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PUTTING A NEW SPIN ON GALAXIES: HORACE W. BABCOCK, THE ANDROMEDA NEBULA, AND THE DARK MATTER REVOLUTION

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1. *Introduction*

When a scientist is the first to perform a difficult type of observation and correctly interprets the result as a significant challenge to then-widely accepted core theories, and the result is later recognized as seminal work in a field of major importance, it is a surprise to find that that work was essentially ignored by the scientific community for thirty years. Such was the fate of the doctoral research on the rotations of the Andromeda Nebula (M31) conducted by Horace Welcome Babcock (1912–2003), who went on to become a very prominent astronomer — in an entirely different sub-field, never working on the subject of his dissertation again. This paper seeks to explain the 'non-reception' of Babcock's work on galactic dynamics and the reasons he did no further work in that sub-field. In particular this paper shows that, contrary to the claims of some commentators, the non-reception of Babcock's work should not be understood as an example of the unjust treatment of a young scientist by the conservative establishment.

In 1939, Horace Babcock published the first detailed spectrographic study of M31.[1] Babcock's spectra of M31, originally obtained for his doctoral dissertation at the Lick Observatory in California,[2] showed a quite unexpected pattern. This unexpected pattern turned out to have massive implications for our understanding of the formation and composition of galaxies. Today Babcock's result is interpreted as an instance of what may be called the 'dynamical discrepancy in astrophysics', popularly known as 'the dark matter problem'. It indicates the need for a thorough overhaul of some of the most fundamental theories and assumptions in contemporary physics and astronomy. In his own time, however, Babcock's result caused barely a ripple within the astronomical community. In fact, despite the consilience provided by several related results obtained by others in the 1930s, it was not until the 1970s that the astronomical community gave the dynamical discrepancy the attention that it deserved, as we realise with the benefit of hindsight. This delay is something of a mystery, not least because the evidence itself did not change significantly in type or quality in the intervening period. As a corollary to explaining the non-reception of Babcock's work on M31, this paper also gives an analysis of some of the historical, sociological and institutional factors responsible for the slow response of the astronomical community to the early evidence for the existence and importance of the dynamical discrepancy.

2. *The Conceptual Background to the Dark Matter Problem*

The dynamical discrepancy is a significant and surprising disagreement between two different but seemingly independently reliable ways of estimating the masses of large-scale astrophysical systems such as galaxies and clusters of galaxies. The internal motions within such systems are inconsistent with the known laws of physics given the distribution of directly detectable matter in those systems — that is, given just the matter directly observable in these systems and the known laws of physics, the rotation rates observed in them cannot be explained. The dynamical discrepancy was first discovered in the 1930s; despite concerted efforts (especially since the 1980s) it remains unresolved today. According to our current view, there are two classes of possible solutions: either (1) there is a great deal more mass in large-scale astrophysical systems than can be detected by any direct means; or (2) our best theory of gravity, Einstein's General Theory of Relativity, does not apply to those systems and therefore needs to be replaced. On the first option, the excess mass is 'dark' in the sense that it neither emits nor absorbs electromagnetic radiation at any wavelength. This is one of the reasons for thinking that the excess mass is likely not any form of matter with which we are already familiar (protons, neutrons, etc.). On the second option, some new theory will be needed to replace General Relativity as the theory describing gravitational interactions taking place over distances equal to or greater than the radius of a galaxy.

Both options (1) and (2) are exciting: by whichever route the dynamical discrepancy is ultimately resolved, we potentially find ourselves now on the threshold of a Kuhnian scientific revolution, either in matter physics or in the theory of gravity. Historians, sociologists and philosophers of science are thus in a position to watch a scientific revolution as it happens — if only the natural world and human ingenuity will conspire to present a viable solution to the dynamical discrepancy. Babcock's work on M31 may be seen as a key piece of evidence, acquired at nearly the earliest moment the result would have been possible to obtain, that this scientific revolution is needed.

An immediate consequence of Isaac Newton's analysis of orbital motion[3] as an inertial motion plus a centripetal acceleration is that for any given central mass there is a unique orbital velocity that results in a stable orbit at a given distance from the centre. (Any slower and the body would drop to a lower orbit; any faster and it would move to a higher orbit or escape the system.) Conversely, if the radius and speed of an orbit are known, the mass of the central body can be calculated by the equation $m = v^2r/G$ (m is the mass, r is the radius of the orbit, v is the speed of orbit, and G is the gravitational constant). It is sometimes useful to plot the orbital speeds of the satellites in a gravitational system against their distances from the centre. Such a plot is called a rotation curve. The hyperbolic shape of the rotation curve for a planetary system like ours (velocity highest near the centre, dropping off asymptotically to zero with increasing distance) is called 'Keplerian', after Kepler's Third Law.

The same principles should apply to larger structures, the difference being that the mass distribution in galaxies and clusters is unlike the mass distribution in planetary systems. On the reasonable assumption that the mass distribution in a galaxy mirrors the distribution of the light it emits, the Keplerian expectation is that orbital velocities should decrease at radii beyond the visible edge of the galaxy. In addition to this general prediction about the shape of a galactic rotation curve, the orbital velocity at a given radius together with a version of the equation mentioned above can be used to calculate the total mass interior to that radius.[4]

3. *Babcock's Contribution to the Discovery of the Dynamical Discrepancy*

By the time Horace W. Babcock began observing for his doctoral dissertation, he had already published three short papers on solar spectroscopy, including one based upon work by, and another with, his father, Harold Delos Babcock.[5] Having essentially grown up at the Mount Wilson Observatory where his father was a noted solar spectroscopist in his own right, the younger Babcock was extremely well placed to make a contribution to the field of astronomy. As some historians have noted, his father raised him to be an astronomer.[6] They are the only father and son to have each won the Bruce medal for distinguished lifetime service to astronomy.[7] This pedigree and the eventual prominence Babcock achieved within the astronomical community make the apparent lack of attention given to Babcock's early work on galactic dynamics even more surprising.

