Slipher and the Nature of the Nebulae

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Why do some discoveries, which appear in hindsight to be obviously major discoveries, have so little impact when they were made.

These are discoveries that were ahead of their time, but for some reason the scientific community was not ready to absorb them.

This issue is related (but not one to one) to why some people don't get the credit for their discoveries

I got interested in Slipher after learning that by 1914 he had observed nebular redshifts up to 1000 km/s: why did this not convince people that the nebulae were extragalactic ?

Start with two major discoveries related to dark matter in galaxies which got little response from the community at the time

- Zwicky and the mass of the Coma cluster (1933, 1937)

- Kahn & Woltjer (1959) on the timing mass for the M31-Galaxy system

and then contrast with the discovery of pulsars (1967) and dark matter in dwarf spheroidal galaxies (1983), both of which had an immediate impact

Then turn to Slipher's work on galaxy redshifts

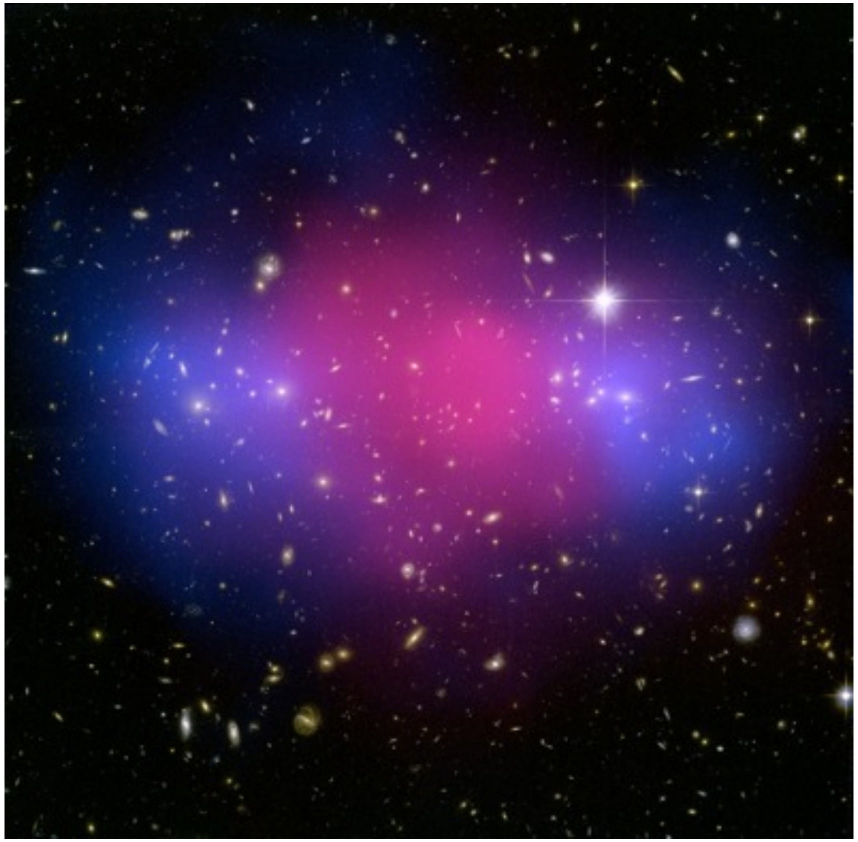
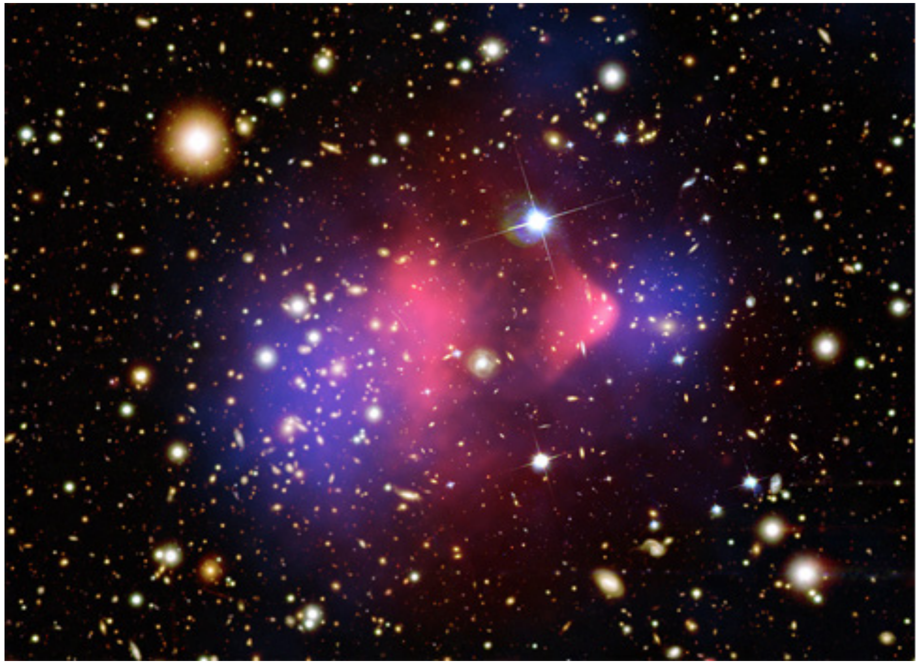
Zwicky's discovery of dark matter in the Coma cluster (1933, 1937)

