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How Much is There of What? Measuring the Mass Density of the Universe

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Abstract

The density of the Universe is, by general agreement, one, in suitable units and with somewhat uncertain partition among a number of constituents, of which perhaps 20%-30% is positive-pressure matter. The present contribution addresses methods of measurement, mainstream results, the more popular constituents, unresolved issues, and some less popular candidates and views, not all quite in this order.

1.1 Introduction

Modern cosmology takes as given general relativity as the correct description of spacetime, gravity, and their relationship (at least back to the time of big bang nucleosynthesis and before) and the interpretation of the velocity-distance relation (Hubble's law) as the expansion of a relativistic spacetime. I will tell you if we get to a paragraph where suspension of belief in this foundation is required.

Hubble (1934) himself reported a density for the Universe of $(1.3-1.6) \times 10^{-30}$ g cm^{-3}, based, he said, on an average nebular mass of $(6-8) \times 10^{8}\, M_{\odot}$ and a nebular density of $(20-30) Mpc^{-3}$. Hubble, incidentally, always said and wrote "nebulae." Galaxies was Shapley's word.

Hey, not half bad, you may say, until you remember, first, that his value of H_0 was 536 km s^{-1} Mpc^{-1} (accurate to within ± 10%, just like all modern values), so that the critical density was 5.4×10^{-28} g cm^{-3}, and, second that Zwicky (1933) had already published a velocity dispersion for the Coma cluster implying a mass-to-light ratio for its members nearly a factor 100 larger than the 3 $M_{\odot}/L_{\odot}$ assumed by Hubble to get from galaxy brightnesses to galaxy masses. If you would like to say that he was measuring the density in luminous, stellar material and got 0.3% of the closure density, you will again be impressed (this time correctly) by the nearness to modern numbers.

But darker clouds were already gathering, with Sinclair Smith (1936) reporting an average mass per nebula of $2 \times 10^{11}\, M_{\odot}$ for members of the Virgo cluster whose radial velocities he had measured, Eric Holmberg (1937) finding an average of $10^{11}\, M_{\odot}$ from velocity differences for a handful of binary galaxies (in work inspired by Knut Lundmark, another undersung hero of this wild territory), and Horace Babcock's (1939) rotation curve for M31 with a last, fuzzy data point at 380 km s^{-1}, implying a mass of $10^{11}\, M_{\odot}$. All of these were for a Hubble parameter of 500 km s^{-1} Mpc^{-1}, so you can multiply them by some favorite number between 5 and 10. Notice that all the methods—cluster velocity dispersions, pairwise velocity differences, and rotation curves—are still in use.

Another modern method belongs to this pre-historic period, gravitational lensing of the sort we now call strong, of one galaxy by another, advanced by Fritz Zwicky (1937) first as a way of confirming general relativity, second as a way of studying distant galaxies that would otherwise be too faint to see, and third as an independent way of measuring the masses of the galaxies that function as lenses.

Then, of course, there was a war and a period during which a large fraction of creative young astronomers concentrated on stellar physics (because of the 1939 breakthrough on hydrogen fusion as the dominant energy source and the post-war advent of digital computers), but, by 1961, at the time of a pair of conferences associated with the Berkeley IAU General Assembly (Neyman, Page, & Scott 1961; McVittie 1962), it was generally accepted that the large velocity dispersions of clusters of galaxies, especially the ones that look relaxed and smooth, constitute a problem that astronomers should be thinking about.

