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EXTRAGALACTIC DISTANCE SCALES: H_0 FROM HUBBLE (EDWIN) TO HUBBLE (HUBBLE TELESCOPE)

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Abstract. In the first fifty years after Edwin Hubble announced a linear relationship between distances and redshifts of external galaxies, the accepted value of his constant dropped by (or the Universe expanded and aged by) a factor of 5 to 10. More recently, different groups, often using nearly the same data, have passionately defended distance scales that differ by about a factor of two. The sections of this review explore (1) the history of extragalactic distance scales, (2) the relationships between the Hubble constant, H_0 , and other cosmological parameters, (3) types of distance indicators, (4) ways of measuring distances in practice, (5) values of H_0 reported recently on the basis of these methods, (6) the continuing discrepancies between the ‘long’ and ‘short’ distance scales, and (7) prospects for future convergence on a single, global value of H , so that we can all get back to doing other things. The units of the Hubble constant are $\text{km s}^{-1} \text{Mpc}^{-1}$ (or reciprocal time), and no one now strongly favors any value outside the range $40\text{--}90 \text{ km s}^{-1} \text{Mpc}^{-1}$ (time scales of 11–25 Gyr).

1. Historical Introduction

The story of the Hubble constant logically begins just where the Curtis–Shapley (Curtis, 1921; Shapley, 1921) debate on the distance scale of the Universe ended, with Hubble’s (1925) discovery of Cepheid variables in several nebulae that we now recognize as galaxies within the Local Group. This settled the issue of the existence of external galaxies.

1.1. THEORETICAL UNDERPINNINGS

Most modern cosmological work assumes the correctness of Einstein’s 1916 theory of general relativity, or at least something with the same classical limit. His own first solution to the GR equations (Einstein, 1917) was, notoriously, a static universe, achieved by introducing a sort of repulsive force now called the cosmological constant. Whether that constant has a non-zero value in the real Universe is still debated (and Einstein having voted ‘no’ is not evidence). Einstein’s universe had a uniform density of matter, uniquely related to the value of the cosmological constant, and a characteristic size scale.

Other early solutions came from Willem de Sitter (1916, 1917), whose space-time was static and empty but caused test particles to display Doppler effects (primarily redshifts) and Alexander Friedmann (1922, 1924), describing everexpanding models with zero and negative curvature. Georges Lemaitre (1925, 1927) independently rediscovered expanding model universes. Howard Percy Robertson

(1928, 1935) and Arthur Gordon Walker (1935) put the mathematics of de Sitter, Friedmann, and Lemaitre spaces into essentially modern form, and one now normally speaks of the Friedmann–Robertson–Walker (FRW) solutions for homogeneous, expanding model universes.

Lemaitre (1927) showed that the Einstein static universe is unstable, a small push sending it into eternal expansion (or contraction), and the de Sitter solution was widely discussed in the community, leaving astronomers with some expectation, up until about 1930, that the correct relationship between distances and velocities might be a quadratic one, as predicted in that solution. Lemaitre (1927), however, combined some very early redshift measurements with a distance indicator of his own and suggested a linear relationship with a slope of about $600 \text{ km s}^{-1} \text{ Mpc}^{-1}$.

Measuring Hubble's constant is an interesting activity only if we live in a relativistic, expanding universe of finite age. What are the alternatives, and how firmly can they be rejected? The ink was not dry on Hubble's classic 1929 paper before Fritz Zwicky (1929) suggested that photons might simply lose energy and become redshifted as a result of travelling long distances. This is generally called tired light, and there are both theoretical and observational reasons for rejecting it. On the theoretical side, we seem to have a good theory for the behavior of photons (quantum electrodynamics), and the Feynman graphs for tired light sum to zero.

On the observational front, tired light fails three tests at varying levels. First of these is the Hubble–Tolman (1935) surface brightness test. The energy we receive from a distant object (in $\text{erg s}^{-1} \text{ cm}^{-2} \text{ steradian}^{-1}$, for instance) will scale as $(1+z)^{-1}$ in a tired light universe and as $(1+z)^{-4}$ in a relativistically expanding one (the extra powers come from the rate of arrival of photons and geometrical factors, see Sandage, 1988, for a detailed discussion). Sandage and Perlmutter (1991) reported a $(1+z)^{-4}$ dependence some time ago, and Pahre et al. (1996), who initially doubted that existing data were adequate to make the distinction, now concur. Incidentally, the other Hubble–Tolman (1935) test is numbers of galaxies as a function of redshift and apparent brightness. It has been declared moot a countably infinite number of times.

A second test also derives from the work of Richard Chase Tolman (1934). It consists simply in recognizing that black body radiation in an expanding universe will cool with the expansion. This has arguably also been seen (Songaila et al., 1994).

The third test is, to my mind, the most persuasive one. In an expanding universe, events seen from far away will be time dilated by a factor $(1+z)$, while in a tired light universe they will merely look faint. The absence of a correlation between time scale and redshift in the variability of quasars, etc., has been one of the props of the 'non-cosmological redshift' school. But there are better clocks. Zwicky (1939) and Olin C. Wilson (1939) both noted that supernovae, being very bright and rather homogeneous, might be good distance indicators. Wilson in addition remarked that time dilation of their light curves should be detectable at large redshift, if the expansion were real. This has now been seen (Perlmutter et al., 1995, and an

unpublished data base of about half a dozen events with $z = 0.3\text{--}0.5$ studied by the same group; Leibundgut et al., 1996, reporting the first event from a separate project). Oh, and in case you wondered, the lack of redshift-time scale correlation in quasars is apparently a selection effect, arising from the facts that you see only very bright objects at the largest distance and that the real time scale is inversely proportional to luminosity (Netzer et al., 1996).

The second alternative to relativistic expansion of space-time itself is motion of galaxies into previously existing (probably Euclidean) space from some center. If you wait long enough and the galaxies initially had a wide range of speeds, the effect will be an isotropic, linear velocity-distance relation, so long as you are looking from somewhere near the center. The main objection is that, given the very small values of anisotropy of the microwave radiation background, we would have to be most unCopernicanly near the center to make this work.

The third alternative is steady state cosmology, in which new matter appears at just such a rate as to keep the average density of an expanding universe constant. Hermann Bondi and Thomas Gold (1948) and Fred Hoyle (1948) had among their motivations the need for a longer cosmic time scale than the reciprocal of the contemporaneous value of H (about $500 \text{ km s}^{-1} \text{ Mpc}^{-1}$, H^{-1} about 2 Gyr). Steady-state supporters were probably never a majority of the astronomical community, and gradually fell away between about 1955 (when counts of radio sources showed that there had been many more in the past) and 1965 (when the implications of the microwave background radiation began to be fully appreciated).

The redshift-distance or Hubble relation has quite different meanings in these alternatives. In a tired light universe, it would be well defined and, presumably, a prediction of some quantum field theory. In an exploding universe, the relation is both meaningful and an indicator of the time since the explosion, while in steady state the value of H tells us only the average age of galaxies to be found in some representative volume. If redshifts have a large non-cosmological component (Arp, 1989) or if the real relation is a quadratic one (Segal and Nicoll, 1996), then the Hubble constant is not even very well defined. Every one of these alternatives continues to have a few defenders, and my ignoring them hereafter is either prejudice or good judgement, depending on which side of the fence you sit on.

1.2. OBSERVATIONS AND THEIR INTERPRETATION BEFORE HUBBLE

Vesto Melvin Slipher measured the very first Doppler shift for a spiral nebula, and also all of the next few dozen. The first was M31 (Slipher, 1914, though the plate was taken the night of 3–4 December, 1912), with a blueshift of about 300 km s^{-1} (a true Doppler, not cosmological, effect, due to the rotation of the Milky Way and our orbit around M31). A few years later (Slipher, 1917) there were a couple dozen, positives outnumbering negatives by 21 to 4. NGC 584 at $+1800 \text{ km s}^{-1}$ was as faint as he could reach with the Lowell Observatory 24". Milton Lasell Humason took up the task using the 60" and 100" telescopes at Mt. Wilson and rapidly reached

+7900 km s⁻¹ for NGC 7619 in the Coma clusters (Humason and Pease, 1929). Humason lived to use the 200" Hale telescope with some regularity and retired in 1957 having achieved a 'personal best' and world record of +60 000 km s⁻¹ for the Hydra cluster. He believed, rightly, that larger distances would not be possible with the detectors (photographic plates mostly) of that era.

During the time that redshifts were accumulating, several important items of interpretation surfaced. First, Ernst Julius Öpik (1922) placed M31 (the Andromeda Nebula) at an unprecedentedly large distance of 450 kpc by assuming that it had the same mass to light ratio as the solar neighborhood (about three in solar units) and was rotating at the speed of 157 km s⁻¹ implied by Slipher's spectra.

Second, Gunnar Malmquist (1920, 1924) explained his bias. The idea is that, if you are studying a class of objects whose real brightnesses vary over some range, then you will see the full range among nearby objects, but only the brightest ones far away, and so be misled into thinking them closer than they are, because they look so bright. Proper correction for the bias requires that you know the full distribution of brightnesses for the distant objects. Taking this to be the same as the distribution for nearby ones is an assumption that cannot always be justified. Any large gathering of observational cosmologists even today will include at least one person who thinks that someone else in the room does not understand the Malmquist effect. Sometimes it only takes two.

Third, a number of astronomers began to look at Slipher's velocities and attempt to extract the solar motion relative to spiral nebulae. Among these, the most important (at least in retrospect) were Carl Wittz (1921, 1924), who included a constant K (redshift) term in his solution and Knut Lundmark (1925) whose K -term was allowed both linear and quadratic dependence on distance. Not surprisingly, Lundmark found a quadratic term. It was negative and seemingly small, but would have limited observable velocities to a maximum of about 3000 km s⁻¹. Curiously, much of the secondary literature seems to assume that he found a positive quadratic term (as indeed one does if Malmquist bias is recklessly neglected). Duerbeck and Seitter (1996) provide a critical discussion of the values of the Hubble constant implied by these early analyses. The main problem, then as now, was in coming up with accurate distances for the galaxies involved.

1.3. THE CONTRIBUTIONS OF EDWIN P. HUBBLE

Hubble's (1929) correlation of redshifts and distances was very rapidly accepted by a large fraction of the contemporary astronomical community, at least partly because his distance estimates were much more consistent than any previous set. They were ultimately based on the relationship between pulsation period and average luminosity of Cepheid variable stars, as calibrated by Harlow Shapley (1918, 1919). Shapley's calibration was badly wrong (because of neglect of interstellar absorption and galactic rotation and the effects of small number statistics, Fernie,

1969), but it was consistent, including disk Cepheids, globular cluster Cepheids, and RR Lyrae stars into a single P–L relation.

Hubble had Cepheid light curves for the six closest of his galaxies and used them to conclude that the brightest stars of any kind had absolute magnitudes of -6.3 . The next 13 galaxies were plotted using this assumption, and the last four, in the Virgo cluster, on the assumption that an average galaxy has $M_v = -15.2$, like his first 17. The largest velocity plotted was about 1100 km s^{-1} , though Humason (1929), reported $+3779 \text{ km s}^{-1}$ for NGC 7626 in the same journal, and it has often been said that this more distant point ‘guided Hubble’s eye’ in drawing his straight line through the points. The graph actually shows two lines, with slopes of 465 and $513 \text{ km s}^{-1} \text{ Mpc}^{-1}$, and the text mentions a possible value of 530 as well. A modern eye examining the plotted points inevitably concludes that Hubble was perfectly honest about the random errors of his result. The problem lay, as nearly always, in systematic errors.

Humason’s ever-increasing velocities extended Hubble’s graph to larger and larger distances (Hubble and Humason, 1931; Humason, 1936; Hubble, 1936), but never changed the slope by more than about one sigma around the 1929 value. Tolman (1929) spoke of ‘Hubble’s relationship’ almost immediately. The symbol H , now universal, first appears in Robertson (1955).

1.4. FROM HUBBLE TO SANDAGE

The ubiquity of interstellar absorption had been discussed intermittently for decades, but was generally accepted only after Trumpler (1930) showed that a plot of angular diameter vs apparent magnitude for open star clusters curved in a way implying that the distances for the apparently fainter clusters were too large. In fact, they looked faint partly because they were far away but also partly because their light was being absorbed by interstellar dust. The effect had not been conspicuous in Shapley’s Cepheid calibration because the globular clusters are mostly at high galactic latitude, relatively unabsorbed by just the right amount to compensate for their Cepheids actually being much fainter than ones in the galactic disk.

