The Standard Model of Cosmology: A Skeptic's Guide

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Summary. — The status of the standard cosmological model, also known as "ΛCDM" is described. With some simple assumptions, this model fits a wide range of data, with just six (or seven) free parameters. One should be skeptical about this claim, since it implies that we now have an astonishingly good picture of the statistical properties of the large-scale Universe. However, the successes of the model cannot be denied, including more than $1000\,\sigma$ worth of detection of CMB anisotropy power. The model is older than most modern astrophysicists seem to appreciate, and has not fundamentally changed for more than a quarter of a century. Tensions and anomalies are often discussed, and while we should of course be open to the possibility of new physics, we should also be skeptical of the importance of 2–$3\,\sigma$ differences between data sets until they become more significant. Still, today's SMC is surely not the full story and we should be looking for extensions or new ingredients to the model, guided throughout by a skeptical outlook.

1. – What is the standard model of cosmology?

The currently best-fitting picture for describing the statistics of the Universe on large scales, the standard model of cosmology (or SMC), is often known as ΛCDM, since it's a model in which the matter is mostly cold and dark (i.e. effectively collisionless and with no

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electromagnetic interactions, CDM), with the bulk of the energy density of the Universe behaving like vacuum energy (i.e. like the cosmological constant of general relativity, Λ). But things are even more specific than that, with the values of only about half-a-dozen free parameters being enough to make a Universe that looks statistically just like the one we live in – and several of those parameters are now known to a really impressive level of precision. So the "SMC" is now quite precisely prescribed.

It is an astonishing achievement of modern cosmology that we have come to have such a successful model, especially when one considers that there is no a posteriori reason to expect things to be this simple. In physics we are driven to accept a model for several reasons – certainly that it fits the data, but also because of some less well defined notion of æsthetics. The simple group theoretical underpinnings of the standard model of particle physics and the elegance of the field equations of general relativity are obvious examples of this. Sure, they fit lots and lots of experimental data, but they're also really *nice*! But for cosmology, no one would claim that the SMC is beautiful, or even that it has to be correct because all alternatives are uglier. Certainly the SMC has some degree of simplicity (since it doesn't need many free parameters), but why do those parameters have the values that they do (see Sects. **2** and **3**)? And why aren't there lots of other parameters required (see Sect. **4**)? Despite the fact that nothing in the basic cosmological picture has changed since the early-to-mid-1990s (see Sect. **5**), most cosmologists are expecting something else to be just around the corner. After all, surely the SMC can't be all there is?

In these notes I'd like to bring some attention to the idea that we should be skeptical [1] here, since we're dealing with very large themes. A model that purports to describe the whole of the observable Universe should be met with a decent dose of incredulity! It's important that we retain a healthy level of skepticism when discussing any such claims. But at the same time we should also remember to be skeptical about *counter*-claims (Sects. **6** and **7**) that haven't passed the same level of scrutiny. And we should keep in mind criteria that define what skepticism is (Sect. **8**), so that we can isolate the successes of the SMC, while remembering that parts of modern cosmology's lore remain quite speculative (Sect. **9**).

2. – The parameters and assumptions of the SMC

Let's be explicit about the standard model by giving the modern values of its basic parameters. Right now the determination of these quantities is driven by cosmic microwave background (CMB) anisotropy experiments, and in particular by results from the *Planck* satellite [2] (supported by many other kinds of data) on the CMB power spectra (which are discussed further in Sect. **4**). Because of this, the basic parameter set is currently given in terms of the quantities that are most directly measured by CMB experiments. This means that the parameters most often discussed in relation to observational constraints are not necessarily the ones that are simplest to explain to the general public, or that are the focus of non-CMB cosmologists. These parameters are listed in Table I. The set consists of: two densities, $\Omega_{\rm b}h^2$ and $\Omega_{\rm c}h^2$ (for baryonic matter and cold dark matter separately, since they have distinct effects on the CMB power spectra), including a scaling of physical density with the dimensionless Hubble parameter, $h \equiv H_0/100\,\mathrm{km\,s^{-1}\,Mpc^{-1}}$; a parameter θ_* that corresponds to the sound horizon divided by the angular diameter distance to last scattering, which quantifies sliding the CMB power spectra left and right; the amplitude A_s of the initial power spectrum of density perturbations, defined at a particular scale, and often given as a logarithm; the slope n of the initial power spectrum as a function of wavenumber; and a parameter τ describing how much the primary CMB anisotropies are scattered by the reionised medium at low redshifts.

Table I. – *Basic cosmological parameters, from a combination of Planck 2015 data and other constraints from BAO, SNe and* H_0 *data (see Ref. [3]). The CMB temperature comes from an analysis of the monopole spectral data by Fixsen [4].*

Physical baryon density	$\Omega_\mathrm{b}h^2$	0.02227 ± 0.00020
Physical CDM density	$\Omega_\mathrm{c}h^2$	0.1184 ± 0.0012
Angular parameter	$100\theta_*$	1.04106 ± 0.00041
Reionisation optical depth	τ	0.067 ± 0.013
Power spectrum amplitude	$\ln(10^{10}A_\mathrm{s})$	3.064 ± 0.024
Power spectrum slope	n_s	0.9681 ± 0.0044
CMB temperature	T_0 [K]	2.7255 ± 0.0006

By far the best determined of these parameters is θ_*, with a signal-to-noise ratio (S/N) of about 2500 (from Table I, or about 2300 from the CMB alone). Then follows A_s, $\Omega_\mathrm{b}h^2$ and $\Omega_\mathrm{c}h^2$, with $S/N \simeq 100$, while n_s and τ only differ from their default values (of 1 and 0, respectively) at $S/N \simeq 5$. Other cosmological parameters that are often discussed include H_0, t_0, Ω_m, Ω_Λ, z_reion, etc., which are not independent, but can be determined from the six parameters in the context of the SMC.

Although it is often stated that there are six basic parameters, there's a seventh that is often ignored. This is the temperature of the CMB today (or equivalently the radiation density), which is constrained using data from the *COBE*-FIRAS instrument [5], as well as from several other experiments (see Ref. [4]). The determination is systematics dominated, with $S/N \simeq 5000$. It is hence more precise than other parameters, and dramatically better determined than other densities. For that reason it is usually considered to be fixed, and not a free parameter at all. However, the precision is starting to approach the cosmic-variance limit, and so if T_0 was measured with much smaller errors, we'd have to consider the fact that we can only measure parameters within our Hubble patch and not actually "background" parameter values (see Ref. [6] for discussion).

