CHAPTER 3.

Surveys and Observations of the Large Scale Structure of the Universe

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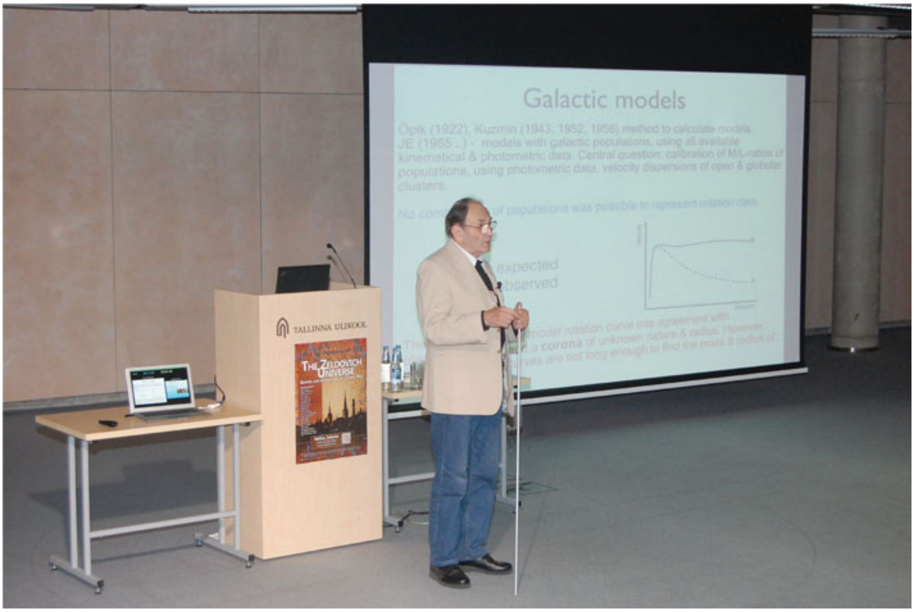

Jaan Einasto during his lecture.

Participants following one of the presentations.
In the center, Varun Sahni and Bernard Jones.

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CHAPTER 3A.

Surveys and Observations: Surveys

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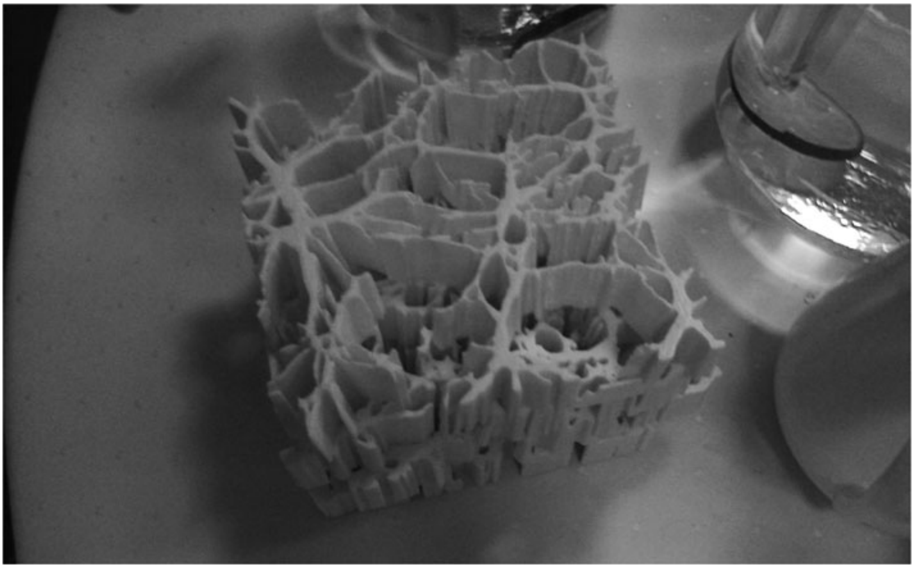

Zeldovich pancakes in a state-of-the-art presentation:
3-D print of the Spine of the Cosmic Web by Miguel Aragon-Calvo.
Photo courtesy: Peter Coles

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The Zeldovich Universe:
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The cosmic web: a selective history and outlook

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Abstract. In the Century since Slipher's first observations, roughly three million galaxy redshifts have been measured. The resulting maps of large-scale structure have taught us much of central importance in cosmology, ranging from the matter content of the universe to the study of the primordial density fluctuations. This talk aims to review some of the key observational and theoretical milestones on this journey, and to speculate about what the future may bring.

1. Introduction: the pre-history of redshift surveys

The large-scale structure seen in the galaxy distribution is one of the most important probes of fundamental cosmology, undoubtedly representing a relic of the earliest times and highest-energy physics. It's sobering to think that all of this was undreamed-of only within living memory, with V.M. Slipher publishing the first galaxy radial velocity in 1913 – the same year in which my Father was born. It's remarkable how little fuss was made of the recent Centenary of Slipher's revolutionary work, although at least a celebratory conference was held at the Lowell Observatory, to mark Slipher's achievements there. For a decade, he had the field of galaxy spectroscopy to himself, and by 1917 had already gathered sufficient data to prove that galaxies had to be redshifted on average (see e.g. Peacock 2013 for details of these achievements).

Beyond about 1923, the frontier of redshift measurement moved to the larger telescopes at Mt Wilson, but things still advanced slowly because of the limitations of photographic plates for recording spectroscopic information. More than three decades of effort yielded the 620 redshifts listed by Humason, Mayall & Sandage (1956), and this logarithmic pace of progress continued even into the mid-1970s, at which point the total number of known redshifts was not greatly above 1000.

Remarkably, a good deal had nevertheless been learned about galaxy clustering by this stage, thanks to careful analysis of the angular clustering seen in projected galaxy catalogues – which were painfully assembled by visual inspection of wide-area photographic survey plates (Shane & Wirtanen 1954). Much of the wisdom of this heroic era was gathered in the classic textbook by Peebles (1980). But by this stage, the field had already embarked on a series of technological revolutions, which have collectively brought us to the present happy glut of data.