Zwicky measured the velocity dispersion of galaxies in the Coma cluster of galaxies. Using virial theorem techniques that are used today, he showed that the mass of the cluster is much larger than the likely sum of the masses of the individual galaxies. A similar result was found by S. Smith (1936) for the Virgo cluster. Zwicky's velocity dispersion is close to present estimates.

This profound discovery got little response, other than Smith's follow-up study for Virgo.

It took another 35 years for the Dark Matter (DM) saga to take off (and Zwicky did get the credit).

And then another 20 years to learn that clusters have the universal baryon content (about 16% of their mass, including the hot gas which dominates the baryons), and the rest in DM, partly in the galaxies and partly in the cluster itself (White et al 1993)

Clusters of galaxies contain galaxies, dark matter and hot gas.

Here, two clusters have collided: the two lots of galaxies and dark matter (blue) have passed through each other but the hot gas (pink) interacts and is left behind. Most of the baryon mass is in the gas ! The dark matter was measured by weak lensing (Hubble and groundbased telescopes): the dark matter lies with the galaxies. The hot gas gives off X-rays measured by the CHANDRA telescope

(Clowe et al 2006)

Why did Zwicky's profound and apparently straightforward discovery get so little response ?

Did the community already regard Zwicky with suspicion
(or did that come later) ?

Why didn't astronomers follow up with spectroscopy of groups of galaxies like the M81 and Leo groups, which would be much easier to study than Coma ?

Or was it due to suspicion of results for which there is no existing theoretical framework ?

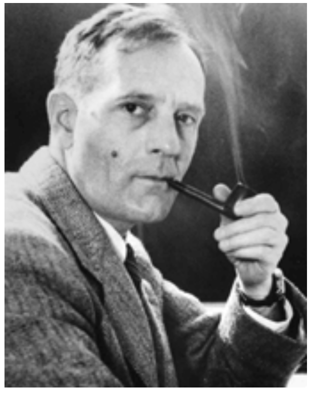
e.g. It is also a good rule not to put overmuch confidence in observational results until they are confirmed by theory
(attributed to Eddington)

Zwicky does now get full credit for his discovery.
This is a counter-example to Stigler's Law of Eponymy (1980), that
no scientific discovery is named after its original discoverer.

Big new ideas only tend to catch on
when the larger scientific community is ready for them
or an established scientist finds a use for them.

If a statement of an idea comes too early,
and a later restatement is accepted by the community,
it indicates that
the community accepts ideas
when they fit into the current framework.

I don't want to dwell much on the appropriation of discoveries by others, or the later failure to recognize discoveries like Slipher's. (That need not be the same issue as Stigler's law).



But Hubble's name comes up frequently in these discussions (see M. Way's talk on Saturday) and it is interesting to ask why. I am aware of at least four examples involving Hubble.