But (to be skeptical about this), we might wonder whether there are other hidden parameters. There definitely are, to some extent, but mostly any additions to the SMC are better cast as assumptions. In fact there are many of these, and it is important to be clear that the six (or seven) parameters of the SMC are only descriptive of the Universe within a specific framework. A list of these assumptions is given in Table II (and the reader can probably think of more).

TABLE II. – *Some assumptions of the SMC. Note that several of these apply to our observable volume (which is the only part of the Universe that we can test) only.*

Understanding the Cosmos is possible for human beings
Physics is the same everywhere and at all times
General relativity is the correct theory of gravity on cosmological scales
The Universe is approximately statistically homogeneous and isotropic
The Universe is spatially flat on large scales
The dark energy behaves like a cosmological constant, with $w = -1$
The dark matter is collisionless and cold for the purposes of cosmology
There are three species of nearly massless neutrinos
There are no additional light particles contributing to the background
Density perturbations are adiabatic in nature
The initial conditions were Gaussian
The running of the primordial power spectrum is negligible
The contribution of gravitational waves is negligible
Topological defects were unimportant for structure formation
The physics of recombination is fully understood
One parameter is sufficient to describe the effects of reionisation

All of these assumptions are testable, and they all *have* been investigated. Many of them are tested through putting limits on extensions to the SMC, e.g. checking whether the curvature is consistent with flat space, whether there's evidence for modified gravity, non-trivial dark energy (i.e. $w \neq -1$), or non-Gaussianity, or whether there are signs of the effects of massive neutrinos or cosmic strings (e.g. see Refs. [7, 3]).

Nevertheless, this is definitely a place where we need to exercise caution. The confidence with which we know the values of the basic set of six (or seven) parameters depends on this being the full parameters space. If there are more ingredients in the actual model, then the parameters in the basic set will have larger uncertainties. For example, if we consider models that allow curvature then the constraints on w are very much weakened. Hence we need to look carefully at these tests. Right now there is no strong evidence for any additional parameter, but we fully expect that there will be more ingredients required as the data improve, e.g. that the effects of massive neutrinos or primordial gravitational waves will eventually be measured. And there may be genuine surprises of course, like multiple kinds of dark matter or dark energy, or important extra components, such as magnetic fields or isocurvature modes.

Nevertheless, there *has* been caution exercised, and despite attempts to find evidence for additional parameters, the basic set continues to fit very high signal-to-noise data – particularly the CMB power spectra.

3. – The numbers that describe the Universe

Since it appears that the set of numbers required to statistically describe the cosmological model has just seven elements, then these values become important quantities that should be better known, among astronomers and non-astronomers alike. Many people follow the detailed statistics of their favourite sports teams, or can name the capital cities of various countries, or give the sequence of colours of the rainbow, or list the wives of Henry VIII in order, or name the actors who have played their favourite time-travelling alien. Almost everyone learns the list of planets in the Solar System, through the mnemonic about pizzas (that no longer includes pizza!). So why don't most humans know the numbers that describe the Universe that we live in?

Perhaps one of the problems is that the usual six parameters coming from CMB anisotropies are quite esoteric. This becomes apparent as soon as one tries to explain the values in Table I to the general public. However, these six arcane numbers (together with the assumptions that we've already discussed) span the space of all parameters, and hence it's easy to present versions that are simpler (like the age of the Universe, t_0, or the density of some component, like $\rho_{\rm M}$) in more familiar units. Let us highlight a few variants of quantities that are useful in describing our Universe, in the hope that some of them may catch on! Further examples along these lines can be found in the paper "Cosmic Mnemonics" [8].

Table III. – *Variants on the numbers that describe our Universe.*

Characteristic scale on the CMB sky, $\theta_* \simeq 0.6^\circ$ (think eclipse!)
Radius of observable Universe $\simeq 400\,$Ym
Age of the Universe $t_0 \simeq 5\,$trillion days $\simeq 5 \times 2^{200} t_{\rm Pl}$
Age of the Universe is triple the age of the Earth, $t_0 \simeq 3\,t_\oplus$
$H_0\,t_0$ is slightly less than 1, and $H\,t$ will be unity in about 1 billion years
H_0 will asymptote to the value $56\,{\rm km\,s^{-1}\,Mpc^{-1}}$ in the far future
Cosmological constant, $\Lambda \simeq 10^{-35}\,{\rm s^{-2}}$ ("ten square attohertz")
Critical density, $\rho_{\rm crit}$, corresponds to 5 proton masses per cubic metre
Density ratios, $\Omega_{\rm c}/\Omega_{\rm b} \simeq 2\,\Omega_\Lambda/\Omega_{\rm m} \simeq 5.3$
Density parameter for photons, $\Omega_\gamma \simeq \alpha^2$
Variance of density in spheres is unity at about $9\,$Mpc (no h^{-1})
Amplitude of position-space density perturbations on Hubble scale, $\sigma \simeq 6 \times 10^{-6}$
Temperature at last scattering epoch $T_{\rm CMB} \simeq 3000\,$K (think M giant!)
Age at last-scattering epoch, $t_{\rm rec} \simeq 370\,$kyr
Age at reionisation, $t_{\rm reion} \simeq 600\,$Myr
Number of particles in observable Universe $\simeq \alpha^{-42}$

With enough effort, it's easy to find numerological coincidences. One should obviously be skeptical about claims of significance for such things though! For example, from the table we see that the number of particles in the observable Universe (mostly photons) is about α^{-42} (where α is the fine-structure constant), and additionally in the standard model, the Earth forms at a redshift corresponding to $z = 0.42$. These facts could be used to suggest a link with Douglas Adams' universal answer.

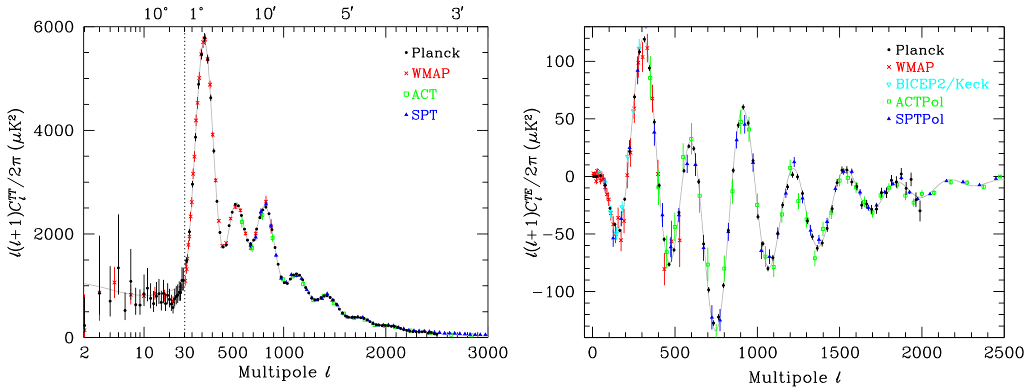

Fig. 1. – CMB temperature anisotropy power spectrum (left) and temperature-polarisation cross-power spectrum (right), from *Planck*, *WMAP*, BICEP/Keck, ACT and SPT (see Ref. [9] for full references). This demonstrates the current precision with which these power spectra have been measured.

