The Future of Cosmology

GEORGE EFSTATHIOU

Institute of Astronomy, Madingley Road, Cambridge, CB3 OHA. England.

Summary. — This article is the written version of the closing talk presented at the conference ‘A Century of Cosmology’ held at San Servolo, Italy, in August 2007. I focus on the prospects of constraining fundamental physics from cosmological observations, using the search for gravitational waves from inflation and constraints on the equation of state of dark energy as topical examples. I argue that it is important to strike a balance between the importance of a scientific discovery against the likelihood of making the discovery in the first place. Astronomers should be wary of embarking on large observational projects with narrow and speculative scientific goals. We should maintain a diverse range of research programmes as we move into a second century of cosmology. If we do so, discoveries that will reshape fundamental physics will surely come.

1. – Introduction

It is a privilege to be invited to give the closing talk at this meeting celebrating ‘A Century of Cosmology’. I have taken the liberty of changing the title of the written version to make it shorter and snappier. Of course, I will not be able to cover all of cosmology and I am not a clairvoyant. What I will try to do is to review a small number of topics and use them as a guide to how our subject might develop. I am told that a good high court judge leaves everybody in the courtroom dissatisfied. I have borne this in mind in preparing this talk. Rather than congratulating our community on its remarkable progress, I have tried to pose some difficult questions. If readers are dissatisfied with some, but not all, of my answers, I will have succeeded with this talk.

It is a cliche, but nonetheless true, that our subject has undergone a revolution in the last decade. When I was a graduate student, astronomers were still struggling with photographic plates and had measured redshifts for only a thousand or so galaxies. A galaxy redshift of a half was regarded as exotically high – almost primordial. Extragalactic astronomy from space was in its infancy – a few bright X-ray sources and little more. The contrast with the present day is striking. We now have space satellites covering radio frequencies through to gamma rays. Observational cosmology has moved with a vengeance into survey mode, with large teams dedicated to special projects such as surveys of galaxy redshifts, distant supernovae, weak lensing, and so on. The era of projects...
geared towards ‘precision cosmology’ is upon us, with WMAP providing the archetypal example. Many of the cosmological parameters that we have been struggling to measure for decades are now constrained accurately, possibly to higher precision than many astronomers actually care about. The talks at this meeting provide ample testament to how cosmology has changed.

Cosmology has clearly been successful and we have learned a lot about our weird and wonderful Universe. But has hubris set in? Have our successes led to unachievable expectations concerning the future? (‘Irrational exuberance’ as Alan Greenspan famously referred to the dot-com boom). I will try to answer these questions by considering three topics in this article. The first two, the search for gravitational waves from inflation and constraints on the equation of state of dark energy, use cosmology to test fundamental physics. The third topic, the non-linear Universe, may or may not lead to new results of relevance to fundamental physics. If not, does this mean that a topic such as galaxy formation is a less worthy problem than understanding dark energy or inflation – a cosmological equivalent of weather forecasting? To what extent should we judge projects by their potential to test fundamental physics rather than to improve our understanding of complex non-linear phenomena? These are difficult, and for some people emotive, questions. But we must face up to them as we move into a second century of cosmology.

2. – The Search for Tensor Modes

Inflation is a compelling theoretical idea. The almost perfect agreement between the cosmic microwave background (CMB) anisotropy measurements (particularly by WMAP) and theoretical predictions based on inflation has been interpreted by many, though not all, cosmologists as evidence that inflation actually happened. A key prediction of inflationary models is the existence of a stochastic background of gravitational waves. Such a background is potentially detectable via a \( B \)-mode polarization signature in the CMB \([1, 2]\). If only we could measure this \( B \)-mode signature, so the thinking goes, we would have incontrovertible evidence for inflation that should convince the staunchest of skeptics. A detection of tensor modes would set strong constraints on the dynamics of inflationary models and would fix the energy scale of inflation via,

\[
V^{1/4} \approx 3.3 \times 10^{16} r^{1/4} \text{ GeV},
\]

where \( r \) is the tensor-scalar ratio (defined so that for \( r \approx 1 \) tensor and scalar modes contribute nearly equal amplitudes to the large angle temperature anisotropies, see \([3]\) for a precise definition).

