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Slipher, galaxies, and cosmological velocity fields

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Abstract. By 1917, V.M. Slipher had singlehandedly established a general tendency for 'spiral nebulae' to be redshifted (21 out of 25 cases). From a modern perspective, it could seem surprising that the discovery of the expansion of the universe was not announced at this point. Examination of the data and arguments contained in Slipher's papers shows that he reached a more subtle conclusion: the identification of cosmological peculiar velocities, including the bulk motion of the Milky Way, leading to a beautiful argument in favour of spiral nebulae as distant stellar systems. Nevertheless, Slipher's data actually contain evidence at $>8\sigma$ for a positive mean velocity, even after subtracting the best-fitting dipole pattern owing to motion of the observer. In 1929, Hubble provided distance estimates for a sample of no greater depth, using redshifts due almost entirely to Slipher. Hubble's distances turned out to be flawed in two distinct ways: in addition to an incorrect absolute calibration, the largest distances were systematically under-estimated. Nevertheless, he claimed the detection of a linear distance-redshift relation. Statistically, the evidence for such a correlation is less strong than the simple evidence for a positive mean velocity in Hubble's sample. Comparison with modern data shows that a sample of more than twice Hubble's depth would generally be required in order to reveal clearly the global linear expansion in the face of the 'noise' from peculiar velocities. When the theoretical context of the time is examined, the role of the de Sitter model and its prediction of a linear distance-redshift relation looms large. A number of searches for this relation were performed prior to Hubble over the period 1924–1928, with a similar degree of success. All were based on the velocities measured by Slipher, whose work from a Century ago stands out both for the precision of his measurements and for the subtle clarity of the arguments he employed to draw correct conclusions from them.

1. Introduction

This talk does not pretend to be a professional exercise in the history of science, but all cosmologists have a fascination with origins: thus we should expect to have a reasonable familiarity with the developments that set our subject in motion. There is indeed a conventional narrative that has been repeated in compressed form in innumerable classrooms and public talks – generally centring on Hubble (1929) as having provided the first observational evidence for an expanding universe. But this conference represents the convergence of many individual trajectories of re-evaluation, all reflecting a growing recognition that our standard tale is seriously at variance with the actual events.

Chief among the casualties of this over-simplification has been V.M. Slipher. His existence was not hidden, and he appears on the first page of the textbook by Peebles (1971), which was a great influence on my generation of cosmologists. But a proper appreciation of Slipher's work was perhaps hindered by the fact that all of his major papers appeared in the obscurity of internal Lowell reports, or journals that ceased publication. In 2004, I tried and failed to discover electronic versions of Slipher's seminal papers anywhere on the web. But the Royal Observatory Edinburgh is fortunate enough to possess an outstanding collection of historical journals, so I was able to track down the originals and make scans. Since then, I am proud to say that my complaints to ADS have had an effect: not only are Slipher's main papers all now listed (Slipher's 1917 masterpiece lacked any entry whatsoever in the database), but you can also find the scans I made through ADS.

Reading these papers cannot fail to generate an enormous respect for Slipher as a scientist: they are confidently argued, and make some points that are astonishingly perceptive with the aid of 21st-century hindsight. Given the confusion that naturally attended the first engagement with modern cosmological questions, this is all the more impressive. When all is unclear, it is tempting to hedge papers with so many qualifications that no conclusion ever emerges – but the mark of a great scientist is to stick your neck out and state firmly what you believe to be true. Slipher achieves this on a number of occasions, and his conclusions have stood the test of time.

The intention of this presentation is to try to illuminate the magnitude of Slipher's achievements by viewing them through the eyes of a working cosmologist, and going back to the analysis of the original data. In particular, by comparing with modern data, the aim is to understand why Slipher did not use the general tendency for galaxies to be redshifted as evidence for the expansion of the universe – but how he came to reach an under-appreciated conclusion of similar importance.

2. Slipher's great papers

Before focusing on Slipher's most important paper (Slipher 1917), it is worth giving a brief overview of his achievements during the period when he was the lone pioneer of 'nebular' spectroscopy. During all of this, it should be clearly borne in mind that the nature of the nebulae was unclear in this period; although the 'island universe' hypothesis of distant stellar systems was a known possibility, a considerable weight of opinion viewed the spiral nebulae as planetary systems in formation.

In Slipher (1913) the blueshift of Andromeda was measured to be $300\,\mathrm{km\,s^{-1}}$. This velocity was very high by the standards of the time, and there could understandably be skepticism about whether this really was a Doppler shift (cf. the quasar redshift controversy). Slipher trenchantly asserts that "...we have at the present no other interpretation for it. Hence we may conclude that the Andromeda Nebula is approaching the solar system...". Since the blue shift is now believed to be induced by dark-matter density perturbations, it is amusing to note Slipher's speculation that the nebula "might have encountered a dark star".

Slipher (1914) was unknown to me prior to my 2004 archival search, but appears to be the first demonstration that spiral galaxies rotate. This would make Slipher a figure of importance, even if he had done nothing else. A striking contrast with modern 'publish or perish' culture is Slipher's statement that he believed he had data showing the tilt of spectral lines, but was not fully satisfied; therefore he waited an entire year until he could repeat and check the results.

The paper in which Slipher presented his results to the American Astronomical Society (Slipher 1915; August 1914 meeting) is perhaps the most well-known. Out of 15 galaxies, 11 were clearly redshifted, and he received a standing ovation after reporting this fact. It must have been clear to all present that this was an observation of deep significance – even if the interpretation was lacking at the time.

The 1917 paper is the most extensive of Slipher's works on nebular spectroscopy, but surprisingly it seems to be less well known than the papers of 1913 and 1915, and I had never seen any mention of its contents prior to reading it for the first time in 2004. The redshift:blueshift ratio has now risen to 21:4, but it is the interpretation that is startling.

3. Slipher's intellectual leap of 1917

Although the mean redshift of the 1917 sample is large and positive, Slipher does not draw what might today be regarded as the obvious conclusion:

> The mean of the velocities with regard to sign is positive, implying that the nebulae are receding with a velocity of nearly 500 km. This might suggest that the spiral nebulae are scattering but their distribution on the sky is not in accord with this since they are inclined to cluster.

The term "scattering" clearly denotes a tendency to recede in all directions, which must be regarded as the most basic symptom of an expanding universe. The reason Slipher does not state this as a conclusion is because there is an issue of reference frame. Astronomers of this era were completely familiar with the fact that the Sun moves with respect to the nearby stars, inducing a dipole pattern in the observed velocities. It must therefore have seemed entirely natural to fit a dipole pattern to the sky distribution of velocities. Slipher makes this analysis, deducing a mean velocity of $700\,\mathrm{km\,s^{-1}}$ for the Sun and thus noting that we are not at rest with respect to the other galaxies on average. He then makes a tremendous intellectual leap, which is described in language of a beautiful clarity:

> We may in like manner determine our motion relative to the spiral nebulae, when sufficient material becomes available. A preliminary solution of the material at present available indicates that we are moving in the direction of right ascension 22 hours and declination -22° with a velocity of about 700 km. While the number of nebulae is small and their distribution poor this result may still be considered as indicating that we have some such drift through space. For us to have such motion and the stars not show it means that our whole stellar system moves and carries us with it. It has for a long time been suggested that the spiral nebulae are stellar systems seen at great distances ...This theory, it seems to me, gains favor in the present observations.