In 1938, Babcock took spectra of the spiral galaxy M31 using the 36-inch Crossley reflecting telescope at the Lick Observatory with a photographic spectrograph built by his doctoral supervisor Nicholas Mayall. In his dissertation, Babcock's goal was to study the internal motions within M31, and compare its rotation curve with its light (and presumed mass) distribution. M31 is the brightest of the spiral nebulae, so it made an obvious target for this first study of the details of the spectrographic motions within a spiral nebula. Despite the relative brightness of M31 as compared to other nebulae, however, it is really quite dim, so obtaining good spectrographs of its fine details was no mean feat: some of Babcock's exposures lasted ten or even twenty hours (taken over several nights).

Prior to Babcock's study of M31, Pease and Slipher had each taken spectrographs of the entire bright inner regions of edge-on spirals, in which they noticed the spectra were inclined to the vertical.[8] They (correctly) interpreted this as being due to the rotational motion of the objects they studied — one side was moving away from us and its light was red-shifted, while the other side was moving towards us and its light blue-shifted.

Babcock's spectra were significant in part because they were the first to cover small interior regions of a spiral galaxy and the first to extend well out along the spiral arms. Babcock measured the relative shifting of the distinctive spectral lines of hydrogen and oxygen in light coming from stars and gas clouds at various radii from the centre of M31, and then plotted the results in a rotation curve.

Babcock concluded his 1939 paper by noting that there was a large discrepancy between his measured rotation curve and the Keplerian expectation that the velocity of rotation should decrease at radii beyond the edge of the visible disk. Instead, Babcock's measurements indicated that the rotation at the extremity of M31 is in fact constant or even increasing. Babcock wrote:

> A new discrepancy is now directly apparent when the rotations of [M31 and the Milky Way] are compared, for the nearly constant angular velocity of the outer parts of M31 is the opposite of the 'planetary' [i.e., Keplerian] type of rotation believed to obtain in the outer parts of the [Milky Way].[9]

Babcock was relying on the best information then available about the rotation curve for our own galaxy when he made this comment. We now know that the Milky Way has the same non-Keplerian pattern as the rotation curve for M31. Also, we now have rotation curves for thousands of spiral galaxies and in every case the pattern is very similar to that of M31. The fact that the rotation curve is non-Keplerian by itself requires that there is a great deal more mass present in the system than would be expected from the distribution of the light. Babcock calculated that the mass-to-light ratio for M31 is 50 (that is, for each unit of light equivalent to the Sun's output emitted by M31, it is calculated that the corresponding mass present is 50 times that of the Sun), a value that is "much greater than that for the same [ratio] in the vicinity of the Sun".[10]

In fact, the situation is even more serious than Babcock realized. In 1970, using more modern technology, Rubin and Ford compiled a similar spectrographic rotation curve of M31 and found the same result as Babcock, out to even larger radii.[11] The increasing sensitivity and discerning power of modern astronomical technology means that rotation curves can now be extended to many times the radius of the visible disks of spirals: gas clouds orbiting as far as 10 times the radius of the optical disk from their parent galaxies (detected at infra-red wavelengths) are measured to have orbital velocities that are no slower than those of the stars at the visible edge of the disk. In short, there is no Keplerian decrease in orbital velocity in spirals even at extreme radii.

Using related methods, parallel results have been obtained for other kinds of astronomical systems, including elliptical galaxies and clusters of galaxies. The implications are far-reaching. It is bad enough that the discrepancy between the dynamical mass and the visible mass in astrophysical systems is very great, but in many ways it is even worse that the rotation curves are non-Keplerian. It could have been that galaxies had Keplerian rotation curves but with greater magnitudes than would have been expected given their visible masses. Instead, the non-Keplerian rotation pattern demands one of two things. Either (1) the majority of the mass in a galaxy or cluster is not distributed where the light is but instead is most likely distributed in a non-luminous, quasi-spherical halo extending to several times the visible radius of the system, or (2) gravity is very different than we thought in that, beyond some large distance, gravitational attraction does not fall off according to the inverse square law! Babcock, of course, did not take his conclusions so far. Though we now know that (1) implies that the additional mass cannot be the type of matter with which we are familiar from ordinary experience (that is, it must be non-baryonic), this was a possibility that did not occur to Babcock, Fritz Zwicky or the other early discoverers of the phenomena today grouped together under the heading of the astrophysical dynamical discrepancy. For Zwicky, "dunkle Materie" referred simply to unusually dim but otherwise ordinary matter, not a special kind of unusual matter;[12] Babcock expresses essentially the same idea when he says that

> the great range in the calculated ratio of mass to luminosity in proceeding outwards from the nucleus suggests that absorption plays a very important role in the outer portions of the spiral, or perhaps that new dynamical considerations are required, which will permit of a smaller relative mass in the outer parts.[13]

4. *On the (Non-) Reception of Babcock's Result*

With hindsight, knowing as we do that Babcock's result and a few related results obtained in the same period were harbingers of the dark matter revolution, it seems remarkable that the astronomical community essentially ignored those results. Only in the 1970s did the astronomical community finally begin to act as if the dynamical discrepancy was real and significant, and only then did the now-booming industry of formulating and testing various hypotheses to account for the discrepancy get its start. There was no major difference in the evidence available in 1939 as compared to 1970. It is true that a few additional studies accumulated in the intervening thirty years, that a greater number of galaxies and clusters were studied, and that the later studies were of somewhat better quality because of improvements in technology.[14] However, there was no change in the fundamental character of the evidence, so the modest accumulation of these results is not enough by itself to explain the sudden and dramatic 'phase transition' in astronomers' attitudes toward the dynamical discrepancy in the mid-1970s.