However, the problem is not a simple one for at least three reasons:

[A] The expected signal will be small:

Firstly, any \( B \)-mode signal from inflation will be incredibly small and difficult to detect. The direct upper limit on \( B \)-modes from WMAP polarization measurements corresponds to a \( 1\sigma \) upper limit of about \( r \lesssim 1 \) \([4]\). From parameter fitting it is possible to set a \( 2\sigma \) indirect limit of \( r \lesssim 0.36 \) with plausible theoretical assumptions \([5]\). A primordial \( B \)-mode of this amplitude would produce an \emph{rms} anisotropy signal of only \( \sim 0.35 \mu K \), \emph{i.e.} about a factor of 20 times smaller than the \emph{rms} anisotropy in \( E \)-modes. This is well below the sensitivity levels achievable by WMAP. The Planck satellite \([6]\) scheduled for launch in 2008 will be considerably more sensitive than WMAP but will struggle to constrain \( B \)-mode anisotropies.
This is illustrated in Figure 1 which shows power spectra from a simulation matched to the sensitivity level of the Planck 143 GHz polarized detectors. (This simulation has been done at half Planck resolution, but with the noise level adjusted to match the sensitivity expected for the 143 GHz detectors). The simulation employs a realistic focal plane geometry, scan strategy, low frequency detector noise and Galactic mask, as described in [7]. The B-mode spectrum for \( r = 0.15 \) becomes noise dominated at \( \ell \approx 20 \). Even if all sources of systematic error are kept under control, Planck will struggle to detect anything more than a few multipoles in the B-mode even if the tensor amplitude is as high as \( r \sim 0.15 \).

Improving the sensitivity further requires large arrays of detectors. Several ground based/sub-orbital B-mode polarization experiments are either planned or in progress (examples include BICEP [9], QUIET [10], SPIDER [11] and CLOVER [12]). Groups in both the USA and Europe have considered designs for a B-mode optimised low resolution (\( \sim 30' \)) space satellite. It is certainly possible to conceive of experiments with the raw sensitivity to probe tensor-scalar ratios of \( r \lesssim 10^{-2} \). However, it is not yet clear whether systematic errors can be reduced to below this level.

[B] Polarized foregrounds will be dominant:

In the case of temperature anisotropies, WMAP and other experiments have shown conclusively that at frequencies of around 100 GHz Galactic emission over much of the sky is small compared to the primordial anisotropy signal. It is therefore possible to estimate the temperature power spectrum accurately by masking out a relatively small area of the sky around the Galactic plane. The situation in polarization is very different,
particularly for large angle B-mode anisotropies. Outside the P06 polarization mask
defined by the WMAP team, the Galactic polarized signal at 65 GHz contributes
\( \sim 0.7 \mu K \) at low multipoles [4], compared to an expected B-mode signal of \( \sim 0.05 \mu K \) if
\( r \sim 0.1 \). Thus, one must be able to subtract polarized foregrounds in \( Q \) and \( U \) maps to a
precision of order a percent to detect a primordial B-mode with \( r \sim 0.1 \) at low multipoles.

Foreground removal is not a well posed problem and it is not easy to assess the accuracy
of any given technique. Even for temperature anisotropies, no experiment has yet been
done with enough frequency channels to fit and subtract a realistic parametric model of
the foreground components. Luckily, foregrounds are unimportant for the temperature
anisotropies over much of the sky and so it doesn’t matter critically how foreground
cleaning is handled. But it certainly does matter for polarization. The ultimate limit
of polarized foreground cleaning is not yet known and this should be borne in mind
when assessing experiments with instrumental sensitivities capable of achieving \( r \sim 0.01 \)
or better. The dominance of polarized foregrounds at low multipoles suggests that an
effective strategy for detecting B-modes is to target the multipole range \( \ell \sim 50 - 100 \)
at frequencies of \( \sim 100 \) GHz in the cleanest regions of the sky. This approach requires
much higher sensitivity than Planck, but lowers the foreground contribution compared
to any primordial signal and has the added advantage that such an experiment can be
done from the ground. This is the strategy adopted by BICEP, QUIET and CLOVER.