2. Selected observational revolutions

2.1. *The CfA survey*

The first big step on the road to modern studies of the cosmic web was taken when the CfA survey applied the tools of the electronic revolution: image-intensifier tubes

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as detectors instead of photographic plates, and digital computers to undertake semi-automated analysis of the data. As a result, Huchra *et al.* (1983) were able to assemble a coherent sample of 2401 redshifts, suitable for the first true analyses of the 3D galaxy distribution. The CfA efforts continued, leading to the iconic slice picture of the cosmic web (de Lapparent, Geller & Huchra 1986), which was the first clear demonstration of the void-filament network with which we are now so familiar (although the void phenomenon was already known: Gregory & Thompson 1978). By the time they terminated, in 1995, the CfA surveys had accumulated over 20,000 redshifts.

2.2. *The LCRS*

The CfA efforts were impressive, but the rate of progress was limited by the need to observe galaxies one at a time. Even a larger telescope and more sensitive instrumentation only help to a degree, since there are unavoidable overheads associated with changing from one target to the next. What was required was a means of observing many galaxies in parallel. A number of approaches were tried, starting with objective prisms applied to early Schmidt photographic plates, but such an approach is unsatisfactory both through object overlap and non-rejection of the sky background. The latter problem can be beaten by applying a slit mask, but spectral overlap remains; in addition, it is hard to cover a very large area in this way. Thus the real revolution in redshift surveys came with the application of fibre optics. A number of groups developed this technique, particularly at the Steward Observatory and the Anglo-Australian Observatory (see e.g. Hill 1988 and Gray 1988 for reviews of the early work). The largest amount of early astronomical results from fibre instrumentation certainly came from the AAO, especially when the initial approach of plugging fibres into a pre-drilled plate (FOCAP) gave way to the more flexible approach where fibres could be placed by a robot at arbitrary positions on the focal plane (AUTOFIB).

But in terms of cosmological information, it is important for redshift surveys to cover a large area of sky, and the AAO's initial efforts were limited by the restricted Cassegrain field of view. Thus, the first true wide-area redshift survey to use fibre multiplexing was the Las Campanas Redshift Survey. Shectman *et al.* (1996) give details of this effort, which measured 26418 redshifts over the period 1988–1994. The LCRS fibre system used 112 fibres over a field 1.5 degrees square, employing pre-drilled plug plates, to cover an eventual area totalling 700 deg^2. The main achievement of this survey was, in their own words, “the end of greatness”. Previous redshift surveys had seen features that were comparable to the size of the survey (the ‘great wall’ being the imaginative name given to one such structure), and it was fair to question whether these studies as yet constituted a fair sample of the universe. But the LCRS observed equal fields in the Northern and Southern hemispheres, and the statistical character of these two sub-surveys were closely comparable. This was a major achievement, setting the scene for the work that followed.

2.3. *The 2dFGRS*

The success of the AAO fibre experiments showed that the power of the technique on the Anglo-Australian 4m could be increased by moving to a wider field. This required the construction of a new corrector to allow the fibres to work at the prime focus. The initial planning started around 1988, with formal approval given in 1990 for the project to construct the ‘two-degree field’ system: a 2-degree diameter corrected field with 400 fibres placed by a robot. 1994 saw the formation of UK-Australian consortia to carry out major galaxy (and quasar) redshift surveys with the new facility, for which observing eventually started in 1996. The galaxy survey (2dFGRS) was based on an input catalogue derived from the UK Schmidt photographic plates scanned at Cambridge with the APM

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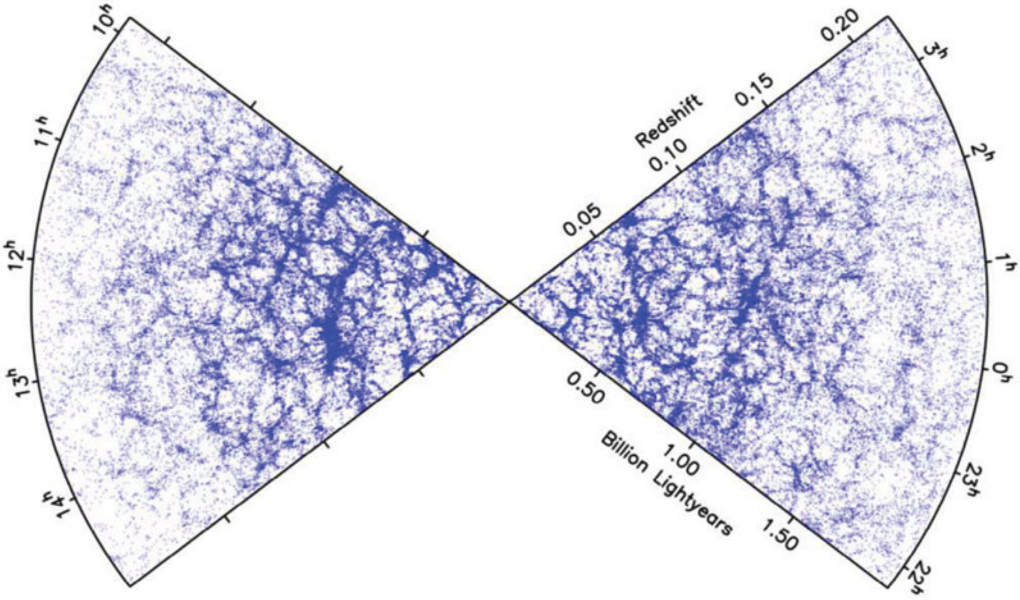

Figure 1. The 2dFGRS view of the local universe, based on 221,414 redshifts. This is the highest-fidelity picture we have of a large expanse of the local cosmic web.

machine (blue 'J' plates) and at Edinburgh with SuperCOSMOS (red 'F' plates). By 2003, redshifts had been measured for 221,414 galaxies over approximately 2000 deg^2, to an extinction-corrected depth of $B_J < 19.45$ (Colless *et al.* 2003). The resulting image of the local cosmic web (Figure 1) remains the highest fidelity picture we have (the deepest truly wide-area fully-sampled survey).

