How dark matter came to matter

A history of missing mass, 1930-1974

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CONCLUSION

"It has long puzzled me why senior folks [...] did not SHOUT about this promptly from the rooftops," MIT astronomer Alar Toomre wrote in an email, referring to how the early observations of flat rotation curves were already indications for the existence of dark matter.[1] Why did people not see that flat rotation curves evidenced the existence of dark matter? Why was the evidence for dark matter not immediately recognized? This issue, in a sense, has been at the core of this dissertation. The central aim of this thesis was to show that the observations currently recognized as evidence for the existence of dark matter – the phenomena of flat rotation curves of galaxies and the discrepant velocities of galaxies in clusters – were not unequivocally recognized as such when they were observed. Although these observations are obvious evidence for dark matter in retrospect, this was not the case when these phenomena came to light.

In the case of flat rotation curves, there had been two groups who, around 1973, observed that the rotational velocity of several galaxies stayed constant at large radii: Seth Shostak and Dave Rogstad at Owen's Valley Radio Observatory, and Morton Roberts and Arnold Rots at NRAO and Westerbork Synthesis Radio Telescope. Both groups noted that their data did not match the previously assumed planetary-like description of rotations: the Brandt curve. Roberts advocated the existence of these flat tails of rotation curves, but, as Toomre noted, the flat rotation curves, and particularly the high mass-to-light ratio that was associated with them, were not directly embraced by the community: other observers had obtained contrary measurements that supported the old expectations that the rotation of galaxies should be planetary-like or "Keplerian."

The discrepancy in the velocity of galaxies in clusters had already been noticed by Fritz Zwicky and Sinclair Smith in the

1 Alar Toomre, correspondence with the author, 31 March 2015.

1930s for two clusters, the Coma and Virgo clusters. Both authors had suggested that the high velocities of galaxies in clusters could imply the existence of yet-to-be-detected mass that stabilized these clusters. In the 1950s, when more clusters with similar problems were observed, an alternative explanation was suggested by Viktor Ambartsumian: that the large velocities of galaxies in groups and clusters implied that clusters were exploding apart. The ambiguity in explaining the dynamical state of galaxy clusters remained until at least the mid-1970s. Around that time, space-based experiments also detected intergalactic gas in the Coma cluster and concluded there was too little mass to stabilize this cluster – intergalactic mass seemed not to be able to solve the problem.

So, to repeat Toomre's question, now using the words of Morton Roberts: "[w]hat took us so long to accept it?"[2] Let us turn this question around, and ask ourselves why researchers *did* see that these observations evidenced the existence of dark matter. As just noted, there were many reasons *not* to believe that the mentioned phenomena were evidence for dark matter. One of these reasons was that the determinations of masses of galaxies and clusters had been troubled by delicate uncertainties – e.g. sidelobes in the case of radio telescopes, and group membership decisions in the case of galaxy clusters. Only when the results from the rotations of galaxies and the dynamics of galaxy clusters were combined did they take definite form as evidence. This happened in the context of cosmological research that flourished in the early 1970s, and in which the mass density of the universe was an important parameter. The existing determinations of mass were translated into the new field of "physical" cosmology, and here, at the intersection of physics and several astronomical subfields, the object of missing mass emerged and acquired clear contours.

2 M. S. Roberts, "M 31 and a Brief History of Dark Matter," in *Frontiers of Astrophysics: A Celebration of NRAO's 50th Anniversary: Proceedings of the conference held 18-21 June, 2007, at the National Radio Astronomy Observatory, Charlottesville, Virginia, USA*, ed. Alan H. Bridle, James J. Condon, and Gerteh C. Hunt, vol. 395 (n.p.: ASP Conference Series, 2008), 288.

Flat rotation curves and the mass discrepancy in galaxy clusters were connected and identified as part of the same issue by Ostriker, Peebles and Yahil, and Einasto, Kaasik and Saar in 1974. Both groups of astronomers and physicists each published a paper in which they summarized available determinations of the masses of galaxies, and showed that these implied that this mass increased linearly with a galaxy's radius. Galaxy masses were 10 times larger than was previously thought, and the papers made a strong case for the existence of a "halo" (Ostriker et al.) or "corona" (Einasto et al.) of unseen mass around galaxies. Both groups concluded that, as a consequence, the mass density of the universe had also been underestimated by a factor of 10.