This argument is a dizzying shift of perspective: we start in the Milky Way looking out at the nebulae, from whose dipole reflex motion Slipher correctly infers that the entire Milky Way is in motion at a previously undreamed-of speed. This is almost as shocking a discovery as Copernicus's proposal that the Earth is in motion. But then the perspective shifts, and suddenly Slipher imagines himself to be within one of the nebulae – looking out at the Milky Way and other nebulae: since they all have rms motions in the region of $400\,\mathrm{km\,s^{-1}}$, they must clearly all be the same kind of thing. Hence the nebulae are hugely distant analogues of the Milky Way. This, remember, is 8 years before Hubble detected Cepheids in Andromeda and settled the 'island universe'

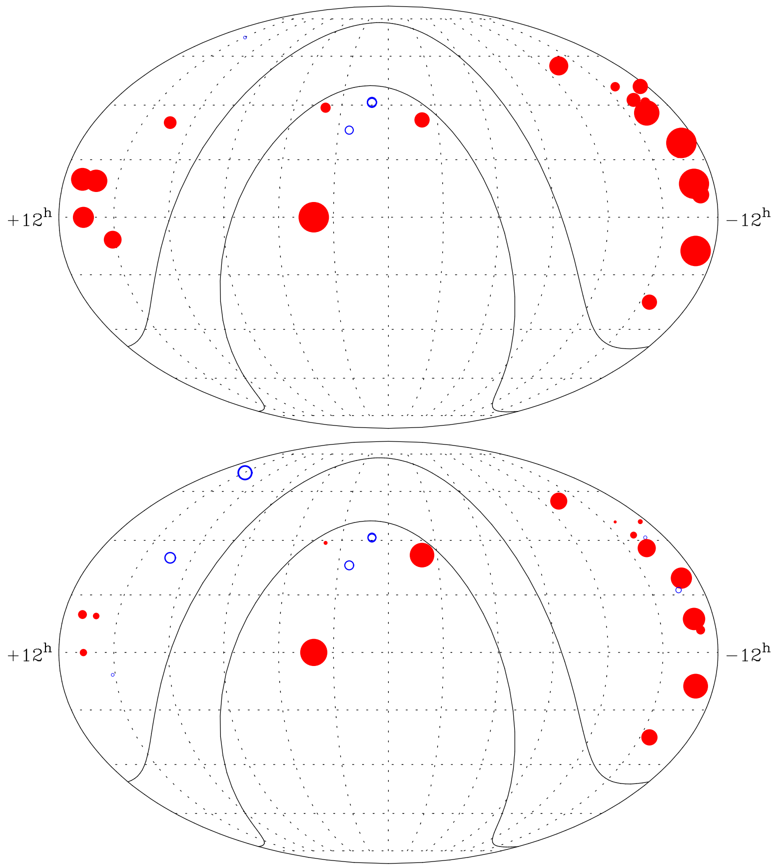

Figure 1. The sky distribution of Slipher's 1917 galaxies, in Mollweide projection of celestial coordinates, indicating the galactic plane at $b = \pm 15°$. Velocities covering the range $-300\,\mathrm{km\,s^{-1}}$ to $+1000\,\mathrm{km\,s^{-1}}$ are coded red solid (redshift) or blue open (blueshift), with the width of the symbol being proportional to the magnitude of the velocity. The top panel shows the observed velocities; the lower panel shows the same data after removal of the best-fitting dipole. Removal of the dipole reduces the mean velocity from $502\,\mathrm{km\,s^{-1}}$ to $145\,\mathrm{km\,s^{-1}}$ with a dispersion of $414\,\mathrm{km\,s^{-1}}$.

question directly. Slipher was not actually the first to consider measuring the motion of the Milky Way in this fashion (see Sullivan 1916; Truman 1916; Young & Harper 1916; Paddock 1916; Wirtz 1916, 1917; I thank Michael Way for pointing out these references). But all these investigations used Slipher's data, and somehow their conclusions lack his confidence and compact clarity regarding the physical implications – although it would be interesting to know if he was motivated by these earlier papers.

Given the neatness of the argument that Slipher uses here, one can hardly complain that he does not focus on the fact that the mean redshift is non-zero, even after adjustment for the best-fitting dipole. Indeed, this feature is not statistically compelling: the mean redshift after dipole subtraction is $145\,\mathrm{km\,s^{-1}}$ with an rms of $414\,\mathrm{km\,s^{-1}}$, which is only a 1.8σ deviation from zero. This transformation of the data can be seen at work in the sky distributions of Slipher's data shown in Figure 1: the largest velocities are concentrated around $\alpha = 12^h$, $\delta = 0°$ (dominated by the Virgo cluster), and so can be heavily reduced by an appropriate dipole – even though the pattern of residuals shown in the lower panel of Figure 1 is clearly non-random.

It is a great pity that Slipher did not revisit this analysis with the redshifts he continued to accumulate. Rather than write a further paper, he was content simply to have these results appear in Eddington's (1923) book (page 162). By this time, there were 41 velocities, of which 36 were positive; the most negative remained at the $-300\,\mathrm{km\,s^{-1}}$ of M31, whereas 5 objects had redshifts above $1000\,\mathrm{km\,s^{-1}}$, including $1800\,\mathrm{km\,s^{-1}}$ for NGC584, which came close to doubling the maximum velocity of the 1917 data. If we repeat Slipher's 1917 analysis with the expanded 1923 dataset, the mean velocity after subtracting the best-fitting dipole rises to $201\,\mathrm{km\,s^{-1}}$ with an rms of $508\,\mathrm{km\,s^{-1}}$; this is now a 2.5σ deviation from zero, and so Slipher's velocities alone give a very clear signal of a general tendency towards expansion (in fact, for reasons explained below, this analysis greatly underestimates the significance of the effect). Eddington does not attempt a dipole analysis, but his discussion of Slipher's data clearly focuses on the high mean value of the raw data as representing a general tendency for galaxies to be redshifted, albeit with some dispersion. However, this is not simply an abstract statement about the pattern of the numbers: for reasons explained in the following section, Eddington had a theoretical expectation of a general redshift.

4. The theoretical prior

By the time of Slipher's 1917 analysis, the theorists were on the march. Two years after the creation of General Relativity, Einstein (1917) had created his static cosmological model, introducing the cosmological constant for the purpose. This is a wonderful paper, which can be read in English in e.g. Bernstein & Feinberg (1986), and the basic argument is one that Newton might almost have generated. Consider an infinite uniform sea of matter, which we want to be static (an interesting question is whether Einstein was influenced by data in imposing this criterion, or whether he took it to be self-evident): we want zero gravitational force, so both the gravitational potential, Φ and the density, ρ have to be constant. The trouble is, this is inconsistent with Poisson's equation, $\nabla^2\Phi = 4\pi G\rho$. The 'obvious' solution (argues Einstein) is that the equation must be wrong, and he proposes instead

$$\nabla^2\Phi + \lambda\Phi = 4\pi G\rho, \tag{1}$$

where λ has the same logical role as the Λ term he then introduces into the field equations. In fact, this is not the correct static Newtonian limit of the field equations, which is $\nabla^2\Phi + \Lambda = 4\pi G\rho$. But either equation solves the question posed to Newton by Richard Bentley concerning the fate of an infinite mass distribution; Newton opted for a static model despite the inconsistency analysed above:

> . . . it seems to me that if the matter of our sun and planets, and all the matter of the universe, were evenly scattered throughout all the heavens, and every particle had an innate gravity towards all the rest, and the whole space, throughout which this matter was scattered, was but finite; the matter on the outside of this space would by its gravity tend towards all the matter on the inside, and by consequence fall down into the middle of the whole space, and there compose one great spherical mass. But if the matter was evenly dispersed throughout an infinite space, it would never convene into one mass. . .

