, American Institute of Physics.]
Russell read Hubble’s paper for him, and it shared the prize for the best paper at the AAAS. Curtis had been at the meeting, but had already taken the train home before the paper was read. In any event, all the principals had got the news long before the session on New Year’s Day of 1925 put it on the official record.

It was perhaps fortunate for Helen Sawyer Hogg that Shapley had come East, and was therefore only a peripheral player in the denouement of the island-universe debate. But in Cambridge Shapley set up the astronomy graduate school from which Helen and her husband Frank obtained two of the earliest doctorates. It was there, with Harlow Shapley, that she developed her love for variable stars in globular clusters, a field that Shapley had worked so energetically to promote. I am particularly pleased, therefore, that I could end this brief series of episodes from the early history of nebulae with Shapley and with variable stars.

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BIBLIOGRAPHICAL NOTES