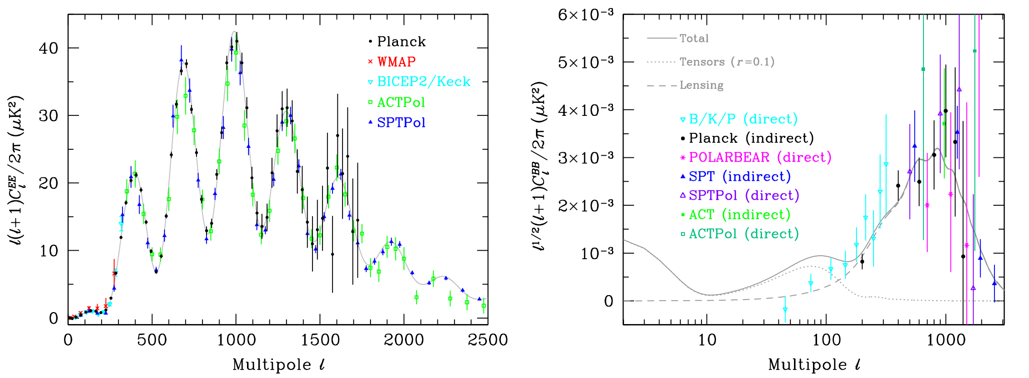

Fig. 2. – Polarisation EE (left) and BB (right) power spectra from several experiments (see Ref. [9] for references). The BB spectrum here is scaled by a power of ℓ that makes it possible to see all three of the expected peaks (from reionisation, recombination and lensing).

4. – Information in the SMC

Cosmological information comes from many sources. However, at the present time, the CMB dominates the constraints on the SMC. This "era of precision cosmology" can be seen through plots of the current status of the power spectra coming from the CMB.

Figure 1 shows the TT (which dominates the information) and TE (which is catching up) spectra, while Fig. 2 shows the EE (now also impressive) and BB (still in its infancy) spectra. The grey line is the 6-parameter ΛCDM model fit to the *Planck* TT data, and one can see how well it matches the other power spectra.

When we add up the total $(S/N)^2$ from the *Planck* CMB power spectra, over the part of the sky conservatively believed to be free of foreground emission, we find that the *Planck* TT, TE and EE measurements together correspond to about $900\,\sigma$. Adding the higher multipole measurements from ACTPol and SPTPol means that today's CMB power spectrum determinations together represent more than $1000\,\sigma$ of detection. If we were skeptical about the success of the SMC, then we should take note that it requires just a few simplifying assumptions and seven free parameters to fit this huge amount of information – quite a remarkable achievement.

How does the constraining power of the CMB work? The simple answer is that it just depends on the number of modes that are measured, where the mode amplitudes $a_{\ell m}$ come from expanding the sky as $T(\theta, \phi) = \sum_{\ell m} a_{\ell m} Y_{\ell m}$. Since the anisotropies are Gaussian, then each $a_{\ell m}$ gives just a little bit of information about the expectation value of the power in the $a_{\ell m}$s, C_ℓ (or equivalently, the variance), and the total constraining power is just about counting the number of modes. In more detail, if we have a cosmic-variance-limited experiment, with $\Delta C_\ell = \sqrt{2/(2\ell+1)}C_\ell$, then the total signal-to-noise ratio in the power spectrum is

$$(1) \qquad (S/N)^2 \equiv \sum_{\ell=2}^{\ell_{\rm max}} (C_\ell/\Delta C_\ell)^2 = \frac{1}{2}\sum_{\ell=2}^{\ell_{\rm max}} (2\ell+1) = \frac{1}{2}\left[\ell_{\rm max}(\ell_{\rm max}+2) - 3\right] \simeq \ell_{\rm max}^2.$$

But since the number of modes is just $\sum_{\ell=2}^{\ell_{\rm max}} \sum_{m=-\ell}^{+\ell}$, then this means that the total $(S/N)^2$ is just half the number of modes.

To the extent that through *Planck*, we've measured all the modes out to $\ell \simeq 1500$ (over a large fraction of the sky), and the damping plus foregrounds means that we can't go far beyond $\ell \simeq 3000$ (say) for primary CMB anisotropy measurements, then we're a good way through gathering all the information we can get from C_ℓ^{TT}. But what about polarisation? Assuming for the moment that C_ℓ^{BB} is negligible, then the existence of both C_ℓ^{TE} and C_ℓ^{EE} would seem to confuse matters. But really the situation is simply that we can measure the scalar field E, in addition to T, for each pixel. And hence, provided that we measure both C_ℓ^{TE} and C_ℓ^{EE} out to some $\ell_{\rm max}$, then we have exactly twice as much information as we would obtain from C_ℓ^{TT} alone. This means that the total "information content" can be defined to be just a count of the number of modes probed, in any of the CMB fields.

Of course not all information is equal. For example, *any* large-angle BB measurement would provide us with an entirely new kind of information, enabling us to determine an additional parameter (r, the tensor-to-scalar ratio) that is otherwise hard to constrain. Moreover, it is well known that adding polarisation data helps to break some parameter degeneracies. So we'd like to know how the parameter constraints map onto the power