This advance in size of nearly an order of magnitude with respect to the LCRS, with corresponding improvement in volume spanned, is what allowed 2dFGRS to pass the threshold in precision needed to make qualitatively new discoveries. These include accurate measurements of the power spectrum, yielding a precise matter density and the detection of Baryon Acoustic Oscillations (Percival *et al.* 2001; Cole *et al.* 2005), and the measurement of Redshift-Space Distortions (Peacock *et al.* 2001). The latter was originally seen as a further means of measuring Ω_m; but as explained below, RSD has now emerged as a leading means of testing theories of gravity.

It should be pointed out that the impact of a given project depends on timing, and the context of developments elsewhere. 2dFGRS made especially strong advances because of parallel developments in CMB anisotropies; both classes of study complemented each other, tying down degrees of freedom that would otherwise have been degenerate (e.g. the shape of the power spectrum largely measures $\Omega_m h$, not Ω_m, and it has a further degeneracy with the slope of the primordial spectrum, n_s). As a result, LSS information from 2dFGRS played a major part in, for example, the cosmological interpretation of the initial results from WMAP (Spergel *et al.* 2003).

2.4. *SDSS*

All the surveys listed so far suffered from one common disadvantage: they were only part-time tenants of their telescopes. So time allocation committees had to perform the delicate juggling act of giving sufficient time to allow the surveys to make worthwhile progress, while not stifling the community wanting to use the telescope for diverse smaller projects. For example, the 2dFGRS used 272 AAT nights over a 5-year period – less than

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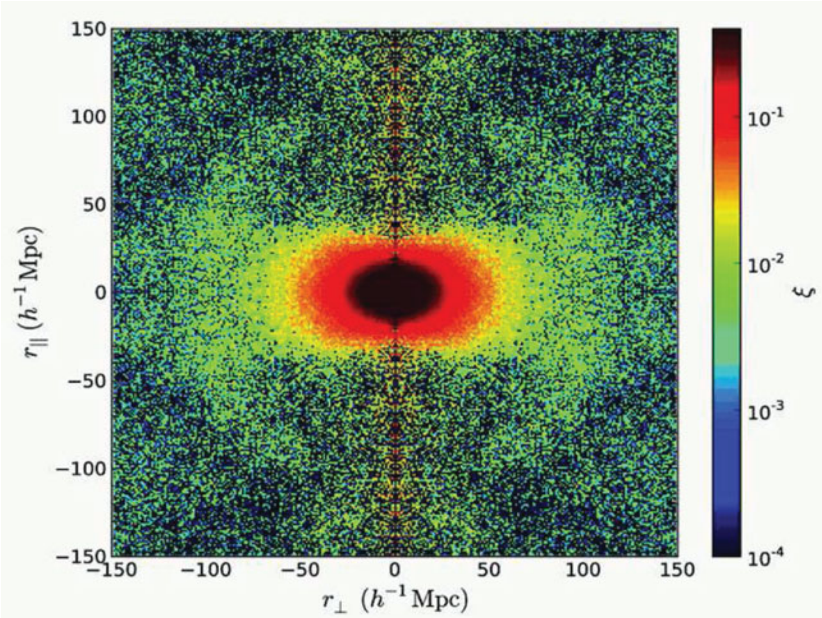

Figure 2. The redshift-space correlation function of BOSS LRGs (Samushia *et al.* 2014). The apparent radial coordinate is affected by peculiar velocities, yielding the marked apparent deviations from circular symmetry around $20\,h^{-1}$ Mpc. This figure also nicely shows the ring around $100\,h^{-1}$ Mpc corresponding to the Baryon Acoustic Oscillation feature.

1/3 of the available dark/grey time. In contrast, the SDSS was able to operate with a dedicated telescope, albeit one split between imaging and spectroscopy. The number of fibres went up slightly: 600 at the time of the 1998 commissioning, rising to 1000 in SDSS-III, and the field size was 3 degrees. But with a much smaller 2.5m mirror, SDSS would have been a less powerful facility than 2dF without the ability to make use of all observing time. Indeed, the main galaxy redshift survey, limited at $r = 17.77$, is somewhat less deep than 2dFGRS as a result.

But the most powerful step taken by SDSS was to exploit its own multicolour imaging, which was used to generate the input catalogue. This permitted the selection of a probe uniquely suited to accurate statistical measurement of LSS properties: the Luminous Red Galaxies. Given the limited light grasp of the SDSS mirror, efficient surveying to large redshifts requires the pre-selection of unusually luminous galaxies – and these tend to be the ellipticals, which can be picked out by their lack of short-wavelength emission. Moreover, such objects have strong spectral features, allowing successful determination of redshifts even when the spectra are of low S/N. For a final bonus, such objects are strongly biased, so the clustering signal is enhanced. As a result, large volumes could be surveyed in a dilute way while yielding accurate measurements of clustering. The initial LRG selection worked to $z < 0.5$, and the first LRG results were already sufficiently powerful to detect the BAO signal with only 46,748 redshifts (Eisenstein *et al.* 2005). Subsequent work has refined the selection method, pushing to $z < 0.7$ in the 'BOSS' sub-project of SDSS-III, yielding sample sizes of 690,826 LRGs in the 'CMASS' sample at $z > 0.43$, supplemented by 313,780 at lower redshifts (Anderson *et al.* 2014; Samushia *et al.* 2014).

