PROCEEDINGS OF SCIENCE

CMB and Physics of the Early Universe
Thoughts and Reflections

Malcolm Longair
Cavendish Laboratory, J.J. Thomson Avenue, Cambridge CB3 0HE
E-mail: msl@mrao.cam.ac.uk

This paper is a summary and review of the international conference *The Cosmic Microwave Background and the Physics of the Early Universe*. The conference was dominated by analyses of the three-year WMAP data and the forthcoming launch of the Planck satellite in early 2008. The excellent papers at the meeting explored all aspects of CMB studies and the related physics of the early Universe. The conference concluded with a survey of future missions and technologies needed to detect the B-mode polarisation signature of primordial gravitational waves. This paper summarises some of the highlights of the presentations made at the conference.

CMB and Physics of the Early Universe
20-22 April 2006
Ischia, Italy

 http://pos.sissa.it/

PoS(CMB2006)025

Contents

1. Introduction **2**

2. The Three-year WMAP results **3**

3. The Key Role of Planck **4**

4. The Cosmological Problems **6**

5. The Reionisation Problem **7**

6. The Physics of the Early Universe **7**

7. The Really Tough Cosmological Problems **8**

8. Future Missions **9**

9. Conclusions **9**

1. Introduction

This excellent meeting has been dominated by two topics – the remarkable results of the Wilkinson Microwave Anisotropy Probe (WMAP) and the challenges it raises for the ESA Planck mission, which is now less than two years away from launch. The first thing to proclaim out loud is:

What terrific missions!! Spread the good news!!

When the science is as good as this and the impact upon our understanding of cosmology and of our place in the Universe have been so profoundly changed by the WMAP satellite, it is a matter for real rejoicing. NASA has learned from the experience of the Hubble Space Telescope how the general public can feel involved in such astronomical and cosmological endeavours. Cosmology belongs to everyone and not just the experts. After all, they are stakeholders in these projects though the support provided by their national and international agencies. At a time when pure science budgets are under very severe pressure, we need the support of the general interested public more than ever.

It is very timely to assess what WMAP is telling us. The one clear message from this meeting is the singular importance of the Planck mission in the light of the WMAP results. In my view, the WMAP results have simply enhanced the importance of the Planck project.

2. The Three-year WMAP results

After the exhilaration of the publication of the one-year WMAP data, the three-year results have been awaited with great anticipation. It has been a long wait, but I am sympathetic to the need of the scientists involved to understand the observational data in as much detail as possible before releasing them to the many theorists who are just waiting to exploit any tiny untoward feature of the published results. The wait has been worth it and, whilst recognising that it is good to have a deadline for publication of the results, I applaud the care which has been taken not to rush into print before the data are well understood.

It was very clear from *Nolta's* presentation of the WMAP results, just what a major challenge it has been to understand all the systematics in the data. This is particularly true of the polarisation data which have turned out to be such a key feature of the WMAP results. The polarisation data are quite spectacular in the sense that they provide independent evidence that the now standard picture of primordial scale-free adiabatic perturbations is an excellent model for the evolution of the density perturbation spectrum – it need not have turned out that way. It is impressive that a new data processing pipeline has been used to analyse the three-year data-sets and some of the apparent anomalies in the one-year data-sets have been removed. I formed the impression, however, that there is still a great deal to be understood about the existing data. Nolta stated that the next release will be of the five-year data-set and we all look forward to these results with the greatest interest.

Taken at face value, there was some surprise that the cosmological parameter estimations were somewhat different in the analysis of the three-year as compared with the one-year data. Many cosmologists were taken aback, for example, by the change in the best estimates of the epoch of reheating and reionisation of the intergalactic gas. In my view, all this reflected was that the meaning of one- and two-sigma uncertainties had been forgotten. As an example, I reproduce in Figure 1 the cosmological parameter estimates for the one-year WMAP data combined with the galaxy power spectrum from the SDSS project as presented in the paper by Tegmark and his colleagues (Tegmark et al. 2004).

It is important to look at the horizontal dashed lines which show the one- and two-sigma limits to the accuracy with which the various parameters are determined. The yellow areas show the error estimates using the WMAP data alone and the red areas the improvement in these when the galaxy power spectra from the SDSS are included in the analysis. As the authors show, generally, the inclusion of the SDSS data improves the accuracy of the determinations by about a factor of two. To take only one example, one of the key surprises in the three-year WMAP data was the lowering of the best estimate of the epoch at which the intergalactic gas was reionised. Inspection of the second diagram down in the last column of Figure 1 (labelled $z_{\rm ion}$) shows that a reionisation epoch of 10 was well with the typical uncertainties of the analysis of the one-year data and so there is clearly no conflict between the one-year and three-year data sets.