[C] **We do not know what to expect theoretically:**

Well before the temperature anisotropies were discovered (and well before inflation
was introduced into cosmology) theorists had made well-motivated predictions of their
amplitude and spectral shape (e.g. [13], [14]). Furthermore, under the general paradigm
of an inflationary \( \Lambda \text{CDM cosmology} \) it is possible to compute the \( TE \) and \( EE \) power
spectra accurately. We therefore have a good idea of what to expect for the scalar mode
contribution to the polarization of the CMB, ahead of any experiment. But we have no
such guidance for the B-mode anisotropies from tensor modes.

Consider for, for example, the simple power-law potential (2)

\[
V(\phi) = \lambda \phi^\alpha ,
\]

advocated by Linde [15] and others. Inflation occurs for field values, \( \phi \gtrsim \alpha/\sqrt{2} \), and
the observed amplitude of the scalar fluctuations in our Universe can be reproduced for
suitably small values of the parameter \( \lambda \). (For example, the quartic potential requires
\( \lambda \sim 4 \times 10^{-14} \). In this model, the tensor-scalar ratio and scalar spectral index are given
by,

\[
r \approx \frac{4\alpha}{N} \approx \frac{\alpha}{15} , \quad n_s \approx 1 - \frac{2 + \alpha}{2N} \approx 1 - \frac{2 + \alpha}{120} ,
\]

where \( N \) (assumed to be \( \approx 60 \) [16]) is the number of inflationary e-foldings between the
time that CMB scales ‘crossed’ the Hubble radius and the end of inflation. The quartic
potential is marginally excluded (\( \sim 3\sigma \)) by the WMAP constraints on \( n_s \) and \( r \), but a
quadratic potential provides an acceptable fit [8]. If the quadratic potential is correct, we
expect to find a significant spectral tilt \( n_s \sim 0.97 \) and a high tensor-scalar ratio \( r \sim 0.13 \).

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(1) To \( \ell(\ell + 1)C_\ell/(2\pi)^{3/2} \).

(2) We use natural units, \( c = \hbar = 1 \). The reduced Planck mass is \( M_{pl} = (8\pi G)^{-1/2} = 2.44 \times 10^{18} \) GeV and will be set to unity unless explicitly stated otherwise.
The power-law models are particular examples of so-called ‘high-field’ inflation models (since the inflaton changes by $\Delta \phi \sim (N_\alpha)^{1/2}$ in Planck units during the last $N$ e-folds of inflation). In fact, to produce a detectable tensor component in any foreseeable experiment, inflation must necessarily involve large field variations $\Delta \phi \gtrsim 1$ (sometimes called the ‘Lyth bound’ [17], [18]).

Steinhardt and collaborators [19] [20] have argued that high tensor amplitudes must be expected unless the inflationary potential is unnaturally finely tuned. The argument goes roughly as follows: fluctuations on CMB scales were frozen $N \sim 60$ e-folds from the end of inflation when the field was rolling slowly, $\epsilon = 2(\psi'/\psi)^2 \ll 1$. Now assume that inflation ends when slow-roll is violated ($\epsilon = 1$). This sets ‘natural’ values for the gradients of the inflaton potential,

$$\frac{\psi''}{\psi} \sim \frac{\psi'''}{\psi' \sim \frac{1}{N},$$

leading to the expectations,

$$r \sim \frac{14}{N}, \quad n_s \sim 1 - \frac{3}{N},$$

similar to the predictions of the power law model (3).