As a result of this large survey volume, BOSS currently sets the standard for the most precise measurements of the key LSS statistics, including the redshift-space correlation

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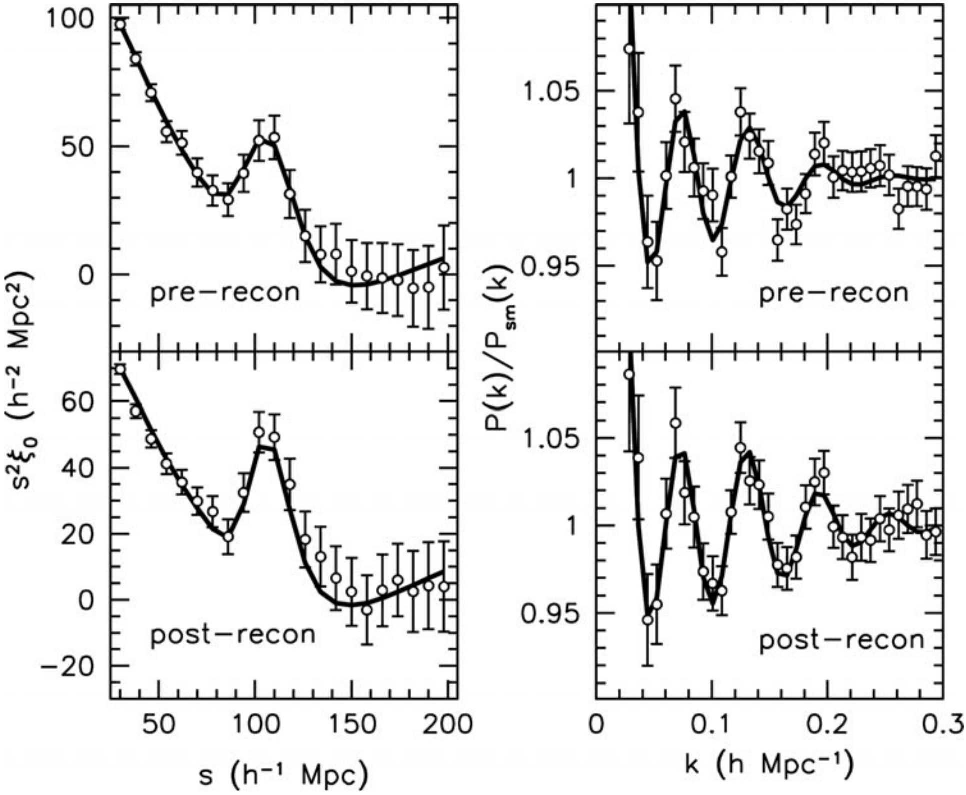

Figure 3. The BAO feature as seen in configuration space and in Fourier space by the BOSS LRG survey (Anderson *et al.* 2014). The precision of the measurement is improved by 'reconstruction', in which an approximate correction is made for weakly nonlinear displacements at the BAO scale. It is striking to compare the quality of this measurement with the first bare detections of only nine years previously.

function (Figure 2), and the location of the BAO scale, with the latter measurement probing the empirical $D(z)$ relation and hence parameters relevant to the cosmic expansion history (Figures 3 & 4).

3. Revolutions in theory

Observational advances have tended to proceed in step with important refinements of theory and/or statistical treatment of data. It is natural that better data should stimulate improved methods, but all the major surveys of recent years have been directed at theoretical targets, even if in some cases the target emerged after the survey had commenced.

In so many ways, studies of large-scale structure rest on the titanic foundations of the book by Peebles (1980). Almost all the statistical concepts and tools used today are set out there, at least in embryo. All that was really missing was a specific theoretical model on which the general methods could be focused – and the CDM model was not long in being developed (Peebles 1982). Next to Peebles, probably the majority of the most important ideas that are in use today derive from three big ideas developed in classic 1980s papers by Kaiser:

(1) **Bias**. It was known empirically that different galaxies displayed different amplitudes of correlations, but this remained a puzzle until Kaiser (1984) showed that

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regions of density high enough to collapse by the present will inevitable show enhanced correlations. Although this breakthrough is commonly cited as the invention of biased galaxy formation, this is incorrect. Initially, it was thought that this phenomenon could arise via a threshold in density below which star formation in galaxies would be suppressed (Bardeen *et al.* 1986), which turned out to be a blind alley. Eventually, it was realised that the correct way to view the Kaiser result was in terms of a bias that depended on the mass of dark-matter haloes (e.g. Mo & White 1996), which then only needs a mass-dependent halo occupation number for a full understanding of the clustering of different classes of galaxy (Seljak 2000; Peacock & Smith 2000).

(2) **Sparse sampling**. Many redshift surveys, from the early CfA surveys to the 2dFGRS, used a simple 'shoot everything that moves' strategy: apply a magnitude limit and obtain redshifts for all galaxies brighter than this over as large an area as possible. But Kaiser (1986) pointed out that this is wasteful. The precision with which cosmological statistics such as the power spectrum can be measured is limited by a mixture of cosmic variance and shot noise, and for the best results with fixed telescope time one should optimise the balance between these. Kaiser found that a fully-sampled survey generally overkills shot-noise suppression: if galaxies all lived in clumps of size N, then most of the clumps have been found once a fraction $\sim 1/N$ of the redshifts have been taken. Beyond this, discreteness noise declines slowly, and one is better covering more area at the same sampling. This strategy was used effectively in the IRAS-selected QDOT survey (Lawrence *et al.* 1999), and is implicitly what has permitted the outstanding success of the SDSS LRG strategy.

(3) **Redshift-space distortions**. The final 1980s revolution concerned the issue that the radial coordinate deduced from redshifts was imperfect through the modification of the observed redshift by peculiar velocities. Although this had long been clear in a general sense, Kaiser (1987) supplied the quantitative analysis of the large-scale linear anisotropic clustering that resulted: $P_s(k,\mu) = P_r(k)(b + f_g\mu^2)^2$, where the s subscript denotes redshift space; μ is the cosine of the angle between the wavevector and the line of sight; b is a bias parameter, and f_g is the logarithmic growth rate of density fluctuations with respect to scale factor, $f_g = d\ln\delta/d\ln a$. This equation has been hugely influential in provoking observational attempts to measure the growth rate, as discussed further below.