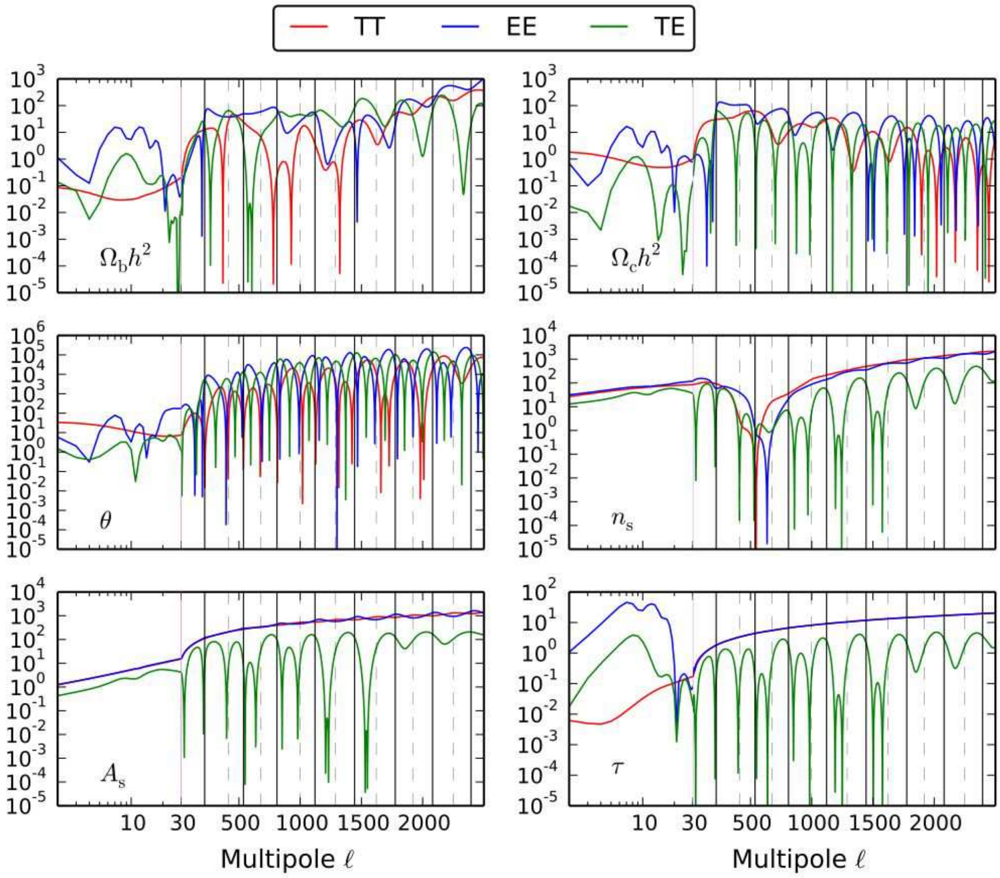

Fig. 3. – Square of signal-to-noise ratio for each multipole, for a cosmic-variance-limited experiment covering half the sky. The results for the TT, TE and EE power spectra are shown on panels for each of the six parameters of the SMC, with a logarithmic horizontal axis for the first 30 multipoles. Vertical lines mark peaks and troughs in the TT power spectrum. These panels show the complex mapping of power spectrum constraints onto parameters over different multipole ranges.

spectrum modes. The proper way to discuss this is through the Fisher matrix, which includes derivatives of the power spectra with respect to the cosmological parameters; this is demonstrated in Fig. 3 and discussed fully in Ref. [10]. One can see that some multipole ranges are particularly important for some parameters, and as we go up in ℓ, so that new peaks or troughs are included, the constraining power can change dramatically.

To focus on one example, the behaviour of the A_s panel is simple – if there was only an overall normalisation to measure, then the constraints would just come from the S/N (and mode counting) expression given in Eq. (1), with polarisation giving equal constraining power to temperature. In that sense, A_s is a "linear" parameter, since there is a simple relationship between its total S/N and the parameter constraint. However, if the dependence is less trivial, then the relationship is "non-linear", and hence the way that the total $(S/N)^2$ is shared out among the parameters is more complicated. A good example is θ_*, which determines the amount by which the power spectra can be slid left and right in multipole – this can be determined to great precision (because of the relative sharpness of the acoustic peak structure), which is why this is the best determined parameter of the SMC today. In fact the total S/N in θ_* from *Planck* is around 2500, which is considerably more than the *total* S/N in the power spectra!

CMB polarisation has yet to become particularly constraining for the parameters of the SMC. But that situation will change as new experiments add modes, doubling at least (or more, since high-ℓ E-mode measurements are not as limited by foregrounds) the information achievable from temperature alone, and providing specifically useful degeneracy-breaking capability. The discussion above can be extended to the BB spectrum, as well as the lensing spectrum $C_\ell^{\phi\phi}$ (which comes from the temperature trispectrum, and gives an additional field, ϕ). This approach is useful for discussing future experimental constraints, and how they map onto parameters. But one thing it tells us is that eventually we'll run out of CMB information. This is essentially because the CMB information is almost entirely restricted to two dimensions. The same thinking can also inform discussions of more ambitious attempts to extract the much larger amount of information contained in 3d surveys – around $(c\,k_\mathrm{max}/H_0)^3$, if we can get all the k modes down to scales k_max [11]. This means that in principle we could one day measure enough modes to give $\gg 10^6\,\sigma$ of power detection.

5. – The venerableness of the SMC

It is clear that the standard model of cosmology is now well established. So well established in fact that a great deal of effort in modern cosmology is directed towards trying to find extensions to the model. For example, searching for evidence that the dark energy is evolving or that more than two parameters are needed to characterise the perturbations. Such searches for "physics beyond the standard model" makes one think of similar endeavours to find evidence to extend the Standard Model of particle physics. When this sort of thing comes up, it has been traditional for cosmologists to claim that the SMC is relatively young and still in an exploratory stage – so it's *nothing like* the chasing of $3\,\sigma$ effects that appears to have motivated much experimental particle physics for decades. But in fact the SMC is actually quite long in the tooth itself by now!

So how old is the SMC? Certainly if one goes back a dozen years to a previous overview by this same author [12], one finds little that has changed. The model is much more precisely determined of course, but all the ingredients are already in place. Indeed, one can go back earlier, e.g. to the paper "What Have We Already Learned from the Cosmic Microwave Background?" (also known as "What Has the CMB Ever Done For Us?") written in 1998 [13], and find that the basic picture is just the same. In fact many expositions of the history of cosmology state that the model became established with the detection of cosmic acceleration in 1998. Of course that was an important part of the story, but I think it's clear that what we now call ΛCDM was *already* the best-fitting model when the supernova data came in and confirmed it – to the extent that even the most skeptical cosmologists had to take Λ seriously. Many papers had already pointed out before 1998 that a collection of results pointed to a flat Λ model being the best way of extending what had previously been called "standard CDM" (or sCDM), i.e. a CDM-dominated model with $\Omega_{\rm M} = 1$ and an initial power spectrum of exactly the Harrison-Zeldovich-Peebles form ($n = 1$). Among these results were: the need for more power on large scales (to match galaxy clustering data); the fact that most measurements of the density parameter tended to give $\Omega \lesssim 0.3$; that few measurements of H_0 gave values in the $\simeq 50\,{\rm km\,s^{-1}\,Mpc^{-1}}$ range that were needed to make the age of the Universe old enough for the stars within it; that the amplitude of density perturbations from the *COBE* satellite's CMB anisotropies pointed towards adiabatic perturbations in a Λ-dominated (rather than open) model; and that indications from smaller-scale CMB anisotropies were suggesting an acoustic scale consistent with flat geometry [14, 15].