There are really two historical questions here. First, why was Babcock's result *not* taken seriously for so long, even though it proved to be both correct and highly significant for our understanding of the universe? Second, why did the astronomical community suddenly take seriously the dynamical discrepancy in the 1970s despite the fact that there was essentially no change in the evidence base? In what follows I attempt to answer the first question, and indicate some factors that provide a partial answer to the second.

4.1. *Accounting for the lack of attention to Babcock's M31 rotation curve*

First consider the question about why Babcock's result was ignored. The astronomer Virginia Trimble is one of the few people to have written substantively on the history of dark matter.[15] Trimble supposes that Babcock's junior status within the profession made it possible for the astronomical establishment to ignore his disturbing result. Why should the word of a newly-minted Ph.D. be allowed to overturn fundamental assumptions about the universe? Trimble goes so far as to suggest that Babcock was so crushed by the poor reception of his work on the rotation of M31 that he switched fields entirely and never worked on galactic dynamics again:

> The large total mass and M/L ratio [derived in his M31 study] were sufficiently different from the numbers then believed for the Milky Way and other spiral galaxies that Babcock was discouraged from submitting his thesis to the *Astrophysical Journal* as was then (and now) the norm. And the reception accorded his presentation at a contemporaneous meeting of the American Astronomical Society was instrumental to his decision to switch from extragalactic to solar astronomy for the rest of his career.[16]

In later work, Trimble includes additional detail:

> Babcock ... who measured the first extended rotation curve for a spiral galaxy ... left the field.... He explained why in a hand-written letter responding to my [Trimble's] inquiry about why no-one (including himself) had followed up on his work for such a long time.... A public talk on his results (at the 1939 dedication of the McDonald Observatory) was heavily criticized by other meeting participants, and he was instructed to publish his thesis as a Lick Observatory Bulletin rather than in the Astrophysical Journal.[17]

This is a compelling story — the brilliant young researcher unjustly oppressed by the conservative establishment. As is all too common with such heroic stories, however, it does not survive scrutiny. The remainder of this section describes the ways in which this particular heroic story fails.

To begin with, Trimble's unjust treatment thesis does not explain why others who made related discoveries around the same time were also ignored. In 1936, Sinclair Smith published a study of the dynamics of the Virgo cluster of galaxies that pointed to the existence of much more mass in the cluster than was visible.[18] Smith was also relatively junior, aged 37 at the time, and he died just two years later; perhaps the fact that he was not alive to promulgate or develop his own work is partly responsible for its obscurity. Fritz Zwicky published a much more sophisticated and detailed study of the same kind, taking the Coma cluster as his object, and reached exactly parallel results. Zwicky's original 1933 paper was followed by several others, including a brilliant theoretical paper in 1937 that first reviewed and critiqued everything known up to that point about how to determine the masses of distant astronomical systems, and then proceeded to invent the idea of using gravitational lensing to measure the masses of galaxies and clusters of galaxies.[19] (Zwicky's 1937 paper was one of 53 selected for inclusion in the centennial volume of the *Astrophysical Journal* in recognition of its status as one of the most important papers published in that venue in its first hundred years.[20]) Zwicky had a reputation as a maverick and a dabbler and he was not well liked within the astronomical community, which might in part explain why his work on the dynamical discrepancy in clusters of galaxies did not at first receive the attention it deserved. No such explanation is available in the case of Jan

Oort, however, who was highly respected and a figure of some authority in the field. In 1932, Oort published a study comparing the dynamical mass in the nearby disk of the Milky Way with its visible mass, and found a small discrepancy.[21]

Taken together, the results of Oort, Zwicky, Smith and Babcock constitute a significant body of independent and mutually reinforcing evidence corroborating the existence the dynamical discrepancy at various distance scales. This should at least have inspired serious further investigations. It didn't. A mere handful of related dynamical studies appeared between 1939 and 1960, and just a slow trickle after that, until the sudden explosion of interest in the mid-1970s. The non-reception of all of this work suggests that there was nothing in particular about Babcock or about his result that is responsible for its lack of impact. So what could it have been?

It is no doubt in general correct that, in the words of Robert W. Smith, "Many scientific results that come to be generally regarded as discredited are dealt with by being ignored in the scientific literature, with the practitioners who are active in a particular area of research aware of what is credible and what can be safely ignored".[22] This cannot be the explanation for the lack of attention Babcock's work received, however. Perhaps a partial explanation of this lack of attention can be derived from the interesting fact that it is extremely rare to find papers in the astronomy literature of the period in which previous results are criticized at all — in particular, observational results are almost never rebutted by later papers.[23] This phenomenon may have partly been due to professional courtesy or the desire to avoid personal controversy in the still relatively small community of astronomers. A more important factor may be that the advent of astronomical photography had simply put basic observational results beyond dispute. The only thing that could possibly be criticized was the reduction of the data, but in most cases that reduction took place according to practices accepted across the astronomical community. It is also worth noting that with the construction of many large telescopes and the development of better photographic technology in this period,[24] there was so much exciting new work to be done that there was little interest in revisiting results someone else had already obtained. This means, to put it crudely, that Babcock's result was treated like any other in the period: they were all more or less ignored in the subsequent literature, and not just the ones everyone thought were wrong.