3.1. *BAO and dark energy*

Probably the main aspect of current LSS studies that was not put in place during the 1980s is the Baryon Acoustic Oscillations – although these were well understood in the context of the CMB during this time, and it was known that the effects on the matter power spectrum would be small if dark matter dominated. One strong stimulus to thinking seriously about BAO effects was the impressive analytical work on the form of the power spectrum by Eisenstein & Hu (1998); it also became clear around the same time that the small baryonic features would survive nonlinear evolution (Meiksin, White & Peacock 1999), so there was a strong theoretically motivated signal to search for.

It should therefore not have come as much of a surprise when evidence for BAO features in the galaxy power spectrum started to become available. The first claim of a detection was made in the initial 2dFGRS power spectrum measurements by Percival *et al.* (2001), with a greater significance in the final data (Cole *et al.* 2005), at the same time as the the initial SDSS LRG results of Eisenstein *et al.* (2005). It should be noted that all these pieces of evidence were model-dependent: the ΛCDM theory made

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a well-specified prediction of BAO features, and this model was statistically preferred to a featureless power spectrum – even though an empirical featureless spectrum was not formally inconsistent with the data. This is exactly the position we occupied with the CMB prior to about 2000: the data at that time constrained very accurately the height and location of the main acoustic peak at $\ell \simeq 220$, even though a spectrum with no peak at all was consistent with the measurements. This represents the additional information injected by having a strong theoretical prior.

In fact, what might be considered surprising about the BAO features is that they are so small. Right from the first calculations of the evolution of cosmological perturbations, it was clear that the resulting transfer function should contain strong oscillatory features as a result of sound waves in the primordial matter+radiation fluid (Peebles & Yu 1970; Sunyaev & Zeldovich 1970). The amplitude of these order unity fluctuations becomes damped only in the presence of collisionless dark matter, which cannot support sound waves (e.g. Bond & Szalay 1983). At the time of decoupling, the matter-radiation fluid has order unity BAO, whereas the CDM has none at all; subsequently, with a much reduced sound speed, the two components fall towards each other. Both have the same phase of Fourier perturbations, but with different transfer functions with respect to some initial fluctuation:

$$\delta_{\rm tot} = \delta_i \left[f_1 T_1(k) + f_2 T_2(k) \right],$$

where f_1 and f_2 are the fractions in the two components. Since about 20% of the matter is baryonic, the overall amplitude of the BAO features in the total power spectrum are correspondingly reduced. Thus, from the moment that data revealed a relatively smooth large-scale galaxy power spectrum (e.g. Peacock & Dodds 1994), we were in possession of rather general evidence for the existence of collisionless dark matter.

In any case, it was quickly realised that the important application of the BAO signal was not so much in constraining cosmological parameters from the shape of the power spectrum; rather, it lay in using the acoustic scale to provide a standard ruler suitable for geometrical cosmology (Blake & Glazebrook 2003; Seo & Eisenstein 2003). One can treat the BAO scale as something completely empirical, observed at different redshifts to yield an angle subtended or the corresponding radial increment of redshift – respectively probing the comoving angular-diameter distance $D_C(z)$ or the epoch-dependent Hubble parameter $H(z)$. In practice, it is normal to combine these into a single effective distance measure derived from averaging over radial shells:

$$D_V(z) \equiv \left(D_C^2(z)\, cz/H(z) \right)^{1/3}.$$

One can then measure $D_V(z)$ subject to an overall normalization, which already places important limits on $H(z)$ and thus the equation of state of dark energy via the Friedmann relation $H^2(z) \propto \Omega_m a^{-3} + (1-\Omega_m) a^{-3(1+w)}$ for a flat universe. But more precise limits can be set if we are prepared to adopt the ΛCDM prediction of the acoustic scale. This is the origin of the impressive plot shown in Figure 4 (Anderson *et al.* 2014), where the BAO data are seen to be consistent with a prediction involving no free parameters (or, rather, the parameters of the simplest flat ΛCDM model can all be determined from the CMB). The precision of this agreement is what permits the current very tight limits on the evolution of dark energy: $w = -1.006 \pm 0.045$ (Planck Consortium 2015).

This has been a great success story for LSS, and there seems no reason why it should not continue. The physics that determines the BAO scale in CMB and LSS is often described as 'simple' – perhaps a little unfair when one considers the decades of detailed work needed to generate a precise theory for the effect – but certainly there is a good case that the signal is robust and independent of the details of galaxy formation. Nonlinear

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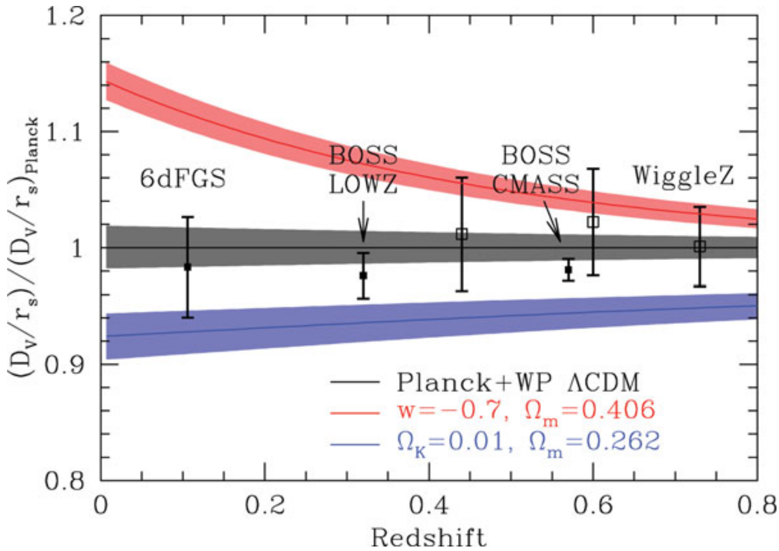

Figure 4. The BAO length scale is calculable given the cosmological parameters of the ΛCDM model. On the assumption of flatness, these can be determined very accurately from the CMB alone, and the BOSS BAO measurements give a completely independent test of the validity of this prediction, which is passed extremely well (figure from Anderson *et al.* 2014). If the model is then generalized to open other degrees of freedom (curvature, or dark energy equation of state $w \neq -1$), these are heavily constrained.

evolution of structure does potentially alter the power spectrum on BAO scales, but to an extent that can be modelled so that systematics in the BAO scale are $\sim 0.1\%$ (Eisenstein *et al.* 2007). Since the sensitivity of distance to w is $d\ln D/dw \lesssim 0.2$, this implies that future experiments should be able to attain 1%–level precision on w, and seeking to measure whether dark energy evolves at this level is a realistic goal for the next decade.