(see e.g. pp. 94-105 of Janiak 2004). With the advantage of hindsight, Newton seems tantalisingly close at this time (10 December 1692) to anticipating Friedmann by over 200 years and predicting a dynamical universe.

But at almost the same time as Einstein's work, the first non-static cosmological model was enunciated by de Sitter (1917) – based on the same Λ term that was intended to ensure a static universe. It is interesting to compare the original forms of the metric in these two models, as they are rather similar:

$$\text{Einstein}: \quad d\tau^2 = -dr^2 - R^2 \sin^2(r/R) d\psi^2 + dt^2 \tag{2}$$

$$\text{deSitter}: \quad d\tau^2 = -dr^2 - R^2 \sin^2(r/R) d\psi^2 + \cos^2(r/R) dt^2 \tag{3}$$

Staring at these naively, it it tempting to conclude that clocks slow down at large distances, $\propto \cos(r/R)$, where R is a characteristic curvature radius of spacetime. In this case, a redshift-distance relation would be predicted to be $z \simeq r^2/2R^2$ i.e. quadratic in distance. But we have made an unjustified assumption here, which is that a free particle (or galaxy) will remain at constant r, which we know does not actually happen. For this reason, the correct redshift-distance relation is linear at lowest order. This was first demonstrated by Weyl (1923; the 5th edition of his book – frustratingly, the common Dover reprint is the 4th edition). This was also shown independently by Silberstein (1924) and Lemaître (1927; see Lemaître 1931 for an English translation). Interestingly, Eddington (1923) proves (on page 161) that test particles near the origin experience an outward acceleration proportional to distance, and from his discussion he clearly sees that this motion will make a contribution to the observed redshift – but he never clearly states that the leading effect is thus a linear term in $D(z)$.

News of this prediction seems to have spread rapidly, and there were soon a number of attempts to look for a linear relation between redshift and distance. It should be made clear that no-one at this stage was thinking about an expanding universe (Friedmann was perhaps an exception, but he was decoupled from the interplay between theory and experiment in the West). The aim was to search for the 'de Sitter effect' and thus 'measure the radius of curvature of spacetime'. This game can be played with any set of objects where radial velocities exist, together with some indicator of distance.

A number of people (Silberstein 1924; Wirtz 1924; Lundmark 1924) tried this, and the paper by Lundmark is particularly impressive and comprehensive. In general, distances to galaxies were lacking at that time, although the detection of Novae in M31 had suggested a distance around 500 kpc, which is not too far off. What Lundmark did was to assume that galaxies were standard objects; thus he was able to estimate distances in units of the M31 distance, based on diameters and on apparent magnitudes (these agree reasonably well). The distances clearly correlate with Slipher's redshifts, as shown in Lundmark's Figure 5 (recreated here as Figure 2). Lundmark was not as impressed with his result as perhaps he ought to have been: "...we find that there may be a relation between the two quantities, although not a very definite one". But despite the scatter, a positive correlation of distance and redshift does exist, of a significance so obvious that it hardly needs formal quantification. Thus by 1924 it was clear that radial velocities tended to be positive, and to increase with distance, even if it was not possible to say with any confidence that the redshifts scaled linearly with distance.

In any case, we reiterate that the physical understanding of the meaning of any distance-redshift relation still had some way to go in 1924. Despite Eddington's insight that there was a kinematical effect at work, the common interpretation of the de Sitter model in the above papers was the static view that redshifts simply probed the curvature of spacetime. And even in 1929 Hubble would mention "the de Sitter effect" and Eddington's argument for a kinematical contribution without actually saying that expansion dominates locally.

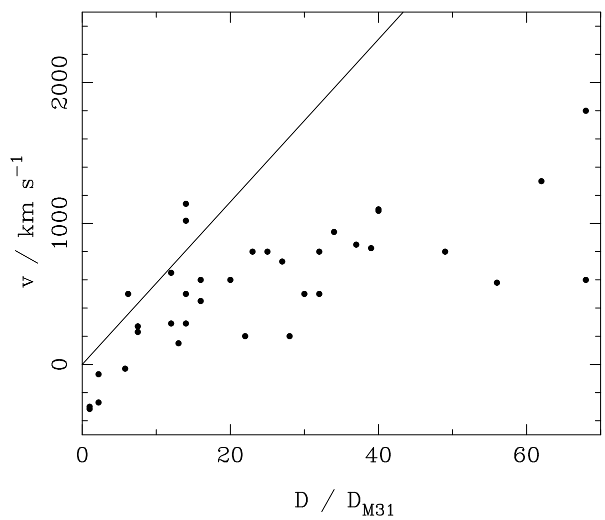

Figure 2. Lundmark (1924) searched systematically for a linear distance-redshift relation, using a variety of classes of astronomical object. His most impressive result was obtained using nebulae. Lacking any direct distance estimates for these, he took a standard-candle approach, in which relative distances were measured using the apparent magnitudes and/or diameters of nebulae; results were quoted in units where the distance to M31 was unity. The solid line shows the modern truth, assuming $H_0 = 73\,\mathrm{km\,s^{-1}Mpc^{-1}}$ and a distance to M31 of 0.79 Mpc. It can be seen that Lundmark's approach works reasonably well out to $D/D_{\mathrm{M31}} \simeq 25$, but thereafter comes adrift as dwarf galaxies are assigned incorrectly high distances (8 further dwarfs exist at larger values of D, and these are not shown).

5. Comparison with modern data

How well might the studies of a Century ago be expected to work with modern data? Today, we can measure relative distances to a precision of order 5% using Cepheid variable stars out to $D \simeq 30\,h^{-1}$ Mpc, or out to almost arbitrary distances using SNe Ia (with the aid of the Hubble Space Telescope in both cases). The traditional distance ladder starting with star clusters within the Milky Way can be used, as in the HST Key Program value of $H_0 = 72 \pm 8\,\mathrm{km\,s^{-1}Mpc^{-1}}$ (Freedman et al. 2001), or a more accurate value obtained by absolute calibration of the Cepheid distance scale using the maser galaxy NGC4258, yielding $H_0 = 73.8 \pm 2.4\,\mathrm{km\,s^{-1}Mpc^{-1}}$ (Riess et al. 2011).

Supernovae are especially useful in studying the expansion at larger distances, since they can readily be detected to $z \simeq 1$ (or beyond with effort) – hence the ability of the SNe Hubble diagram to probe cosmic acceleration. Figure 3 shows the SNe Hubble relation in Lundmark's form out to $D = 60D_{\mathrm{M31}}$, where we can see that the relation has quiet and noisy regions: the deviation from uniform expansion is episodic. If we had data only at $D < 30D_{\mathrm{M31}}$, there would hardly be evidence for any correlation between distance and redshift, much less a linear relation. Things only improve when we probe to 40 or 50 times D_{M31}.

Once we get close enough that Cepheids can be detected (20 Mpc or so) they are a better probe than SNe, since they are simply more numerous while the distance precision is comparable. Figure 4 plots local Cepheid data and shows that, closer than the noisy region at $D = 20D_{M31}$, we are lucky enough to experience an unusually quiet part of the Hubble flow (a fact that has puzzled many workers: e.g. Governato et al. 1997). Although the SNe data show that this is in fact globally unrepresentative, it is clear that one could be forgiven for claiming a well-defined linear $D(z)$ relation given results out to $D = 20D_{M31}$ (although not with any great significance for distance limits twice as small).