The implications of absorption for distance scales and Cepheid brightnesses were only gradually realized, e.g., Gerasimovich (1934) and Mineur (1946). The latter arrived at an average value of about one magnitude per kiloparsec in the galactic plane and made his Cepheids about one magnitude brighter than Shapley’s. But it was to be a few more years before anyone realized that something was fundamentally wrong, except for Lundmark (1946, 1948, 1950) who appears on our stage one last time, pointing out that the distance to M31 from its novae and globular clusters was twice the distance implied by Shapley’s Cepheid calibration.

Hubble’s galaxies were too faint for other reasons, and this also was realized only gradually. Oort (1931), who is almost never mentioned in connection with this subject, compared field and cluster galaxies and concluded that an ‘average galaxy’ was one or two magnitudes brighter than Hubble’s -15.2 . Expressing a

sort of polite puzzlement at the discrepancy, Oort proposed an expansion constant of $290 \text{ km s}^{-1} \text{ Mpc}^{-1}$.

Later, Behr (1951) realized that assuming a fixed absolute magnitude for the brightest galaxy in a cluster would inevitably lead to apparent distances smaller than the real ones by increasing amounts as you look farther away. This is not a reinvention of Malmquist bias (under which assuming a constant value for *average* galaxy brightness leads you astray). Rather, it is a heuristic pre-discovery of what we now call the Scott effect, as she made clear (Scott, 1956). The point is that you need to be able to see a certain number of galaxies to recognize a cluster. At larger distances, you need richer and richer clusters to get enough bright galaxies to spot it. And, eventually you get to clusters richer than any in your local sample, so that they contain rare, super-bright galaxies not present in the calibrating data. Behr's discussion implies a Hubble constant near 240 km s^{-1} , without any Cepheid recalibration.

Matters came to a head at the 1952 Rome General Assembly of the International Astronomical Union. Everyone had assumed that the 200" telescope would easily image RR Lyrae stars in M31. It did not. As Walter Baade (in Oosterhoff, 1954; Baade, 1956) reported, only the very brightest Population II stars, the tip of the red giant branch, were visible on limiting exposures. Within the Milky Way globular clusters RR Lyraes are 1.5^m fainter than the RG tip. Thus Shapley's calibration had to be wrong by the same amount, and M31 at twice its previously advertized distance (bravo Lundmark!). Baade described the implication as an increase in the age of the Universe from 1.8 to 3.6×10^9 years.

Confirmation was already at hand. At the same meeting, Andrew David Thackeray of Radcliffe Observatory, South Africa, reported that he had just found three RR Lyrae stars in the Small Magellanic Cloud. They too were 1.5^m fainter than predicted by the Shapley calibration. RR Lyraes in the Large Magellanic Cloud followed the next year (Thackeray and Adriaan Jan Wesselink, 1953). Gerard Henri de Vaucouleurs (1955) entered extragalactic space soon after, placing the Clouds at an average distance of 52 kpc.

Once it was safe to admit in public that Shapley might have been wrong, recalibrations of the Cepheid distance scale multiplied rapidly. Fernie (1969) reports 19 efforts between 1953 and 1959, and the list is probably not complete. One even came from Shapley himself (Shapley and Virginia McKibben Nail, 1954).

Work on recalibration of Cepheids has continued to the present time, including, of course, the Hubble Space Telescope key project. But the next major changes in Hubble constant arose from other considerations and are largely associated with the name of Allan Sandage. The massive paper of Humason, Nicholas Mayall and Sandage (1956) dealt primarily with the accurate determination of nebular magnitudes and redshifts. But, tucked away in an Appendix, is an estimate of the Hubble constant, differing a good deal from Hubble's own. The brightest single stars are placed at $M_{pg} = -8.5$ (based on examples in M31 and M33, many of them Hubble–Sandage variables) and brightest galaxies in clusters are set equal to

M31 at $M_{pg} = -19.92$. This moved the Virgo cluster out to a distance modulus of $(m - M) = 29$, which, in combination with an average velocity for the cluster of 1136 km s^{-1} , yields a Hubble constant of $180 \text{ km s}^{-1} \text{ Mpc}^{-1}$. They believed this to be correct to within 20%, assuming as usual that the main errors were statistical ones of $0.2\text{--}0.3^m$. No allowance was made for gravitational effects of the Virgo cluster on expansion in its neighborhood.

Two years later, things changed again. Sandage (1958) pointed out that some of Hubble ‘brightest stars’ in galaxies of Virgo and beyond were really entire H II regions and their illuminating central stars and clusters. Thus they were much brighter than previously supposed, moving their host galaxies outward another factor of two to three. Sandage proposed $H = 75 \text{ km s}^{-1} \text{ Mpc}^{-1}$, to within a factor two. I believe this may have been the last realistic set of error bars published for a very long time.

A number of astronomers expressed preference for somewhat larger values at about the same time. These include McVittie (1959) advocating 143–227, Sersic (1960) recommending 125 ± 5 , Holmberg (1958) favoring 134 ± 6 , and Ambartsumian (1961) speaking *ex cathedra* at an IAU invited discourse in favor of the range 70–100 $\text{km s}^{-1} \text{ Mpc}^{-1}$.

Van den Bergh’s (1960a, b) first two published calibrations were 100^{+20}_{-17} and 120^{+25}_{-20} . De Vaucouleurs (1964) first appears on the Hubble constant stage drawing attention to an asymmetry associated with the Virgo cluster, such that $H = 100$ in its direction and 125 elsewhere. This corresponds to what we would now call a Virgocentric infall of 250–300 km s^{-1} . The correct amount of infall to allow for remains a topic of dispute between the supporters of long and short distance scales.

1.5. THE BATTLE LINES HARDEN

At *IAU Symposium 15* (‘Problems in Extragalactic Research’, McVittie, 1962), Sandage (1962) attempted to arrive at a consensus value of the Hubble constant by averaging his own 75 and 82 ± 18 (from H II regions) with the numbers from van den Bergh, Sersic, and Holmberg just mentioned. Unfortunately, he did not use the numbers they had published, but his own renormalization of them to his Local Group Cepheid distance scale, which removed about half of the real spread (or discrepancy, depending on your point of view). This was presumably not in accord with the views of the people thereby renormalized, but resulted in the choice $H = 100 \text{ km s}^{-1} \text{ Mpc}^{-1}$ being very widespread for at least a few years. The custom of hiding remaining uncertainty in the parameter $h = H/100$ dates from that brief era of good feeling. Sersic (1962) was already aware that a Hubble constant near $100 \text{ km s}^{-1} \text{ Mpc}^{-1}$ could still easily lead to an age problem.

By the time of Volume 10 of the Kuiper compendium (Sandage et al., 1975), it was necessary to have two separate reviews of the cosmic distance scale. Van den Bergh (1975, but last revised in September 1969) settled on 95^{+15}_{-12} , while Sandage

(1975, but last revised in August, 1972) declared for 55 ± 5 , and has at times come down as low as 42 ± 11 (Sandage, 1988).

As we enter the modern era, the Sandage and Tammann series of ‘steps toward the Hubble constant’ ended twenty years ago (Sandage and Tammann, 1976) but has been followed by many more of their papers addressing various aspects of the distance scale and age problems, always finding numbers near 50–55. De Vaucouleurs (1969) ended his next assault at 100 ± 10 . He took up cudgles and *Sosies* again almost immediately and laid them down only upon command of higher authority, next to a Hubble constant of 87.3 ± 1.1 (de Vaucouleurs, 1993). Van den Bergh has published an interestingly wide range of Hubble constants in recent years, but opined that ‘on balance’, available evidence suggests that $H_0 \geq 75 \text{ km s}^{-1} \text{ Mpc}^{-1}$ (van den Bergh, 1994).

1.6. OTHER REVIEWS, DEBATES, ETC.

In addition to the reviews mentioned in the previous section, other papers that have attempted to make sense (not always unprejudiced sense) of the distance scale and H include Hodge (1981), de Vaucouleurs (1981), Huchra (1992), van den Bergh (1992), Fukugita et al. (1993), Mould (1995), and Trimble (1997). Books devoted to the subject include Madore and Tully (1986) and Rowan-Robinson (1985). Some discussions have been intentionally or unintentionally organized in the form of a debate (Huchra vs Tammann, 1987; van den Bergh vs Tammann, 1996a, b). A more extensive historical discussion appears in Trimble (1996).

Figure 1 summarizes the material of Sections 1.2–1.5, though Oort (1931) is missing.

2. Relationships Between H_0 and Other Cosmological Parameters

Establishing extragalactic distance scales and measuring the Hubble parameter are sometimes treated as identical, and perhaps even as identical to measuring the age of the Universe. This is a simplification. First, treating the Universe as homogeneous and isotropic is fully justified by properties of the 3K microwave background on the largest scales, but it is something of an assumption from here to the first gigaparsec or so. Thus it is conceivable that the correct, global value of H is significantly different from the one we can measure by most methods (Turner et al., 1992; Zotov and Stoeger, 1995). At any rate, some observers find significant deviations from uniform Hubble flow out to the largest distances they can probe (Lauer and Postman, 1992).

Second, the relationship between H and age is not unique unless you claim to know some other numbers better than they are really known. Start by tying galaxies or clusters to the fabric of space-time (‘co-moving coordinates’). Then all of the dynamics of a homogeneous, isotropic universe in general relativity is contained in

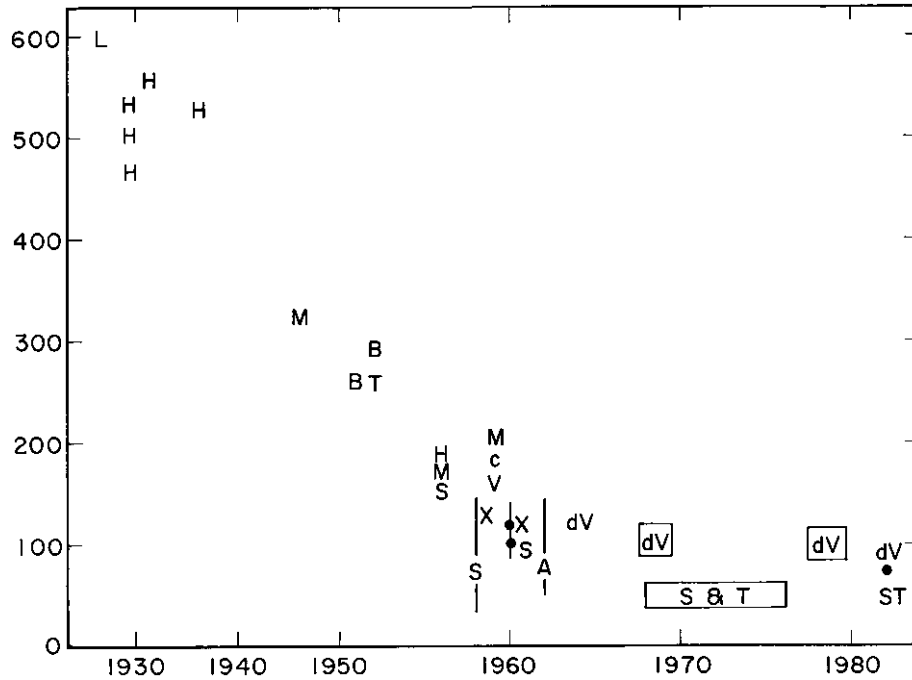


Figure 1. A brief history of values of the Hubble constant. The abbreviations, from left to right, stand for Lemaître, Hubble and Humason, Mineur, Behr, Baade and Thackeray, Humason–Mayall–Sandage, Sandage, McVittie, X = Holmberg, Sersic, solid dots = van den Bergh, Ambartsumian, de Vaucouleurs, Sandage and Tammann. A straight line drawn from the Lemaître point, *L*, through the 1960 Sandage point, *S*, would have passed through zero in another decade. This did not, of course, happen, and the curve levels out into two roughly horizontal branches.

the time-dependent function $a(t)$, the scale parameter, which describes how space expands or contracts. Define $H = (da/dt)/a$ and $q = (da/dt)a/(da/dt)^2$ and you can extract from the Einstein equations, for instance

$$H^2(1 - \Omega) = \frac{\Lambda c^2}{3} - \frac{Kc^2}{a^3},$$

where Ω is the density of all mass-energy in the Universe in units of the density that would just stop the expansion in infinite time. K describes how the geometry of the Universe differs from flat space-time; in these units it is a reciprocal length, in effect the reciprocal of the radius of curvature of the Universe (positive or negative). And Λ is the infamous cosmological constant, here in units of $(\text{length})^{-2}$. Divide both sides by H^2 and all terms are dimensionless; it is in those units that we will generally refer to Λ in the future, so that it is likely to fall between zero and one.