Over the next few years (a) Abell (1965) estimated the density of the Universe to be 10^{-30} g cm^{-3} for $H_0 = 75$ km s^{-1} Mpc^{-1} (that is, 10% of closure density) if his superclusters had masses of 10^{16} $M_\odot$ and filled 10% of space, (b) what we now call weak lensing slipped quietly into the literature (Gunn 1967, though it was initially entitled "a fundamental limit on the accuracy of angular measurements in observational cosmology" and portions of the idea credited to unpublished thoughts of Richard Feynman), and (c) the third primary method of measuring masses for large structures was conceived. A 1965 NRL rocket flight had seen X-rays coming from the general direction of the Coma cluster. Felten et al. (1966) tried to account for these as both nonthermal and thermal emission and ended up favoring the latter. They asked the question, could all the "missing mass" of the cluster (4×10^{48} g) be in hot gas, and, if so, what temperature must the gas have in order to emit all the X-rays? Their answers were yes, and about 10^8 K, close to what you would suppose from the cluster velocity dispersion of 1050 km s^{-1}. This consideration lay nearly dormant until the *Einstein Observatory* began to return a few cluster images with some angular resolution in both surface brightness and temperature (Fabricant, Lecar, & Gorenstein 1980). Thus a review soon after (Forman & Jones 1982) could say with some confidence that a handful of nearby clusters indeed have mass-to-light ratios of 100 or thereabouts and that X-ray gas makes up only about 10% of the total.

Meanwhile, by 1974 (Einasto et al. 1974; Ostriker, Peebles, & Yahil 1974) dark matter had overtaken its competitors as the most likely explanation for large apparent M/L's on all scales. For additional historical material and references see Trimble (1987, 1995, 2001)

1.2 Methods

If you want to measure density, you need two things, a volume and the mass within it. As a rule, volume is easier, provided that someone tells you the correct value of H_0 (Dr. Freedman at this meeting). Not all volumes are spherical, which can be source of systematic error, due, for instance, to end-on prolate clusters being over-represented among X-ray or lensing samples. How large a volume do you need? Well, the whole observable Universe is generally not a good choice (as one sometimes has to explain to students and science journalists), but neither is one too small to be representative. Luckily, the Universe seems not to be fractal or hierarchically structured beyond a few hundred Mpc, and voids and superclusters like the ones near us simply repeat. This means that de Vaucouleurs (1970) was wrong when he said that it was incredibly improbable that he should be writing just when, after centuries of growth in our ideas of cosmic horizons, the largest structures had finally been found. Incredibly improbable, perhaps, but apparently true. Nothing more will be said about volumes here, and we will carry a spare $h = H_0/100$ km s^{-1} Mpc^{-1} around with us for use when needed.

Now, what about measuring masses? This is a good deal harder. You still need somebody to tell you H_0, and there may not be any electromagnetic radiation coming from the volume you want to know about (Kraan-Korteweg et al. 1999). All of the tactics mentioned in § 1.1 are still in use, and some additional ones might be contemplated. They are mentioned here with a cross reference if a speaker focused on a particular one and with just one or two recent literature citations that enable you to get on-system if necessary otherwise. These are: (1) Rotation curves (Sofue & Rubin 2001), (2) binary galaxies (Junqueira & Chan 2002), (3) velocity dispersions in clusters (Rines et al. 2002), (4) X-ray emission from clusters (N. Bahcall; Schuecker et al. 2002), (5) structure on scales larger than clusters, often in combination with some simulation that begins with a primordial fluctuation spectrum (Seljak; Shanks; Annis; Percival et al. 2001; Susperregi 2002), (6) an X-ray cluster variant in which one assumes the cosmic baryon density to have the BBN value and the ratio of total to baryon mass to be that given by clusters (Allen, Schmidt, & Fabian 2002), (7) gravitational lensing (Blandford 2001; Kochanek, Ellis), both strong (Claeskens & Surdej 2002) and weak, by clusters (King et al. 2002), single galaxies (Brainerd, Blandford, & Smail 1996), and by structures larger than clusters (Wittman et al. 2000).

One can imagine (at least) four additional approaches to mass measurements for galaxies and clusters, which have not so far added much. First is escape velocity. If you see a star or galaxy with much more than 1.4 times the local circular velocity, and it seems to have been around a long time, you might reasonably deduce that there is a good deal of mass still further outside than your location, and the largest observed speed tells you how much. This really applies only to the Milky Way and confirms numbers from other data. Second is gravitational redshift. Unfortunately, for typical galaxies and clusters, the wavelength displacement is only about one part in 10^6.