Part of the confusion here arises from the fact that Trimble, in the two quotations above, gets some of the details and context wrong. It is true that Babcock never published anything else on galactic dynamics. Contrary to Trimble's implication, however, he certainly was not ostracized by the community. After his year-long appointment as a research assistant at the Palomar Observatory and four years of wartime service on weapons projects (first on radar at MIT, later on rockets at Caltech), Babcock earned a prestigious permanent position at the Mount Wilson Observatory. He later became the director of the Mount Wilson and Palomar Observatories, he was the prime mover in the founding of the very important Las Campanas Observatory in Chile, and he was renowned both as a scientific administrator and as a designer/builder of outstanding instruments, especially diffraction gratings for astronomical spectroscopy. In 1969, Babcock was awarded the Bruce Medal by the Astronomical Society of the Pacific for lifetime contributions to astronomy, an honour he shares with the likes of Eddington, Hubble, Hoyle and Chandrasekhar.[25] This is hardly the record one would expect from someone supposedly ostracized by his professional community at the beginning of his career.

Another reason to doubt the unjust treatment thesis is that prominent workers in the field did in fact appreciate Babcock's results at the time, contrary to the implications in the quotation from Trimble, above. In the lead paper at the memorial conference for Babcock in 2004, Donald Osterbrock noted that

> W. H. Wright, the Lick Director, arranged for HWB [Horace W. Babcock] to present an oral paper on this M31 rotation curve at the annual meeting of the American Philosophical Society in Philadelphia, and for him to be invited to the dedication of the brand new McDonald Observatory in Texas after it. His paper fitted right into the subject of the symposium, Structure and Dynamics of Galaxies, and he discussed it afterward with Bertil Lindblad and Jan Oort, two of the world experts, who gave review papers there. Babcock's was the first published rotation curve which went significantly out into the main body of the galaxy. Everyone agreed his results were important, but no one had a good explanation for them.[26]

The final clause here may give the best hint about why so little was done with the early evidence for the dynamical discrepancy: the problem was so serious and so unexpected that no one could even begin to try to explain it.

In fact, Sandage tells the story of the early reception of Babcock's work in a completely different way than Trimble:

> Upon graduation from Berkeley, Babcock accepted a year's appointment as a research assistant (equivalent to a modern postdoctoral fellowship) at the Lick Observatory of the University of California. On the basis of his outstanding thesis in 1939 he was invited to the dedication of the 82-inch reflector of the new McDonald Observatory of the University of Texas in 1939, to give a paper on his M31 rotation results. In attendance were a number of the most famous astronomers of the time, including Jan Oort from Holland, Bertil Lindblad from Sweden, Edwin Hubble from Mount Wilson, Harlow Shapley from Harvard, and Otto Struve, host, director of both the Yerkes and the McDonald Observatories. Babcock impressed many at the conference. After his talk on the M31 rotation curve he was, on the spot, offered a job by Struve as an assistant astronomer on the Yerkes staff, with half time to be spent at McDonald in instrument design and construction.[27]

Far from the 1939 conferences being bad experiences for Babcock, then, they seem to have helped launch him on the fast track to a prominent career. Further evidence of a positive reaction comes from the fact that the publication version of the paper Oort delivered at the McDonald Observatory dedication conference attempted (in part) to deduce mass-to-light ratios for an edge-on spiral and found a much larger than expected value. Oort surely would not have been hard on Babcock for coming to a similar result; in fact, he even cites Babcock's work on M31 in that paper.[28] It is worth remembering, too, that Babcock was not an outsider to the field even as a freshly minted Ph.D.: he was already known to many of the leading lights because he had essentially grown up at Mount Wilson where his father was a well-regarded astronomer.

So why did Babcock do no further work on galactic dynamics? In an oral history interview nearly forty years after the event, Babcock cited Edwin Hubble's (non-) reaction as part of the reason, at least. After describing Hubble as "a very dignified person" who tended "to seem a bit cold and standoffish", Babcock relates this incident:

> I had been a graduate student at Lick, and I did a thesis on the rotation of M31, I came to Pasadena and talked with Hubble and with Zwicky and some others about it, Hubble, I think, didn't mean to be discouraging, but his attitude was so cold that quite frankly, it turned me off, I might have had a stronger interest in pursuing research in extragalactic fields, had it not been for that.[29]

It could be that the vicissitudes of memory led Babcock to misreport his experiences in 1939 in his personal letter to Trimble. Perhaps Trimble misread or misremembered that letter herself (she does not quote it in any material I have seen). Even Babcock's non-committal explanation of Hubble's cold attitude seems to have the flavour of a rational reconstruction in response to the interviewer's question rather than a true memory of an event that had any real significant impact on Babcock's early career trajectory.

Perhaps part of the explanation of why Babcock did no further work on galactic dynamics can be found by looking at his scientific bibliography in another way. When we do so, we see continuity rather than a change of direction: the work on M31 stands out as the aberration. Both before and after his doctoral project Babcock worked on subjects in solar and stellar spectroscopy. His first three published papers[30] were on the solar spectrum. After his M31 work, Babcock published a single joint paper on the background radiation of the night sky[31] and then he went back to his true love, stellar spectra. He excelled in that field for the rest of his career. He was renowned as an instrument maker — 'Babcock gratings' were highly sought by observatories around the United States. And as both an observer and a theorist he led important subfields. For example, Babcock was the first to suggest the idea of adaptive optics as a way to compensate for astronomical seeing, he was the first to observe the spectroscopic effects of magnetic fields in stars and our Sun, and he formulated a theory of stellar magnetism that is still the foundation of that field today.[32]

What really needs explanation, then, is not why Babcock worked on things other than galactic dynamics after his Ph.D., but why he did any such work in the first place. A speculative but plausible answer is that, as a spectroscopist, taking detailed spectra of a diffuse object such as M31 would have been an appealing challenge. Also, Babcock's dissertation director, Nicholas Mayall, who suggested the project,[33] certainly was concerned with galactic dynamics. The M31 study, then, would have been a good way to combine their interests and take best advantage of the equipment available at the Lick Observatory.