The manner in which they argued for the existence of this mass illustrates how explanations that drew together different distance scales started to have an important role in cosmological reasoning: dark matter was introduced because it could explain a series of observations at different astronomical scales with a single idea, and, at the same time, the explanation connected with the theoretical preferences for a closed universe that demanded a high value of the mass density of the universe. In these papers, the different cases in which astronomers had talked about missing and unseen mass on different scales were unified – the papers showed 95% of the mass of the universe consisted of something unseen.

WHO DISCOVERED DARK MATTER?

Given what we know now, what can we say about who "discovered" dark matter? Historians of science are often asked who should be credited for a scientific discovery. And so too have there been many who have pondered who should be accurately named the discoverer of dark matter. This thesis has hopefully shown two things: 1. that the usual answers given to this question are wrong; and, 2. that this very question is actually quite problematic.

First, let us start by understanding what is meant by the question "who discovered dark matter?" The question is slightly con-

fusing, because dark matter has not been discovered yet. The nature of dark matter – whether it is axion particles, black holes or otherwise – is yet unknown. If one talks of the discovery of dark matter, then what exactly is being talked about? It seems that in this case the question refers to the fact that there *is* dark matter, rather than not. A more correct version of the question then would be "who discovered that dark matter exists?" (Do note that there is no guarantee that dark matter should exist; some researchers have suggested the problem can be solved with alternative theories of gravity. So, to make things more difficult, a still better version of the question seems to be: who discovered that dark matter should exist, if it indeed exists?) This question gives a strange sense of discovery – discovering without having knowledge of what is discovered – but let us stick with it for now.

Who would be a potential candidate for the title of "discoverer of dark matter's existence"? Fritz Zwicky has sometimes been called the "father" of dark matter. This title is apt if you consider he indeed drew attention to the issue with the dynamics of galaxies in clusters. But Zwicky did not advocate any particular solution to this problem: his conclusion was that clusters were ill-understood, not that dark matter should exist. It is also incorrect and confusing to suggest that Zwicky "coined" the term dark matter. First of all, the term had been used by others before him (e.g. Jacobus Kapteyn). Secondly, we should not forget it was not yet a *name* for a phenomenon or object – like "quasar" was for particular high-energetic radio emitter, or "Pluto" was for a planet. Rather, it should be considered no more than a conjunction of an adjective and a noun, that did not bear the intricate meaning it does today. While Zwicky put the problem with galaxy clusters on the map, he did not discover that dark matter exists.

Horace Babcock could be another candidate for the title. Babcock showed that, contrary to planetary or "Keplerian" orbits, the rotational velocity of Andromeda did not decrease but kept increasing with radius – he showed this velocity was increasing *linearly* with radius. His observations were interpreted by many in the 1940s as showing that galaxies at their edges have high mass-to-light ratios, indicating the existence of dark matter. But Bab-

cock's Ph.D. supervisor Nicholas Mayall came to different conclusions when he studied Andromeda in the 1950s. Also the more detailed work in the early 1970s of Roberts, and Rubin and Ford, did not reproduce Babcock's linear relation. In other words: Babcock's results were shown to be *incorrect*. As Gianfranco Bertone and Dan Hooper have shown, the data he found also does not comply with current estimates of the rotational velocities of galaxies.[3] This fact raises interesting questions, including questions on the historical importance of incorrect results. But mostly, it indicates that Babcock can not be credited with discovering that dark matter should exist.

Vera Rubin has had an enormous influence on the development of the dark matter problem. Yet, although the contrary is often suggested, her role was not that of a discoverer. In her work in 1970 with Kent Ford on Andromeda, the authors focused on the inner parts of the Andromeda galaxy, and made no conclusions on the mass-to-light ratios of the outer edges or otherwise concluded on the flatness of their rotation curve. The authors left whatever happened on the outer edges to be "a matter of taste." Rubin's influence on dark matter research came only later, from 1978 onward, when many scientists were already convinced of dark matter's existence.

Morton Roberts has been one of the few people who advocated that the rotation curves of multiple galaxies were indeed flat. He showed this for three galaxies together with Arnold Rots. But he did not "discover" rotation curves of galaxies were flat, as he relied on the work of many others who had done similar observations in the early 1970s. This included the work of Seth Shostak and Dave Rogstad, who had also shown different galaxies had flat outer ends of their rotation curves that might imply high-to-mass ratios at these radii. By 1974, only half of the rotation curves Roberts compiled were flat at large radii, and not all the early rotation curves implied there should be unseen mass.