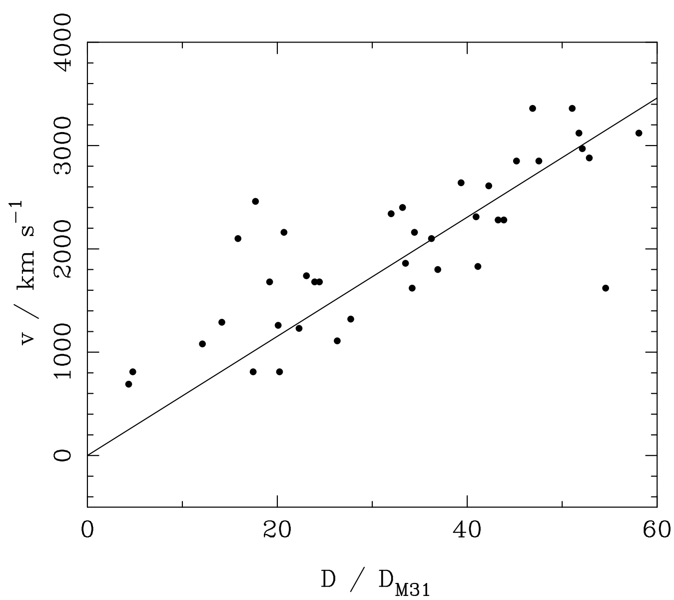

Figure 3. Data on nearby SNe Ia (taken from the compilation of Tonry et al. 2003) give accurate enough distances that we can see clearly the dispersion in the $D - z$ relation caused by peculiar velocities. This is sporadic: we can have lucky regions where the dispersion is low, and others, such as around $D/D_{M31} = 20$, where it blows up. Typically, we can see that high-precision distances to perhaps $D/D_{M31} = 50$ would be required for a convincing demonstration of an underlying linear relation. Again, the solid line shows the modern truth, assuming $H_0 = 73\,\mathrm{km\,s^{-1}Mpc^{-1}}$ and a distance to M31 of 0.79 Mpc.

6. Hubble's 1929 analysis

With the above perspective, what are we to make of Hubble's 1929 paper, in which a relation between distance and redshift was announced? Hubble used a sample of 24 nebulae, 20 of which had redshifts measured by Slipher; and with a maximum redshift of $1100\,\mathrm{km\,s^{-1}}$ the sample is no deeper than that available to Slipher in 1917. One might therefore have expected that the mean redshift after dipole subtraction would not be significantly non-zero. But treating Hubble's sample in the same way as Slipher's

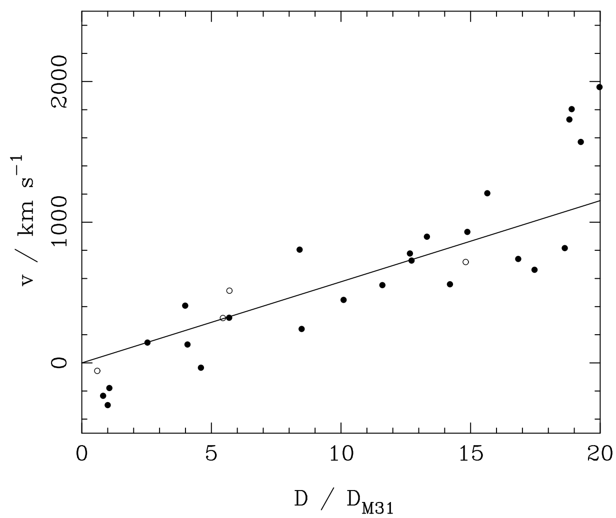

Figure 4. The local Hubble flow is 'colder' than in typical regions, so in fact a linear $D - z$ relation might be detected given data to $D/D_{\rm M31} \simeq 15 - 20$. This is demonstrated by the local Cepheid data (taken from Freedman et al. 2001). Again, the solid line shows the modern truth, assuming $H_0 = 73\,{\rm km\,s^{-1}Mpc^{-1}}$ and a distance to M31 of 0.79 Mpc.

reduces the mean redshift from $373\,{\rm km\,s^{-1}}$ to $197\,{\rm km\,s^{-1}}$ with an rms of $343\,{\rm km\,s^{-1}}$ – which is a 2.8σ deviation from zero. The extra significance is aided by the inclusion of the LMC and SMC: being Southern objects, they constrain the dipole velocity more strongly and prevent solutions in which the mean redshift is made as low as is the case with Slipher's sky coverage. Since the Magellanic systems are so nearby as to be almost part of the Milky Way, the case for including them is not obvious.

Let us now see what is added by Hubble's distance data. His greatest distance was only the rather modest $D = 7.5 D_{\rm M31}$, so one might have expected no significant claims: we have seen from modern data that a linear distance-redshift relation would not reveal itself clearly with data of twice this depth. But in fact Hubble's data do exhibit a correlation between distance and redshift (Figure 5). This plot is made in the Lundmark form used earlier, showing distance as $D/D_{\rm M31}$; and comparing with the line representing modern 'truth' we see that the slope is completely wrong. It should be clearly noted that this is not the same phenomenon as Hubble's overestimation of H_0, because we have used different coordinates. Plotting distance as $D/D_{\rm M31}$ should remove any calibration errors; thus Hubble's distances suffer from an entirely distinct additional problem of internal inconsistency, in addition to the well-known miscalibration of his Cepheid scale. The symptom is effectively that the distances for all the most distant objects are strongly underestimated. This could be suggestive of Malmquist bias: the distances presented by Hubble go well beyond what was possible with Cepheids in those days, so Hubble had switched to using the brightest individual stars as standard candles. These have a substantial dispersion, so the most distant galaxies for which such distances can be inferred will be those where individual stars are abnormally luminous – causing the distances to be underestimated. The effect is however extremely large (roughly a factor 2 in distance), and a simpler alternative explanation is that Hubble may have simply mistaken compact HII regions in the more distant galaxies for indi-

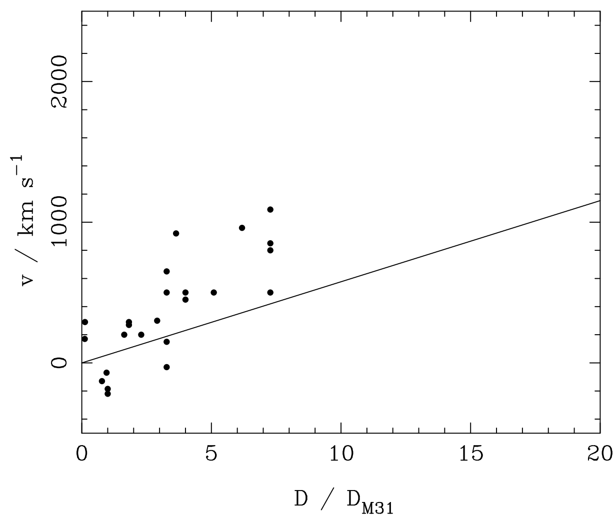

Figure 5. Hubble's 1929 data (largely Slipher's velocities, recall), on the same scale as the previous plot. Again, the solid line shows the modern truth, assuming $H_0 = 73\,\mathrm{km\,s^{-1}Mpc^{-1}}$ and a distance to M31 of 0.79 Mpc. Owing to the distance units as a ratio, the form of this plot is independent of the assumed absolute distance calibration, so that the incorrect Cepheid calibration used by Hubble in deriving $H \simeq 500\,\mathrm{km\,s^{-1}Mpc^{-1}}$ does not contribute here. Nevertheless, the slope of the relation is completely wrong, so Hubble's distance estimates were hugely in error through an independent additional effect.

vidual stars (Sandage 1958). This is undeniably a great irony: by combining Slipher's effectively perfect velocity data with distance estimates that are so badly flawed, Hubble nevertheless routinely receives sole credit for the discovery of the expanding universe (including the assertion that he measured the redshifts, which is frequently encountered in popular accounts – and too often even in those written by professional scientists).