1. At this meeting, we are concerned with the lack of recognition of Slipher's redshift achievements until long after the event. His work had impact on the community at the time but later did not get the recognition in the context of Hubble's expanding universe.

2. The issue with the Hubble redshift-distance law and Lemaitre's contribution has been much discussed recently. Lemaitre's discovery of the law a few years earlier was not recognized properly. A translation of Lemaitre's paper into English omitted the observational section that showed the Hubble law and Lemaitre's derivation of the Hubble constant. Mario Livio has argued that this omission was Lemaitre's choice.

In a book that David Block & I wrote in 2008, we identified two other incidents involving Hubble and the UK astronomer Reynolds

3. The Hubble classification of galaxies was basically invented by Reynolds. Hubble certainly knew about Reynolds work, and they corresponded about classification around 1919 (RAS archives).

4. The other Hubble law: the brightness distribution in elliptical galaxies, which Reynolds discovered. It became known as the Hubble distribution but more recently as the Hubble-Reynolds law.

$$\boxed{I(R) = I_\circ(1 + R/a)^{-2}}$$

There is a view that Hubble was not generous in acknowledging the contributions of others. Some who knew him regarded him poorly in this respect. On the other hand, some of us are careless about picking up ideas and forgetting where they came from. It still happens. Geography, institutional rivalry and culture may also be significant elements in this behaviour. For others, modesty is more important than credit.

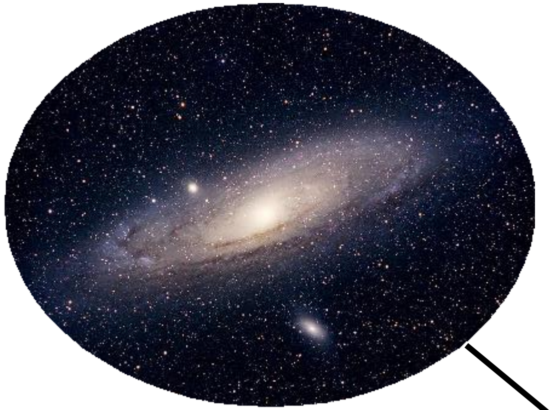
Kahn & Woltjer (1959) on the motion of M31 relative to the Galaxy

(another paper that did not have the impact it deserved)

M31 is about 750 kpc away and approaches the Milky Way at 188 km/s. Adopting the age of the universe, a radial orbit and simple Keplerian dynamics shows that the mass of the (M31- Milky Way) system is about 20 times larger than the likely masses of the stars.

A large transverse component of motion was unlikely (and recently shown to be small) but would anyway make the problem worse. Their total mass was $> 2.10^{12}\ M_{\odot}$ (present estimates are about $3.10^{12}\ M_{\odot}$). Their distance scale and stellar masses were roughly right.

The argument is direct and compelling but made almost no impact at the time: it was brushed off by a couple of unconvincing studies that concluded that extra mass was not needed to bind the LG.



M31

M31 and the Milky Way are now approaching at 118 km s $^{-1}$. Their separation is about 750 kpc

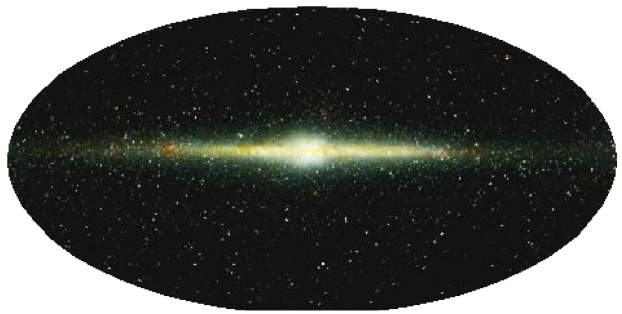
To acquire this velocity of approach in the life of the universe means that the total mass of the Milky Way is at least 120 x 10 ^{10} $M_{\odot}$.

The stellar mass is about 6 x 10^{10} $M_{\odot}$, so the ratio of dark to stellar mass is ~ 20

118 km s $^{-1}$

Milky Way

The dark halo extends out to at least 120 kpc, far beyond the disk's radius of ~ 20 kpc



(Kahn & Woltjer 1959)

Kahn & Woltjer excluded intergalactic stars and argued that the extra required matter was in the form of ionized gas. Their paper went on to explore the distorting effects of this gas on the Galactic disk. Maybe that was a distraction.