Then what about Ostriker, Peebles and Yahil, and Einasto, Kaasik and Saar? These groups had recognized the existence of a new object by combining the work of several other authors and

3 Bertone and Hooper, "History of dark matter," fig. 4, p. 9.

relating it to the mass density of the universe. This dissertation has put a lot of emphasis on their work. But, as the Princeton group itself noted, they had done nothing new. Both groups had collected several sets of data. They themselves did not do a single observation. Their work also did not introduce any new theoretical concepts or calculations. While the object that we now know as dark matter, existing in galaxies and clusters, was shaped in these two papers, it would make very little sense to call their activity a "discovery" – if anything, it more resembled that of eloquent bookkeeping.

My point is this: there was no single discoverer of dark matter's existence. As already argued by for example Thomas Kuhn, trying to find a discoverer misleads us into searching for a discrete event, in which a single person at a specific moment showed something to exist.[4] This thesis has shown that this person or moment does not exist. There was not one person who at one moment acquired indubitable evidence for the existence of dark matter. One of the conclusions of this thesis then also is that the traditional portrayal of the history of dark matter, in which it is said that Fritz Zwicky and Vera Rubin proved dark matter to exist, is incorrect.

Similarly, this thesis has also argued against understanding the history of dark matter as an "accumulation of evidence." The problem with this view, as we can now see, is that it implies that there were discrete steps in which clear-cut evidence was found for a well-developed hypothesis. This was not the case: neither flat rotation curves nor the cluster stability problem were in discrete steps interpreted as clear-cut evidence for an explicit conception of dark matter. These observations only acquired their status as evidence for dark matter later, in the hands of theorists not involved with the observations.

Instead of searching for discrete events or discoverers, this thesis has followed dark matter as it took shape as an object. This has allowed us to study many different conceptual, institutional, technical, and philosophical dimensions in this history. Using the

4 Thomas S. Kuhn, "Historical Structure of Scientific Discovery," *Science* 136, no. 3518 (1962): 760–764. This point can also already be recognized in the work of Ludwik Fleck: Fleck, *Genesis and Development of a Scientific Fact*, 162.

words of Lorraine Daston, we followed "how a heretofore unknown, ignored, or dispersed set of phenomena is transformed into a scientific object," in our case that of dark matter in the universe, "that is capable of theoretical ramifications and empirical surprises, and that coheres, at least for a time, as an ontological entity."[5] We traced early forms, or proto-ideas, of this mass, from the 1930s to 1974, and argued that in 1974 this "dispersed set of phenomena" became a single entity. Through a cosmological unification, where several arguments were lined up, dark matter came to matter as a new object of study. But this "missing mass" did not just come to existence as an epistemic object, for astrophysicists to study, but also as an ontological entity which made up the majority of the universe.

"I don't think there were any other papers which would try to pull things together into a single convincing lecture about a need for unseen halos," Toomre noted in an interview.[6] This "pulling together" captures what was so distinctive about what happened in 1974: the work of the Princeton and Tartu groups drew together different existing mass measurements in a single coherent hypothesis. They converted figures and numbers into a new system of practice, where they obtained new meaning and a different weight. This new system of practice was physical cosmology, where the existing mass determinations of galaxies were now related to the mass of the entire universe. Physical cosmology was what philosopher Bruno Latour has called "centres of calculation": an environment, or strategic position, where observations, calculations and results from various different fields were gathered and new relations were shaped and objects were formed.[7]

What was so particular of physical cosmology, is that, in its practice, unifying explanatory narratives began to have evidential value: connecting different observations to the existence of a single phenomenon became a common way to argue for the reality

5 Daston, *Biographies of Scientific Objects*, 5.

6 Alar Toomre, interview with the author, 9 December 2014, Cambridge, Massachusetts.

7 Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, MA: Harvard University Press, 1987), 223-247.

of this phenomenon.[8] So, not only did already existing observations become evidence for the missing mass hypothesis, but the synthesis of these observations itself also supported the hypothesis. We now should understand that *observations are not evidence in and of themselves*; in our case observations acquired their status as evidence when they were pulled together and functioned in a single explanation. The importance of a cosmological narrative to make observations into evidence illustrates a dictum of Ludwik Fleck: "In the history of scientific knowledge, no formal relation exists between conceptions and evidence."[9]

The way that evidence has been discussed in this dissertation then opens up questions about how evidence for dark matter's existence is still addressed. What does it mean if one says there is "undisputed evidence" that dark matter exists? Or that a particular phenomenon is "smoking gun evidence" for dark matter? Understanding these expressions, not as formal statements but as contingent remarks, can give further insight in the continuous historical development of what it means to have evidence for something that is not directly observed. Similarly, what we learn from the history of dark matter, is that the widespread support for the particle dark matter scenario can be understood from the way it "pulls together" and explains so many different observations, ideas and results that relate to the cosmos. This is what gives the dark matter particle its sense of reality despite not being detected.