Part of the problem of understanding why Babcock's result had so little impact for so long is the difficulty of seeing his result as his contemporaries would have seen it. It is important in this regard to note that in 1939 the very idea of external galaxies was still relatively new.[34] The state of knowledge as late as 1920 was such that Heber D. Curtis and Harlow Shapley, two prominent American astronomers, could hold a public debate on the question of whether or not the nebulae are 'island universes'.[35] By the end of the 1920s Hubble had formulated arguments, based on Cepheids and red-shifts, showing that many nebulae are indeed at very great distances from the Milky Way, and hence that they must be external galaxies. The point here is that the very concept of external galaxies was new and their properties still uncertain, so Babcock's results would not in their own time have been seen to have the same implications as astronomers see in them now. For one thing, it would have seemed possible — even likely, given M31's disturbingly non-Keplerian rotation curve — that later observations of other spirals would show that the rotation pattern of M31 was somehow atypical.

Also relevant here is the fact that spectrographic observations of nebulae were exceedingly difficult to obtain. Indeed, although astronomers (notably Slipher and Pease) had taken spectrographs of the brightest inner regions of spiral galaxies and observed rotation, it was not settled for several years after Babcock's work on M31 even in which direction spirals rotate.[36] As noted above, Babcock's exposures were as long as 10 and 20 hours, even though they were made with some of the very best equipment of the day and M31 is the brightest of the spiral nebulae. It took some time before improved technology made it practical and profitable to pursue detailed spectrographic studies of the internal motions of other galaxies. Follow-up studies to confirm or disconfirm Babcock's result and generalize to other spiral galaxies were thus many years away.

Trimble claims that "The second rotation curve ever measured was that of M33 (Mayall and Aller 1960)"; however, "M33 is now known to be tidally distorted and truncated, so the small mass and M/L they found were inevitable".[37] The fact that M33 is unusual in not having a high mass-to-light ratio perhaps contributed to the lack of momentum in these studies. Unfortunately there is a typographical error in this passage and in the bibliography: the paper from Mayall and Aller was actually published in 1940, so the implication in the passage just quoted that no one worked on galactic rotation curves for twenty years after Babcock is incorrect. Mayall was, of course, using the very instrument Babcock had used two years previously.

Astronomers around this time were wary of reports about observations of motions within spiral nebulae anyway, because of an embarrassing incident that was still fresh in their minds. In 1916, Adriaan van Maanen had published a study of the face-on spiral M101 in which he claimed to have detected, by comparing photographs taken years apart, a rotational motion with a period of roughly 85,000 years; and he later published similar studies of other spirals. Shapley, in the printed version of his debate with Curtis in 1920, had invoked van Maanen's rotation as the basis for a *reductio ad absurdum* of island universes: if M101 were distant enough to be an external galaxy, a rotational period of 85,000 years would translate into a true rotational velocity faster than the speed of light; nothing can move faster than light; hence, M101 is not an island universe. We can now interpret Shapley's *reductio* as a classic illustration of the ambiguity of falsification: the absurdity of superluminal rotation actually proves that van Maanen's rotations are in fact spurious.

When Hubble (in 1925 and 1929) published data establishing that nebulae are indeed at great distances, he knew that his greatest challenge would come from van Maanen's purported rotations.[38] In 1935, Hubble published a paper attacking van Maanen's result.[39] Perhaps because of the rarity of inter-professional critique in this period, the final version was much toned down at the insistence of Walter Adams, the director of the Mount Wilson Observatory where both Hubble and van Maanen worked.[40] Hubble and colleagues independently re-examined van Maanen's photographs and were unable to find any rotation. This is as it should be, since we now know that spiral galaxies are typically on the order of 100,000 light-years in diameter and that typical stellar velocities within galaxies are a few hundred kilometres per second: at our distance and over the time available, no rotation could possibly be detected by looking for changes of relative position in photographs. The van Maanen episode is famous in some quarters as proof that observation is perniciously theory-laden in the sense that it is possible for researchers to 'see just what they are looking for'.[41]

After Hubble's book *The realm of the nebulae* was published in 1936, van Maanen's rotation was almost universally dismissed as false. Notice that this is just two or three years before Babcock worked on M31. This fact also casts an interesting light on what Babcock, quoted above, interpreted as Hubble's cold reception of his spectrographic rotation curve of M31. Perhaps Hubble was reluctant to commit to such rotation so soon after attributing van Maanen's spurious rotation to unknown systematic measurement errors — especially since the conflict with van Maanen seems to have caused Hubble a great deal of stress.[42] A Ph.D. candidate's non-Keplerian rotation curve might have initially seemed to Hubble to be just as implausible as van Maanen's superluminal rotation. One can imagine the young Babcock walking into that situation unawares and misinterpreting the great man's reactions. Hubble must have changed his mind, of course, since he prominently referenced Babcock's work on M31 in an important paper just a few years later.[43]

4.2. *Why was 1974 the turning point for the acceptance of dark matter?*

The foregoing explains why Babcock's result received almost no attention when it was first published.[44] Why did it continue to be ignored into the 1970s, and more generally why did it take so long for dark matter studies to gain momentum? Part of the answer, at least, can be found in a constellation of historical, institutional and sociological factors. From the 1920s through the 1950s, "the properties of stars and their life histories had dominated the interests of astrophysicists in the United States and elsewhere".[45] The Second World War interrupted astronomy generally; many physicists and astronomers took jobs working on defence projects. After the war, the focus of the discipline shifted. Radio astronomy and cosmology, including related observational studies of cosmic expansion and the structure and evolution of stars, were some of the areas of interest. Galactic dynamics was simply not a hot topic.