Several of these arguments were compiled in two essays in 1995, one by Ostriker & Steinhardt [16] and the other by Krauss & Turner [17]. Not everyone was convinced of course, and some nostalgic theorists still tried to cling to the Einstein-de Sitter elegance of sCDM [18, 19]. However, the writing was on the wall, and of all the flavours to add to sCDM, it was apparent by the mid-90s that ΛCDM gave the best fit (even if you didn't necessarily like it!). Indeed it is possible to find earlier papers pointing to this model being preferred by a combination of data – and here the 1990 Nature paper by Efstathiou, Maddox & Sutherland [20] is a particular standout. That's not to say that there weren't papers proposing quite different models at the same time, but just that the currently understood SMC was already there in the early 90s, with reasons to believe that it provided the "best-buy" cosmology.

What this means is that the SMC is older than most people appreciate – something like a quarter of century old, making it more than half the age of the SM of particle physics! As an indication of just how long ago that was, in the early 90s we were using dial-up modems to connect with the internet, the main browser was (the pre-Netscape) Mosaic and the world's first text message was being sent!

6. – Tensions

The idea of "tensions" has already come up. So let's take that particular bull by the horns right now. There are several different minor chinks in the armour of the SMC that are pointed to by various researchers. A list of some of them is given in Table IV. None are sufficiently significant to call them an actual discrepancy, hence the use of the word "tension" [21]. What should a skeptic make of an apparent disagreement between different data sets at the 2–3 σ level?

Well, let's remember that today's CMB data contains more than $1000\,\sigma$ worth of detection. We can ask how many $3\,\sigma$ results there are in $1000\,\sigma$. Since signal-to-noise ratios add quadratically, the answer is the number of times that 3^2 goes into 1000^2, and

Table IV. – *List of claimed tensions (not complete!). Are any of these of consequence?*

The amplitude σ_8 between CMB and cluster abundance
Galaxy cosmic shear versus CMB constraints on $\sigma_8\Omega_{\mathrm{m}}^{0.5}$
H_0 between traditional direct estimates and indirect CMB estimates
Planck versus *WMAP TT* power spectra
Planck high-ℓ versus low-ℓ data
Preference for $A_{\mathrm{L}} > 1$ (apparent lensing effect) in *Planck* data
Small-scale galaxy formation controversies

the answer is *more than 100,000*. So why is there so much focus on specific issues that are barely at the $3\,\sigma$ level?

Of course part of the answer is that we shouldn't accept that the SMC is the last word, but should keep an open mind to other possibilities. There's also a strong motivation to look for flaws that require revisions, since we're all hoping to find fundamentally new physics by further confronting the SMC with cosmological data. And there *may* be evidence of such things lurking in low S/N disparities. The trick of course is to find the hints of disparity that grow from mere "tensions" into genuinely significant differences.

However, concentrating on a few of the $> 100{,}000$ potential 3-ish σ effects seems like a misplaced kind of skepticism. Trying to find minor deficiencies in conventional wisdom seems to me to be a bit like chasing conspiracy theories. In any situation, you can always find *something* that doesn't seem to make sense – but you should be assessing the evidence carefully, bearing in mind the context. Here the context is: (1) that the model (the SMC) fits a number of observational phenomena very well; (2) that some of the uncertainties are of a systematic rather than statistical nature; and (3) that there are a very large number of potential tensions that could be selected from the $> 1000\,\sigma$ of measured information.

Let's look at a couple of aspects of the history of the development of the SMC in order to see if there are any lessons we can learn. Although the SMC was already in place by the early 1990s, there are some observations that have changed considerably since that time. In particular, determinations of the age of the Universe (from estimating the ages of the oldest globular clusters) and determinations of the baryon abundance (coming from Big Bang nucleosynthesis) changed in value in the mid-1990s. Figure 4 shows the situation for cluster ages. The values plotted come from a representative selection of papers by several of the groups working on this problem at that time. In the early 90s the oldest clusters were stated to be perhaps 17 billion years old, with lower limits at around the 15 billion year level. However, in hindsight it is clear that those estimates were incorrect because they were dominated by systematic uncertainties. As other cosmological measurements improved, and it became clear that the Universe had an age that was probably no more than 14 billion years, the cluster ages were revised to become consistent. One might imagine that the change could be traced to one particular effect that was fixed – but that really isn't the case. Instead there were several tweaks made over the years, most

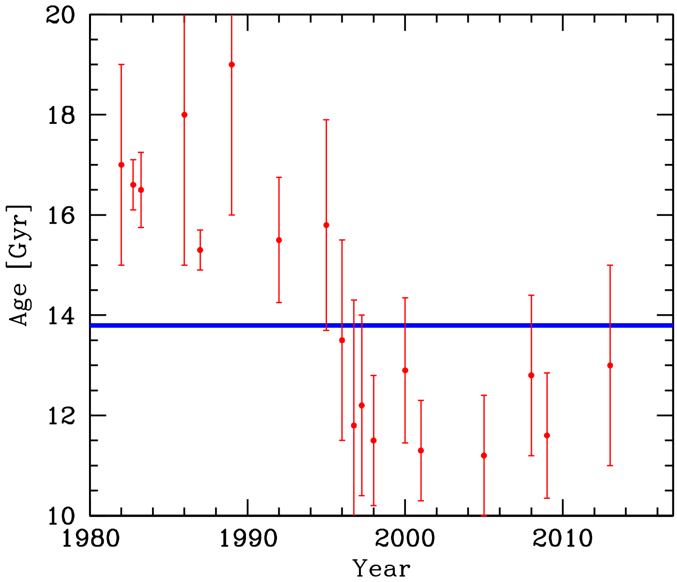

Fig. 4. – Estimated ages of the oldest globular clusters, taken from a representative set of review-like articles. The preferred value dropped dramatically in the mid-1990s. The blue band is the current best-fitting value for the age of the Universe, based on the 2015 *Planck* data (including uncertainty).

of which had the same sign and resulted in the ages of the oldest clusters coming down to around 12 billion years. The situation is really not very satisfying! But I suspect this is often the way things are when the uncertainties are to do with assumptions and approximations in the analysis, rather than just being statistical.

The situation with baryon abundance is fairly similar. But here it is harder to make a plot of the values, since often there were no clear errors given! Instead it was common to write down some feasible range for the baryon-to-photon ratio, which was bracketed by different light-element abundances (with little effort made at that time to designate 95 % confidence ranges, or give $\pm 1\,\sigma$ values, or the equivalent). Despite this difficulty in interpreting the uncertainties in the old results, what is clear is that the preferred value around 1990 was something like $\Omega_{\rm b}h^2 \simeq 0.012$, while 10 years later it was around 0.022. The change corresponded to a lot of σ (with whatever value of uncertainty you used). Again, there wasn't one reason for this change, but probably a list of things contributed to the increase (the availability of damped Ly α systems for deuterium abundance measurements being part of it).