RECEPTION

In the 1974 papers, the Tartu and Princeton groups argued that masses of galaxies were ten times more massive than was previously thought, consisting for their majority out of unseen matter. Yet we should not conflate this moment in the history of the dark matter problem with the moment of its acceptance. It took some four more years before the synthesized issue of missing mass

8 These explanations resemble what Michel Janssen has called "Common Origin Inferences." Michel Janssen, "COI Stories: Explanation and Evidence in the History of Science," *Perspectives on Science* 10, no. 4 (2002): 457–522.

9 Fleck, *Genesis and Development of a Scientific Fact*, 27-28.

that these papers had introduced became more widely accepted. In the final pages of this dissertation I wish to give some indications of how the work of the Tartu and Princeton theorists was received in these years.

Not all were keen on the result of Ostriker and Einasto's groups. "People thought it was just crazy", Ostriker recollected in an interview.[10] "[P]eople reacted to [our argument] with very strong hostility," he said on another occasion.[11] Indeed, the papers and their ideas were controversial. We have already discussed the responses of some astronomers to the idea of flat rotation curves; particularly, the Cambridge group led by John Baldwin, and also Walter Huchtmeier, spoke unfavorable of the conclusion.[12] As discussed in the last chapter, there also had been critical responses to the numerical arguments for galactic halos that Ostriker and Peebles had introduced in 1973. At the 1973 IAU General Assembly in Canberra, Australia, Alar Toomre reviewed the topic of stability in galaxies and Ostriker and Peebles' new stability criterion: "a real migraine," he called it.[13] According to Ostriker, many people quickly confused the 1973 and 1974 papers and their conclusions, adding to the skeptical reception of the latter paper on the masses of galaxies and the universe.[14]

The immediate reception of the papers of Ostriker and Einasto's groups was indeed unsympathetic. Particularly critical of the conclusions of the papers was Geoffrey Burbidge, who had worked on both the stability of clusters of galaxies and the rotation of galaxies. Within a month after the publication of the paper by Ostriker's group, Burbidge drafted a condemning re-

10 Ostriker, interview with the author, 1 December 2014, Princeton, NJ.

11 Lightman and Brawer, *Origins: the Lives and Worlds of Modern Cosmologists*, 275.

12 Groningen radio astronomer Albert Bosma remembered that Gérard de Vaucouleurs also did not believe the results of the flat rotation curves. Bosma recalled an interaction with de Vaucouleurs about his results on flat rotation curves in 1976: "I showed him the great extent of the neutral hydrogen in one galaxy, and he just walked away." Albert Bosma, interview with the author, 24 June 2014, Amsterdam, the Netherlands.

13 Alar Toomre, "How Can It All Be Stable?," in *Highlights of Astronomy , Volume 3. As presented at the XVth General Assembly of the IAU, 1973.*, vol. 3 (Dordrecht: Reidel, 1974), 457.

14 Ostriker, in Lightman and Brawer, *Origins: the Lives and Worlds of Modern Cosmologists*.

sponse. The paper opened as follows: "Contrary to the results obtained by Einasto *et al.* and Ostriker *et al.*, we show that there is no unambiguous dynamical evidence which demonstrates that galaxies have very massive halos."[15] Burbidge analysed all the arguments of the two papers, from rotation curves and double systems, to clusters of galaxies. His main counterargument was a point that had also been made by Ambartsumian almost two decades earlier: that some observations indicated that groups and cluster might be unstable. As Burbidge wrote, the mass increase observed by the two groups "can be considered a real effect only if it is *assumed* that each system is a physical system and furthermore, a bound system."[16] Others who endorsed this critique were Jürgen Materne and Gustav Tammann, of the Hamburg Observatory. They added that the large masses of galaxies also conflicted with observations of the Hubble flow – the smoothness in which the universe seemed to be expanding in every direction – which indicated that galaxies could not be so dense.[17]