3.2. *RSD and modified gravity*

Kaiser's work on redshift-space distortions revealed a quadrupole anisotropy that could be used as a diagnostic of the growth-rate of cosmological inhomogeneities. From an early stage, it was clear that this linear-theory expression required supplementing with allowance for small-scale virialized motions, which can be treated by a radial convolution. In Fourier space, this amounts to multiplying by a function of the radial component of **k** (Peacock & Dodds 1994), which gave rise to the widely-used 'dispersion model':

$$P_s(k,\mu) = P_m(k)\,\frac{(b + f_g\mu^2)^2}{1 + k^2\mu^2\sigma_p^2/2}\,.$$

There is a choice of whether the matter power spectrum, P_m, should be taken from linear theory; in practice, a better match to data arises if it is the full nonlinear real-space spectrum.

From the shape of this equation, the distortion parameter $\beta \equiv f_g/b$ can be extracted. This is complicated by the unknown bias, but this is constrained by the amplitude of the observed large-scale galaxy clustering, $b^2 P_m$ (this real-space quantity can be measured by projecting the 2D redshift-space correlation function along the radial direction). Thus e.g. $b^2\sigma_8^2$ is observable, so the bias-independent quantity $f_g\sigma_8$ can be constructed. If this

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is measured as a function of redshift, it is an informative combination of the differential and integrated growth of fluctuations.

Initially, the RSD phenomenon was seen as an independent way to weigh the universe via the approximate relation $f_g \simeq \Omega_m(z)^{0.6}$, and the first detailed measurement of the effect by the 2dFGRS was focused on measuring Ω_m (Peacock *et al.* 2001). But in more recent years, it has been appreciated that the deeper significance of f_g is that it gives a way no probe modified theories of gravity. This change of perspective was promoted by Guzzo *et al.* (2008), who were also the first to measure RSD at high redshifts ($z \simeq 0.8$). At high redshifts, the Kaiser flattening becomes partially degenerate with the 'Alcock-Paczynski' geometrical flattening which can arise through incorrect choice of the cosmological geometry when calculating the 2D correlations. But with sufficiently good data, this degeneracy can be broken – and the geometrical effect then becomes another useful constraint on the cosmological model (Ballinger *et al.* 1998).

Once we decide to move beyond Einstein's original relativistic theory of gravity, a range of possibilities becomes available (e.g. Clifton *et al.* 2012). From the point of view of LSS, it is simplest to condense this variety into two parameters that express deviations from Einstein gravity on linear scales. We start by writing the metric in terms of two scalar potentials:

$$ds^2 = a^2(t)\left[(1+2\Psi)d\eta^2 - (1-2\Phi)(dx^2+dy^2+dz^2)\right] ;$$

in Einstein gravity, $\Phi = \Psi$ is the Newtonian potential, which obeys Poisson's equation. Note that, in moving away from this model, we are not really abandoning general relativity, despite the common claim that this approach tests GR: there is still a metric, and we seek a description of gravity in terms of relativistic invariants. All that changes is that we abandon the simplest gravitational Lagrangian $\mathcal{L} \propto R + 2\Lambda$ in favour of a more complicated invariant based on the metric (such as the common $f(R)$ models). In the linear limit of small deviations from Einstein, we can parameterise the slip (relation of Ψ and Φ) and the effective strength of gravity in Poisson's equation:

$$\begin{aligned} \Psi &= (1+\alpha)\Phi \\ \nabla^2\Phi &= 4\pi(1+\beta)Ga^2\bar{\rho}\delta. \end{aligned}$$

There is no standard notation for these parameters, as discussed by e.g. Daniel *et al.* (2010).

Rather than working with slip and effective G, a more directly physical parameterisation is to describe the change in the acceleration felt by non-relativistic particles (dictated by Ψ) and relativistic particles (dictated by $\Psi + \Phi$):

$$\begin{aligned} \Psi &= (1+\mu)\Psi_{\rm E} \\ \Psi + \Phi &= (1+\Sigma)(\Psi+\Phi)_{\rm E} = 2(1+\Sigma)\Phi_{\rm E}, \end{aligned}$$

where the E subscript (often written 'GR', which as discussed earlier is inappropriate) stands for Einstein gravity, in which $\nabla^2\Phi_{\rm E} = 4\pi G a^2\bar{\rho}\delta$. These parameters are related to first order to the slip and G parameters by $\mu = \alpha + \beta$; $\Sigma = \alpha/2 + \beta$. In this parameterisation, μ is probed by redshift-space distortions, while Σ is probed by gravitational lensing. But even so, this decomposition leaves open the extent to which the gravitational modification depend on scale and on epoch. The scale dependence is normally ignored, so that we are implicitly determining the properties of gravity on the $\sim$ 100-Mpc scales of LSS. As for epoch dependence, modifications of gravity gain a strong motivation from the acceleration of the universe, with the conjecture that changing gravity may produce this acceleration without the need for dark energy as a physical substance. Acceleration