In addition,

$$H^2 q = \frac{4\pi G\rho}{3} - \frac{\Lambda c^2}{3},$$

where ρ is the current total density (in ordinary units). The actual age of the Universe (elapsed time on a clock honorably carried by a co-moving observer ever since the Big Bang) is

$$\text{age} = \frac{1}{H} \int \frac{da}{\left[\frac{\Omega}{a} + 1 - \Omega + \frac{\Lambda^2 c^2}{3H^2} (a^2 - 1) \right]^{1/2}},$$

which is why it is usually shown graphically.

Each of the other parameters clearly affects the relationship between H and real age, but there is not quite so much freedom as you might suppose. The following subsections briefly address what we know about each, concluding that Ω is at least 0.2 and Λ not more than about 0.7. As a result, you can stretch the age to about 30% larger than H^{-1} . Thus $H = 85 \text{ km s}^{-1} \text{ Mpc}^{-1}$ and a total age of 15 Gyr gets you into trouble for any possible choice of the other numbers, while $H = 50$ and a total age greater than 13.6 Gyr is already in trouble for $\Omega = 1$ and $\Lambda = 0$.

In addition to its importance for the age of the Universe, H or the distance scale feeds directly into our calculations of luminosities (hence energy requirements) for galaxies and quasars, their virial masses and mass-to-light ratios (hence the amount of dark matter present in various systems), and the numerical values of the critical density and luminosity density (hence the fraction of the critical density that can be baryonic, given limits set by Big Bang nucleosynthesis).

2.1. THE DECELERATION PARAMETER, q

The use of q or q_0 (meaning value now) is something of a relic of the time when $a(t)$ was expanded in power series for lack of any better guess. Astronomers from Hubble onward have sporadically felt that they know how to tell whether the expansion was speeding up or slowing down by looking at sufficiently distant galaxies, etc. In practice, all such efforts have been thwarted by the physical evolution of the sources being used as probes, right on down to the present (Kellermann, 1993; Dabrowski et al., 1995).

2.2. THE TOTAL DENSITY

The arguments that Ω must be very precisely one derive from considerations of how the very early universe managed to make itself able to evolve into the one we see (homogeneous, isotropic, with just enough fluctuations to grow into galaxies, not too many monopoles, etc.). For the details, see Kolb and Turner (1990), Peebles (1993), or any other recent, mathematically-oriented text under the magic word ‘inflation’. It ill behoves an old-fashioned observer like the present author (who remembers when it was possible to put plates into the hypo before the developer and lose a whole night’s work) to pass judgement on such matters. It is, however,

worth noting (a) that there is some dissension in the ranks in favor of inflation alternatives with different implications (Cornish et al., 1996; Kamionkowski and Toumbas, 1996) and (b) that inflation more nearly predicts $K = 0$ than $\Omega = 1$, that is, you can make up the difference with a non-zero Λ , which has other virtues.

We can, at least, set a lower limit to the density, based on the largest-scale structures that seem to be bound and the assumption that the mass-to-light ratio of the Universe where you don't see any light is at least as large as where you do see light. The number that comes out is somewhere between 0.1 and 0.3 (Shaya et al., 1995; Jing and Boerner, 1996; Carlberg et al., 1996).

2.3. THE COSMOLOGICAL CONSTANT

This parameter has been described as 'my worst mistake' (by Einstein), as 'unphysical' (by Michel, 1996), and probably worse things. It is rather difficult to get an observational handle on it that is not heavily dependent on already knowing all the other parameters (especially H). William H. McCrea, whose involvement in relativistic cosmology goes back about as far as that of anyone living (e.g., McCrea and McVittie, 1930) has said he doesn't think it can be done. One possibility is to exploit the sensitivity to Λ of the difference between redshift-distance relations in the angular and radial directions (Alcock and Paczyński, 1978), for which the existing data base of quasar pairs needs to grow only a factor of 20 or so (Phillips, 1993).

Meanwhile, a range of values of Λ has been exploited for their effects on simulations of the formation of galaxies and clusters from the early universe, with some disagreement about whether having some is good (Borgani et al., 1995) or bad (Klypin et al., 1996). And, most important for our purposes, the fraction of distant quasars that are gravitationally lensed can put an upper limit on Λ , because strong negative curvature means that there is a lot of space out there at large redshift. Lensed quasars are indeed rather rare (e.g., Kochanek et al., 1995). The corresponding limit has remained at $\Lambda = 0.7$ for some time and is apparently fairly robust (Maoz and Rix, 1993; Wu and Mao, 1996; Matsubara and Yokoyama, 1996; Tomita, 1996; Kochanek, 1996). Some of these authors indicate that some contribution actually helps to explain the lensing data.

2.4. THE AGE OF THE UNIVERSE

Nobody wants to live in a Universe younger than some of its constituent parts. The parts that come closest to telling us their ages are radioactive elements and stars (especially those in clusters). Decaying uranium and thorium (lead is the end product) are very good about telling us how long it has been since the rocks containing them solidified, for instance 4.55 Gyr for many meteorites. To go back further, you need to assume something about formation ratios of the different isotopes (somewhat amenable to measurements) and a good deal about the rate

of formation of stars that synthesize the elements (currently amenable only to ‘adjustments of variable parameters’). Formation of stars massive enough to make U and Th surely began sometime between 10 and 20 Gyr ago in our Galaxy (Cowan et al., 1991; Truran, 1997), which is already long enough ago to be of some interest in connection with large values of both H and Ω .

The standard main age indicator is color magnitude diagrams for globular clusters, showing us the color and luminosity (hence mass) of stars that have just exhausted their hydrogen core fuel and are about to become red giants. In order to get a precise age this way, you need to know many things accurately, including the distance to the cluster, its chemical composition (average metallicity, deviations from solar CNO/Fe, etc., initial helium abundance) its atmospheric structure (connecting observed color with effective temperature), the effects of convection and diffusion on the structure, and perhaps other things (rotation, magnetic fields...) that are generally not even considered. The accumulated uncertainties are fairly large (Catelan and de Freitas Pacheco, 1996; Jimenez et al., 1996).

As a result, all honest ‘best values’ and ‘lower limits’ have uncertainties of a couple of Gyr. Even then, recent determinations do not completely agree. Herewith a representative sample: Bolte and Hogan (1995) 15.8 ± 2.1 Gyr (as always, for the oldest clusters in a group, or ones picked by the authors as plausibly among the oldest, M92 in this case), 14.6 Gyr for a large group (lower limit 12.1, Chaboyer et al., 1996), 16.4 ± 2.6 Gyr (for M68, Jimenez and Padovani), 13.5 ± 2 Gyr (for 8 old clusters, lower limit 9.7 Gyr, Jimenez et al., 1996), and 13.5 Gyr (Chamcham and Hendry, 1996). Even the smaller ones of these are problematic for many combinations of H and other parameters. In addition, it is worth remembering that many of the theorists involved have spent a good deal of time over the past few years wondering how to make the clusters younger, and one wonders what would happen to the ‘best fits’ if suddenly there were all the time in the world available. The simplest way to make the clusters younger is to re-expand the Galaxy back to a size somewhere between the classic Oort constants (with which we were 10 kpc from the center) and the old Shapley calibration (that put us at 20 kpc) rather than the current best value of 8.5 kpc. More distant clusters means brighter, more massive Main-Sequence stars with shorter lifetimes!

Can we check on whether something might have gone terribly wrong with these ages? Not entirely. There is some understanding of how and when white dwarfs cool and fade, so that magnitudes and colors of the faintest white dwarfs in globular clusters could provide independent age estimates. Unfortunately, the stars are so faint and the photometric accuracy required so great that it will take a space telescope a good deal larger than HST to carry out the project. One globular cluster in the Large Magellanic Cloud is about as old as ours (Mighell et al., 1996a), but this is not really independent, since it also comes from a color magnitude diagram (and one with quite poorly determined colors).

Beyond the Magellanic Clouds, we cannot resolve individual main sequence stars, and estimates of stellar age populations are based on integrated colors and

spectra, for which age and metallicity are not fully separable. But many workers have tried, reporting ages in the range 13–17 Gyr for other spirals (Sommer-Larsen, 1996), dwarf spheroidals (Mighell et al., 1996b), and elliptical galaxies (Bressan et al., 1996)

A slightly different sort of age problem has surfaced quite recently as astronomers have begun to find relatively ordinary galaxies, with reddish colors and nearly solar metallicity, at large redshifts. For a given combination of H and the other parameters, you can say how old the Universe was at that redshift, and then ask whether it was old enough for the galaxies you see to have formed and evolved as much as they seem to. The answer in a few cases (Dunlop et al., 1996; Ohta et al., 1996; Omont et al., 1996) is that, once again, it is easy to get into trouble with combinations involving large H and Ω and zero or small Λ .

2.5. THE AVERAGE BARYON DENSITY

This is about as far from the hard path of distances as we should probably venture to wander. There are, however, connectors. The early, hot, dense universe is generally blamed for the production of most of the helium we see, all of the deuterium, and the smidgeon of lithium-7 seen in the oldest stars. Just how much of each you get out of the Big Bang depends on all sorts of bits of physics (including the number of stable species of neutrinos), but also very much on the ratio of baryons (protons and neutrons) to photons. The photon number density is known accurately from the temperature of the microwave background radiation. Thus, in turn, one sets both upper and lower limits on $\Omega_b h^{-2}$.

Two potential problems are floating around. The first is whether you can actually get a consistent fit to all of the nuclides supposedly produced in the big bang (Copi et al., 1995). Recent detection of helium-3 in the nearby interstellar medium (Gloeckler and Geiss, 1996) and the possibility of rather more primordial deuterium than has generally been advertized (Rugers and Hogan, 1996) have gone some way to make this look more like a problem in galactic chemical evolution than in Big Bang physics.

The second problem is whether the real universe actually has a baryon density in the range $\Omega_b h^{-2} = 0.007\text{--}0.024$ allowed (Copi et al., 1995, with a little extra slop allowed if the early universe was inhomogeneous, Mathews et al., 1996). Seeing too few is not a problem. You can always invoke (and probably should) dark baryons, in the form of substellar objects or whatever appeals to you. Seeing too many is a problem! At redshifts of 2–3, most of the gas seems to be in the form of clouds that we see as absorption lines in the spectra of still more distant quasars (Meylan, 1995). Here and now, there is quite a lot of gas in rich clusters of galaxies, where it is hot enough to emit X-rays. It is ‘too much’ (that is, there is some sort of baryon catastrophe, White and Fabian, 1995) if and only if (a) the few clusters that have been analyzed have been treated with sufficient care that their having 10–30% of their total mass in hot gas is a reliable conclusion, (b) they are typical of all the

other galaxies in the Universe that are not in rich clusters in terms of baryonic to total mass, and (c) the total density is the closure one. If not, not.

2.6. LOOKING AHEAD

Most of us (rightly) distrust panaceas. Nevertheless, under some circumstances, fluctuations of the microwave background on scales $\lesssim 1^\circ$ may contain signatures that are separately sensitive to Ω , Λ , the spectrum of the initial perturbations in the early universe, and even H . Among the necessary conditions are that those perturbations be adiabatic ones and that there not have been any significant reionization of gas after the recombination epoch near $z = 1000$ (Jungman et al., 1996; Lange, 1996).

3. Methods of Measuring Distances

It seems to me that there are five different ways of getting hold of distances to astronomical objects. First, if you know the real luminosity, L , of something in erg s^{-1} and measure the apparent luminosity, l , in $\text{erg s}^{-1} \text{ cm}^{-2}$, then $d = (L/4\pi l)^{1/2}$. These are the standard candles, and the method must have been discovered by the first paleolithic tribe to light campfires in more than one place. Absolute and apparent magnitudes were invented to make them look more difficult.

Second, if you know the real linear size, h , of something in astronomical units and measure its apparent size or angular diameter, θ'' , on the sky, then $d(\text{pc}) = h(\text{AU})/\theta''$. These are the standard meter sticks. Geometrical parallax is one of them, where the object whose linear size you know is the orbit of the Earth.

Third, if you know the real speed, V , of something in km s^{-1} perpendicular to your line of sight and measure its proper motion, μ , in arc sec yr^{-1} on the sky, then $d = V/4.74\mu$. I think of these as speedometers, and the generic problem is that usually you know V along the line of sight and μ in the plane of the sky. Light echoes are a special case, where $V = c$.