Why did this paper have so little impact ?

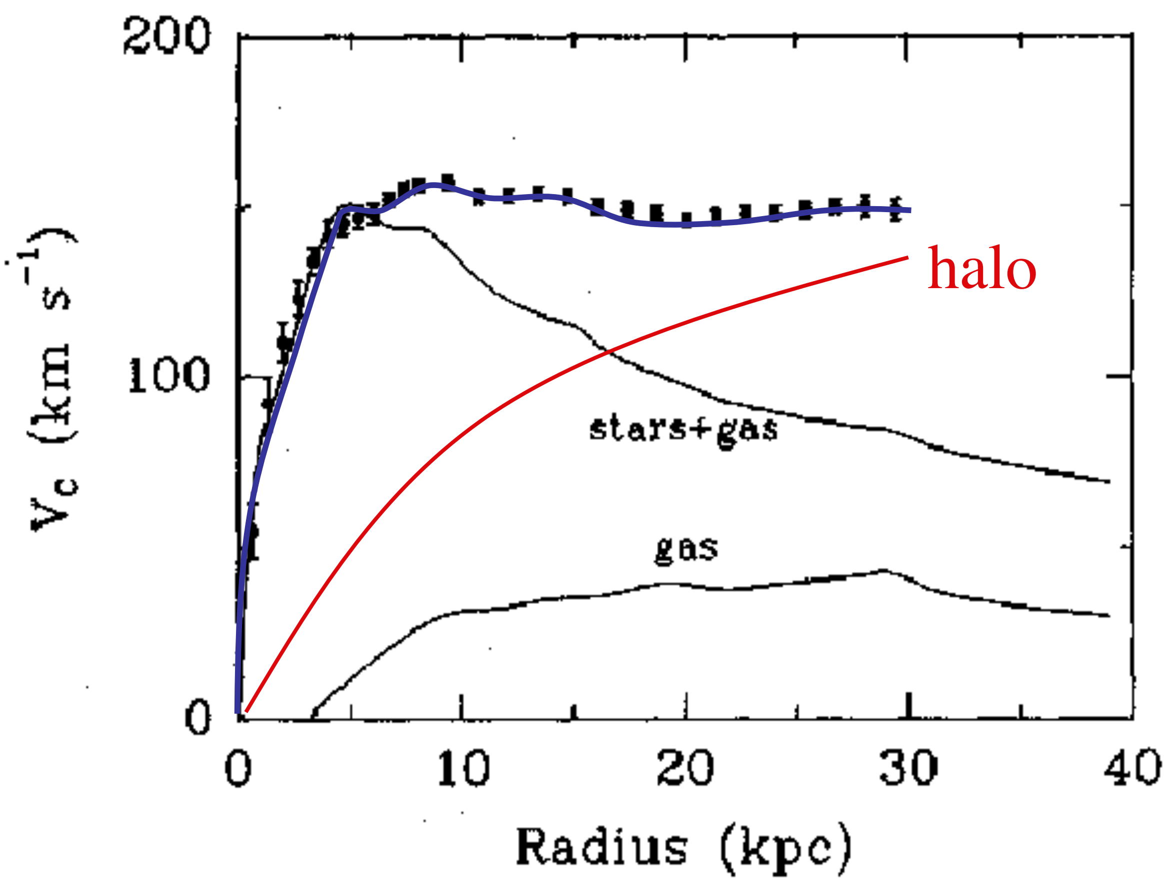
The argument is simple and correct and has survived to the present. Kahn and Woltjer were both very respectable and well-regarded researchers. Their work could have started the dark matter revival in 1959.

My guess is that there was simply no theoretical framework within which to interpret this observation. The weak contrary evidence provided a welcome escape.

In the 1970s, the 21-cm rotation curves of spiral galaxies showed that they have massive and extended dark halos. The argument was no clearer, but it made an impact. There was a theoretical framework by then (even though it turned out to be wrong).