Trimble writes:

> Two fairly obvious things kept [the period 1939–61] from being a golden one for dark matter research. First, there was a war going on and then a good deal of postwar reconstruction.... Second, it was a golden age of stellar structure and evolution research, because a basic understanding of nuclear reactions as the energy source for the sun and stars had been achieved just as WWII broke out and because computational power was rapidly expanding during and after the war. Thus many outstanding astronomers focused on stars.[46]

The annual reports of the National Science Foundation (NSF) from 1952 through 1970 reveal that common topics of research in astronomy (as far as can be determined from the titles of the successful NSF individual research proposals) included: stellar composition and evolution; variable stars; binary stars; stellar motions; the interstellar medium; studies of the Sun; observations of asteroids, the planets and the Moon; supernovae; and radio astronomy. A very small proportion (well under 5%) of these grants went to projects that were even loosely related to galactic structure and dynamics. Similarly, only a handful of the hundreds of NSF-sponsored conferences and scientific seminars in this twenty-year span were related to galactic structure and dynamics. Insofar as funding trends are indicative of real research trends in the field, the NSF annual reports reinforce the idea that studies of the structure and dynamics of galaxies and clusters of galaxies were of limited interest before the 1970s. (But note that this information about NSF funding is not a definitive indication of what topics were considered important in the field because a significant portion of the astronomical research in this period was supported by well-funded private organizations such as the Carnegie Foundation that operated the Mount Wilson and Palomar Observatories.)

The factors just mentioned provide a plausible explanation for why dark matter research did not gain much momentum before 1970. Now consider the question of why the dynamical discrepancy was finally (and relatively suddenly) taken seriously in the 1970s. Three factors come to the fore.

First, as mentioned by Trimble,[47] two important review articles summarizing the dynamical evidence then available happened to appear in leading journals in 1974. For members of the astronomical community, seeing all the various lines of evidence gathered together had a strong psychological impact that converted many to believers in dark matter; as Trimble puts it, "Since then, a mainstream astronomer who seriously doubted that we are somehow not readily seeing 90% or more of the stuff of the Universe has found himself in the position of having to justify his discordant views".[48] This was especially true since there was a clear indication that the mass discrepancy increased linearly with the scale of the system being considered — from the local Milky Way to galaxies to clusters of galaxies, the proportion of dark matter present was greater and greater. The fact that the mass discrepancy appeared in systems of various kinds at various distance scales helped push aside some of the objections that had been raised about dark matter in clusters — that cluster members were not actually gravitationally bound, or that the high measured velocities were foreground effects rather than real traces of a dynamical discrepancy. These sorts of objections do not apply to galactic rotation curves, for example, and so in the context of the total evidence the apparent strength of those objections was diminished. Even if foreground effects did pollute some cluster measurements, the evidence still clearly called for dark matter (or a new theory of gravity, though this option did not garner much attention for some time).

Second, in 1973 another paper[49] had used numerical simulations to prove that the spiral structure observed in many galaxies is dynamically unstable unless spirals are embedded in massive spherical haloes. This provided a theoretical reason to expect rotation curves like those produced by Babcock for M31 since it now seemed necessary for there to be a great deal of invisible mass beyond the edge of the visible disk. Given a spherical halo of great mass extending well beyond the optical edge of a spiral galaxy, the rotation curve of the optical part has to be flat or rising.

Third, by the mid-1970s the so-called 'marriage of particle physics and astronomy' had been consummated and one fruit of that union was that particle physics finally provided a framework for thinking about what the dark matter could be, namely exotic fundamental particles with unusual properties. In particular it may be noted that cosmological arguments based on the production of light elements in the earliest moments after the Big Bang suggested that the universe should be full of neutrinos — nearly undetectable, individually almost massless but in such large quantities that together they were expected to constitute the majority of the total mass of the universe. Although the initial theories about the particulate nature of dark matter proved ultimately to be untenable, the idea that there exist fundamental particles that are individually very difficult to detect yet collectively massive enough to be the dark matter (at astrophysical as well as cosmological scales) has persisted to the present time. This is the case despite the fact that almost every candidate that has been explicitly described so far has turned out to not be empirically adequate to the totality of the evidence available, and despite the fact that none of the remaining candidates has any particularly strong evidence in its favour. Weakly interacting massive particles (WIMPs, fundamental particles predicted by the Standard Model of particle physics that interact with baryonic matter only through the weak nuclear and gravitational forces) remain the preferred potential solution to the dynamical discrepancy and for the missing mass at cosmological scales as well.

In short, then, the maturation of the discipline by the mid-1970s, to the point where the theoretical resources became available to begin to see possible solutions to the dynamical discrepancy — even to expect the sort of rotation curves that Babcock had produced — finally gave astronomers the space in which to work seriously on questions related to dark matter. Without this framework on which to hang their ideas, so to speak, astronomers had no way to understand the problem let alone move toward its solution. The advent of this theoretical framework, by itself, may be the most significant reason that the dynamical discrepancy began to be taken seriously at this time and not earlier.

5. *Conclusion*

Today, the dynamical discrepancy in astrophysical systems is accepted as one of the most important open problems in the astronomical sciences. Both observational and theoretical studies continue to occupy significant journal space.[50] Many different projects attempting to directly detect dark matter particles have been seriously pursued since at least the 1980s. Unfortunately, no candidate particles have been confirmed to exist, though many have been ruled out. The evidential context also leaves open the possibility — however unpopular it may be within the scientific community — that the solution will involve a revision to the action of gravity over very long distances.[51]

Horace Babcock's work is an important piece of the early history of dark matter studies that probably deserved more attention than it received. As Sandage puts it,

> Babcock was the first to discover '*dark matter*' in the outer regions of [M31]. He was, of course, justly proud of this major discovery, and became deeply disappointed in later years when the discovery, repeated by the next generation of astronomers, was credited in the public media to others with no mention of its originator. But it was characteristic of Babcock to avoid self-promotion, never trying to sell his discoveries, and therefore often failing to receive the proper credit for his multiple accomplishments. It was this characteristic of quiet competence that made him such an effective director in later years of the joint observatories of Mount Wilson and Palomar.[52]

Sandage exaggerates slightly here: in his published work Babcock himself never suggested that anything like what we now think of as dark matter was responsible for the rotation curve he measured in M31. Still, Babcock's apparent disappointment in not being recognized as the first to discover the discrepancy between observed rotation curves in spirals and the theoretical expectation based on their light profiles is quite natural.