Fourth, one can sometimes measure the ratio of two quantities that depend on different powers of distance. Then, if you know the correct value of the ratio, distance falls out immediately. M/L is such a ratio when the virial theorem applies to a system of angular size θ and apparent brightness l , with $d = V^2\theta L/4\pi GMl$. Or, at least, it would be such a ratio if we knew what M/L should be for a galaxy or cluster. Öpik (1922) found the first modern distance to M31 by assuming M/L equal to that in the solar neighborhood.

The fifth category includes methods that have been calibrated with respect to items in the previous four sections (especially standard candle Cepheid variables), but whose physical underpinnings are not at all well understood. The Tully–Fisher relation is an example. It is tempting to call these voodoo methods.

A good distance indicator is, for preference, bright (so you can see it far away), common (so you won't be done in by small number statistics), narrow-minded (in

its range of luminosity, size, velocity, or whatever, so that you won't have to worry too much about Malmquist-type corrections), and capable of being understood. This last point is what Eddington must have had in mind when he said that he refused to believe an observation until it was confirmed by theory.

There are non-controversial examples of the first three types of indicators. For instance, if you study a moderate-resolution stellar spectrogram and conclude that it is of type G2V, with Population I composition, small $v \sin i$, weak calcium H and K reversal (etc.), you can take the luminosity of the corresponding star to be $1 L_{\odot}$ with reasonable confidence. Similarly, among the meter sticks, binaries whose orbits can be determined in more than one way (spectroscopic + eclipsing; visual or astrometric + spectroscopic) can yield good distances. The first of these seems to have been a study of Delta Orionis by Stebbins (1915), using, rather remarkably, a selenium photometer to see the eclipses. His account of the project includes an expression of gratitude for generous support over five years from the Rumford Fund, totalling \$750.

Non-controversial speedometers include the moving cluster method applied to the Hyades (and probably many other clusters when there are *Hipparcos* proper motions for them), statistical and secular parallax for suitably distributed populations of stars, and the expansion of spherical planetary nebulae, supernova remnants, and nova shells. The controversy, of course, is over whether they are suitably distributed or spherical! There are at present no non-controversial ratio or voodoo methods.

Each class of method has also had its historic failures. For instance, astronomers from Herschel to Kapteyn had put the Sun near the center of a small galaxy (universe) by using stars as standard candles (star gauging) and neglecting interstellar absorption. Some of the early efforts at correlating spiral redshifts with distances used meter sticks (apparent angular diameters of galaxies) as the distance indicator and failed or found odd relationships because there were no small galaxies in the more distant groups. Robert Trumpler's (1930) use of open clusters as meter sticks, on the other hand, counts as a major success.

Shapley's calibration of the Cepheid period-luminosity relation using statistical parallax (a speedometer) also stumbled over the neglect of absorption and, to a lesser extent, galactic rotation. Ludwig Silberstein's (1925) attempt to correlate red and blue shifts of both spiral nebulae and globular clusters with their distances counts as failed voodoo, since at most one person (Silberstein, 1926) understood the physics.

An alternative way of classifying methods of distance determination describes how they are calibrated. Primary indicators have geometrical parallaxes or are found in clusters whose Main Sequences can be fit to the Hyades (this includes Cepheid variables). Secondary indicators are calibrated on Cepheids and other primaries (for instance the luminosity function of planetary nebulae); tertiary ones on secondaries (including the brightest galaxy in a rich cluster). And a fourth class includes methods where the calibration comes from knowing something about the physics of the objects involved (for instance assuming that all type Ia supernovae

flash $0.7 M_{\odot}$ of CO to iron peak elements). Of course the classification we really want is good vs bad, and this does not correspond to any separation made above!

4. Methods in Current, Past, and Future Use

The underlying problem is that the candles (etc.), in which we have the most faith, like Cepheid variables and spectroscopic, eclipsing binaries, are relatively faint and can be seen only in galaxies where gravitationally-induced velocities conceal the expected smooth expansion. Just how far out you have to go to avoid this is not clearly established, but Virgo (the limit for Cepheids, even with intensive use of the Hubble Space Telescope) is not far enough, unless you have a great deal of confidence in modelling of the dynamics of the local supercluster. Thus it is that the sections following (and the literature of the subject) mention such a very large number of possibilities and worry about which is best, which agree, and what can be done about it all.

The game plan of the HST Key Project team (e.g., Kennicutt et al., 1995) is to obtain light curves for Cepheids in about 18 galaxies in the Virgo cluster or closer and use them to calibrate a number of secondary indicators, including the Tully–Fisher relation (between 21 cm line widths and infrared galaxy luminosity), surface brightness fluctuations (in E and early S galaxies), the luminosity function of planetary nebulae, the luminosity of type Ia supernovae (assuming them to be standard candles), the luminosities of type II supernovae (treated through model atmospheres of the non-black body emissivity of their photospheres, the expanding photosphere method), the $D_n - \sigma$ relation (for elliptical galaxies, between diameter and central velocity dispersion), and the globular cluster luminosity function. These, in turn, will calibrate brightest cluster members, the only rung of the traditional distance ladder that extends beyond about 100 Mpc.

4.1. CEPHEIDS, RR LYRAES, AND OTHER CANDLES FOR LOCAL USE

The variable stars of which Delta Cephei is the (untypical) prototype have been mainstays of the distance ladder from Hubble to Hubble. Shapley's original calibration used statistical parallax (radial velocities and proper motions plus the assumption of random motions in space). Modern determinations of the zero point rely on about ten galactic Cepheids in open clusters (unfortunately none with the long pulsation periods that characterize the brightest stars and are most important for larger distances) and the Baade–Wesselink method, in which the changing radial velocity of the stellar photosphere is integrated through time to get a linear dimension, and apparent brightness plus color (effective temperature) yield an angular dimension (a meterstick method). This latter also has problems (Albrow and Cottrell, 1996; Butler et al., 1996) connected with the detailed structure of atmospheres and where the lines form.

The Cepheid scale is under control to the extent that, assuming the zero point to be the same for Milky Way and Large Magellanic Cloud stars, the Key Project Team (Kennicutt et al., 1993) and Tammann et al. (1996) agree that the LMC has a distance modulus, $(m - M) = 18.5^m \pm 0.1$ (de Vaucouleurs, 1993, last placed it at 18.38). Freedman and Madore (1996) are reasonably confident that the zero point (absolute magnitude of a Cepheid with a particular period) of the P–L relation is not very sensitive to metal or helium abundance in the stars. Others (Stift, 1995; van den Bergh, 1995) are not so sure. Di Benedetto (1995) for instance, would place the LMC rather further out at $(m - M) = 18.64^m + 0.02$. Part of our confidence in Cepheid distances comes from understanding more or less how the pulsation works, though, as always, ‘more work is needed’ on period ratios where more than one mode is unstable (Buchler et al., 1996) and on the discrepancy between star masses implied by such mode ratios and those implied by evolutionary tracks (Antonella et al., 1996). On the whole, other candles usable over similar distances agree with the Cepheids within likely error bars (next few paragraphs), suggesting that, at most, 0.2^m remains to be gained or lost here.

RR Lyrae variables pulsate by more or less the same (opacity) mechanism as the Cepheids and are understood about as well, but belong to the old, metal-poor Population II. There is a systematic discrepancy when you look at RR Lyraes and Cepheids (or W Virginis stars) in the same nearby galaxy or globular cluster, in the sense that the RRL distance is always smaller by 10% or so (van den Bergh, 1995; Sasselov, 1997). Van den Bergh, at least, bets on the Cepheids as more reliable.

Many other kinds of stars have been proposed as potential standard candles. Most have in common with Cepheids that they cannot be seen beyond the Local Group, and so can serve only as calibrators or checks for other methods. The list, not necessarily complete, includes non-variable horizontal branch stars, asymptotic giant branch and post-AGB stars, Mira variables, semi-regular red supergiant variables, entire Hertzsprung–Russell diagrams or branches thereof (OB Main Sequence, AGB, RG), the tip of the red giant branch (Sakai et al., 1996; Soria et al., 1996), Hubble–Sandage variables, the brightest non-variable stars (Sandage et al., 1996; Sohn and Davidge, 1996), stellar luminosity functions, W Virginis stars, stellar associations, faintest resolvable stars, and the BCD (Barbier, Chalonge, Divan) calibration of supergiants in terms of three rather mysterious spectroscopic parameters.

Many of these have gone rather out of fashion (see Hodge, 1981; de Vaucouleurs, 1993 for additional discussion and references), others are just being tried (see contributed papers in Livio and Donahue, 1997). One item that surprised me is just how sharp the tip of the red giant branch is in HR diagrams of nearby galaxies (Freedman and Madore, 1996), apparently because a wide range of stellar masses ignite helium at nearly the same luminosity, and more massive or more evolved (AGB) stars are rare. Some of the methods remain useful as bridges from distances where Cepheids can be studied to the regime of rich clusters or provide consistency checks on more popular methods (Ajhar et al., 1996; Huterer et al., 1995).

Critical readers will have noted that some of these ‘standard stars’ are advertized as truly being of a single, fixed brightness (the brightest red or blue supergiants or Hubble–Sandage variables, the RG tip). Others are really relationships between brightness and something else you can measure (pulsation period, spectral type). Another critical point is that interstellar absorption in both our own and the other galaxy concerned is already an issue at this stage. The Key Project Team (Kennicutt et al., 1995) believes that the residual error associated with uncertain extinction is not more than $\pm 0.1^m$. This is, perhaps, true within the Local Group, although values differing by more have been assumed by different workers (tables in de Vaucouleurs, 1993). Not surprisingly, those who get the smallest distances have allowed for the most absorption.

4.2. MEHR LICHT (BRIGHTER STANDARD CANDLES)

Hubble identified the first of these brighter candles – the average galaxy. He also (accidentally) used another, bright H II regions, while mistaking them for individual stars (Sandage, 1958). Other examples are planetary nebulae, globular clusters, novae, supernovae (optical and radio; note that ones calibrated by expanding photosphere or Baade–Wesselink methods are meter sticks), and whole galaxies, whether the brightest one in a rich cluster or more ordinary sorts for which color, luminosity class, line strength, or central surface brightness is a luminosity indicator. Methods in which you attempt to find twin galaxies to our own (‘Sosies’ in de Vaucouleurs’ terminology) or to others whose distance you think you know, and assume them to have the same brightness, also fall in this class. As in the previous section, we find that some of these are supposed to be of truly unique brightness (brightest cluster galaxies, the peak of the luminosity function of globular clusters), others have luminosity correlated with some other measurable property in a way that makes sense (novae with maximum luminosity a function of decline rate), some are correlations we do not understand (luminosity vs surface brightness, Holmberg, 1958) and so might just as well be listed under the voodoo methods, and some may or may not be correlated with anything (type Ia supernovae).

Hodge (1981) and de Vaucouleurs (1993) provide references to many of the older methods that are no longer in fashion, and contributed papers in Livio and Donahue (1997) present a few that may belong to the future, including a sharp break the luminosity function of H II regions (‘Strömgren transition’) that occurs when the ionized regions go from being ionization limited to density limited, and radio supernovae (one of which has a ‘speedometer’ calibration, 1979C). Virtually every method has been discussed at some time by people from both the ‘long’ and ‘short’ distance scale points of view, but each camp also has its favorites. For instance, the luminosity functions of planetary nebulae seem to give systematically smaller distances than do type Ia supernovae.

The *globular clusters* of M31 and other Local Group galaxies were described by their discoverer (Hubble, 1932) as systematically fainter than those of the

Milky Way. This was, of course, because his galaxies were too close, and the recalibrated Cepheid scale implied by the non-detection of RR Lyraes in M31 made other populations comparables with ours (Shapley, 1955). The same year, Baum (1955) used a comparison between the brightest globulars in M87 and M31 to put the former (and the Virgo cluster) at a distance of 10 Mpc, implying a Hubble constant of about $150 \text{ km s}^{-1} \text{ Mpc}^{-1}$. He also opined (wrongly we would now say) that galaxy diameters would be better indicators at large distances than galaxy brightnesses because of the (erroneous) Stebbins–Whitford effect, which suggested that luminosity and color evolution were more rapid than size evolution.