Third is the Sachs-Wolfe (1967) effect. The idea is that photons drop down into gravitational potential wells and have to climb back out to get to us, taking sufficient time that the well has changed with the expansion of the Universe (getting shallower if it is still a linear perturbation) and/or growth of the perturbation (getting deeper). The two possible changes fight each other, but at least interesting limits may be possible (Boughn & Crittenden 2003).

Fourth is back-calculation from velocity data—the large-scale deviations around smooth Hubble flow, known for more than a quarter century (Rubin et al. 1976a,b) and attributed to structures like the Great Attractor (Burstein et al. 1986), then unattributed (Mathewson, Ford, & Buchhorn 1992), and then perhaps attributed again (Vauglin et al. 2002).

Of course you could just assume a fixed mass-to-light ratio, after the fashion of Opik (1922) and Hubble (1934), though if your purpose in attempting a mass measurement is to be able to say something about M/L ratios, this is probably not a good choice. The closest thing found in modern dark matter studies is the "maximal disk" assumption (Bissantz, & Gerhard 2002, who are for; Debattista, Corsini, & Aguerri 2002, who are against).

And, finally, there are global approaches, for which the effective volume is much of the observable Universe, such as luminosity distance vs. redshift for Type Ia supernovae (Perlmutter, Phillips) and analysis of fluctuations in the cosmic microwave background (Spergel;

Lange; Readhead; de Bernardis et al. 2002) . Comparison of a simulation of formation of large-scale structure with the real world is also a sort of global method, and the one that arguably led to widespread recognition that the Universe was probably not closed by positive-pressure matter of any sort. At the time this was widely called "the failure of standard cold dark matter," and Hamilton et al. (1991) were among a number of groups that pointed it out more or less simultaneously.

1.3 Results

These are conveniently divisible into (a) numbers close to what most of us now expect for M/L and $\Omega_{\rm m}$, (b) reconciliation of discordant numbers for what should be the same quantity (the mass of a particular cluster from velocities, X-rays, and lensing, or cluster vs. more global values for total matter density), and (c) minority views in favor of either very small matter densities or values close to unity and larger divergences.

The broadest-brush version of the expected, best-buy, result has not changed enormously since the papers of Ostriker et al. (1974) and Einasto et al. (1974). That is, the mass-to-light ratio in solar units included within a given scale length increases more or less monotonically from 0.5 to a few for star clusters and galactic disks to something like 10 for the inner parts of galaxies, probed with rotation curves, globular clusters, and halo gas, to 20–30 for the extreme reaches of galaxies and binary pairs, to 100 or so for groups, and tapers off at 200–300 for rich clusters and superclusters, showing no signs of rising higher when you look globally. What is clearer now than then is that there is a real range of M/L values at each length scale, and not just error bars. Some dwarf spheroidal companion galaxies (perhaps even some globular clusters) have their own dark matter halos, and some do not (Braine et al. 2001; Kleyna et al. 2001; Bromm and Clark 2002; Fellhauer & Kroupa 2002; Prada & Burkert 2002). And while there are apparently no truly empty halos (Haynes et al. 2002; Miralles et al. 2002), some are less full of luminous baryons than others and so can have very large M/L's (Sahu et al. 1998; Tully et al. 2002). Historically, these have been difficult or impossible to find, but weak lensing surveys of "empty fields" are beginning to fill in the inventory.

1.3.1 Conventional Recent Results

On the whole, galaxies have continued to yield galaxy-type numbers: 7.4×10^{10} $M_\odot$ for the Milky Way inside our orbit (Olling & Merrifield 2001, an M/L of 3–4), M/L_B = 22 for M81 out to 21 kpc (Schroder et al. 2002) and $(1-2) \times 10^{12}$ $M_\odot$ for the whole Local Group and M81 group (Gallart et al. 2001; Karachentsev et al. 2002a,b). Binary galaxies (e.g., M/L = 17.6 for a bunch of pairs of large ellipticals; Junqueira & Chan 2002) also fall where they have been accustomed to fall, between inner galaxy and cluster numbers.

Both masses and M/L ratios get larger as you look at larger assemblages (Adams & Mazure 2002, an application of early-release SDSS data). Their mean value of cluster M/L_B is $360h$. Girardi et al. (2002) also record the increase of M/L with scale going from 0.5 to 300×10^{12} $M_\odot$ for the entities considered.