The 21-cm rotation curve of NGC 1398

Begeman et al 1991



The stars and the gas together do not provide enough gravity to explain the rotation: we need the extra gravity of the dark halo

The Kahn & Woltjer (1959) story about the Galaxy & M31 is dynamically very straightforward. So is the dynamics of the flat rotation curves (1970s). One made an impact and the other did not.

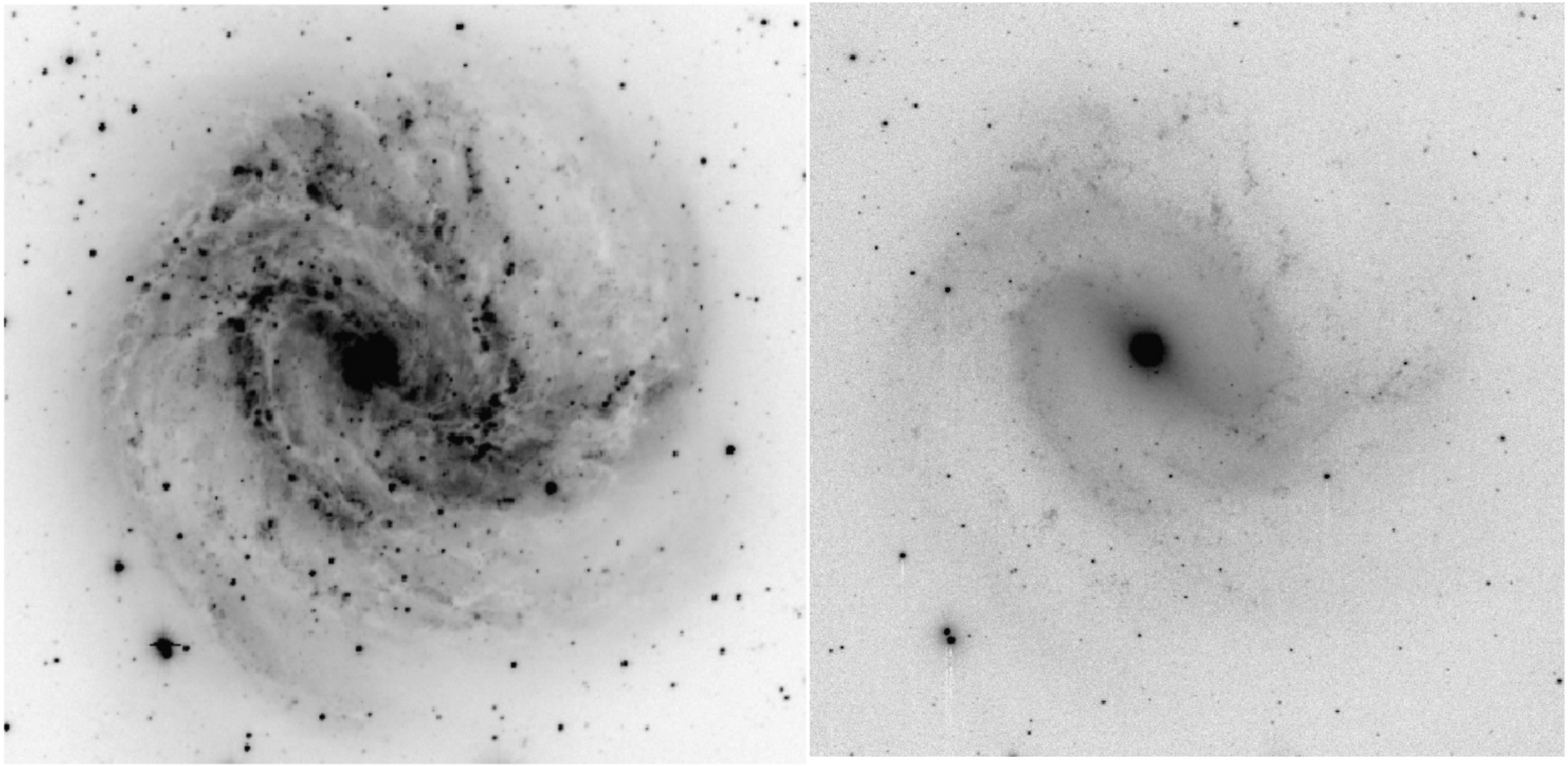
The idea of DM from rotation curves started controversially around 1970 and was based on poor data but was taken seriously. Why was it taken seriously ? In the 1970s, the idea of the massive dark halo got unexpected support from theoretical arguments (Ostriker & Peebles 1973) about the need for DM to stabilize disks against bar formation. These arguments turned out to be irrelevant because most galaxies do have bars. In fact, we now know that dark halos are needed to *sustain* bars via angular momentum transport. The controversy ended around 1978 when high quality 21-cm rotation curves became available from WSRT.