The brightest clusters in a galaxy are not, however, good candles, because the number of clusters can range from a few to a few thousand (remember the Scott effect). The peak of the luminosity function is, however, much more promising. Modern work goes back nearly 20 years (Hanes, 1979) and continues to find that the peak is a quite remarkably weak function of galaxy type and luminosity (Whitmore in Livio and Donahue, 1997; Harris in Livio and Donahue, 1997). There is, however, some variation, even between the Milky Way and M31 (Reed et al., 1994), neither of which is a very good calibrator for the cluster-rich giant elliptical galaxies that can carry this candle out to $m - M = 36$ or thereabouts. In addition, the cluster system of M87 appears to be bimodal in both $N(L)$ and composition (Elson and Santiago, 1996), and there is evidence from galaxies in the Fornax cluster that peak luminosity may depend on the environment in which a galaxy finds itself as well as (or instead of) on its Hubble type or luminosity (Blakeslee and Tonry, 1996). Not surprisingly, the method leads to large distances when applied by people who believe in large distances (Sandage and Tammann, 1995) and to small distances when applied by people who believe in small distances (Jacoby et al., 1996).

Planetary nebulae reprocess much of the ultraviolet radiation from their central stars into a few optical emission lines, especially when they are young. Individual ones are very far indeed from being standard anythings (to the point where distances within the Milky Way are disputed over factors of three or more). Hodge (1966) and later Ford and Jenner (1978) suggested that one might do better with the luminosity function (as in the case of globular clusters as candles). The recent work is virtually all associated with the names of Jacoby (1980) and Ciardullo et al. (1989). Their point is that the number of PNe as a function of brightness in the 5007Å line has a very sharp edge (observationally, and understood as an effect of the evolutionary tracks of the central stars) that is nearly the same for all galaxies.

An important virtue of PNe as a distance indicator is that they occur in all stellar populations and so in all kinds of galaxies. Unfortunately, it is not quite so certain that the bright edge is the same for all kinds. Jacoby et al. (1996) believe that all is well, and that the planetaries (though visible at best only out to the Virgo cluster) confirm a short distance scale. Predictably, the other camp disagrees (Bottinelli et al., 1991, for instance). It may be possible to allow for galaxy size and stellar population effects (Soffner et al., 1996), but this is not the same as saying that everybody will then agree. Even supporters of the method allow that there are

compact, emission line ‘overluminous’ objects that have to be ignored to use PNe as standard candles (Jacoby et al., 1996), or you end up with large distances. A few nearby planetaries have ‘speedometer’ calibrations of their distances, but it seems unlikely that this will be possible outside the Local Group in the near future (20 km s^{-1} is $1 \mu\text{arc sec yr}^{-1}$ at 4 Mpc).

Novae have been invoked as distance indicators in many different ways, as pure standard candles (Lundmark, 1946; McLaughlin, 1942, 1945) at peak brightness, as standard candles when 15 days past peak brightness (Buscombe and de Vaucouleurs, 1955), as candles with a correlation between peak brightness and rate of fading (Zwicky, 1936, writing before he had separated off supernovae from common novae), and as collective candles based on luminosity function or duration of visibility (see Jacoby et al., 1992; van den Bergh, 1988). As the data base grew larger and cleaner (McLaughlin, 1945; Arp, 1956; Rosino, 1973) the correlation of maximum brightness and decline rate grew tighter. In addition, we now have a good theoretical understanding of the underlying physics (Shara, 1989; Livio in Livio and Donahue, 1997). Bright equals fast because both are the result of a relatively small amount of mass being ejected (we are talking about numbers in the ballpark of $10^{-5} M_{\odot}$). The ejected mass, in turn, depends on how much accreted hydrogen a white dwarf has to build up before the bottom of the envelop is hot and dense enough to explode. Necessary accreted mass is smallest where the white dwarf is massive and the accretion rate rapid.

Novae in Virgo lead to a distance of 18.6 ± 3.7 Mpc (Della Valle and Livio, 1995), honest error bars wide enough to offend very few; though with slightly smaller ones, ‘long’ and ‘short’ nova-based distances begin to slide out of each others’ arms (van den Bergh, 1988; Sandage and Tammann, 1988). The Key Project Team have not included novae among the secondary distance indicators that they think are worth calibrating with Cepheids (Kennicutt et al., 1995) because of possible confusion with other kinds of variables and the large amount of observing time needed to find, and get good light curves for, the events. Among virtues of novae are their commonness (compared anyhow to supernovae) and potential for calibration as speedometers (Ringwald and Naylor, 1996, is a recent example).

Supernovae are the Jekell, Hyde, and kudzu (a semi-tropical weed that grows overnight over everything) of distance scales of the 1990s with more than a dozen numerical values of H reported in the last year alone (not all independent). Wilson (1939) and Zwicky (1939) thought of them as potential pure standard candles, in the days when only one type had been recognized. Later refinements include separation by types, correlation of peak brightness with decline rate (Pskovskii, 1984; Phillips et al., 1987), modeling of light curves and spectra as a function of initial energy input (Arnett, 1982, and many other efforts), and fitting of model atmospheres to observations (Branch and Patchett, 1973; Kirshner and Kwan, 1974; Wagoner 1977), this last belonging to the ‘standard meter stick’ category and analogous to Baade–Wesselink methods of calibrating the luminosity of Cepheid variables. In principle, both of the major types (nuclear detonation/deflagration = Ia; core

collapse = II, etc.) could be treated by any of the methods. In practice, most people have regarded the nuclear explosion type as the more suitable standard candle and the core collapse events as most amenable to atmospheric modelling.

Type Ia supernovae have occurred in a handful of galaxies with distances based on Cepheids observed with HST. Other things being equal, one expects that bright = slow = lots of Ni^{56} made in the explosion. How strong the correlation really is and whether one improves one's value of H by correcting for it remain in dispute (Tammann, Saha, Kirshner, Branch in Livio and Donahue, 1997; Tammann and Sandage, 1995; Nugent et al., 1995). It is known that there are a few anomalously faint events. The 'pure standard candle' camp says that they can be clearly identified and left out of the distance scale measurements. Others disagree. There is also disagreement about whether anomalously bright events exist that must also be left out. How to handle the statistics of SNe Ia is one of the main outstanding issues that separate the current long and short scale parties (van den Bergh vs Tammann, 1996, 1997). Both groups use most of the same, nearby calibrating SNe, though different distant ones. Neither picture looks entirely satisfactory if you did not draw it. According to the 'short' distance scale, the distant objects are systematically fainter than the nearby calibrators (a sort of inverse Malmquist bias); while according to the 'long' scale, the dispersion of absolute brightnesses is incredibly tiny.

Curiously, you can broaden the distribution by correcting for absorption of light in the parent galaxies of Ia's. According to van den Bergh (1996), this is perfectly reasonable. Absorption occurs mostly in spiral galaxies, where the progenitors of the supernovae (even Ia's) will have had larger masses on the Main Sequence, be able to make more Ni^{56} , and therefore be brighter.

From the Eddington point of view ('observations confirmed by theory'), type Ia supernovae remain an unsatisfactory candle because we have been unable to identify their progenitors. Cases have been made for merging pairs of white dwarfs (with a combination of large mass and short period not found in any known system) and recurrent novae (Branch et al., 1995), for recurrent supersoft X-ray binaries (Kahabka, 1995), and against some other likely-sounding combinations (Prialnik and Livio, 1995). One might have thought that the half dozen or so assorted supernovae seen in our own Galaxy in the past 2000 years would be of some use as calibrators, but this proves not to be the case (Schaefer, 1996).

Whole galaxies were Hubble's (1929) candle for all but the closest objects. They remain in the repertoire in several different ways, as brightest cluster members (Mould, Postman, Duemmler in Livio and Donahue, 1997), as members of morphological and luminosity classes (Sandage, 1996, and much earlier work referenced there), and as one side of correlations (like Tully-Fisher, $D_n - \sigma$, etc., discussed under voodoo methods). In all cases, Malmquist bias may be important. A large real spread in luminosities leads to large Malmquist corrections, and, therefore, to a relatively small Hubble constant.

4.3. STANDARD METER STICKS

The quintessential astronomical meter stick is the Earth's orbit around the Sun, and geometrical parallax is far and away the oldest method belonging to this category. I will say nothing more about it here, except that the 100 000 or more improved and new parallaxes expected from *Hipparcos* will stretch the fundamental distance scale out to about a kiloparsec and take in at least a few examples of many different interesting kinds of stars (Cepheids, RR Lyraes, nuclei of planetary nebulae...) that live in our zoo. Public release is currently scheduled for June 1997. Binaries with astrometric and spectroscopic orbits are, in principle, equally clean but are necessarily all very close to use and made of very common kinds of stars.

Spectroscopic eclipsing binaries, if sufficiently nearly edge-on and free of reflection effect in their light curves and atmospheric variability in their spectral line profiles are, in principle, equally good. Modern technology would seem to permit their study within at least the nearer globular clusters. A MACHO-class search is required to identify the few suitable systems per cluster that fall near the Main-Sequence turn-off, but one is rewarded not only with an accurate cluster distance (for calibration of RR Lyrae and other Population II candles) but also with the precise luminosities and masses of turn-off stars, which will make a major contribution toward narrowing the possible range of cluster ages (Paczynski in Livio and Donahue, 1997). See also Gaposchkin (1938, 1940) on the method.

A second category of meter stick includes a variety of 'galaxies and parts of galaxies' that one might reasonably claim to know the physical sizes of. Use of H II regions was pioneered by Sersic (1960) and revitalized by Kennicutt (1979). An interesting variant considers the correlation between the size of an H II region and the range of velocities in it (Melnick, 1977). Other potentially useful structures are the diameters of globular clusters, diameters of rings in ring galaxies, separation of spiral arms or the disk scale length in spirals of a given type (or resembling some nearby galaxy like M31 or M101), and the largest H II rings, loops, or superbubbles. De Vaucouleurs (1993) kept all of these in his inventory, Sandage (1993) and van den Bergh (in Livio and Donahue, 1997) have retained only diameters of whole galaxies, and the Key Project Team has not thought any of them worth calibrating.

In two relatively new approaches, the thing whose linear size you know is 'distance light travels in a given time'. Both are bedeviled by your having to know a good deal about the three-dimensional geometry of the objects involved. The first light echo ever seen was that of Nova Persei 1901. Nebulosity seemed to be moving out at remarkable speed, but to turn this into the distance to the 'flash bulb' or that to the reflecting screen, you need to know how the reflecting dust is distributed around the point source (Couderc, 1939; Felten, 1991). For supernova 1987A, the projected shape of the reflected light largely defines the dust distribution, and a distance of 55 kpc can be determined with considerable precision (Panagia et al., 1991; Panagia in Livio and Donahue, 1997). One has, of course, still to worry about just where the supernova exploded relative to the center of the galaxy along

the line of sight. An unsuccessful application of light echoes was Lynden-Bell's (1977) discussion of superluminal motions of VLBI components of radio sources. It led to a plausible answer ($110 \pm 10 \text{ km s}^{-1} \text{ Mpc}^{-1}$), but such quasar components are not, in fact, echoes.

Gravitational lensing of quasars (like supernovae if you understand their physics well enough to predict their brightnesses) enables us to jump right over the entire cosmic distance ladder and land on the roof (or in the rain barrel). Gravitational lensing of one galaxy by another was considered by Zwicky (1937). Refsdal (1964) pointed out that if the galaxy in back happened to experience a supernova explosion, then you would see two images of it as well, arriving at different times. He did the arithmetic for the simplest possible case of a spherically-symmetric lens and redshifts small enough to permit Euclidean geometry. No lensed supernovae have yet been caught, but the first lensed quasar, O957-561 (Walsh et al., 1979) is variable at both optical and radio wavelengths and far enough away ($z = 1.4$) that a precise distance would uniquely determine H (or some combination of H and q).

Qualitative analysis of the time delay between the two images is easy. For a given angular separation on the sky (and a given mass distribution in the lens), the longer the delay, the farther away the object is. Although O957 is the only quasar for which anyone has yet suggested a very firm value of H , it has two problems. First, the time delay at optical and radio wavelengths seemed to be different. This ambiguity has now been resolved in favor of the shorter time delay, about 1.1 years, and smaller distance (Thomson, 1997; Turner in Livio and Donahue, 1997). In addition, however, the actual lens is a combination of a single galaxy and its host cluster, introducing additional complexity into the analysis (see Kundic et al., 1995, for a concise discussion). The resulting 'best value' of H ($82 \text{ km s}^{-1} \text{ Mpc}^{-1}$) thus has large and uncertain error bars (Grogan and Narayan in Livio and Donahue, 1997). The inventory of lensed qso's is now a dozen or more, and some may eventually prove to be cleaner distance indicators (e.g., UM 425, Courbin et al., 1995) than O957.