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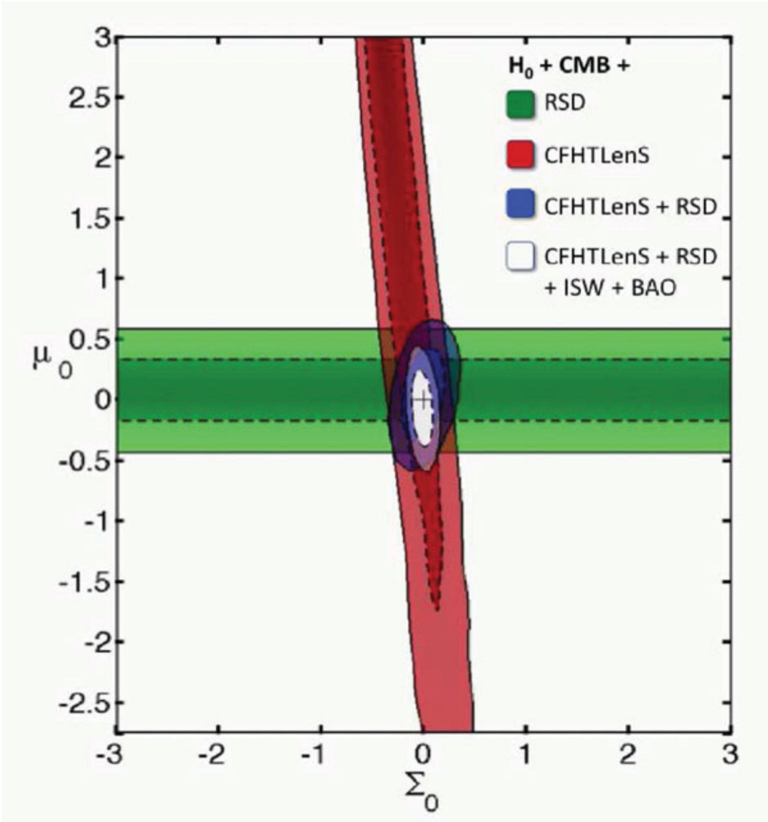

Figure 5. Limits on the two principal linear modified-gravity parameters, from Simpson *et al.* (2013). Both non-relativistic accelerations (from RSD) and relativistic accelerations (from gravitational lensing) seem to be within $\sim 20\%$ of their standard values.

is a late-time phenomenon, so it is reasonable to assume that modifications of gravity are negligible at early times (apart from anything else, we would prefer not to spoil the good agreement between the standard model and the CMB or primordial nucleosynthesis). Thus it is usual to scale $(\mu, \Sigma) \propto a^3$ or $\propto \Omega_v(a)$.

As we have seen, the μ parameter influences the growth rate of density fluctuations: this is often parameterised as $f_g = \Omega_m(a)^\gamma$, where the standard value of $\gamma \simeq 0.55$ changes when gravity is modified. According to Linder & Cahn (2007), this index is well approximated by

$$\gamma = \frac{3}{5 - 6w}\left(1 - w - \frac{\mu(a)}{\Omega_v(a)}\right),$$

where $\mu(a)/\Omega_v(a)$ will generally be assumed to be nearly constant in order that the modifications vanish at high z, so it can be taken to be μ_0/Ω_v. The sensitivity to μ_0 is therefore

$$\frac{d\ln f_g}{d\mu_0} = \frac{-3\ln\Omega_m(a)}{(5-6w)\Omega_v}.$$

For data at $z = 1$, $\Omega_m(a) \simeq 0.77$, so $d\ln f_g/d\mu_0 \simeq 0.10$ (as compared to 0.47 at $z = 0$), so high-redshift measurements probe μ_0 less effectively (although this comes entirely from the model assumptions, and it is still of interest to look at the high-redshift f_g in order to be sensitive to a broader range of possibilities). The highest redshift probed to date is

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$\bar{z} = 0.8$ via the VIPERS project (de la Torre *et al.* 2013), which measured $f_g\sigma_8$ to 17% precision. Combining lower-redshift BAO data (especially from BOSS), together with measurements of gravitational lensing, we can set limits to the parameters governing modified gravity, specified by the current values μ_0 and Σ_0. Current constraints of this sort are shown in Figure 5, where we see that standard gravity in both its relativistic and non-relativistic respects is consistent with current data at about the 20% level.

Future experiments will change this sensitivity to the sub-% level; do we have any reason to expect that a deviation from Einstein will be seen? Certainly there is no compelling model that leads us to expect a signal at this level, so a null result is probably the favoured prior expectation. Nevertheless, carrying out such tests has been a hugely positive development for cosmology. In previous generations, the correctness of Einstein gravity had to be assumed in order to reach cosmological conclusions; but now we can validate this fundamental assumption, rather than having to take it on trust.

4. Summary and conclusions

In this brief overview, we have taken stock of the growth in understanding of the large-scale structure of the universe from its inception almost exactly a century ago, to the beautiful precision of present-day measurements. Over 2 million galaxy redshifts are known, a total that would probably have been unimaginable to the early pioneers. As summarised above, this advance has been possible through a series of technological revolutions, which have allowed the stockpile of redshifts to advance exponentially with time over more than three decades. As illustrated in Figure 6, the doubling time of this advance has been a mere 3.5 years; at this rate, less than 50 years remains before spectroscopic information for the visible universe would be complete.

Naive exponential extrapolation is guaranteed to generate comical errors in due course. As a child, I owned an early-1960s book that looked ahead correctly to human landings on the Moon in 1969 – but the same book also informed the reader that landings on Mars would follow in 1984. Similarly, one can list several reasons why the spectroscopic exploration of the universe may not continue to grow at its recent astonishing rate. The fundamental problem is that there is no new technology: efficiency of digital detectors has saturated; future degrees of spectroscopic multiplex will be only moderately larger; primary mirrors for spectroscopy will become slightly larger. This does not add up to a revolution. It is somewhat disappointing to think that DESI, as the leading spectroscopic survey project around 2020 (Levi *et al.* 2013), will have the same mirror size and 'only' about 10 times the number of fibres of 2dF – an instrument 25 years older. Moreover, the cost of such an instrument can tend to increase linearly with the number of fibres, as this increases the number of spectrographs to be replicated. DESI will also increase its power over 2dF by becoming a dedicated instrument that uses all the time on its telescope; this factor, together with the increase in numbers of fibres and a lowering of S/N target will amount to two orders of magnitude in terms of the numbers of redshifts gathered with respect to 2dFGRS. But the cost has arisen substantially, also. 2dF cost about \$10M for the hardware, which dominates the cost of 2dFGRS; DESI's budget is in excess of \$100M, of which about 1/3 comes from supporting the sheer number of nights required. Even though the cost per redshift has fallen if we assign the entire cost of 2dF to 2dFGRS, this approach is becoming expensive. Add to this the fact that increasing depth of imaging surveys really requires a larger mirror, and we can see that limits are being reached. A spectroscopic survey 'dream machine' with 10m mirror, 10,000 fibres over a 4-5 degree field, would clearly be in the same billion-dollar league as e.g. LSST,