Another interesting aspect of Hubble's analysis is that he assumes from the start a model that includes a linear $D(z)$ relation as well as a reflex dipole:

$$v = HD - \mathbf{v}_\odot \cdot \hat{\mathbf{r}}. \tag{4}$$

The famous $v - D$ plot from his 1929 paper shows not the raw velocities, but rather the velocities corrected by the dipole that best-fits the above relation – i.e. the plot has been manipulated in order to make a linear relation look as good as possible. Admittedly, Hubble does state that the data "...indicate a linear correlation between distances and velocities, whether the latter are used directly or corrected for solar motion.", but we are not shown the uncorrected plot – and we have seen in the case of Slipher's data that the Solar motion can change the picture very substantially.

It is therefore worth looking carefully at the statistics of the various samples that have been discussed, and these are collected in Table 1. From this, it is apparent that Hubble was a little fortunate with his 1929 data: the mean redshift after dipole correction is substantially more significant than Slipher's 1917 results – and more so than even Slipher's much deeper data of 1923. But this ceases to be true when the LMC and SMC are removed from the 1929 sample. Hubble's sample is therefore poised to deliver evidence for an expanding universe, even before adding distance data.

Table 1. Statistics of various early redshift samples, showing the influence of correction for Solar motion. This is quoted in Cartesian components within a J2000 coordinate system, in units of $\mathrm{km\,s^{-1}}$. The dipole is the least-square fit to a dipole-only model; but nevertheless the mean residual ($\langle v \rangle$) can be significantly positive, when the population standard deviation (σ_v) is converted to a standard error ($\sigma_v/N^{1/2}$). In the case of Hubble (1929) we show results with the full sample and also excluding the LMC/SMC.

Sample	N	$v_x^{\odot}$	$v_y^{\odot}$	$v_z^{\odot}$	$\langle v \rangle$	σ_v	Significance
S17	25	0	0	0	502	422	5.9σ
S17	25	566	−356	−268	145	414	1.8σ
S23	41	0	0	0	571	439	8.3σ
S23	41	467	−856	−298	201	508	2.5σ
H29	24	0	0	0	373	371	4.9σ
H29	24	462	−317	−117	197	343	2.8σ
H29	22	0	0	0	386	385	4.7σ
H29	22	426	−205	−200	159	370	2.0σ

Table 2. The equivalent of Table 1, but now assuming a model of an explicit non-zero (constant) mean velocity. Note that, with respect to the fits of Table 1, the best-fitting dipole is different and the dispersion in the residuals is smaller – representing a more significant detection of a non-zero mean.

Sample	N	$v_x^{\odot}$	$v_y^{\odot}$	$v_z^{\odot}$	$\langle v \rangle$	σ_v	Significance
S17	25	246	25	−430	566	328	8.6σ
S23	41	81	−109	661	805	364	14.1σ
H29	24	323	−267	113	315	306	5.0σ
H29	22	322	−483	424	493	286	8.1σ

Table 3. Statistics of Hubble's 1929 data. The population standard deviation, σ_v, about the best-fitting linear model is given with and without dipole correction. Results are given with the full sample and also excluding the LMC/SMC. The units of H are $\mathrm{km\,s^{-1}}$, since distances are in units of D_{M31}.

N	$v_x^{\odot}$	$v_y^{\odot}$	$v_z^{\odot}$	H	σ_v
24	0	0	0	373	371
24	67	−219	189	462	192
22	0	0	0	386	385
22	68	−235	205	467	199

Because of these non-zero mean velocities, we should make it clear that there are (at least) three distinct models worth considering:

$$\begin{aligned}&(1)\ v = -\mathbf{v}_{\odot}\cdot\hat{\mathbf{r}}\\ &(2)\ v = \bar{v} - \mathbf{v}_{\odot}\cdot\hat{\mathbf{r}}\\ &(3)\ v = HD - \mathbf{v}_{\odot}\cdot\hat{\mathbf{r}}.\end{aligned} \tag{5}$$

Model 1 is all that has been considered so far. Hubble (1929) quotes $H = 513 \pm 60\,\mathrm{km\,s^{-1}Mpc^{-1}}$, which represents an 8.6σ detection of a linear $D(z)$ (model 3) in comparison to model 1 (pure dipole). But this is not the right question, since we have seen that the mean redshift in Hubble's data is clearly non-zero. Since model (3) naturally predicts a non-zero mean, we have to ask whether the distance data add anything significant beyond this fact. There are a number of ways in which this can be assessed, and the simplest is to look at the size of the residuals about the best-fitting linear+dipole model. Table 2 shows these results, again with and without the Magellanic systems.

The striking feature of Table 2 is that the standard deviations in the residuals are smaller than in Table 1. This may seem puzzling at first sight, since in each case the only correction made to the data has been to remove a dipole. But in model 1 (which is what was used by Slipher 1917), we are attempting to minimise the mean square velocity, not the dispersion about the mean; this naturally pushes the mean low. If we are open to the possibility of a non-zero mean, then we need to minimise the standard deviation. This yields a different dipole and a more secure detection of the mean. Indeed, the remarkable conclusion of Table 2 is that Slipher's data alone provide a very secure detection of a non-zero mean velocity: 8.6σ in 1917 and 14.1σ in 1923. This significance is slightly overestimated because of the reduction in degrees of freedom caused by best-fitting the dipole. But this would only be a slight effect – especially with the $N = 41$ of 1923. This huge significance vindicates Eddington's ready acceptance of a non-zero mean velocity without the need for a detailed analysis.

We now consider the fits of model 3, which are given in Table 3. Model 3 is clearly a better fit than model 2: for Hubble's full sample, the rms residual is reduced from $306\,\mathrm{km\,s^{-1}}$ to $192\,\mathrm{km\,s^{-1}}$ – but is this reduction significant? The question is whether the low rms might be simply a statistical fluctuation downwards from a true value of around 300. For Gaussian distributions, the standard deviation, ϵ, on the estimate of the population standard deviation, s, is $\epsilon = s/[2(N-1)]^{1/2}$. We are therefore comparing 192 ± 28 with 306 ± 45, which is only a 2.1σ difference. But this statement applies only to independent samples, whereas we have the same data fitted with two different models. A better way to deal with this objection, plus the issue that the dipole is fitted to the data, is to use Monte Carlo: we hypothesise that there is no information in Hubble's distances, so we randomly permute them, and fit in each case a model of linear $D(z)$ plus dipole. This allows us to compute how often the rms is lowered by as much as is observed relative to model 2. The answer is about 1 in 23,000 for Hubble's full sample (a 3.9σ deviation), or 1 in 3000 if we ignore the LMC/SMC (a 3.5σ deviation). Thus the distance estimates do contain evidence for a correlation between distance and redshift – but at a lesser additional degree of significance than the basic fact that the mean redshift tends to be positive.