Type II supernovae provide meter sticks if you analyze their light curves with non-black-body model atmospheres and allow for scattering of light. The resulting 'grey bodies' tend to be less bright (closer) than you would guess (Schmidt et al., 1994, e.g.).

4.4. SPEEDOMETERS

Most of the standard applications of $V_r \text{ (km s}^{-1}\text{)} = 4.74\mu \text{ ("}\text{yr}^{-1}\text{)} d(\text{pc})$ work only locally. These include statistical and secular parallax (one uses the reflex of the solar motion as the velocity to be measured; the other uses the random motions of stars); expansion parallaxes for planetary nebulae, nova shells, and supernovae; and the moving cluster method (think about how a bound cluster of stars would seem to be expanding on the sky if it were coming at you with a definite speed; now look at the thing from a random angle). One might imagine using radial velocity

and proper motion dispersions within globular clusters to determine their distances, but the required precision is of order 10^{-6} arc sec yr $^{-1}$.

A new and very exciting speedometer lives in galaxies with disks of H₂O maser sources around their cores (which probably contain moderate mass black holes). For the prototype source, NGC 4258, a combination of radial velocities of the maser emission and VLBI imaging of the positions of the sources strongly indicates that there is motion in Keplerian orbits. Proper motion at about 3.5×10^{-5} arc sec yr $^{-1}$ is expected. That it has not already been seen sets a lower limit of 5.4 Mpc to the source distance (Greenhill et al., 1995). NGC 4258 is not quite far enough away for a precise measure of its distance to tell us much about H . But we expect on both observational and theoretical grounds that more distant maser disks will be found and that secular acceleration, confirming Keplerian motion, will also be detectable (Neufeld and Maloney, 1995; Wilson et al., 1995; Kiekemeier et al., 1995).

4.5. RATIO METHODS

Mass-to-light ratio in spiral galaxies was the first method of this sort, applied to M31 by Öpik (1922), taking $M/L = 2.6$ (in solar units) for the visible part of the Galaxy. It was perhaps last seriously applied by Roberts (1969), using rotation speeds measured at 21 cm and $M/L = 4.3$. He found $H = 105$. Soon thereafter, dark matter came crashing down around our heads, and it would be a brave fin-de-siècle astronomer who claimed to know the correct mass-to-light ratio of any galaxy in any units.

A method called ‘velocity ratios’ turns up in some tables of techniques for finding H . It is less profound than it sounds. The idea is just that you take the distance ratio, based on some reliable indicator, between a nearby galaxy or cluster (too close to be in the smooth Hubble flow) and the measured redshift of the more distant object and thereby figure out what the proper cosmological redshift of the nearby one should be. M101 to Virgo and Virgo to Coma are common steps.

The criterion for a gas disk in a spiral to be stable against clumping and all turning into molecules is a ratio method I had not encountered until this year (Zasov and Bizyaev, 1996). They assume the instability occurs at the radius (in arc sec) where surface density in H I and H₂ are equal and then, using measured rotation speed, calculate where it should be in kiloparsecs. I suppose this could also have been included among the meter sticks.

The two ratio methods in most active use at the moment are surface brightness fluctuations and the Sunyaev–Zeldovich effect. On the whole, the former is popular with ‘short distance scale’ supporters and the latter with ‘long distance scale’ advocates.

Surface brightness fluctuations are a classic example of Poisson statistics (and are known to radio astronomers as $P(D)$, Scheuer, 1957). The idea has apparently floated around the optical astronomy community for a long time, but it was first clearly described and applied by Tonry and Schneider (1988) and has only recently

been taken up by others (Sodemann and Thomsen, 1996). I plagiarize shamelessly the following description from Tonry and Schneider. Suppose all galaxies (or all elliptical galaxies, or all bulges, or some such) consist entirely of stars of one luminosity L , and that for a galaxy at a given distance from you, 100 stars contribute to the light hitting one of your pixels. The pixel-to-pixel fluctuations will be 10% on average, $N^{1/2}/N = 10/100$, and the total light coming into a pixel will be $100L/4\pi d^2$. For a galaxy at twice the distance, light from 400 stars will hit your pixel, which will collect just as many photons. But the variations will be only 5% on average, $N^{1/2}/N = 20/400$. Thus, if you know L , you know d . Now correct this for the ‘detail’ that real galaxies consist of stars of a range of luminosities in some distribution, $N(L)$, that depends on the color band you choose, population type, and whatever else. The method currently reaches out to the Virgo and Fornax clusters (Tonry, 1991; and in Livio and Donahue, 1997), and if it is not, in the end, regarded as one of the best ways of measuring distances, it is nevertheless a very interesting way of studying stellar populations (Jensen in Livio and Donahue, 1997; Sodemann and Thomsen, 1996).

The Sunyaev–Zeldovich effect (Zeldovich and Sunyaev, 1970, but first available to English speaking astronomers as Sunyaev and Zeldovich, 1972) is the decrement in 3K Rayleigh–Jeans photons when there is hot gas along the line of sight, which scatters those photons up to higher energy without changing their numbers. The first reported detection of the effect (Parijskii, 1972) was nearly simultaneous, but it was the better part of a decade before everyone agreed that it had been seen in more than 1 ± 1 cluster. Zeldovich and Sunyaev suggested looking for the effect as a way of being sure that X-ray emission from rich clusters of galaxies really was thermal Bremsstrahlung rather than inverse Compton scattering by relativistic electrons. That issue was actually settled by the discovery of iron lines in spectra of several clusters before the microwave decrement had been convincingly demonstrated.

Simultaneous observation of X-ray emission and S–Z decrement tells you the distance to the cluster because the latter depends on the integral of n_e along the line of sight ($n_e \theta d$, for instance, where θ is the observed angular size of a spherical cluster) and the former on n_e^2 times the volume of the cluster ($n_e^2 \theta^3 d^3$, for instance, again assuming sphericity and uniform electron density). Thus you have two equations in two unknowns and can solve for n_e and d , separately. Strictly speaking, you get, within the framework of the FRW models, a somewhat motley combination of luminosity and angular diameter distances, but this matters only if you are also out to find q_0 .

Who first realized that solving the two equations for the distance of a cluster of known redshift was a way of finding H ? I am not quite sure. Cavalieri et al. (1977) have the right equations in slightly awkward form (they were thinking about how to find clusters that had forgotten to form any galaxies) and say ‘a measure of $\Delta T/T$... implies some assessment of the distance... by comparison with the X-ray flux... (Equations (1) and (4))’. Gunn (1978) presents the method as his own and does not cite Cavalieri et al. (1977). He also gives the equations in the most transparent

possible form, assuming Euclidean space, in this set of lectures delivered in April 1978 at Saas-Fee. Finally, Silk and White (1979) incorporate a FRW metric and do the relativistically correct calculation, leading up to the suggestion that one nearby cluster and one at $z = 1$ could uniquely determine both H and q . They also cite no previous considerations, except those of Zeldovich and Sunyaev.

Finally, Birkinshaw (1979), with the first S–Z decrement measurement from which a value of H was extracted, mentions explicitly that he had the idea from Longair, who was another of the lecturers at the, 1978 Saas-Fee summer school, who had it from Gunn. Birkinshaw’s and other early, preliminary distance scales from the S–Z effect were all in the ‘long’ camp. It is easy to think of why this might be wrong. If clusters are not spherical, S–Z decrement will be easiest to see for prolate ones viewed end-on. But the linear length you associate with a given angular diameter will then be too big and your distance too large.

Now that the decrement is routinely seen in many clusters (e.g., Carlstrom et al., 1996; Grainge et al., 1996) out perhaps as far as $z = 0.3$ (Andreani et al., 1996), it is both possible and important to consider complete, X-ray selected samples of clusters, rather than individual objects, and to include upper limits on the S–Z decrement where it is not seen. Even then, quite small values of H seem to be favored (Lasenby in Livio and Donahue, 1997). Nor am I sure enough that the technique should be renamed the Cavalieri–Danese–de Zotti–Gunn–Silk–White method to advocate this strongly!

4.6. VODOO

All distance indicators that make use of some correlation between real luminosity (or real physical size) and something you can measure without knowing distance, where the correlation is not very well understood, belong in this category, including a good many mentioned in previous sections. The classification as voodoo is, I believe, due to Robert Kirshner, and it is here restricted to correlations that are supposed to tell you about whole galaxies, of whose formation and evolution we have rather little theoretical understanding beyond the level of the ‘adjustable parameter.’

Hubble’s scheme for classifying galaxies (though it was initially rejected at the 1928 general assembly of the International Astronomical Union) has stood the test of time remarkably well. It was, however, essentially one-dimensional, like spectral types OBAFGKM before luminosity classes were added. Van den Bergh’s (1960c) second dimension (the DDO system) was explicitly a luminosity classification for spirals, based on degree of development of their arms, with long, well-developed arms indicating the brightest galaxies. De Vaucouleurs’ (1979a) luminosity classifications, involving complex combinations of surface brightness and shape parameters, was a somewhat remote descendent, not much used by other groups.

Most of the recent correlations refer to only one (Hubble) type of galaxy, elliptical or spiral. Elliptical galaxies display a reasonable correlation between optical brightness and central velocity dispersion (roughly to the fourth power), now called the Faber–Jackson (1976) relation. The updated, improved version is called $D_n - \sigma$ (Dressler et al., 1987; Faber et al., 1989, the authors often collectively referred to as the Seven Samurai). D_n is the diameter of the B isophote at the level 20.75^m (arc sec) $^{-2}$ and σ is the central velocity dispersion. So far, this correlation has been used primarily for finding deviations from smooth Hubble flow-evidence for large-scale structure revealed by galaxies having the wrong distances for their measured redshifts. There has been no large elliptical with a well-calibrated distance that could be used to pin down the zero point of $D_n - \sigma$ accurately. Cepheid distances to galaxies in the Virgo and Fornax clusters are rapidly changing this.

The corresponding dynamical correlation for spiral galaxies is called the Tully–Fisher (1977) relation, or, when infrared luminosities are used, the IRTF (Aaronson et al., 1979). As in the case of ellipticals, total visible or infrared luminosity scales roughly as V^4 , where V is the width of the 21-cm emission line of neutral hydrogen. Because most spiral galaxies have constant rotational velocity in their outer regions (‘flat’ velocity curves) the line edges are quite well defined. Optical or radio images are used to determine the orientations of the galaxies and correct the observed line width to what it would be if the galaxy were seen edge-on (observed apparent brightnesses must also, of course, be corrected, to what would be seen face-on).

All practitioners agree that the TF relation is a good distance indicator, capable of reaching out to large distances and tracing the Hubble flow beyond even the largest inhomogeneities in clustering and motions of galaxies, and the very largest advertized inhomogeneities have indeed been found using it (e.g., Lauer and Postman, 1992, who conclude that anisotropies are less than about 7% on the scale $z = 0.05$).

Nevertheless, values of the Hubble constant that are reported as coming primarily from Tully–Fisher calibrations differ by very considerably more than their claimed errors bars, for instance 84 ± 4 km s $^{-1}$ Mpc $^{-1}$ (Ford et al., 1996) vs 48 ± 5 (Sandage, 1994). There is still some discussion about how best to handle statistical issues (Triay et al., 1996). But this is not the core of the problem. Disagreement sets in very close by, for the only two galaxies (M31 and M33) that have ground-based Cepheid calibrations and line widths as large as most of the more distant galaxies employed ($\gtrsim 150$ km s $^{-1}$), amounting to about 15% for the distance of M33.

The largest discrepancy arises, however, when corrections are made for Malmquist bias. The very small number of local calibrating galaxies define a TF relation with only 0.16^m dispersion. If this is also correct for more distant samples (e.g., Bernstein et al., 1994), then the correction is exceedingly small, and you are likely to end up with H in the range 70–85 km s $^{-1}$ Mpc $^{-1}$. There is again general agreement that the dispersion is smaller for very bright galaxies than for dwarfs (Meurer et al., 1996; Hoffman in Livio and Donahue, 1997) and large for peculiar galaxies (Willick et al., 1996). But Federspiel et al. (1994) conclude that $\sigma = 0.4^m$ for

even the brightest, leading to a large Malmquist correction and a small value of H . With even a little bit of luck, the full set of spirals to receive Cepheid calibrations with HST should reveal the full range of dispersion around the mean TF relation locally. But, of course, arguments about environment-dependent effects will still be possible.