How seriously should we take this argument? My view is that we should be skeptical of such simplistic arguments in the absence of a fundamental theory of inflation. For example, in the last few years there has been a lot of interest in the idea of brane inflation (see e.g. [21] for a review). In one particular scenario, inflation arises as a D3 brane moves towards an anti-D3 brane (or stack of anti-branes) sitting at the bottom of a ‘throat’ in the flux compactified bulk in Type IIB string theory [22]. There are many variants of this idea: the bulk may have many throats (with the standard model brane lying in a different throat to the ‘inflationary’ branes); inflation may occur by the usual slow-roll mechanism, or via the ‘UV’ Dirac-Born-Infeld (DBI) mechanism, in which the velocity of the inflation branes is fixed by the limiting speed in the warped geometry. It may be possible to realise ‘IR’ DBI inflation in which branes roll out of a throat into the bulk. The phenomenology of these types of models has been discussed recently in [23], [24], to which we refer the interested reader. These models must satisfy a geometrical constraint on the volume of the warped throat [25] which, in turn, sets a limit on the inflaton field variation during inflation of

$$\Delta \phi < \left(\frac{4}{N_C}\right)^{1/2}.$$  

Here, $N_C$ is the background number of charges which is usually $N_c \gg 1$ in this type of model. Applying the Lyth bound, the constraint (6) restricts the tensor-scalar ratio to be small. In [23] some specific parameter combinations were found which gave $r$ as high as $r \sim 4 \times 10^{-3}$, but generically $r$ is expected to be many orders of magnitude smaller and hence unobservable. For the ‘IR’ DBI models, the analysis of [24] suggests $r \lesssim 10^{-13}$. Kallosh and Linde [26] have argued for an even more stringent bound of $r \lesssim 10^{-24}$ on string inflation models by imposing the requirement that the Hubble constant during inflation is smaller than the gravitino mass, which they assume is in the TeV range. Possible ways of evading the geometrical and gravitino bounds are discussed in [23] and [26], but the key point is that at present there are no compelling theoretical predictions.
for the amplitude of the tensor component. Phenomenological models can be constructed which lead to a high tensor amplitude, while brane inflation models can be constructed with unobservably small tensor amplitudes.

Given this theoretical uncertainty, is it worthwhile building experiments that cover only a small range in $r$? My view on this is as follows (see [27] for a more detailed discussion). It is feasible to design experiments (at relatively low cost) to probe a tensor-scalar ratio as low as $r \sim 10^{-2}$. By probing the multipole range $\ell \sim 50 - 100$, it should be possible to monitor the accuracy of foreground removal by demonstrating the reproducibility of any putative detection in different clean regions of the sky, and as a function of frequency. A failure to detect tensor modes at this level would rule out ‘chaotic’ inflationary models such as (2) and other examples of ‘high-field’ inflation. This is a well-motivated and achievable goal.

But if we fail to detect tensor modes at the level of $10^{-2}$, what then? Do we continue the search with more complicated experiments (perhaps a new CMB satellite). An experiment to probe $r \sim 10^{-4}$ would be formidably difficult, yet such an experiment would improve the constraints on the energy scale of inflation by a small factor of only $\sim 3$ (equation 1). In my view, the case for such an experiment is weak unless there are strong theoretical reasons to favour this narrow energy range.

So, my strategy would be to pursue an aggressive experimental campaign to achieve a limit $r \sim 10^{-2}$, since this seems feasible and would rule out an important class of inflationary models. But without strong theoretical motivation, I would not blindly continue the search to still lower amplitudes. In this case, it may well be more profitable to test models of inflation by designing experiments to detect, for example, non-Gaussianity or signatures of cosmic strings.

In the last few years we have become used to a wealth of new high precision information from CMB experiments. The temperature anisotropies have been relatively easy to analyse and they have had important consequences for many areas of astrophysics and cosmology, e.g. theories of the early Universe, models of structure formation, primordial nucleosynthesis, neutrino masses, ages of the oldest stars and so on. Experiments designed to probe tensor modes are very different, as summarized in the following table. They are technically demanding, will be difficult to analyse, and may well lead to a null result which, though of interest to inflationary model builders, would likely have little impact on the wider astrophysics community.

| Temperature Anisotropies | Large-Scale B-mode Anisotropies |
|--------------------------|---------------------------------|
| Broad science case: early and late universe | Narrow science case: test of inflation |
| Know what to aim for | No idea of what to expect |
| High signal | Small signal |
| Foregrounds unimportant | Foregrounds dominant |

3. – Dynamical Dark Energy or Cosmological Constant?