The picture that emerges from this study is thus that Hubble's 1929 work was perhaps more an exercise in validation of a linear $D(z)$ than a discovery. Hubble's closing quote that "...the velocity-distance relation may represent the de Sitter effect..." shows that he was certainly aware of the theoretical prediction that motivated earlier studies, such as that of Lundmark (1924). Hubble is not explicit in his introduction about the role that theory played in his work, although he did state that previous (unnamed) investigations had sought "...a correlation between apparent radial velocities and distances, but so far the results have not been convincing". Since this previous work was motivated by a search for the de Sitter effect, we can conclude that Hubble was influenced by the same theoretical prior as Lundmark in 1924 – and it is debatable which of these investigations achieved greater success in tracking down their quarry.

7. Peculiar velocities today

7.1. Velocities and structure formation

Slipher's demonstration that the Milky Way is not at rest is as revolutionary a moment as Bradley's proof in 1728 from stellar aberration that the Earth is in motion. We see this effect today most clearly in the dipole component of the Cosmic Microwave Background, which measures the Solar motion as $368\,\mathrm{km\,s^{-1}}$. The fact that this differs from Slipher's $700\,\mathrm{km\,s^{-1}}$ is further proof that his sample of galaxies is not deep enough to be a fair sample of the universe, from which one could really expect to measure the expansion.

But Slipher's data were deep enough to show that all galaxies have a random component to their velocities, so that the universe contains a peculiar velocity field. These deviations from the general expansion have been of great importance in cosmological research over the past several decades. The significance of peculiar velocities is that they must have their origin in the gravitational forces that cause the growth of cosmic inhomogeneities. If the dimensionless density fluctuation, δ is defined by $\rho = (1+\delta)\langle\rho\rangle$, then conservation of mass requires

$$\frac{\partial\delta}{\partial t} = -\nabla\cdot(\mathbf{1}+\delta)\mathbf{u} \simeq -\nabla\cdot\mathbf{u}, \tag{6}$$

where u is a comoving peculiar velocity: the physical peculiar velocity is $\delta\mathbf{v} = a\mathbf{u}$, where $a(t)$ is the dimensionless cosmic scale factor. The last equality holds in the linear limit of small density fluctuations.

The perturbation growth rate is more commonly written in terms of the logarithmic derivative, f_g:

$$f_g \equiv \frac{\partial\ln\delta}{\partial\ln a} = \frac{1}{H\delta}\frac{\partial\delta}{\partial t} = -\frac{1}{H\delta}\nabla\cdot\mathbf{u}. \tag{7}$$

In this form, the growth rate depends purely on the density of the universe, and in years gone by this was seen as a powerful route towards measuring the matter density. Today, with the density measured accurately via the CMB, the focus has shifted to using the growth of structure as a test of Einstein's relativistic theory of gravity. This boils down to the common approximation

$$f_g \simeq \Omega_m(a)^\gamma \tag{8}$$

where $\gamma \simeq 0.55$ for Einstein gravity, largely independent of the value of any cosmological constant, but non-standard gravity models can yield values that differ from this by several tenths (Peebles 1980; Lahav et al. 1991; Linder & Cahn 2007).

The motivation for thinking about deviations from Einstein gravity is not simply that it is always a good idea to verify fundamental assumptions of a field where possible.

The possibility that Einstein's theory may be incorrect derives its motivation from the most radical constituent of modern cosmology: the deduction that roughly 75% of the mean density is contributed by a nearly uniform component termed dark energy. So far, the properties of this substance are empirically indistinguishable from a cosmological constant or vacuum energy, but are we really sure that the dark energy exists? The doubt comes not through uncertain data, but because the inference derives entirely from the expansion history of the universe, which is interpreted via the Friedmann equation

$$H^2(a) = H_0^2 \left(\Omega_r a^{-4} + \Omega_m a^{-3} + (1 - \Omega_{\rm total}) a^{-2} + \Omega_v\right). \tag{9}$$

Empirically, it is impossible to match the data on $H(a)$ using only known matter constituents without adding a constant term on the right-hand side. But this might simply say that the Friedmann equation is wrong; it could be that some alternative to Einstein gravity might generate a Friedmann equation containing a constant term without needing to introduce dark energy as a physical substance. The way to distinguish between these options is to look for a scale dependence of any gravitational modifications, and the peculiar velocities associated with the growth of structure are a perfect tool for this job, since they measure the strength of gravity on scales of $\sim 10 - 100\,h^{-1}$ Mpc. As a result studies of the growth rate of perturbations have, together with gravitational lensing, assumed huge importance in recent years as an industry has built up around cosmological tests of gravity (see e.g. Jain & Khoury 2010).

7.2. Direct velocity measurements

There are two main ways in which the growth rate can be measured, and the first to receive attention was the most direct: estimate the peculiar velocity field from data. To do this requires some means of estimating distances, since

$$\delta v = v - HD \tag{10}$$

(assuming low enough redshifts that the cosmological and Doppler peculiar redshifts simply add; at higher redshifts we should multiply the $1 + z$ factors). Taking the divergence of the peculiar velocities inferred in this way is problematic since we only observe the radial component. This can be cured by adding the assumption that density perturbations under gravitational instability are expected to be in the growing mode, in which the velocities are irrotational. Thus $\mathbf{u} = -\nabla\psi$, where ψ is a velocity potential – which can be measured by integrating along the line of sight (Bertschinger & Dekel 1989).

There are two problems with this method. The difficulty of principle is that the divergence of $\mathbf{u}$ is proportional to δ times f_g, so we need to know the absolute level of density fluctuations. This is not so easy when using galaxies as tracers, because they are *biased*: $\delta_{\rm gal} \simeq b\delta$ on large scales. Thus we measure not Ω_m^γ, but Ω_m^γ/b.

The second difficulty is the practical one: the only tracers of peculiar velocities that have high space densities are galaxies, so we need to treat them as some kind of standard candles in order to deduce distances. Even with luminosities calibrated by an internal velocity (the 'Tully-Fisher method' for spirals; the 'fundamental plane' for ellipticals), the distances are good to only around 20%, and this scatter necessitates careful statistical treatment in order to avoid Malmquist bias and related effects.

A number of studies appeared in the 1990s claiming that these problems could be cured (e.g. Sigad et al. 1998), and the consistent result was a high value of Ω_m^γ/b, close to unity. It was possible to argue from the statistics of the collapse of dark-matter haloes that b should never be very much less than unity for any given class of galaxy (e.g. Cole & Kaiser 1989; Mo & White 1996), and therefore these results were seen as supporting a high matter density – most naturally $\Omega_m = 1$. This flat model was known to be in good agreement with the early limits on CMB fluctuations; these ruled out low-density open models, so that a cosmological constant was the only option if a low matter density $\Omega_m \simeq 0.2$ was preferred. A good body of evidence existed at that time (ranging from large-scale galaxy clustering to the baryon fraction in rich clusters) to suggest that $\Omega_m = 1$ was too high, so there were strong arguments in favour of a Λ-dominated model (Efstathiou et al. 1990); the last resistance to such a model crumbled with the arrival of the high-redshift supernova data in the late 1990s.