The tension that perhaps attracts the most current attention is that some of the most recent and precise values for the Hubble constant, determined using standard distance-estimation techniques, appear higher than the value determined from the SMC parame-

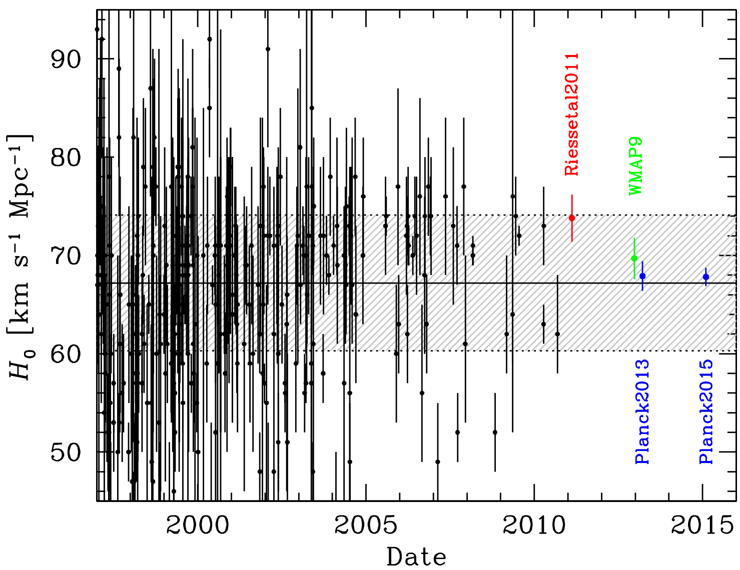

Fig. 5. – Estimates of the Hubble constant between 1997 and 2010, compiled by John Huchra (black points [22]). These values were combined into a best estimate (using practices for combining nuclear physics data) by Pritychenko [23], with this value and uncertainty represented by the hatched region. The new value published by Riess et al. in 2011 [24] is shown by the red point. The estimate derived from the full *WMAP* data set is shown in green [25]. The values obtained from the 2013 [7] and 2015 [3] *Planck* data releases are the two blue points.

ters that best fit CMB data. Figure 5 shows a large collection of published H_0 determinations from the period 1997–2010, including the error bars. The horizontal band gives an average of these data (using procedures developed to deal with apparently disparate nuclear and particle physics data [23]). The newer value from Riess et al. (2011) [24] is indicated, together with values coming from *WMAP* and *Planck*. Other recent values could be added, but wouldn't change the basic picture. Placed in this context we can see the systematics-dominated history of attempts to directly determine H_0 (which certainly goes back earlier than 1997!). We have to assume that most sets of authors of historical values believed that their published uncertainties were a fair representation of their confidence in the data. And yet it's clear that at any given time the errors were being underestimated by most groups. Perhaps the situation is genuinely different now, and the new (smaller) error bars are correct. But it's hard not to be skeptical.

In any case, we know that measurement techniques are continuing to improve, and that if this CMB-versus-direct-determination tension in H_0 arises from some genuinely new physics, then the statistical confidence in the differences will grow. Time will tell.

Turning to the example of *Planck*'s large-scale versus small-scale constraints, there are some additional issues for the skeptic to keep in mind. As discussed in the Planck

Collaboration Intermediate LI paper [26], this situation is not as simple as it might at first seem. There are certainly parameter shifts when one considers *Planck* low-ℓ versus *Planck* high-ℓ data, and these shifts may seem to be at the 2–3 σ level. However, the parameter space has six (or maybe five, considering that τ is hardly measured) dimensions, with many other directions in this space corresponding to particular parameter combinations. So one will see a $2\,\sigma$ deviation in *some* direction more than 5 % of the time, and hence it's necessary to take into account the whole parameter set when assessing this kind of tension. When that is done, the differences of low-ℓ versus high-ℓ parameters have a probability to exceed above 10 % (i.e. nothing to write home about). On top of that, it's unclear how one is choosing the angular scale to look for a split. And there is also a difficulty in assessing how unlikely it might be for a data excursion to map onto a parameter shift. The conclusion is that *Planck* and *WMAP* are in spectacular agreement where they overlap, and the shifts seen at higher multipoles are just about as big as you'd expect the shifts to be. That's not to say that there might not also be problems with some of the foreground modelling, or indeed some physics missing from the SMC – it's just that the data don't *require* these things at the moment.

7. – Anomalies

The other word that is much heard when discussing the CMB is "anomalies". What is meant here is a feature (at a low level of significance) that appears to be unexpected in the SMC, pointing to perhaps some kind of non-Gaussianity or breaking of statistical isotropy. There are several examples that have been suggested over the years, with different researchers claiming importance for one or other. Table V gives a partial list. It seems extremely hard to believe that *all* of these are pointing to deficiencies in the conventional picture. One should be skeptical of each of them, particularly because of the issue of a posteriori statistics. This issue is one that causes enough debate among cosmologists, that I'm going to discuss it in some detail.

TABLE V. – *List of claimed temperature anomalies. This is not intended to be a complete list (and several of these anomalies are related to each other).*

Low quadrupole and other low-ℓ modes
Deficit in power at $\ell \simeq 20$–30
Low variance
Lack of correlation at large angular scales
The Cold Spot
Other features on the CMB sky
Hemispheric asymmetry
Dipole modulation
Alignment of low-order multipoles
Odd-even multipole asymmetry
Other features in the power spectrum

The problem is that all of these "anomalies" had their statistical significance assessed *after* they were discovered. Hence, in order to fairly determine how unlikely they are, it is necessary to consider other anomalies that may have been discovered instead. Statisticians call this the "multiplicity of tests" issue, which I think is the most helpful way to think about it.

Let's take the so-called CMB Cold Spot as an example (as shown in the left-hand panel of Fig. 6). The probability of finding a cold region of exactly this size and shape in exactly this particular direction is obviously vanishingly small. No one would consider such a calculation to be useful, and at the very least would appreciate that the spot could have been found in any direction – hence to assess the significance one could look in simulated skies for similarly extreme cold spots that occur anywhere. The probability determined in this way then becomes of order 0.1 %. However, a specific scale was chosen for the spot, or to be more explicit, a filter of a particular shape and scale was chosen. It turns out that the Cold Spot isn't very extreme if a purely Gaussian filter is used, but is pulled out at higher amplitude by a "compensated" filter (e.g. what is often called a "Mexican hat") with a scale of about $5°$. This means that one should marginalise over the scale (within some reasonable bounds) and over a set of potential filter shapes (that one might have chosen) as well. On top of all this it's obviously clear that one needs to consider *hot* spots as well as cold spots (this may seem so self-evident that it doesn't need to be stated, but in fact several papers have *only* assessed the significance of cold spots). And the situation is more complicated than that, since if there had been a fairly conspicuous pair of neighbouring spots, or a hot spot diametrically opposite a cold spot, or even a triangle of spots, then one might equally well have been writing papers about the anomalous feature that was discovered.