Other concordant numbers (given the various bands used for L and the sometimes unstated values of H_0) include a mean of $239h_{50}$ (Hoekstra et al. 2002) and one of 470 ± 180 (Dye et al. 2002). A King et al. (2002) value of M/L_K = 40 sounds as if it might have crept down from the previous paragraph, as indeed it has, since it pertains only to the inner core of the cluster (coming from weak lensing also in the K band). Superclusters have, of course, even larger masses in excess (as Abell concluded) of $2 \times 10^{16}\, h^{-1}\, M_\odot$ (Caretta et al. 2002).

For the global numbers, one is perhaps entitled to be surprised at their stability. A majority of panelists at a discussion within the 1997 IAU General Assembly in Kyoto voted for a set very close to what the majority of speakers at this conference are voting for (65, 0.3, 0.7, 0.1, etc., and it is left as an exercise for the reader to attach these to the symbols normally used to indicate the quantities). This point of view was thoroughly dominating the literature by 1998, when I recorded more than 20 papers reporting $\Omega_m = 0.3 \pm 0.1$, of which Eke et al. (1998) was particularly well written. Indeed a sort of Monrovian era of good feeling might be said to have set in when Lineweaver (1999) and Bahcall et al. (1999) concluded that you could bring together numbers from bound clusters of galaxies, larger-scale structures, the apparent brightnesses of supernovae (Perlmutter; Phillips; Hamuy), and "early" (meaning anything before January 2003) measurements of the acoustic peaks in the spectrum of fluctuations of the CMB and get mutually consistent results with the 0.3/0.7 split between stuff with positive pressure and stuff with negative pressure.

On the whole, this concordance has persisted. Numbers for Ω_m published in the last year that overlap 0.3 at the $1-2\,\sigma$ level include, with the probes used, 0.32–0.38 (Susperregi 2002, analysis of four redshift surveys), 0.35 ± 0.1 (Borgani et al. 2001, *ROSAT* clusters), 0.25 ± 0.1 (Roukema, Mamon, & Bajtlik 2002, clustering of QSOs in the 2dF survey), 0.3 ± 0.04 (Allen et al. 2002, relaxed X-ray clusters in the *Chandra* database but assuming that they have their fair share of baryons, which is perhaps unfair), 0.20 ± 0.03 (Percival et al. 2001, 2dF again, assuming scale invariant primordial fluctuations), 0.2 ± 0.1 (Hoekstra et al. 2001, weak lensing by groups), 0.17 (Bahcall & Comerford 2001, clusters), and, the most honest errors bars we've seen, $\Omega_m\, h^2 = 0.02$–0.3 (Schuecker et al. 2002, X-ray clusters), plus, of course, nearly all of the analyses of CMB data presented at this meeting and by Miller, Nichol, & Batuski (2001), Pryke et al. (2002, DASI), Elgarøy, Gramann, & Lahav (2002), Abroe et al. (2002, Maxima), de Bernardis et al. (2002, Boomerang and DASI), and the one to read, if you're reading only one, 0.28 ± 0.11 (Bridle et al. 2001), again bringing together data from supernovae, microwave background, and large-scale structure.

1.3.2 Discrepancies and Reconciliations

Neta Bahcall at this meeting and elsewhere has noted that the numbers coming from global considerations tend to be somewhat larger than the best values of Ω_m from recognizable clusters of galaxies and that this may constitute a problem. The alternative is that, just as there are some very diffuse baryons that we are only now learning to spot, there may be some very diffuse CDM that we have not quite learned how to spot.

A second issue is particular clusters for which different methods of analysis yield different total masses. A typical example is RX J1347−1145, for which Cohen & Kneib (2002) conclude that the virial mass is the smallest, the X-ray mass is the largest, and the lensing mass comes in the middle. They suggest that the cause might be a merger-in-progress, which has shocked the gas (making it hotter than it "ought" to be) while the two separate velocity distributions have not yet had a chance to discover that they now belong to a single, more massive cluster. Other discrepancies are typically in the same order, typically not very large, and typically can be explained or explained away (Rines et al. 2002 on A2199; Czoske et al. 2002 and Machacek et al. 2002 on A2218; Allen, Schmidt, & Fabian 2001, Hoekstra et al. 2002, and Athreya et al. 2002 on MS1008−1224).