From this point on, direct use of peculiar velocity estimates has been somewhat neglected. No convincing explanation has really been given for why the 1990s velocity measurements gave what is now considered to be too high a density, and indeed discrepant results continue to exist in the form of apparent 'streaming velocities' that are inconsistent with what we think we know about the mass distribution (e.g. Watkins et al. 2009; Kashlinsky et al. 2009). But this is probably a common situation in science: where the evidence for a standard model is strong, discrepant results are most likely to be flawed and so a community is rightly reluctant to invest too much effort in understanding what has gone wrong. Sometimes this approach will ignore the key to a revolution, of course, but the large dispersion in individual peculiar velocity estimates means that a claimed rejection of ΛCDM based on such data will continue to be treated skeptically.

7.3. Redshift-space distortions

Nevertheless, peculiar velocities remain a major tool in conventional cosmology. This is because of the existence of major galaxy redshift surveys, where up to 10^6 galaxies are used to build up a picture of the 3D distribution of luminous matter. Such surveys have turned out to be fantastic statistical tools, because the power spectrum of fluctuations contains characteristic lengths that can be measured and used as a diagnostic of conditions in the early universe. Chief among these are the relatively broad curvature in the spectrum around the horizon size at matter-radiation equality, and the sharper feature at the acoustic horizon following last scattering. These are mainly sensitive to the density of the universe, and gave some of the first evidence for low-density models, as mentioned above. Today, the frontier is to measure the angular scale corresponding to these lengths as a function of redshift, mapping the $D(z)$ relation with standard rulers.

But the 3D picture given by redshift surveys is distorted in the radial direction by peculiar velocities, and in a complicated way that is correlated with the actual structures to be studied. Rather than being a bug, this is a feature: it causes observed galaxy clustering to be anisotropic in a way that allows a very precise statistical characterization of the amplitude of peculiar velocities.

Redshift-space distortions of clustering were first given a comprehensive analysis by Kaiser (1987). In the limit of a distant observer, where all pairs subtend small angles, the apparent anisotropic power spectrum for some biased tracer is given in linear theory by

$$\begin{aligned} P(k,\mu) &= P_m(k)(b + f_g\mu^2)^2 \\ &= b^2 P_m(k)(1+\beta\mu^2)^2; \quad \beta \equiv f_g/b, \end{aligned} \tag{11}$$

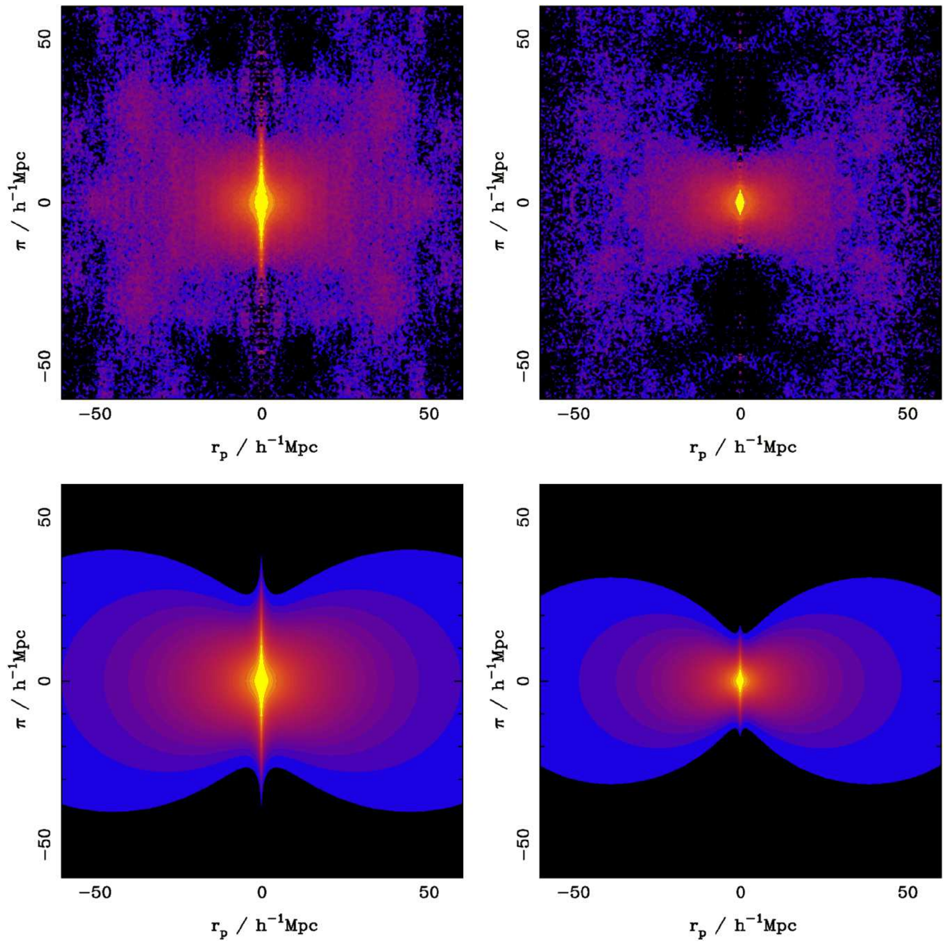

Figure 6. Redshift-space clustering as measured in the GAMA survey, split by colour of galaxy, together with theoretical models allowing for different degrees of linear flattening and of the pairwise dispersion (red galaxies to the left; blue to the right). As expected, the higher-mass haloes hosting red galaxies give them a higher pairwise dispersion and a higher bias – thus a lower large-scale flattening.

where $P_m(k)$ is the linear matter power spectrum, f_g is the desired growth factor, $f_g \equiv d\ln\delta/d\ln a$ and b is a linear bias parameter. It is common to find this model extended to allow for 'Fingers of God', in which the density field is convolved radially by random virialized velocities in haloes. Most usually an exponential pairwise velocity distribution is adopted, with rms σ_p (expressed as a length), leading to Lorenzian line-of-sight damping in Fourier space:

$$P_s(k,\mu) = b^2 P(k)(1+\beta\mu^2)^2/(1+k^2\mu^2\sigma_p^2/2). \tag{12}$$

The original derivation is only valid for small density fluctuations in the linear regime, but this expression has been used with some success inserting the non-linear real-space power spectrum of galaxies in place of $b^2P(k)$.

An example of such modelling is shown in Figure 6, which presents preliminary results from the GAMA survey (Driver et al. 2011). Here we see the galaxy population split by colour, with the result that the red population shows larger fingers of God, and less pronounced large-scale flattening (a smaller value of β). Both these results can be understood in terms of the typical mass of the dark-matter haloes hosting the galaxies: where this is larger, the small-scale velocity dispersion is larger and the large-scale clustering amplitude increases (which reduced β, since it is $\propto 1/b$).

The bias parameter is hard to predict a priori, meaning that this method is unable to yield a direct measurement of the perturbation growth rate without additional assumptions. The way this is dealt with in practice is to realise that the real-space clustering amplitude of galaxies is observable, so that the bias can be measured if a model for the mass fluctuations is assumed. At the level of a consistency check, this can be a standard ΛCDM model taken from CMB and other data. A slightly more general way of putting this is to say that galaxy data determine $b\sigma_8$, where σ_8 is the usual normalization measure of density fluctuations: the linear-theory extrapolated fractional rms variation when averaged in spheres of radius $8\,h^{-1}$ Mpc. Thus the slightly unlovely combination $f_g(z)\sigma_8(z)$ can be measured in an approximately model-independent fashion. A compilation of recent estimates of this quantity is shown in Figure 7, which shows impressive consistency with the standard model, indicating that Einstein's relativistic theory of gravity can be verified at about the 5-10% level on scales $\sim 10 - 30\,h^{-1}$ Mpc over a wide range of cosmological time. This is hardly yet at the level of precision of solar-system tests, but this limit will in due course be brought down to the per cent level by future experiments such as ESA's Euclid satellite. This should be launched around 2020 (Laureijs et al. 2011), and will provide redshifts for around 50 million galaxies in a redshift band around $z \simeq 2$, as opposed to current studies which are based on in total around one million galaxies in the smaller volume at $z < 1$.