The discovery of an accelerating Universe [28], [29] has led to an explosion of papers on the phenomenology of ‘dark energy’ (for a recent review see e.g. [30]). On the observational side a large number of ambitious projects have been proposed to constrain the equation of state of dark energy and its possible evolution (summarized concisely in the Report of the Dark Energy Task Force, [31]). However, phenomenology should not be confused with fundamental physics. The fact that it is easy to construct a bewildering
variety of models dynamical dark energy does not mean that any of them will turn out to be right. In my view, much of the astronomical community has got the problem of dark energy out of perspective. There is no sound theoretical basis for dynamical dark energy, whereas we are beginning to see an explanation for a small cosmological constant emerging from string theory. Furthermore, observational data favour a cosmological constant over dynamical dark energy. In this Section I present three arguments for why the cosmological constant should be given higher weight as a candidate for the dark energy than phenomenological dynamical models.

3.1. Occam’s Razor. – For a scalar field $\phi$ rolling in a potential $V(\phi)$, the condition that dark energy begins to dominate at the present time requires the well-known fine-tuning,

$$V(\phi_0) \sim 3H_0^2 \sim 1.2 \times 10^{-120} \sim (10^{-3} \text{eV})^4.$$  \hfill (7)

This fine-tuning is imposed by fiat in all models of dynamical dark energy. For generic potentials, if the scalar field is to show interesting dynamical behaviour, it must be nearly massless \[32\]

$$m_\phi = \left(\frac{V''}{2}\right)^{1/2} \ll H_0 \sim (10^{-33} \text{eV}).$$  \hfill (8)

Now, the observational constraints on the equation of state parameter $w = p/\rho$ are already closing in on the cosmological constant value $w = -1$. For example, Spergel et al. \[8\] combine WMAP observations of the cosmic microwave anisotropies with the Supernova Legacy Survey \[33\] and find $w = -0.967^{+0.073}_{-0.072}$. Similar limits on $w$ have been found by other authors using a variety of cosmological data sets and theoretical assumptions (e.g. \[34\] \[35\] \[36\] \[37\]). If the dark energy is a scalar field, then the field must be moving slowly $\frac{1}{2}\dot{\phi}^2/V \ll 1$. The equation of motion of the scalar field then imposes a constraint on the derivative of the potential,

$$\left|\frac{V'}{V}\right| \approx \sqrt{3} \left(1 + \frac{w_{\phi_0}}{\Omega_{\phi_0}}\right)^{1/2}.$$  \hfill (9)

A value of $w_{\phi_0}$ close to $-1$ therefore requires yet another fine-tuning in addition to those of (7) and (8), namely that the derivative of the potential is small in Planck units. Consider, for example, the archetypal ‘tracker’ potential \[38\]

$$V(\phi) = M^{4+\alpha}\phi^{-\alpha}.$$  \hfill (10)

The attractor solutions for this potential, subject to the constraint $\Omega_{\phi_0} = 0.72$, give $w_{\phi_0} = -0.411, -0.495, -0.643, -0.768, -0.864$ for $\alpha = 6, 4, 2, 1$ and 0.5. If the experimental constraints continue to tighten around $w = -1$, the fine tuning required by (9) becomes even more acute. For example, if we could constrain $w$ to lie in the range $-1 \lesssim w \lesssim -0.97$, the attractor solutions of (10) would require $\alpha < 0.1$, which most readers must surely find contrived.