I do not really think that these systematic disagreements constitute "a problem," since they are generally in the direction you would expect from on-going mergers and how the various methods probe gravitating matter. They may, however, mean that one cannot use agreement of lensing numbers with the others as a confirmation of general relativity in comparison to some other theory of gravity with the same weak-field limit. Remember that a Newtonian argument also yields gravitational bending of light, by half the GR angle. Theories that do not have Newtonian gravity as the weak-field, small-velocity limit, may, however, still be ruled out (Edery 1999).

Great was the wailing and gnashing of teeth when, into a world already disenchanted with standard (closure density) CDM came the first few Type Ia supernovae, with luminosity distances that seemed to require Ω_m much larger than Ω_Λ (Perlmutter et al. 1997). Have faith! said we of unbounded (this is a pun) optimism. It will all go away. And sure enough it did (Garnavich et al. 1998; Riess et al. 1998, 2001; Perlmutter et al. 1998, 1999).

Next, there remain some measurements of Ω_m by various methods, published within the last few years, that do not fall within the $1-2\sigma$ band of the official value 0.3. A few are significantly smaller, e.g. Reiprich & Böhringer (2002, with a best value of 0.12 and a firm upper limit of 0.31 from X-ray clusters and the assumption of a Press-Schechter luminosity function), 0.13 (Arnaud, Aghanim, & Neuman 2002, more X-ray clusters, but reaching to z = 0.8), and an upper limit of 0.1 from three clusters with X-ray and weak-lensing data (Gray et al. 2002).

Most discordant values, however, are well above the consensus band, perhaps only because, if a quantity has to be positive, there is a lot more phase space above 0.3 than below. Thus Ω_m close to one (and Ω_Λ close to zero) has been found a better, or at least as good, a fit as 0.3/0.7 to the following data sets:

(a) The K-magnitude redshift relation for strong radio galaxies, on the assumption that ones at $z = 0$ are more massive than the ones at large redshift (Inskip et al. 2002).

(b) The clustering perpendicular to and along the line of sight of QSOs in the 2dF survey (Hoyle et al. 2002, an application of the Alcock-Paczyński test).

(c) The contribution of gravitational lensing to quasar counts (Croom & Shanks 1999).

(d) Non-linear clustering (Szapudi et al. 1999).

(e) And more (cited in Trimble & Aschwanden 2000 and earlier papers in that series), including Pen (1997), who used the classic angular diameter-redshift test like the one that led Loh & Spillar (1986) astray a decade before. Just this year, finally, one of these $\Theta - z$ measurements has been found inconsistent with a large matter density (Lima & Alcaniz 2002).

Finally, there are alternative theories of gravity, motivated at least partly by the desire to avoid having any dark (or anyhow non-baryonic) material in galaxies. If you would like to chase these down, possible starting points are Mannheim (2001) on conformal or Weyl gravity (he is for it) and Milgrom (2002) on Modified Newtonian Dynamics (MOND; he invented it and is still in favor of it). The underlying principles of MOND (a fixed minimum gravitational acceleration) are simple enough that we bears of very little brain can see how it might be tested, by comparing mass measurements made by different techniques (and therefore how it might fail). Those of conformal gravity are not.

Authors who insist on doing cosmologically sensitive calculations, like the distribution of v/c ratios for QSO jets (Wang et al. 2001) under the assumption that H_0 = 100 km s^{-1} Mpc^{-1} and q_0 = 1/2 probably also belong in here. So, perhaps, does Harrison (1993), who pointed out that, at the time, it was not so easy as you might suppose to exclude a universe with $H_0 = 10$ km s^{-1} Mpc^{-1}, $\Omega_m = 10$, plus very large deviations from Hubble flow. This is, I think, no longer true.