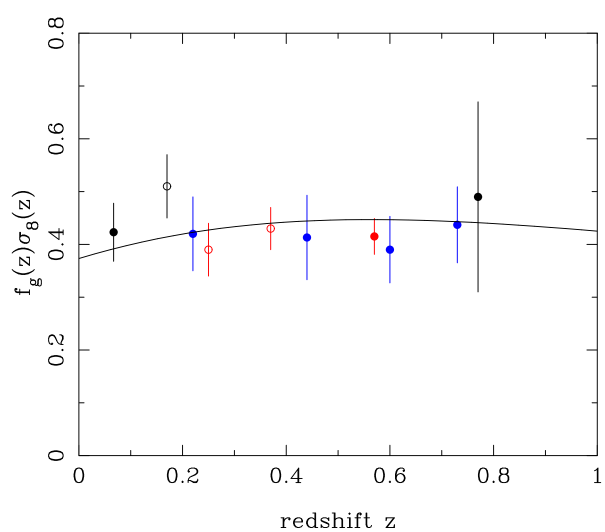

Figure 7. A plot of various measurements of the linear growth of density perturbations inferred from redshift-space distortions (see e.g. Beutler et al. 2012 for a compilation of the data). The solid line shows a default flat Λ-dominated model with $\Omega_m = 0.25$ and $\sigma_8 = 0.8$, which matches the data very well (perhaps too well: 9 out of 10 measurements agree with the theory to within 1σ).

8. Discussion and conclusions

It is irresistible to speculate about how Slipher might feel were he able to hear us talk calmly of having measured a million galaxy redshifts, and how we plan to increase this number a hundredfold – when each of his measurements cost him several nights standing alone in the cold. But from the anecdotes aired at this meeting, one suspects he might not have been all that jealous, since he seems to have found a deep attraction in the basic process of observing. And it is undeniable that something is in danger of being lost as we pursue large-scale cosmology with an industrial efficiency; there are declining opportunities for young astronomers to work at telescopes and experience that sense of a mystical connection to the cosmos that comes from standing by a telescope in the dark under a clear sky. As the machines become larger, one way of retaining that sense of wonder is to remember the efforts of the pioneers.

And Slipher was a great pioneer; not simply through his instrumental virtuosity in achieving reliable velocities where others had failed, but through the clarity of reasoning he applied. Respect for what he did and did not claim can only be increased by the exercise presented here of analyzing his data as if the information was freshly available, and trying as best we can to rid the mind of modern preconceptions. At the depth to which he worked, and with the restrictions of sky coverage, it was hard for the signature of a general expansion to stand out. But it is hard not to wonder what would have happened if data for more southerly or even slightly more distant galaxies had been available; Slipher's comment in 1917 that generally positive velocities "...might suggest that the spiral nebulae are scattering..." suggests that he would have been open to the conclusion of a general expansion. As shown above, such a result can actually be obtained from Slipher's 1917 data (a $>8\sigma$ detection of a non-zero mean velocity, even after allowance for the best-fitting Solar dipole), and the signal rises to 14σ with the expanded dataset that Slipher gave freely to Eddington and others in 1923. It is more than a little surprising that no-one attempted to repeat Slipher's 1917 work with this expanded material, in which case Slipher could have been clearly established as the discoverer of the expanding universe.

Unlike Hubble and other workers from the 1920s, Slipher in 1917 lacked the theoretical prior of a predicted linear distance-redshift relation, which de Sitter only published the same year. Slipher was simply looking for a message that emerged directly from the data, and it is therefore all the more impressive that he was able to reach his beautiful 1917 conclusions concerning the motion of the Milky Way and the nature of spiral nebulae as similar stellar systems. But this is characteristic of Slipher's work: right from his early assertion that the velocity of M31 must be Doppler in origin, he was willing to stick his neck out and state firm conclusions when he believed that these were supported by the data. Rather than using hindsight to regret that he did not focus on the non-zero mean velocity of his data, we should look on with admiration at how much he was nevertheless able to learn from the observations he had gathered.

By adding distance data to existing velocities, Hubble (1929) claimed not only that the mean velocity was a redshift, but that redshift correlated linearly with distance. We have seen that Hubble was fortunate in a number of ways to have been able to make such a claim with the material to hand: (1) peculiar velocities are unusually low in the local volume; (2) his mean redshift was higher than Slipher's in 1917, despite the sample containing no greater velocities; (3) he included the LMC and SMC, which could be viewed as unjustified; (4) his distance estimates were flawed in two distinct ways. Also, Hubble considered from the outset only the hypothesis of a linear relation between distance and redshift, and never asked how much his information added to the simple statement that the mean velocity was positive (which we have seen accounts for the majority of the statistical weight in his result). Hubble admitted that he was following up previous searches for a distance-redshift correlation, and these studies were explicitly motivated by the theoretical prior of the de Sitter effect. If this prediction had been absent in 1929, one wonders if claims of a linear distance-redshift relation would have been made at that time.

If the data in 1929 were really too shallow for a truly robust proof of a linear distance-redshift relation, when was this first seen unequivocally? Credit is often given to Hubble & Humason (1931), who pushed the maximum velocity out to $20,000\,\mathrm{km\,s^{-1}}$ – ten times what had been achieved by Slipher. But the distances used in that paper were based on the same unjustified assumption used by Lundmark in 1924: that galaxies could be treated as standard objects. Indeed, Hubble gives a pre-echo of this argument in his 1929 paper, referring to the large redshift of NGC7619. Because galaxies at these distances lacked any sort of well-justified distance estimates, one could imagine that the 1931 paper should have received a good deal of critical skepticism – but by this time a linear $D(z)$ was already regarded as having been proved.

In fact, right through the 1980s, cosmology journals and conferences were treated to a continuing critique of a linear $D(z)$ as deduced from galaxy data by Irving Segal (e.g. Segal 1989). Segal made major contributions to quantum field theory, and could hardly be dismissed as a crank; the basic problem is that, even when calibrated dynamically as in the Tully-Fisher method, the scatter in galaxy properties is so large that getting distances to better than around 20% is not feasible. Thus it was only really in the 1990s, with HST extending the reach of Cepheids and SNe Ia giving accurate distances, that we could verify what had been generally assumed to be true since 1929/1931.

But if the work on the distance scale in the 1990s closed the chapter on the local distance-redshift relation that was begun in the 1920s, Slipher's other main legacy to modern cosmology remains as relevant as ever. The peculiar velocity field that he discovered has become one of the centrepieces of modern efforts to measure the nature of gravity on cosmological scales. Hence we have come full circle, from assuming the correctness of Einstein's relativistic gravity (and of the de Sitter solution in particular) to search for evidence of expansion in the 1920s, to the present-day use of data on peculiar velocities to tell us if the theory is correct. Slipher would probably have been happy to see things being done in this direction.

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