An assortment of plausibility arguments have been made that one or another of the scenarios for galaxy formation ought to produce something like the TF and $D_n - \sigma$ relations, but the real world is probably more complex (Eisenstein and Loeb, 1996), and I believe that the relations are not physically understood at the present time.

A relatively new player on the voodoo stage is the discovery that the surface-brightness profiles of dwarf spheroidal galaxies become steeper with decreasing luminosity (Young and Currie, 1994). They have looked only at the Fornax cluster, but it should be possible to carry out a similar analysis for Virgo (van den Bergh, 1994). The correlation for dSph's between central surface brightness and total luminosity has already been applied in both clusters (Bothun et al., 1989). Young and Currie (1995) report that they cannot quite reach the core of Virgo.

4.7. THE VIRGO CLUSTER

The poor old Virgo cluster(s) is not even rich enough to make it into the Abell catalog, and compared to Abell clusters it has relatively few very bright elliptical galaxies (one of which is M87 or Virgo A, and the others aren't, including the brightest, NGC 4472), relatively many spirals and irregulars, and a great many dwarf spheroidals. It is just a little too close to us to be a good direct probe of Hubble flow and a little too far away to have its stellar populations well studied from the ground. It is, nevertheless, our nearest neighbor, and a major, broad step on the climb to an extragalactic distance scale.

De Vaucouleurs (1961, 1973) was of the opinion that the concentrations of spiral and elliptical galaxies were really separate entities, separated by as much as 5 Mpc and 300 km s^{-1} , with the spiral cloud further away. This remained his view to the end (de Vaucouleurs, 1993), though the gap eventually shrunk to about 250 kpc (cf. de Vaucouleurs, 1982).

In the context of long vs short distance scales, Virgo continues to present two major problems. First, what is our real distance from its center(s)? And, second, what would our Hubble velocity relative to it be if it were not there, exerting gravitational forces on us? The smallest distance reported in modern times is 11.8 Mpc (de Vaucouleurs, 1982a) and the largest 27.7 (Sandage, 1993), though these are extreme even by the standards of the two camps. Reasonable representative values are 21.3 Mpc (Sandage and Tammann, 1995), 14.5 Mpc (de Vaucouleurs, 1993), and 15–17 Mpc (van den Bergh, 1994, 1996). The Key Project Team, making use of Cepheids found first in M100 and later in additional Virgo spirals,

have reported distances between 16 and 17 Mpc for the cluster center, with errors of 1–1.5 Mpc (Ferrarese et al., 1996; Freedman and Madore, 1996).

Now, what is our ‘real’ cosmological velocity relative to the center of the Virgo cluster? First, you must measure enough red (and blue!) shifts that your average is well defined and correct it to the center of the Local Group. Even here there are disagreements at the 50–100 km s^{−1} level (Binggelli et al., 1993; Jergen and Tammann, 1993, the latter incorrectly cited by Sandage and Tammann, 1995). Next you have to allow for what is often called ‘Virgocentric infall’, meaning that we and Virgo are moving apart from each other slower than would be the case without gravitational attraction between us (not that we are literally falling in). De Vaucouleurs (1964) first pointed out the effect and thought it might be about 250 km s^{−1}. Reviewing a much larger data base, I (Trimble, 1987) said 250 ± 50 . Values from 100 to 450 km s^{−1} have been suggested in recent times, but, as van den Bergh (1996) points out, it is possible to short-circuit part of the problem, using the ratio of distances between Virgo and the Coma cluster and the better-known cosmic velocity of Coma (simply because it is more distant), to arrive at a corrected $v = 1300 (\pm 10\%) \text{ km s}^{-1}$ for the Hubble part of the velocity between us and Virgo. Sandage and Tammann (1995) have naturally adopted a smaller number, near 1180 km s^{−1}.

The bottom line is that, without the Virgo cluster, discovery of the linear Hubble law would have come much later than it actually did, but the subsequent history might have been a happier one. And, lest I should leave the impression that error bars are moving together in friendship, it must be mentioned that values of Virgocentric infall published in the last year include $-52 \pm 67 \text{ km s}^{-1}$ (Gudehus, 1995), $+224 \pm 90 \text{ km s}^{-1}$ (Bureau et al., 1996), and $+350 \text{ km s}^{-1}$ (Schmidt and Karachetsev, 1996), but these last pick $d = 18 \text{ Mpc}$, so their Hubble constant is an average one.

5. Recent Evaluations of the Hubble Constant

Everyone who has discussed this topic since about 1965 has been forced either to ‘take sides’, to attempt the sweet reason of compromise (usually thereby making enemies on all sides), or simply to plot all values published during some period. All diagrams of ‘all the news that is fit to print’ fill the space from 45 or 50 to 85 or 100 km s^{−1} Mpc^{−1} quite densely, and, contrary to popular superstition, wiping out the points published by some particular author or group you happen to disapprove of does not much reduce the scatter (Kennicutt et al., 1995).

Figure 2 (a and b) is my version of a complete set. It overlaps only slightly the Kennicutt et al. (1995) plot; the pair of high values with small error bars near the end of 1993 (de Vaucouleurs’ last contribution to the subject) are roughly the match point. The time scale suddenly expands from years to days in Figure 2(b), so as to accomodate values reported in the symposium to be published as Livio and Donahue (1997), in the order people spoke. Points shown as X’s rely heavily

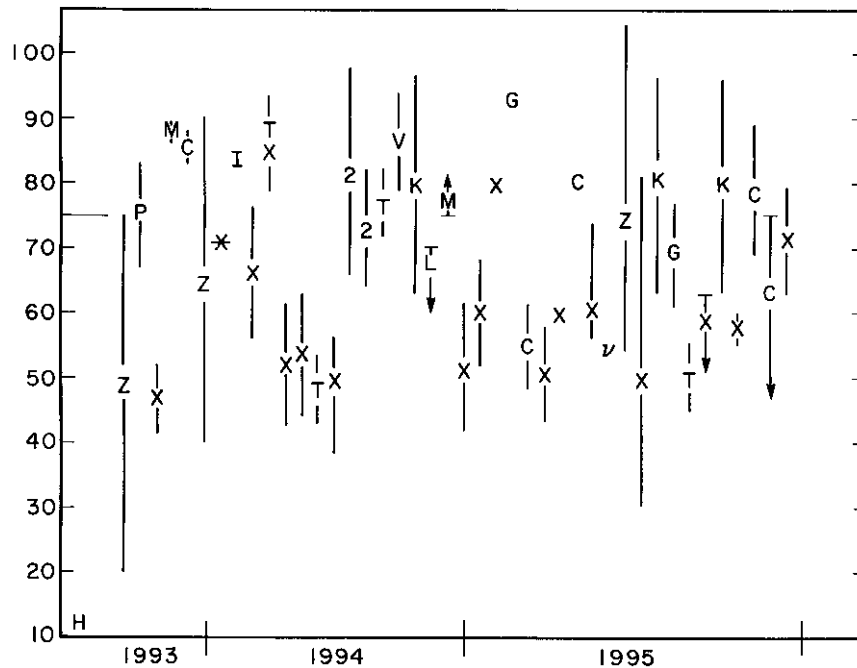


Figure 2a. Values of the Hubble constant published in recent years. The horizontal bar at left is the median ($75 \text{ km s}^{-1} \text{ Mpc}^{-1}$) of about a dozen numbers that appeared in 1992 and early 1993. The others are individual papers, coded by the primary distance scale indicator used. *B* = surface brightness fluctuations; *C* = globular clusters; *G* = properties of whole galaxies (brightness, scale length, stability); *H* = Harrison's extreme inhomogeneous model; *I* = infall to Virgo; *K* = HST Key Project Team; *L* = gravitational lensing; *M* = multiple methods (Virgo, Cepheids, brightest cluster galaxies, supernovae...); *P* = planetary nebulae; *T* = Tully-Fisher; *V* = Cepheids in Virgo other than Key Project; *X* = type Ia supernovae; *Z* = Sunyaev-Zeldovich effect; * = individual bright stars; 2 = type II supernovae; ν = particular kind of unstable neutrino. Absence of error bars means either that none were given or that they are smaller than the symbol.

on type Ia supernovae. The codes for other points are explained in the caption. It is essentially impossible to keep such a plot complete and up-to-date. In reviewing my notes for this discussion, I found three values published in late 1995 or early 1996 that were accidentally left out, and 11 that reached library shelves between early June and mid August 1996. These are shown summarily in Table I. Some of the more interesting ones (in addition to the sources of Virgocentric infall velocities mentioned at the end of the last section) come from Markevitch et al. (1996, Sunyaev-Zeldovich effect), Grogin and Narayan (1996, on lensing of qso O957), Branch et al. (1996, Ia supernovae corrected for parent galaxies etc.), Ruiz-Lapuente (1996, Ia models), and Eastman (1996, type II supernovae by the expanding photosphere method).

Figure 3 also derives from the Space Telescope Science Institute workshop (Livio and Donahue, 1997). Most of us would, I think, perceive a correlation about like that shown by the dashed line, plus an outlier or two. In fact, the

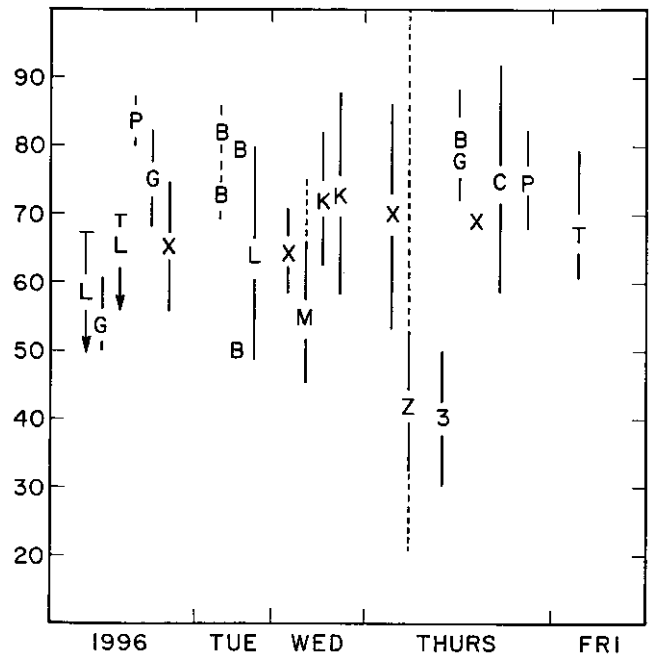


Figure 2b. Values of the Hubble constant published in 1996 and presented in review talks at the May Space Telescope Science Institute symposium on extragalactic distance scales (Livio and Donahue, 1997). Distance indicator codes as for Figure 2(a), plus 3 = first Doppler (acoustic) peak in the power spectrum of microwave background fluctuations.

Table I
Values of H_0 published in summer 1996 or Omitted from Figures 2 and 3

Numerical value	Method	Numerical value	Method
46–50	Virgo infall	44–57	M33, M100 look-alikes
42–110	Sunyaev–Zeldovich	56 ± 6	type Ia supernovae
55 ± 3	type Ia SNe	57 ± 4	type Ia SNe
74 ± 7	Tully–Fisher	$\leq 62 \pm 3$	globular clusters
82.5	lensing of 0957	57 ± 4	type Ia supernovae
68 ± 15	Ia models	55 ± 3	type Ia SNe
73 ± 10	Virgo infall	73 ± 7	type II supernovae

horizontal coordinate is simply poster number (approximately, author’s surname in alphabetical order).

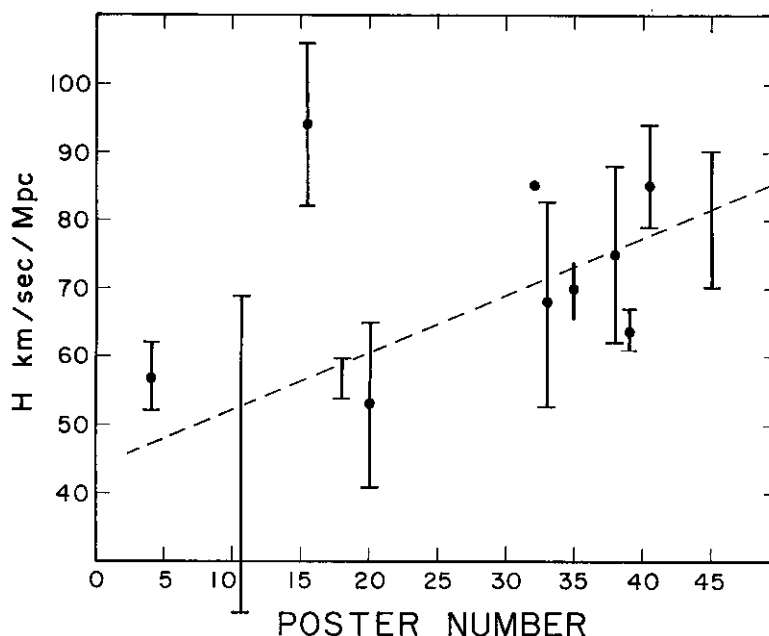


Figure 3. Numerical values of the Hubble constant reported in poster contributions at the STScI symposium (Livio and Donahue, 1997), plotted vs poster number (roughly, surname of the senior author, in alphabetical order). The dotted line is of course, not a physically meaningful correlation, though it looks about as persuasive as many in this discipline.