Given the length of time it took the astronomical community to come to terms with the evidence for dark matter, it is reasonable to say that no single scientist was the discoverer of dark matter. However, Horace Babcock does deserve to be credited with priority in the discovery of the dynamical discrepancy in spiral galaxies. His work on M31 may not have been influential in the sense of inspiring the community of researchers to follow his lead, but he was both first and correct. While Babcock deserves credit for discovering the non-Keplerian rotation curve in M31 and recognizing that it poses a significant problem for current theories, we cannot fairly say that he discovered dark matter in the current sense of the phrase. This is not a shortcoming in Babcock or an opportunity he squandered; rather, dark matter simply could not have been discovered at the time Babcock was working on M31 because so many aspects of the background information and evidence were then still several decades in the future.

The main missing element in the study of dark matter before the 1970s was a theoretical framework for understanding what potential solutions to the dynamical discrepancy could possibly be like, and this was not formulated until particle physics and cosmology put forward the notion of the existence of large quantities of nearly-undetectable fundamental particles. That this development would take almost forty years from the initial discovery of the dynamical discrepancy was not something that anyone could have anticipated. That we have still not yet settled on a solution almost another forty years later is perhaps even more surprising, especially given the rapid pace of scientific research today.

For the reasons discussed in this paper, it is easy to understand why Babcock's result fell into obscurity for so many years despite its correctness and significance. This was not a case of the unjust treatment of a junior scientist by a conservative establishment. Significant figures in astronomy did think highly of Babcock's spectrographic rotation curve of M31 and agreed with the conclusions he drew from it — they just did not know what to make of the result. The cold reception Babcock felt he got from Hubble can possibly be attributed to Hubble's negative personal feelings about the controversy over van Maanen's spurious rotations, rather than having anything to do with Babcock's own work. The van Maanen episode may also have contributed to other astronomers' reluctance to take up galactic rotations in this period. Hubble must ultimately have come to see value in Babcock's work on M31 since he included it as part of his proof regarding the direction of rotation in spirals. Many observational papers in this period received little or no discussion in the subsequent literature; so the fact that Babcock's paper received so few citations does not distinguish it from many others and is not a sign of the astronomical community's tacit dismissal of his work. Finally, Babcock's scientific bibliography shows continuity rather than a change of direction after his dissertation research: after studying M31 he simply returned to the kind of work he was already doing beforehand. Babcock made a highly successful career studying the spectra of stars, with just a brief foray into galactic spectroscopy.

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2004. Parts of the present work were also presented at the Seven Pines Symposium, "The Unknown Universe: Dark Energy and Dark Matter", in Minnesota in 2008. I am grateful to Michel Janssen, Lee Gohlike and the organizers for inviting me, and I benefited greatly from interactions with the participants at that conference. The paper then unfortunately sat in my files for years after I started working in administration, until I was finally able to complete it in 2013. I am grateful to George Dehner and to the editor and referees of this journal for helpful comments on this version.

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2. Horace W. Babcock, "On the rotation of the Andromeda Nebula", Ph.D. diss., University of California, Berkeley, 1938.
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13. Babcock, *op. cit.* (ref. 1), 50.
14. Some of the other observations of note include the following: Erik Holmberg, "A study of double and multiple galaxies together with inquiries into some general metagalactic problems", *Annals of*

the Observatory of Lund, vi (1937), 162–173; M. Schwartzschild, "Mass distribution and mass–luminosity ratio in galaxies", *Astrophysical journal*, lix (1954), 273–84; F. Kahn and L. Woltjer, "Intergalactic matter and the Galaxy", *Astrophysical journal*, cxxx (1956), 705–17; Thornton Page, "Radial velocities and masses of double galaxies", *Astrophysical journal*, cxvi (1952), 63–80; "M/L for double galaxies, a correction", *Astrophysical journal*, cxxxvi (1962), 685–6.