But despite these initial critiques, Ostriker expressed strong confidence about their newly found cosmic contribution of mass. He emphasized its importance at an invited talk at the National Academy of Sciences in New York: "[the problem] arises when dynamical questions are asked, including the basic one of whether the Universe will expand forever or whether gravitation can ultimately reverse the expansion."[18] His calculations with Peebles and Yahil implied that "most of the mass is not in ordinary (solar) type stars, but some other dark form," Ostriker noted.[19] Ostriker, who was the editor of a relatively new journal called *Comments on Astrophysics*, also asked Roberts to write a piece on rotation curves for the journal. In it, Roberts elaborately

15 G. R. Burbidge, "On the masses and relative velocities of galaxies," *The Astrophysical Journal* 196 (1975): L7.

16 Ibid., L9.

17 J. Materne and G. A. Tammann, "On the stability of groups of galaxies and the question of hidden matter.," in *Stars and Galaxies from Observational Points of View, Proceedings of the Third European Astronomy Meeting, Tbilisi, Georgia*, ed. E.K. Kharadze (Tbilisi, Georgia: Mecniereba, 1976).

18 J. P. Ostriker, "Galaxies: Outstanding problems and instrumental prospects for the coming decade (A Review)," *Proceedings of the National Academy of Sciences* 74, no. 5 (1977): 1772.

19 Ibid., 1773.

discussed flat rotation curves.[20] Einasto kept defending the conclusions of his co-authored paper too, and helped set up a session devoted to the hidden matter problem at the Third European Astronomical Meeting, in 1975, in Tbilisi, Georgia.[21]

Ostriker and Einasto were not alone in supporting their hypothesis. We have seen that Jan Oort on multiple occasions had supported the idea that galaxies might be more massive than was traditionally thought. Among other supporters was Berkeley astronomer Ivan King, who had worked on the dynamics of clusters of galaxies. "The most serious problem in extragalactic astronomy today is the notorious 'missing mass'," King said in 1977, during a conference on the evolution of galaxies at Yale University. "It is an extremely disquieting one," he noted, "[c]an we really claim to know anything about the nature of the universe if we don't know the properties, or even the nature, of 90 per cent of its material?"[22]

Cosmologists were among the first to accept the existence of dark material in the universe. This was particularly clear in work on the large scale evolution of mass in the universe. In Cambridge, where the missing mass had been a much discussed conundrum, Martin Rees and a recently graduated student, Simon White, introduced an influential theory for galaxy formation in 1978 in which the existence of dark matter was one its premises. They wrote:

> A satisfactory theory of galaxy formation must account for the large amount of non-gaseous 'dark matter' which apparently provides 80 per cent of the virial mass in clusters like Coma and which may constitute massive halos around large galaxies.[23]

20 E.J. Öpik, "Comments on astrophysics and space physics," *Icarus* 11, no. 3 (1969): 437; M. S. Roberts, "The Rotation Curves of Galaxies," *Comments on Astrophysics* 6, no. 4 (1976): 105–111.

21 Einasto, "Cosmology Paradigm Changes," 18-19.

22 Ivan R. King, "Introduction: Galaxies and Their Populations - The View on a Cloudy Day," in *The Evolution of Galaxies and Stellar Populations (Conference at Yale University, May 19-21, 1977)*, ed. B.M. Tinsley and R.B. Larson (New Haven, CT: Yale University Observatory, 1977).

23 S. D. M. White and M. J. Rees, "Core condensation in heavy halos: a two-stage theory for galaxy formation and clustering," *Monthly Notices of the Royal Astronomical Society* 183, no. 3 (1978): 342.

Dark matter became an integral part of thinking about structure in the cosmos – a topic which both Jim Peebles and Jaan Einasto worked on extensively in the 1970s. In this context, missing mass was no longer a curious concept but a central thesis on which the theory of structure formation was being built; dark matter was no longer an outcome, but the input on which theories of the universe started being constructed.