And if observed redshifts are not the (correctly done) sum of cosmic expansion plus peculiar motion through space (Arp 1971; Bell 2002) then all bets are off.

1.4 What is it All?

Neutrinos, now that their rest masses have been (more or less) measured, are no longer a "dark matter candidate." Rather, they are simply part of the inventory, to be included in calculations where appropriate. Other speakers (e.g., Silk; Sadoulet) will have told you about the respectable candidates for the rest of the non-baryonic, non-leptonic $\Omega_m = 0.25$ or thereabouts, but it probably falls to me to tell you about the less conventional ones that have made it into the literature in the past few years. There are enormous numbers of these. Some are variants on the main stream, for instance (a) self-interacting dark matter, which had a brief vogue (Spergel & Steinhardt 2000) before shattering on the reefs of conflicting observations (Yoshida et al. 2000), (b) decaying dark matter, one very specific version of which (Sciama 1990) made a prediction that was not confirmed by a search for the expected product photons (Davidsen et al. 1991), but other versions of which remain viable and perhaps even useful for the "cusp problem" (Cen 2001), and (c) annihilating dark matter (Craig & Davis 2001).

Other candidates are at least tied to portions of physics that one feels one ought to understand, for instance cosmic strings (Spergel & Pen 1997), decaying domain walls (Dvali, Liu, & Vachaspati 1998), and topological defects in general (Digal et al. 2000), and a whole zoo of scalar fields and particles (Cormier & Holman 2000; Dabrowski & Schunck 2000; Goodman 2000; Hu & Peebles 2000).

Still others have been put forward by people of whom one thinks so highly that surely there must be something in the ideas, even if one isn't quite sure what it is. Examples include small, cold gas clouds (Gerhard & Silk 1996), dust with charge = mass in the $c = G = 1$ units of GR (Bonnor 1996), and white dwarfs that were never luminous stars (Lynden-Bell & Tout 2001).

And this still leaves 25 or so apparently distinct proposals for the dominant dark matter that I have spotted over the years and cited in the Ap9x and Ap200x series (with another 10 or so waiting for Ap02). A quick and dirty way to decide whether you should take them seriously is by noting whether they were published in *Astrophysics and Space Science* (perhaps not), in *Physical Review Letters* (probably yes, at least if you are a particle physicist), or one of the more-tightly refereed journals of astronomy and astrophysics, meaning either that at least it didn't contradict existing observations at the time, or that the referee got worn down. Here is a subset of my favorites, deliberately not ordered by that variable.

- Solid hydrogen (White 1996)
- Rydberg matter (which, by the way means atoms in highly excited orbits like $n = 109$, not something at 13.6 eV (Badiei & Holmlid 2002)
- Q balls (Kusenko & Steinhardt 2001)
- Clusters of MACHOs with cluster mass $4 \times 10^4\ M_\odot$ (Kerins 1997)
- DAEMONS (Drobyshevski 2000)
- Decaying B-balls (Enqvist & MacDonald 1998)

- Planck-mass remnants from the evaporation of primordial black holes (Alexeyev et al. 2002)
- A vector-based theory of gravity (Jeffries 1996)
- Spin = 0, charge = 0 bosons of mass 10^{-34} eV with a time-dependent scalar field (Israelit 1996)
- Short-lived particles that are created continuously from vacuum quantum fluctuations during the gravitational formation of galaxies (Majernik 1996)
- Point-like masses (in the MACHO range) with attached gas halos, so that they produce non-gray lensing events and have escaped detection (Bozza et al. 2001)

A comparable number of papers put observational or laboratory limits on various candidates from axions and nuclearites to black holes of both very large and very small masses.

1.5 Conclusions

It is difficult to summarize the cosmic density situation as it is today in any way very different from what Ivan King said in 1977 or I did in 1987: there is an enormous body of evidence indicative of gravitating mass which does not emit or absorb its fair share of light (which evidence we can abbreviate with the phrase “dark matter”), but we haven’t a clue what most of it is. I should perhaps just add that there are a good many more things than there were in 1977 that we now know that it is not, though the inventory of remaining candidates is strange, wondrous, and growing.

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