The point is that in each Hubble patch (with the CMB sky being an independent realisation of the underlying power spectrum), there will be features on the sky or in the power spectrum, that appear anomalous. One has to consider the set of potential anomalies in each patch in order to assess whether a feature is extreme enough to get excited about. In practice 2–3 σ anomalies go away when you marginalise over these possibilities, but $\gtrsim 5\,\sigma$ anomalies would remain anomalous after maginalisation.

A criticism of this way of thinking is that it's just *too* skeptical! The argument is that if you try hard enough to marginalise over possible tests then you can make *anything* appear to be insignificant. I don't think this is true, since you have to be reasonable here (as in any assessment of statistical evidence, where there is always some subjectivity). And I stand by the claim that it's hard to make $5\,\sigma$ effects go away, while 2–3 σ effects that are subject to a posteriori statistics should always be viewed with extreme skepticism.

Another way to look at this is to make an analogous study of something that you are confident is genuinely random. This was done in the paper with Dr. Frolop [27], comparing the CMB anomalies with patterns in the digits of π. Several examples are given there, but let's just pick one. As illustrated in the right panel of Fig. 6, there are six consecutive occurrences of the digit "9" at the 762nd digit of π. Assuming that the digits are random, a simple calculation (considering the number of ways of placing the

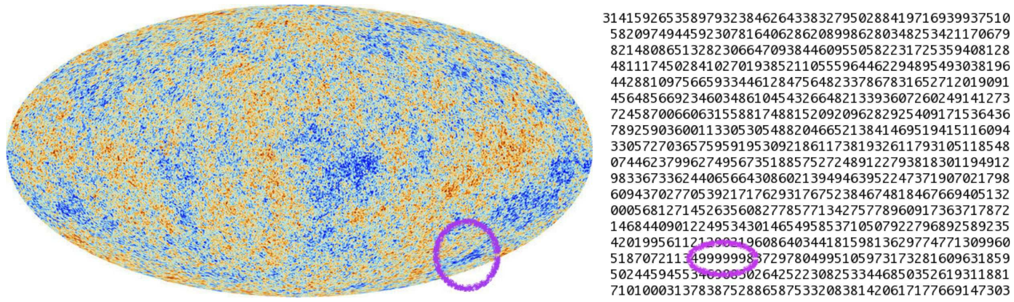

Fig. 6. – Left: map of the CMB sky from *Planck*, with the position of the so-called "Cold Spot" indicated. Right: the first 900 digits of π, showing the "hot spot" of six 9s (also known as the Feynman point).

run of 9s in the first 762 digits, and the number of ways of picking the other 756 digits) gives a probability of $756/10^6$. Of course the run of six numbers didn't have to be 9 (even although you could consider 9 to be special because it's the highest), and hence the probability for any run of six is 10 times greater – but that's still less than 1 %! So why does this not shake our faith in the digits of π being random? The answer is that a posteriori statistical effects can be subtle. In this particular case, we would have found an equal probability for obtaining a run of *five* 9s at an earlier digit, or *four* 9s even earlier, and so on. When including this hidden "multiplicity of tests", the probability becomes of order 10 %, i.e. not small enough to decide that there are messages written in the digits of π! And in addition to all of that, there are other patterns that might also have been remarked upon if they had been found, a run of "123456" for example, or an alternating series like "9090909". Perhaps these aren't quite *as* striking as six 9s, but they should get *some* weight in considering the set of tests, and hence in the assessment of the significance of the anomaly. I'm convinced that if one went to the trouble of looking for such things, there would appear to be something conspicuous in most chunks of π (in every 1000 digits, say) – just like there are some apparent "anomalies" in every Hubble patch's CMB sky.

Despite all these words of caution, let me add that one should *still* continue the search for anomalies, since any genuinely significant large-scale oddities could be signs of exciting new physics (e.g. see discussion in Ref. [28]). And of course sometimes $3\,\sigma$ things will become $5\,\sigma$ when more data are included – so it's worth continuing these investigations. A problem is that the large-angle temperature field has already been well mapped, and those data are now limited by cosmic variance. So the only way to make progress is to include new data, such as from CMB polarisation [29]. What would be particularly good would be to find some kind of natural explanation for an anomaly, with no (or very few) free parameters, which also makes a clear prediction for some *new* observable, such as polarisation, lensing, or 21-cm observations. If there's such a prediction, and a $3\,\sigma$ result is found, then that really would mean 99.7 % confidence.

8. – The nature of skepticism

I've said a lot about being skeptical – but what does that really mean? What I'm talking about here is the concept philosophers might call "scientific skepticism", which involves questioning assertions that lack empirical evidence. I believe this to be a fundamental part of scientific inquiry. It can be summed up through the phrase "extraordinary claims require extraordinary evidence" (popularised by Carl Sagan) – and obviously that applies well to cosmology, through its grandiose themes, just as it does to pseudo-science. Science isn't completely mechanical and dispassionate, since it includes speculation and creativity as part of the process of development – but that's not the same as accepting every new idea that comes along. At the other end of the spectrum, it's also important not to fall into the trap of "denialism", i.e. adopting a position that rejects every claim *even if* there's good evidence to support it (like climate change, or dark matter perhaps).

To be a bit more explicit about skepticism, let me pick the writings of a particular modern philosopher, namely Mario Bunge, who has written extensively on the topic of scientific epistemology. Among other definitions, he describes how any authentic science must include "changeability, compatibility with the bulk of the antecedent knowledge, partial intersection with at least one other science, and control by the scientific community" [30]. These ideas give a little more content to the notions of hypothesis testing, falsifiability, parsimony, etc., that we learn about in school. And they make clear that the skeptical approach is central to the establishment and evolution of scientific ideas.

As examples of topics that fail to meet these criteria and land up in the pseudo-science category, Bunge lists "astrology, alchemy, parapsychology, characterology, graphology, creation 'science', 'intelligent design', Christian 'Science', dowsing, homeopathy, and memetics". However, Bunge also states that "cosmology is still rife with speculations that contradict solid principles of physics"! He says that for good reason – the SMC lives within the domain of "physical cosmology" and has passed a wide array of tests, but, on the other hand, the most theoretical aspects of cosmology are indeed in an entirely different conjectural realm. Hence it is important to separate the concrete parts of modern cosmology (the answers to the "what" questions) from the areas where we are still speculating wildly (and trying to find answers to the "why" questions).