Even though the theory turned out to be irrelevant … people believe observations that fit into some theoretical framework, even if the observations have a sounder basis than the theory. It allows them to come to grips with startling observations (Eddington)

An unbarred spiral

A barred spiral

In the 1970's, barred galaxies were thought to be rare. Now we know that bars are very common in disk galaxies - about 70% of disk galaxies (including our Galaxy) show some kind of central bar structure in the near-infrared.



The nearby spiral galaxy M83 in blue light (L) and at 2.2μ (R)

The blue image shows young star-forming regions and is affected by dust obscuration. The NIR image shows mainly the old stars and is unaffected by dust. Note how clearly the central bar can be seen in the NIR image

An unexpected discovery that had an immediate impact

Discovery of pulsars in 1967 by Hewish, Bell et al was followed by quick stream of papers : after the "little green men" interlude, focus quickly moved to the current spinning neutron star explanation.

Why did it get such a quick response ? Because the basic theoretical framework was already in place, from Baade & Zwicky (1934) and later theoretical developments: people quickly made the connection.

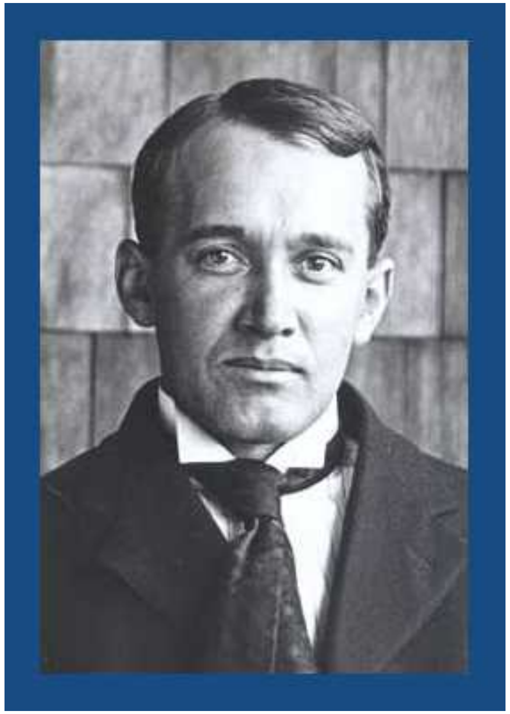
Another example is the discovery of the high fractions of DM in dwarf spheroidal galaxies, starting with Faber & Lin (1983) and Aarsonson (1983). Arguments were flimsy, but there was only limited scepticism. This is still an active field today: current estimates of M/L are > 1000 for ultrafaint dSph

Why did it get such a quick response ? Because the basic observational infrastructure of dark matter in galaxies was already there, plus the White & Rees (1978) ideas on the role of DM in galaxy formation.