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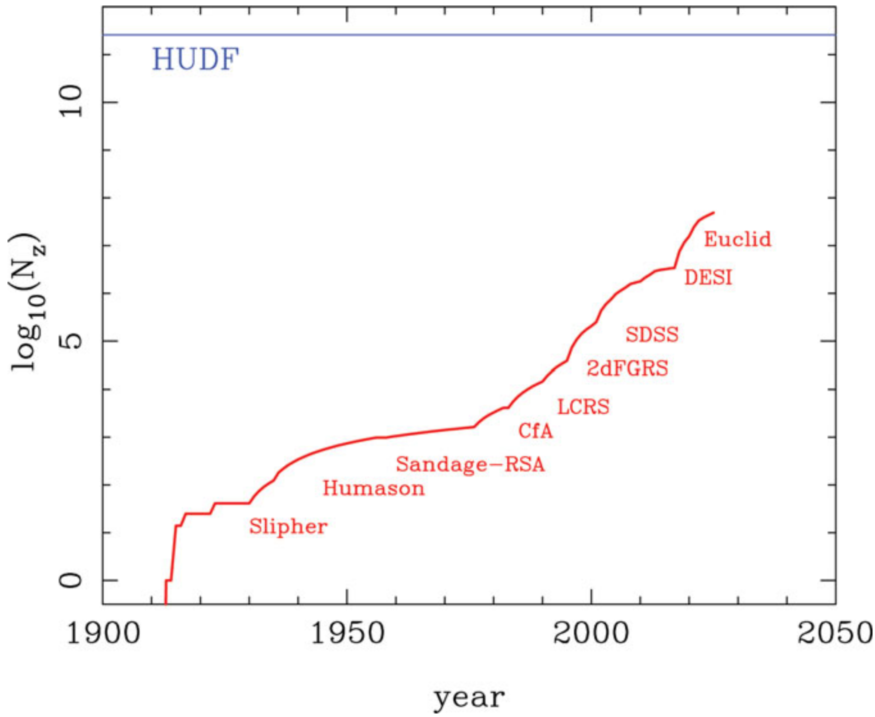

Figure 6. The approximate cumulative measurement history of galaxy spectroscopic redshifts, with selected major landmark studies indicated. The upper line marked 'HUDF' corresponds to the Hubble Ultra Deep Field, extrapolated to all sky (i.e. every galaxy in the visible universe). Over several decades, we have sustained a 'MooreZ Law', in which the stock of redshifts doubles every 3.5 years. On this basis, spectroscopic exploration of the universe of galaxies may be expected to cease in about 2060.

and thus would probably take the same 20+ years to come to fruition. This cannot be the way ahead.

The nearest thing to a new technology on offer is the approach taken by Euclid, measuring redshifts via slitless spectroscopy. This is of course also a very old approach, given a new lease of life because the sky background is very low in space. But this too has its limitations: object overlap is a major headache unless the spectral resolution is made unacceptably low. Also, working in space inevitably escalates the cost, pricing Euclid's ~ 25M expected redshifts at several times \$100M. Thus, both on ground and in space, it is hard to see how to take current approaches to the next level of $\sim 10^9$ redshifts. Two possible ways ahead aim to achieve the equivalent science goals by sacrificing some element of detailed spectroscopy on individual objects. The first approach is extreme photometric redshifts, using narrow-band filters to estimate redshifts with a precision of around 0.5% in z. Two competing Spanish-led projects are pursuing this approach (J-PAS and PAU) (Benitez *et al.* 1014; Martí *et al.* 2014). This approach should work well for BAO studies, although the redshift precision is equivalent to a velocity of worse than $1000\,\mathrm{km\,s^{-1}}$, so RSD studies will be challenging. Also, the restriction to optical bands means that the accessible redshift range will substantially overlap the volume already studied by BOSS, and subject to the same cosmic-variance effects.

Alternatively, we can ask why we bother to measure redshift from individual objects, when we only care about how the emission is distributed on 100-Mpc scales. The philosophy of intensity-mapping experiments is to measure this directly by sacrificing spatial

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resolution in exchange for sky coverage. In order to avoid being degraded by sky background emission, this approach is most effective in the radio. The first experiment likely to demonstrate this approach in practice is CHIME – the Canadian HI Mapping Experiment (Bandura *et al.* 2014). This will map a hemisphere with about 0.4° resolution (tens of Mpc), with a frequency resolution of 0.1% over $z = 0.8 - 2.5$. The lack of angular resolution will make removal of foregrounds more challenging, but this could be a revolutionary approach. The frequency precision is good enough that the results should be useable for RSD as well as BAO.

But how far the field will be able to make use of expanded datasets depends on theoretical and modelling developments. We have seen that it is already hard to extract the growth rate correctly at the 1–2% level from existing data, and improving this precision will be a challenge. The BAO systematics are simpler, so probably there is no reason that this method cannot be pushed robustly well below the current 1% level. There is good reason to be hopeful in both areas, since future datasets will yield large samples of very different galaxies, so there will be a strong motivation to continue theoretical efforts until the correspondingly improved deductions about dark energy and gravity are available. It's hard to know whether we should really expect to find a deviation from the ΛCDM model in this way; but it is profoundly important that these fundamentals of the model can be tested rather than assumed. To have achieved so much in this direction in only a century of effort should be a source of great pride to this community.

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