Occam’s razor suggests that a cosmological constant is a more economical explanation of the observational data than a dynamical model carefully chosen to satisfy the fine
tunings of (7)- (9). Some authors have attempted to quantify Occam’s razor by computing Bayesian Evidence or related information criteria (for example [39], [34], [40], [41], [42]). However, in the cosmological context Bayesian Evidence is difficult to compute and sensitive to assumptions concerning prior distributions of parameters (3). It is therefore difficult to get a precise measure of how much the data favour model ‘A’ over model ‘B’, but evidently the more the data forces us towards $w = -1$, the better the case for a cosmological constant compared to more complex models.

It is also worth noting that if, after a lot of hard work, the observations tighten around $w = -1$ to high precision (say to an accuracy of a percent or so), we will not be able to rule out dynamical dark energy. Such a constraint at low redshift ($z \sim 0.5$, depending on the nature of the cosmological data, e.g. supernovae, lensing, baryon oscillations) will constrain the potential to be very nearly flat for field values $\phi \sim \phi_0$ (equation 9), but it is easy to construct dynamical models in which the potential changes shape outside the redshift window probed by the data leading to an abrupt change in $w_\phi$ [44]. A double exponential potential,

$$V(\phi) = M_1^4 e^{-\lambda_1 \phi} + M_2^4 e^{-\lambda_2 \phi},$$

with $\lambda_1 \gg \lambda_2$, $\lambda_2 \ll 1$ provides an example [45]. With a double exponential it is possible to construct models which make a transition from the attractor solution $\Omega_\phi = 3(1+w_B)/\lambda_1^2$, $w_\phi = w_B$, at high redshift (where $w_B$ is equation of state of the background fluid) to the late time attractor at low redshift with $w_\phi = -1 + \lambda_2^2/3$, $\Omega_\phi = 1$. The parameters of this type of model can easily be adjusted to give a dynamically significant dark energy density at high redshift while mimicking a cosmological constant to arbitrary precision at low redshift. No matter how precise the observations become, we will always be able to construct models of this sort [44]. But, of course, without strong theoretical motivation, such models are not likely to be taken seriously. I mention this point here to emphasize that one cannot take a purely empirical approach to the problem of dark energy. Dark energy surveys must be assessed within a theoretical (and not purely phenomenological) framework.

3’2. The String Landscape. – A fundamental theory of the cosmological constant must involve quantum gravity. Quantum mechanics is required to endow the vacuum with an energy density and gravity is required if we are to ‘feel’ the effects of this vacuum. At present, string theory is our best bet for a consistent quantum theory of gravity, so it is reasonable to ask what string theory has to say about dark energy. There has been substantial progress on this question in the last few years (see, for example, the reviews by Polchinski [46] and Bousso [47], [48]). As is well known, string theory requires either 9 + 1 or 10 + 1 spacetime dimensions. Six or seven spatial dimensions must therefore be compactified so that they remain hidden from us. It now seems that a huge number, perhaps $10^{500}$ or more, metastable vacua may exist depending on the choice of compact manifold and different values of magnetic fluxes wrapped over different homology cycles [49] [50]. These vacuum solutions form the so-called ‘landscape’ of string theory [51].

The existence of a landscape of vacua raises the possibility of an anthropic explanation for the cosmological constant, as first advocated by Weinberg [52]. To some people, anthropic reasoning is abhorrent and represents a retreat from conventional science. I

(3) For inference problems in which Bayesian Evidence is more easily interpreted see [43].
disagree with this view. An anthropic explanation for \( \Lambda \) requires a very special theoretical framework, placing restrictive conditions on fundamental theory. In particular [48]:

- the value of \( \Lambda \) must ‘scan’, either continuously or with sufficiently close spacing to account for the small value of \( 10^{-120} \);
- vacua with small values of \( \Lambda \) must be realised (i.e. populated);
- vacua must exist that are consistent with the physics of the standard model;
- they should admit a period of inflation to produce a big Universe containing matter, radiation and the fluctuations necessary to form non-linear structure by the present day.

These are non-trivial conditions. Although we do not yet know the details of how these conditions might be satisfied\(^{(4)}\), the landscape of string vacua at least offers the possibility of a theoretical framework within which they may be met. String theory was not designed to solve the cosmological constant problem, yet it may contain all of the ingredients necessary to realize Weinberg’s anthropic prediction of \( \Lambda \). If this is correct, \( \Lambda \) must be much more strongly favoured than dynamical dark energy.