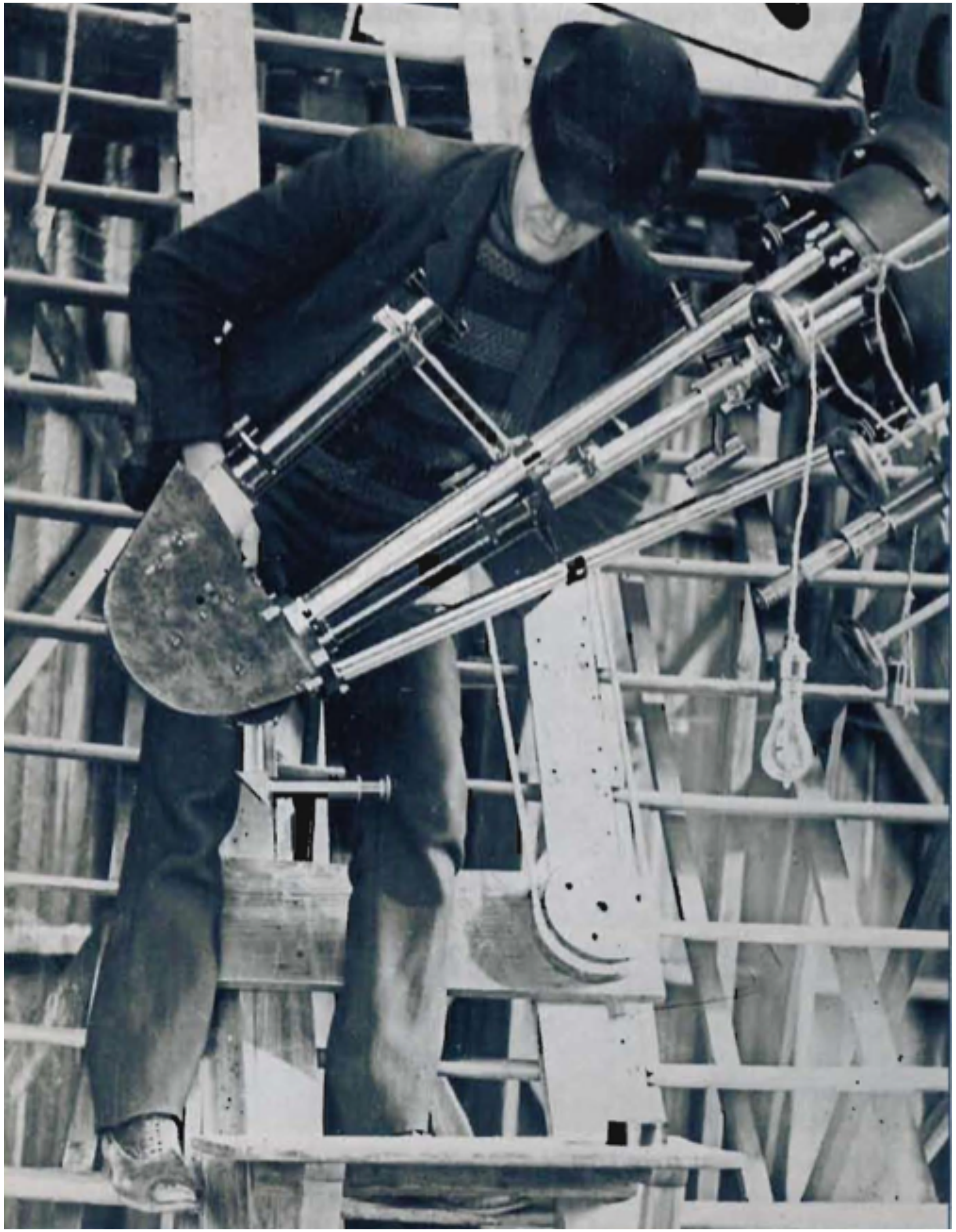
Slipher's contribution

Slipher had expertise in spectroscopy. In 1904, he wrote a paper on the Lowell spectrograph, built by the Brashear company for planetary spectroscopy, to go on the 61 cm Clark refractor.

Lowell requested Slipher to get spectra of nebulae: major challenge because of their low surface brightness. Slipher had the expertise required. He modified the instrument for nebular spectroscopy: this advance in technology made it possible to measure nebular redshifts.



For about a decade, Slipher was almost the only person measuring velocities of nebulae, although there are mentions of confirmations by Wright (Lick), Wolf (Heidelberg) and Pease (Mt Wilson). They, and Fath (Lick) and others, had already acquired nebular spectra but had not measured their radial velocities



Slipher and the Lowell spectrograph

The nebular exposures were long: 20 to 40 hours.