15. Virginia Trimble's work on the history of dark matter has appeared in various astronomical venues since the 1980s. See, for example: "History of dark matter in galaxies", in Terry D. Oswalt and Gerard Gilmore (eds), *Planets, stars and stellar systems*, v: *Galactic structure and stellar populations* (Dordrecht, 2013), 1091–118; "Dark matter", in Noriss S. Hetherington (ed.), *Encyclopedia of cosmology: Historical, philosophical and scientific foundations of modern cosmology* (New York, 1993), 148–58; "History of dark matter in the universe (1922–1974)", in B. Bertotti, R. Balbinot and S. Bergia (eds), *Modern cosmology in retrospect* (Cambridge, 1990), 355–62; "Dark matter in the universe: Where, what, why?", *Contemporary physics*, xxix (1988), 373–92; and "Existence and nature of dark matter in the universe", *Annual review of astronomy and astrophysics*, xxv (1987), 425–72. Some works of Sidney van den Bergh cover similar ground, for example: "A short history of the missing mass and dark energy paradigms", in Vicent Martínez, Virginia Trimble and María Jesús Pons-Bordería (eds), *Historical development of modern cosmology*, Astronomical Society of the Pacific conference series, cclii (2001), 75–84; and "The early history of dark matter", *Publications of the Astronomical Society of the Pacific*, cxi (1999), 657–60.
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17. Trimble in Oswalt, *op. cit.* (ref. 15), 1100.
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22. Robert W. Smith, "Beyond the Galaxy: The development of extragalactic astronomy 1885–1965, part 2", *Journal for the history of astronomy*, xl (2009), 71–107, p. 73. In the passage quoted Smith is making a general point; he is not referring to Babcock or early dark matter studies.
23. In the summer of 2002, I spent several weeks in the library at the Harvard-Smithsonian Center for Astrophysics in Cambridge, Massachusetts. Part of the work I carried out there was a close examination of all the titles and abstracts published from 1930 to 1950 in the main astronomy journals, in key observatory reports, and in the astronomy sections of *Science abstracts*. The pattern that emerged was very clear: astronomy papers in that period almost never explicitly included critiques or contradictions of prior work. There are, of course, exceptions to this pattern. Compared to other academic fields (for example, philosophy), however, the relative lack of inter-professional critique is remarkable.
24. Albert van Helden, "Building large telescopes, 1900–1950" and John Lankford, "The impact of photography on astronomy", both in Owen Gingerich (ed.), *Astrophysics and twentieth-century astronomy to 1950: Part A* (Cambridge, 1984).
25. Abell, *op. cit.* (ref. 7).
26. Osterbrock, *op. cit.* (ref. 6), does not mention his source for this information, though the situation is described very similarly in Allan Sandage, "Horace Welcome Babcock", *Proceedings of the American Philosophical Society,* cl (2006), 151–60, http://www.amphilsoc.org/sites/default/files/proceedings/150108.pdf, accessed 20 December 2013. The Babcock papers at the Huntington Library in California are from his time as observatory director and (at least according to the finding aid) they do not contain any materials relevant to the reception of Babcock's work on

M31. Note that the McDonald Observatory conference title Osterbrock gives in the quotation, Dynamics and Structure of Galaxies, is probably a slip. Oort, who spoke at the event, refers to it as “the symposium on galactic and extragalactic structure”: Jan Oort, “Some problems concerning the structure and dynamics of the galactic system and the elliptical nebulae NGC 3115 and 4494”, *Astrophysical journal*, xci (1940), 273–306, p. 273 n. 1. Note, too, that while Trimble’s writings seem to run together a meeting of the American Astronomical Society and the McDonald dedication conference, Helen Sawyer Hogg’s (delightful) first-person account of the McDonald dedication and surrounding events refers to a nearby meeting of the southwestern division of the American Association for the Advancement of Science (AAAS rather than AAS) followed by a ‘symposium on galactic and inter-galactic structure’ associated with the observatory dedication itself: Helen Sawyer Hogg, “The dedication of the McDonald Observatory”, *Journal of the Royal Astronomical Society of Canada*, xxxiii (1939), 201–9, p. 204 and p. 207. The University of Texas library system holds an item entitled, “Program of the dedication of the McDonald Observatory and astronomical symposium on galactic and extragalactic structure, Fort Davis, Texas, May 5–8, 1939”.

27. Sandage, *op. cit.* (ref. 26), 154.
28. Oort, *op. cit.* (ref. 26), 305.
29. Interview of Horace W. Babcock by Spencer Weart on 25 July 1977, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD, USA, http://www.aip.org/history/ohilist/1038.html, accessed 20 December 2013. I am grateful to the Niels Bohr Library of the American Institute of Physics for permission to quote from this oral history interview.
30. See ref. 5.
31. Horace W. Babcock, “Radiations of the night sky photographed with a grating”, *Publications of the Astronomical Society of the Pacific*, li (1939), 47–50.
32. For details of Babcock’s illustrious later career see: Abell, *op. cit.* (ref. 7); Weart’s interview with Babcock, *op. cit.* (ref. 29); Arthur Harris Vaughan, “Obituary: Horace Welcome Babcock, 1912–2003”, *Bulletin of the American Astronomical Society*, xxxv (2003), 1454–5; George W. Preston, “Horace Welcome Babcock (1912–2003)”, *Publications of the Astronomical Society of the Pacific*, cxvi (2004), 290–4; Sandage, *op. cit.* (ref. 26); and George W. Preston, “Horace Welcome Babcock (1912–2003): A biographical memoir” (Washington, DC, 2007), http://books.nap.edu/html/biomems/hbabcock.pdf, accessed 23 December 2013.
33. Babcock, *op. cit*. (ref. 1), 51.
34. See, for example: Smith, *op. cit.* (ref. 22), 71, and Robert W. Smith, “Beyond the Galaxy: The development of extragalactic astronomy 1885–1965, part 1”, *Journal for the history of astronomy*, xxxix (2008), 91–119, p. 91.
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39. Edwin P. Hubble, “Angular rotations of spiral nebulae”, *Astrophysical journal*, lxxxi (1935), 334–5.
40. Smith, *op. cit.* (ref. 22), 74. As part of the ‘settlement’ imposed by Adams, van Maanen published a companion piece to Hubble’s paper acknowledging that systematic errors “make it desirable to view the motions with reserve”: Adriaan van Maanen, “Internal motions in spiral nebulae”,

Astrophysical journal, lxxxi (1935), 336–7, p. 337.

41. See, for example, Noriss S. Hetherington, *Science and objectivity: Episodes in the history of astronomy* (Ames, IO, 1988), 83–110.
42. Smith, *op. cit.* (ref. 22), 73.
43. Hubble, *op. cit.* (ref. 39). A referee for this journal points out another possibility, namely that that Hubble did not change his mind, and was not really cold towards Babcock's results, and that Babcock was misremembering events. The referee further suggests that since Mayall was something of an acolyte of Hubble, it is plausible to think that Hubble would have known about Babcock's rotation curve before the two visited. These are good points.
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