9. – Beyond the SMC

We'd all like to understand where the whole Universe comes from, or explain away the dark matter and dark energy. Speculation is certainly good, but *believing* your speculation (before it has passed any tests) is bad science! The correct approach should be to investigate the consequences of your idea and try to determine if there are definitive predictions that can be confronted with data.

I feel that there's a kind of malady that infects some cosmologists, where pretty much any outlandish and unorthodox idea is considered at the same level as the conventional picture – rather than giving it a higher degree of skepticism, like all extraordinary claims deserve. Perhaps part of the blame here is that modern physics in general, and the

SMC in particular, contain some fairly bizarre-sounding concepts. We teach students about quantum mechanics and black holes, that we can build a model for the whole observable Universe, that there are hypothetical particles that dominate all matter and that a negative-pressure fluid is driving the cosmic acceleration. So perhaps students start to think that any hare-brained scheme is equally worth pursuing?

I can't shake the feeling that a good dose of skepticism would help keep things in perspective.

An example of this is inflation (see [31] and [32] for discussions). It is undoubtedly an appealing idea, and there is a great deal of circumstantial evidence to support it – so I think it's entirely reasonable to be a *fan* of inflationary cosmology. But since inflation is really a framework rather than a model, we can't assert that any of the observations actually prove that inflation is correct [33]. It seems reasonable to assume that whatever picture turns out to describe the early Universe, and generates the perturbations, it will contain some of the features of the current inflationary paradigm. But I don't think we can proclaim that we know that it will include *all* the ingredients – not until we have some more direct evidence.

However, one of the problems with assessing the merits of inflation is that there isn't a good alternative. Sure, there might be some ideas suggested as counter-proposals, but they tend to seem much more ad hoc, or create more problems than they solve, or have predictions that are less well developed. And the same issue applies more broadly across other "alternative" theories. The SMC has been developed over decades and the calculations are relatively straightforward (involving Gaussian perturbations, linear theory, well-understood physics, etc.) – but there's no reason to expect the same to be true for some unconventional new idea. So if an alternative is proposed, then it's not trivial to determine whether it can match the precision tests of the SMC. We just have to be a little patient until the calculations can be done accurately enough.

Despite the need to be open to alternatives, when there are clear predictions, it's still important to be skeptical if they just don't fit the data. As an example, we call the dominant form of energy in the Universe "dark energy", as though its properties were mysterious and unknown – and a huge amount of effort is going into measuring its equation of state (w as a function of redshift) with increasing precision. But the reality is that all measurements so far are consistent with this component being simply vacuum energy with $w = -1$. I've heard people say that it's much more likely to be a model with $w \neq -1$, since $w = -1$ has zero probability! But really, there's no sensible model that gives a definite prediction other than pure vacuum, and so we're left with the notion that there's just a universal constant, Λ, that gives a small (but non-zero) energy density to empty space.

Another example is dark matter. It's obvious that an alternative explanation for galaxy rotation curves *might be* that we can modify our theory of gravity. And there have been several suggestions along those lines (see e.g. [34]). However, the evidence for dark matter comes from a lot more than rotation curves of galaxies, e.g. the depth of cluster potential wells and measurements of gravitational lensing. But in fact the most robust evidence for dark matter comes from the CMB anisotropies – there is *no* model for fitting the power spectra that doesn't include a lot more CDM than baryonic matter. Here we have a choice between abandoning GR (or even Newtonian gravity) or just imagining that there's a component of matter that's not very shiny! Even without guidance from data, it seems fairly clear that the parsimonious explanation is to have a particle that's like a heavier version of the neutrino. But the skeptic should come down more heavily on the side of CDM when comparing with clustering, lensing and (particularly) CMB data.

A related issue is the evaluation of some of the small-scale puzzles associated with galaxies. It has become common to propose models ascribing these to some property of the dark matter (just strong enough to detect, without messing up the SMC predictions entirely). However, galaxy formation is a complicated business [35], involving non-linear complexity, hydrodynamics, feedback processes, etc. Since we *know* that we don't fully understand baryonic physics, we should be skeptical of assertions that some new property of dark matter has been discovered because of indications coming from non-linear scales.

Despite the examples given here, the SMC is in no sense a complete model, and there will surely be several additions eventually. Table VI lists some potential questions relating to physics *beyond* the SMC. Will one of these lead to the next breakthrough? Right now the path to progress isn't at all clear. Maybe it will turn out to be something else entirely, something unexpected and outlandish – but only if the evidence strongly supports that.

Table VI. – *Physics beyond the SMC. Which of these questions will turn out to be fruitful?*

Where did the parameters come from?
Did inflation happen?
Can we explain the value of Λ?
Why is $\Omega_c/\Omega_b \simeq 5.3$?
Are any anomalies or tensions worthy of attention?
Can we detect primordial gravitational waves?
Can we detect primordial non-Gausianity?
Are there missing ingredients to the SMC?
Will neutrino properties be measurable?
Can we predict reionisation from first principles?

10. – Conclusions

I've given an overview of the current status of the Standard Model of Cosmology, the SMC, and stressed how important it is to maintain a healthy level of skepticism when assessing the successes of this model, and in evaluating the merits of extensions to it.

So when should one be skeptical and when not? That's the trick of course! Obviously the aim is to be *right*, and it's never clear how to forecast the future. There was a time when hardly anyone believed that the solution to the Solar neutrino problem lay in the properties of neutrinos – but a small number of people got it right before the rest of us. Similarly, some people saw that ΛCDM fit most of the data while many others in cosmology were working on things like "open CDM" or "mixed dark matter". Since no practicing cosmologist believes that the current SMC will be the last word on a statistical description of the Universe, then there are surely developments that are yet to come. The goal is (somehow) to pick the 2–3 σ effects that grow to be important parts of the model – and by implication, part of this process involves ignoring most of the other claims for chinks in the SMC's armour.

There were times in the history of cosmology when it was fairly clear what directions were going to be fruitful for pursuing calculations or observations. I think it's not just that we have the benefit of hindsight – it really was the case that at one time studying hot versus cold dark matter was obviously a good idea, and at some other time developing the theory of CMB anisotropies or building experiments to probe degree-scale anisotropies were clearly worthwhile. However, right now it's not at all obvious where cosmology is going next.

This means that this is either the worst time or the best time to be a cosmologist! If you have a good idea (and it turns out to be right) you could find yourself on your own making the next major contribution to our understanding of the whole of the Cosmos.

* * *

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