Linear dispersion of 140 tenth-meters per mm (140 Å/mm) which is adequate to see rotation

(With original optics, the dispersion was about 11 Å/mm or R ~ 22,000)

From Bartusiak 2009

The Slipher papers

1913: measured the radial velocity of M31. This appears to be the first radial velocity of a spiral nebula. Its velocity ~ -300 km/s.

> That the velocity of the first spiral observed should be so high intimates that the spirals as a class have higher velocities than do the stars and that it might not be fruitless to observe some of the more promising spirals for proper motion.

1914: measured the rotation of NGC 4594. It has a high surface brightness bulge, velocity ~ 1000 km/s, rotational velocity ~ 200 km/s

1915: radial velocities for 15 spirals: 13 are positive, up to 1100 km/s. Mean is about 400 km/s, about 25 times the average stellar velocity.

1917: longer review on nebulae (about 10,000 were catalogued at the time). The spirals have mainly absorption-line spectra but some emission lines are seen.

How accurate are Slipher's velocities, relative to modern values ?
σ = 112 km/s, close to his own estimate.

By 1917, he had velocities of 25 spirals up to 1100 km/s, all positive except for Local Group galaxies and M81. The mean velocity of the nebulae was now about 30 times the average velocity of the stars.

It has for a long time been suggested that the spiral nebulæ are stellar systems seen at great distances. This is the so-called "island universe" theory, which regards our stellar system and the Milky Way as a great spiral nebula which we see from within. This theory, it seems to me, gains favor in the present observations.

(Slipher 1917)

Slipher was well aware of the significance of his observations. The velocities of the nebulae are much larger than of the Galactic stars. He inferred that they lie outside the Milky Way. Hertzsprung wrote to Slipher in 1914 to make the same point. Although some (like Reynolds) had their doubts about the data, Slipher had a convincing response, with confirmation of the velocities from others. Slipher's work was well known. Why did it not settle the issue ?

The velocities of stars were known from the work of Boss, Campbell, Kapteyn and others to increase with spectral type, from about 6 km/s at B to 15 at K and then to 27 for the planetary nebulae (Smith 2008). (This effect is still not properly understood.)

Could the nebulae be Galactic objects, much further up this evolutionary chain ? Seems unlikely: their high velocities indicated that they were not gravitationally bound to the Milky Way.

By 1916 there were already some ideas about obscuring material in the MW. It was long known that the nebulae avoided the Galactic plane, which again favored an interpretation putting them outside the MW at that time. Further support came from observations of extragalactic novae, but van Maanen's proper motions in nearby galaxies confused the issue.

In the 1920 Shapley-Curtis debate,   Shapley argued that the nebulae are just nearby clouds and the universe is one big Galaxy. Curtis argued that the nebulae are galaxies like our own, far outside the Milky Way. In the end, Hubble's 1923 cepheid work on M31 settled the question.

Some similarity of this controversy and the DM controversy of the 1970s, but in reverse. The DM story was supported by erroneous theory, while the island universe story was delayed by erroneous observations. Progress is not always linear.

Conclude that Slipher's situation is not at all comparable with Zwicky's. Slipher's work made an impact at the time, but his problem of recognition came later. Speculate that without van Maanen's confusion, the issue would have been more clearly defined. Maybe the debate would not have happened and Slipher would have got the credit for identifying the nebulae as extragalactic, which I think he deserves.

CONCLUSION

I find it hard to believe that the high velocities of the nebulae were not regarded as conclusive evidence for their extragalactic nature.

Slipher had shown convincingly that the wavelength shifts in the spectra of the nebulae were consistent with the Doppler shift, and others had reproduced his velocities.

Kapteyn had already made a fairly accurate estimate of the mass of the MW, and it seemed clear that the nebulae could not be bound to the MW

Some M/L numbers

Stellar population of [Fe/H] = 0 and age (5,10 Gyr) has M/L = (3,5)

MW with $M_* = 6 \times 10^{10}$ has $L \sim (2,1.2) \times 10^{10}$. If $M_T = 2 \times 10^{12}$ then its M/L is about (100, 160).

Current M/L estimate for Coma is about 160 so could be all in the galaxies.