6. Residual Disagreements

The brief era of good feeling around 1960–1965 when $H = 100$ suited enough people to be stuck into lots of calculations has not recurred. Many astronomers for whom the distance scale is input rather than output have taken to assuming intermediate values of 65, 70, or $75 \text{ km s}^{-1} \text{ Mpc}^{-1}$, and there have been hints at conferences and in the handling of papers that anyone far outside this range need no longer be taken seriously. I think this is a mistake, and have mentioned in the preceeding sections specific areas of disagreement. They are summarized here.

The absorption of light in our own galaxy can be approximated by a cosecant law or measured from place to place using high latitude stars of known spectral type. The absorption in B at the North Galactic Pole has always been zero in analyses by Sandage and Tammann. De Vaucouleurs chose $A_B = 0.2$ (from systematics of galaxy colors in his catalogues). Recently, Knude (1996) has concluded that the average value is $A_B = 0.06$ for the half-thickness of the galactic disk, but that there is a real range from 0.0 to 0.2.

The slope of the period-luminosity relation of Cepheid variables in any single color band is known observationally to vary hardly at all (Freedman and Madore, 1996, and many others). It remains just barely conceivable that the zero point is

much more sensitive to metallicity or some other aspect of stellar populations than has been allowed for. Freedman (in Livio and Donahue, 1997) noted that this is one of the few remaining wild cards in the Key Project program. Their rational error budget allows only $\pm 0.1^m$; extreme long and short calibrations probably still differ by at least 0.2^m .

Moving outward, we next come to the question of our real, cosmological velocity relative to the center of the Virgo cluster. While some very small error bars have been published (or implied by other numbers), van den Bergh (1996, 1994) believes that the true uncertainty remains about 10% around a likely value of 1300 km s^{-1} . This is, of course, another 0.2^m .

The situation with respect to type Ia supernovae is rather complicated. The seven events described in Sandage et al. (1996) have a maximum absolute B magnitude of -19.49 (unweighted mean). The seven listed by van den Bergh (1996), with five events in common, and excluding SN 1885 and the spectroscopically anomalous 1991bg have a weighted mean of $M_B(\text{max}) = -19.54$. For once, something agrees within the stated error bars. But van den Bergh also considers eight events in E and SO galaxies in Virgo, and their mean (with a distance near 17 Mpc) is -18.76 , almost a whole magnitude fainter than the average when they are excluded. Thus any distance scale or Hubble constant leaning heavily on supernova data is going to depend on which mean you think is more appropriate for the more distant objects. By this point on the ladder, several contributions from absorption, Virgocentric infall, and so forth have tangled themselves up, but there is clearly another 0.2^m or more that can be more or less specifically attributed to handling of statistics of SNe Ia.

The final major disagreement concerns the width of the Tully–Fisher relation between spiral luminosities and H I linewidths and, therefore, the amount of Malmquist bias that must be corrected for in going to large distances. No one supposes that the width is zero, but the ‘short’ and ‘long’ scale proponents have settled on roughly 0.16 and 0.40^m for the bright end of the relation. A valid calculation does not simply say that this makes a 0.24^m difference in calibrations of distance scales attributable to this specific disagreement, but the right answer is not so very different (see the Spaenhauer diagrams in Federspiel et al., 1994, and, no, I don’t know who Spaenhauer was, but I promise to tell you if I find out before proof time).

We have, in other words, identified five sources of disagreement, each of which does or could amount to about 0.2^m . If you added these in quadrature, everybody would still be friends. But this is not what happens. Rather they are more or less summed, leading to 60–70% difference from shortest to longest distance scales. The Key Project Team has recently modified their ‘best buy’ H to 73 ± 15 (both random and systematic error, Freedman and Mould in Livio and Donahue, 1997). Strong advocates of planetary nebulae and surface brightness fluctuations are centered somewhere around the $82.5 \text{ km s}^{-1} \text{ Mpc}^{-1}$ implied by a particular lensing model of the quasar O957, and 65% of 82.5 is indeed 53.6, a very typical ‘long scale’ value of H .

It is perhaps worth reiterating here that any H greater than about $60 \text{ km s}^{-1} \text{ Mpc}^{-1}$ lives very precariously with the ages of the globular clusters (van den Bergh, 1996, and many other places). De Vaucouleurs (1982, 1993) gradually came to favor a non-zero cosmological constant as the most likely solution, although his numbers required one larger than gravitational lensing considerations now seem to permit (Section 2).

Is there a ‘Trimble value’ of the Hubble parameter? Frankly no. I would not bet more than a bottle of good wine (red for preference) on any band narrower than $30\text{--}120 \text{ km s}^{-1} \text{ Mpc}^{-1}$. I can, after all, remember when a straight line drawn through a plot of published H ’s as a function of time (the left part of Figure 1) was predicted to go through zero in the mid-1970’s.

7. Looking Ahead

Key Projects for the (Hubble) Space Telescope were chosen in 1985 by a very broadly based peer advisory committee. Three were selected, a medium-deep survey (moderately long exposures of random bits of sky), quasar absorption lines, and an effort to pin down the extragalactic distance scale to 10% or better. Proposals to accomplish these tasks were specifically invited in the first HST cycle of general observer proposals. The first time assignment committee chose one proposal in each of the three designated areas in 1989. The winning distance scale proposal has had four co-principle investigators (M. Aaronson, W. Freedman, R. Kennicutt, and J. Mould) and involved another dozen senior astronomers and a comparable number of graduate students and postdocs. The TAC also decided to award time to a smaller team headed by A. Sandage for the purpose of observing Cepheids in galaxies that have hosted type Ia supernovae.

Following the discovery of the optical flaw in the primary mirror of HST, a subset of the first TAC reconvened, designated a (small) subset of proposals originally chosen to receive time during the first cycle (including the three key projects) and endorsed the desire of the guaranteed time observers and the key project teams to defer much of their observing until after the hoped-for repair mission. The later TACs of cycles 2 to 6 have continued to award time to both groups interested in the distance scale (Disclaimer: the present author served on the cycle 1 committee and its post-launch version, and on the cycle 3 and 6 committees, but claims no credit for their collective wisdom).

Observations directed at measuring H indeed began during cycle 1 and continued slowly during the remainder of the ‘pre-fix’ period and at accelerated pace following the enormously successful repair mission. Data collection for the Medium Deep Survey and QSO Absorption Line Projects is now complete. Data collection for the distance scale KP should be nearly complete within cycle 6 (February 1997, roughly the time of the next expected servicing mission) and entirely completed in August 1998, with final data analysis to be accomplished by the end of that year.

Observations of galaxies selected by the Sandage team have also continued. These data will eventually all become part of archives accessible to any astronomer with the time, expertise, and equipment to download large files.

The results are slowly creeping into refereed journals and conference proceedings (e.g., Ferrarese et al., 1996, with the definitive discussion of M100, the first Virgo galaxy; Saha et al., 1996, on the Cepheid calibration of NGC 4536, host of SN 1981B;; Silbermann et al., 1996; and Graham et al., 1996, on the Cepheid populations in galaxies in small groups with redshifts of $700\text{--}800\text{ km s}^{-1}$; Sandage et al., 1996, on Cepheids in NGC 4639, host of SN 1990N; Mould et al., Freedman et al., and a number of poster contributions in Livio and Donahue, 1997; and a number of projects best described as ‘in preparation’ or ‘under contemplation’). One can reasonably expect to have everything in print by the end of the decade.

It is likely that most of the goals described in Kennicutt et al. (1995) will actually be met – that is, enormous increases in the number of galaxies, groups, and clusters with primary, Cepheid, distances that can be used to calibrate the secondary indicators they chose to emphasize. These are six: the Tully–Fisher relation, surface brightness fluctuations, the luminosity function of planetary nebulae, type Ia supernovae, type II supernovae by the expanding photosphere method, and $D_n - \sigma$. Notice what a small fraction this is of all the methods mentioned in Section 4, and de Vaucouleurs, at least, would have said that this is a sad mistake. The Sandage team is focussing almost entirely on Cepheid calibrations of SNe Ia.

The goal in all cases is to know the average absolute magnitude (and scatter) of each secondary indicator to an uncertainty between 0.03 and 0.13^m . And I believe that each group involved will feel that it has achieved these goals by the time the last papers appear.

Under the circumstances, a critical reader may feel that it is a silly time for anybody to be taking the trouble to write a review of extragalactic distance scales and the Hubble constant. You may be right. A similar number of years after the Curtis–Shapley debate, Hubble found Cepheids in Andromeda, and everybody knew the answer. Like Fermi, I find it difficult to make predictions, especially about the future. But, in this case, I will venture one. It is that, come 2000 (when I finally rotate off the organizing committee of the Commission on Galaxies of the International Astronomical Union), a significant fraction of the astronomical community will feel that the issues of the distance scale and H have not yet been fully resolved; or, at any rate, that they don’t much like the answer and that there are still conflicts among H^{-1} , the other measurable parameters, and the apparent age of the Universe.

What else can conceivably be done? First, absorption in our own and other galaxies must be dealt with on a case-by-case basis. Magellanic Cloud Cepheids now have light curves in 7 colors (Freedman and Madore, 1996), with the scatter least at K . Type Ia supernovae that are found in the future in galaxies with redshifts of $3000\text{--}30\,000\text{ km s}^{-1}$ (or thereabouts) need the same kind of multi-color treatment.

Next, population issues need to be dealt with by both finding and understanding the scatter in surface brightness fluctuations, in the peaks of luminosity functions of globular clusters, in the brightest planetary nebulae in a galaxy, etc. There are parts of this program suitable for users of HST, observers with access to large and moderate ground-based telescopes, and theorists galore. Similar remarks apply to the scatter in the Tully–Fisher and $D_n - \sigma$ relations. Though I am not optimistic about these being understood any time soon, it must be possible to get consensus on how big they are by looking at enough galaxies (and then throwing away most of the data, which is the only guaranteed way to triumph over Malmquist and Scott).

Progress can undoubtedly be made on the other parameters, Ω , Ω_b , Λ , and q , through continued efforts to define and model large-scale structure and streaming of galaxies and clusters. MAP, the US satellite intended to look for 1° structure in the 3K background is expected to be returning data before the year 2000, potentially providing a completely independent handle on H itself, as well as the other quantities. The European project, COBRAS/SAMBA, should have better angular resolution (and so a better chance of defining the several parameters independently), but is an eon or two further in the future. What happens if the Hubble parameter found in this way disagrees significantly with the one derived from Cepheids, supernovae and all the rest?

Finally, it is enormously important to find a clock that is completely independent of the Main Sequence turn-off mass in globular clusters but of comparable accuracy. Radioactive nuclides found in the solar system present the traditional problem of ‘model dependence’ (that is, what was the galaxy doing before the solar system formed?). Detection of thorium in one very old star (Snedden et al., 1996) is therefore of great importance. We still don’t know what the galaxy was doing before CS 22892–052 formed, but it hadn’t been doing it for nearly so long! Cowan et al. (1996) have examined the Th/Eu ratio, and conclude that thorium in the star has been decaying away for at least 11.5 Gyr, with the most probable age more like 15–17 Gyr. Given thorium in a handful of stars of slightly different age and metallicity, one could perhaps even trace out the early history of r -process nucleosynthesis, and find out what the Galaxy was doing.

Thus, while there is no chance that this, or any other epoch 1996, review of the distance scale can be definitive, it can perhaps serve as a sort of signpost to the road we think we are following. A distinguished American admiral is supposed to have explained his generous distribution of various awards and his parsimony in handing out promotions by saying: ‘Medals are cheap. Ships are expensive.’ Similarly, reviews are cheap; telescopes are expensive.

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