3.3. Back to square one. – The smallness of the cosmological constant is a fundamental problem in theoretical physics. We must therefore understand what we are doing when we construct phenomenological models of dark energy. Why should we write down a potential such as (10) rather than the potential

\[
V(\phi) = M^{4+\alpha} \phi^{-\alpha} + V_0 \tag{12}
\]

In other words, why should we assume that the cosmological constant is zero in writing down a potential for dark energy? Despite many years of effort no mechanism has been found that can enforce \( V_0 \) to be zero [56]. In fact there are good theoretical reasons to suggest that no such mechanism exists [48]. The assumption that \( V_0 \) is zero is therefore not benign. It raises the fundamental question of why the vacuum energy and ‘bare’ cosmological constant cancel exactly, to which there is no known answer.

4. – The Non-Linear Universe

We have learnt a lot about fundamental physics by studying the non-linear Universe at low redshifts. Highlights include the discovery of dark matter in galaxies and clusters, evidence for the hierarchical assembly of galaxies (as expected in the cold dark matter model) and the discovery of the accelerating Universe. Will this link between fundamental physics and astronomy remain as strong in the future? Perhaps not. Unless there are some new surprises, we probably already know enough about the initial fluctuations and cosmological parameters to predict the low redshift Universe. Understanding the formation of the first stars, or the formation and evolution of galaxies, then becomes an exercise in complex non-linear physics, of no more relevance to fundamental physics than weather forecasting.

On the other hand, there is plenty of scope for surprises, for example:

\(^{(4)}\) In addition, there is still no compelling solution of the ‘measure problem’ that bedevils the interpretation of eternal inflation and the string landscape (see e.g. [53], [54], [55]).
• evidence of topological defects, such as cosmic superstrings [57];

• clues to the nature of the dark matter *e.g.* evidence of dark matter annihilation or direct laboratory detection;

• firm evidence for dynamical dark energy;

• evidence of non-minimal coupling between dark matter and dark energy;

• non-Gaussianities in the primordial fluctuations, perhaps indicative of brane inflation [58];

• features, such as a large spectral index variation or a sharp spike, in the fluctuation spectrum;

• evidence of modifications to General Relativity, perhaps associated with higher dimensional physics [59];

• observational signatures of other universes [60].

Any one of the above would constitute an important discovery, and some might well be considered revolutionary. But there is no guarantee that we will discover any of these things. Suppose that several years from now the WMAP ‘concordance’ cosmology still holds up, what then? The non-linear Universe may then be of little interest to fundamental physicists, but it nevertheless poses problems that are interesting in their own right. Finding extra-solar planets, understanding how they form and whether they harbour life are interesting problems, though they will not tell us anything new about fundamental physics. Similarly it is important to develop an understanding of the rich phenomena that we observe in the Universe such as, supernovae, supermassive black holes, quasars, galaxies, gamma-ray bursts. Astronomers should offer no apologies about studying Nature in all her complex glory.

5. – Prognosis

5.1. The Future of Precision Cosmology. – We have seen very remarkable progress in the last few years in constraining theoretical models and determining cosmological parameters using a diverse set of astronomical surveys. There have been some problems along the way, for example, the apparent high optical depth for reionization in the first year WMAP data [61] and tension between different supernovae and galaxy redshift data sets [62], [63], but these have not been too serious. We have been lucky. The next generation of large surveys will be aimed at much more difficult problems, such as constraining \( w \), absolute neutrino masses, \( B \)-mode anisotropies etc and there is no guarantee that systematic errors can be controlled to the required level of precision. Progress is therefore likely to be slower than we have become used to, with more false results. I would advocate the following:

(i) new surveys should be designed with as many internal redundancy checks as possible;
(ii) perform complementary surveys since consistency between different types of astrophysical data provides powerful tests of systematics;
(iii) when making the case for ambitious new experiments do not succumb to über-forecasting – include realistic errors, foregrounds, systematics *etc.*
5.2. *The Future of CMB Experiments.* – Post Planck, most CMB experiments are targeting either high resolution observations of secondary anisotropies (the Sunyaev-Zeldovich effect in particular) or low resolution observations of B-mode anisotropies. However, the science case for B-mode experiments is highly specialised and so we need to think carefully about their likely impact. As discussed in Section 2, sub-orbital and ground based experiments aimed at achieving tensor-scalar ratios of $\sim 10^{-2}$ are well motivated. Detection of tensor modes above this level would constitute a major discovery, providing firm evidence that inflation took place and fixing the energy scale of inflation. Failure to detect tensor modes at the $\sim 10^{-2}$ level would rule out an important class of models that has played an influential role in the development of inflationary cosmology. But we need to think carefully before pressing the case for a new CMB satellite to detect a tensor-scalar ratio in the range $10^{-2} \lesssim r \lesssim 10^{-4}$. Bearing in mind that a low resolution polarization satellite is a single goal mission, that foreground polarization is dominant, and systematics need to be controlled to an unprecedented level, it only makes sense to target this narrow range in $r$ if there are compelling theoretical reasons to do so. Otherwise, a failure to detect a tensor mode would have little scientific impact. The science cases for other types of CMB experiments are difficult to judge at this stage. Hints of non-Gaussianity, or cosmic strings, could motivate a new generation of CMB experiments with high potential for scientific discovery.

5.3. *Unravelling The Nature of Dark Energy.* – In Section 3, I presented arguments for why the cosmological constant should be strongly favoured over more exotic dark energy candidates such as quintessence. Does this mean that there is no point in supporting new dark energy surveys [64]? Of course not! The arguments presented in Section 3 are not intended to dissuade people from ambitious programmes to probe the nature of dark energy. But they are intended to influence the nature of the programmes themselves. If you are at all persuaded by the arguments of Section 3, then you should expect that future experiments will simply strengthen the case for a cosmological constant. By all means design experiments to test for dynamical dark energy, but expect failure! Dark energy surveys should therefore be designed to have a broad *astrophysical* science case, so that if we find nothing fundamentally new about dark energy we will at least learn something interesting about astrophysics. Simon White, using quite different arguments based on the sociological implications of dark energy surveys, has reached similar conclusions. Surveys narrowly focused on dark energy will have little scientific impact on both astronomy and theoretical physics if they merely tighten the limits around $w = -1$. They will have even less impact if systematic errors prevent them from achieving their stated goals.

5.4. *The Non-Linear Universe.* – We are extremely fortunate in having a large array of expensive facilities with which to study the Universe. We are even more fortunate to have major new telescopes and observatories on the horizon, including ALMA, Pan-STARRS, Herschel, JWST, LSST, LISA, ELTs and so on. This diverse range of facilities guarantees that astronomy will remain a vibrant subject for many years to come. In Section 4, I listed ways in which new physics could influence what we see today. Most of these effects are so speculative that the likelihood of observing any one of them is very small. In judging scientific projects, one must strike a balance between the importance of a scientific discovery against the likelihood of making the discovery in the first place. In my view, we should continue using our generous array of facilities, together with theoretical insight and increasingly powerful computers, to build up a picture of how
complex objects in the Universe formed and evolved. Astronomy should not succumb to fundamentalism – understanding the complexity of our Universe is an important problem in its own right. We therefore need to maintain a diverse range of research programmes, rather than assigning resources to projects with narrow and highly speculative science goals. If we maintain diversity, surprises of relevance to fundamental physics will surely come.

The story of astronomy is one of unexpected discovery after unexpected discovery. This is why our subject is so interesting. It is extremely unlikely that the ‘concordance’ ΛCDM model is the last word in cosmology. There will be surprises in store and they will have revolutionary implications for fundamental physics. Is my confidence in this an example of irrational exuberance? Perhaps, but history is definitely on my side.

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