Around the same time new work was done on double systems of galaxies that supported the dark matter hypothesis, and a large number of new rotation curves were published of which many showed flat features.[24] The latter included 14 rotation curves from Nathan Krumm and Edwin Salpeter with observations from the Arecibo radio telescope, 25 curves from Albert Bosma with the Westerbork Radio Synthesis Telescope, and 10 curves from Vera Rubin, Kent Ford and their colleague Norbert Thonnard with the Kitt Peak telescope, using their image tube technology.[25] The latter group noted that their observed flat rotation curves "take on added importance in conjunction with the suggestion of Einasto, Kaasik, and Saar (1974), and Ostriker, Peebles, and Yahil (1974) that galaxies contain massive halos extending to large r."[26] Roberts himself reflected in an interview that "[t]o unambiguously establish [flat rotation curves], it took Bosma's thesis." This, Roberts argued, was because Bosma showed rotation curves to be flat for such a large amount of galaxies, and that they were flat beyond the optical regions of galaxies.[27] Bosma's work "overwhelmingly confirmed" that the old extrapolations of the Brandt curve did not hold and rotation curves were flat, a 1979 review paper read.[28]

24 E. L. Turner, "Binary galaxies. II. Dynamics and mass-to-light ratios.," *Astrophysical Journal* 208 (1976): 304–316.

25 N. Krumm and E. E. Salpeter, "Rotation curves, mass distributions and total masses of some spiral galaxies.," *Astronomy and Astrophysics* 56 (1977): 465–468.Rubin et al., "Extended rotation curves of high-luminosity spiral galaxies. II - The anemic SA galaxy NGC 4378"; Bosma, "The distribution and kinematics of neutral hydrogen in spiral galaxies of various morphological types."

26 Rubin et al., "Extended rotation curves of high-luminosity spiral galaxies. II - The anemic SA galaxy NGC 4378," L111.

27 Morton Roberts, interview with the author, 20 November 2014, Alexandria, Virginia.

28 Faber and Gallagher, "Masses and Mass-To-Light Ratios of Galaxies," 144.

This review paper, written by Sandra Faber and John Gallagher of Lick Observatory and University of Illinois, represented how the sentiment had changed: by 1979 the astronomy community largely embraced the conclusions of Einasto, Kaasik, and Saar, and Ostriker, Peebles and Yahil. Faber and Gallagher's 53-page review paper, titled "Masses and Mass-to-Light Ratios of Galaxies," was "especially concerned with the current status of the 'missing mass' problem," the authors wrote.[29] The paper covered all the different parts of the argument in detail, discussing mass determinations on the basis of rotation curves, double galaxies, small groups, and large clusters. "After reviewing all the evidence, it is our opinion that the case for invisible mass in the Universe is very strong and getting stronger," they wrote in their conclusion. "We think it likely that the discovery of invisible matter will endure as one of the major conclusions of modern astronomy."[30]

By 1979, the idea that the universe consisted of much more mass than can be seen had found wide support. This did not mean that discussions on the evidence for the dark matter hypothesis stopped completely. Where galactic astronomers and their rotation curves were central to establishing the argument, they also were the ones who were most critical of the hefty conclusions that were drawn from their observational work. As astronomer Robert Sanders has illustrated, one counter argument came from Agris Kalnajs, an astronomer at the Australian Mt. Stromlo Observatory.[31] In 1982, during a IAU Symposium in Besançon, Kalnajs showed that many of the observed flat rotation curves could be modeled with the traditional idea of a *constant* mass-to-light ratio. "[E]veryone came back from that meeting and said 'See. We don't need massive halos. There's no more dark matter. At last we got rid of it. It's all wrong.'," Ostriker recollected in an interview.[32]

Kalnajs' point, besides further triggering the interest of astronomers working on galaxies, in the end had little impact on

29 Faber and Gallagher, "Masses and Mass-To-Light Ratios of Galaxies," 136.
30 Ibid., 182.
31 Sanders, *The Dark Matter Problem: A Historical Perspective*, 57-68.
32 Lightman and Brawer, *Origins: the Lives and Worlds of Modern Cosmologists*, 276.

the acceptance of the dark matter hypothesis. His remarks came at a time when the existence of dark matter already had been accepted by many scientists in and, most of all, outside of astronomy. By 1982, particle physicists had become dominant in the discussion of the dark matter problem. These physicists did not discuss whether dark matter exists, but, instead, *what* it consists of. In the early 1970s, it had been suggested that dark matter consisted of neutrinos, and by the early 1980s there were many reasons to believe this particle would indeed have the exact mass that was needed. In the end, the neutrino turned out not be the right dark matter "candidate." But the idea that the dark matter was an elementary particle stuck, and by the mid-1980s, experiments to detect the yet-unknown dark matter particle started being proposed – the first of many experiments to come. The history of how dark matter became a particle, however, is the topic of another book.