On the other hand, it is important to understand why the amplitude of the cross-polarisation signal decreased significantly between the two data releases. I am sure this is buried in the new data analysis procedures used to extract the polarisation signal. It also raises the issue of how stable the present analyses will be to further improvements of the data analysis procedrues. I am very sympathetic to the WMAP team in the really great methodological problems of extracting wholly reliable results from these very large data sets.

PoS(CMB2006)025

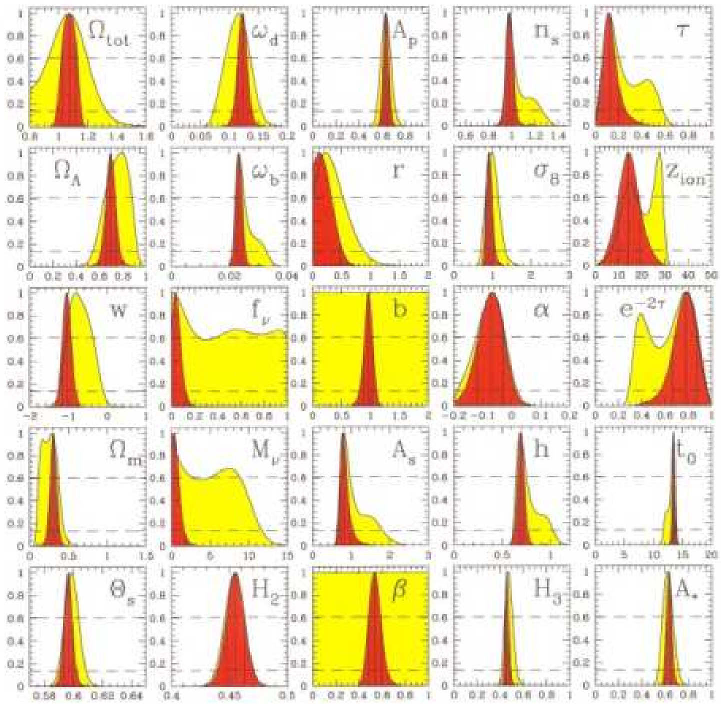

Figure 1: The results of the analysis of the one-year WMAP data (yellow areas) and the combined WMAP and SDSS data (red areas) by Tegmark and his colleagues in order to estimate a wide range of cosmological parameters (Tegmark et al. 2004). The horizontal dashed lines show the one- and two-sigma uncertainties in the parameter estimates.

There are still many issues to think about. For example, one of the obvious anisotropies of the whole-sky maps, pointed out to me by Lyman Page, is the apparent large-scale anisotropy between the northern and southern galactic hemispheres. There is clearly a large region of negative flux in the south as compared with the north. Attempts to explain this in terms of large scale vorticity in the Universe, such as that provided by the Bianchi VIIh template for the WMAP sky, resulted in a best fit with $\Omega_0 = 0.5$, but this value of Ω_0 is inconsistent with the best independent estimates.

So far as the estimation of cosmological parameters is concerned, it was interesting to learn that the new methods of parameter estimation described by *Lasenby* and *Hobson* provided "evidence" that all the data can still be accommodated by a six parameter family. The value of their innovative procedures is that, although we are at liberty to introduce additional parameters beyond the six concordance values, there is no "evidence" that these are necessary. The reader should be aware that I am using the term "evidence" in parentheses since the word has to be treated in the technical sense which they discuss in their presentation. One way of thinking of their approach is as a formalisation of Occam's razor.

3. The Key Role of Planck

It is impossible to overstate the importance of the WMAP observations of the Cosmic Microwave Background Radiation since they have become one of the cornerstones for the whole of modern geometrical and astrophysical cosmology. Planck has many key roles to play in extending our understanding of cosmology and the determination of cosmological parameters. For me, one of the most important aspects of the mission is that it is a totally independent experiment and the data reduction will be carried out entirely independently of the results of the WMAP project. My instinct is that the WMAP data are genuinely 'variance-limited' out to wavenumbers $l \sim 500$ and this should mean that we cannot do much better than WMAP for these wavenumbers. However, the security of the WMAP results depends crucially upon understanding the *systematics* rather than the *noise* in the data products and it is wonderful that we will have a completely new independent image of the sky to study the consistencies and anomalies.

Over and over again, I come back to worrying about the problem of the foregrounds, both unpolarised and polarised, and wonder how well these are really understood. *Burigana's* analysis of the radio background emission illustrated very clearly the magnitude of the problem on the basis of ground-based radio intensity and polariation maps of the sky. While the large scale components of the polarised emission can be dealt with, there is concern about the flctuations which are also known to be present in the radio data. I was recently strongly impressed by the new technique of Rotation Measure Synthesis, which has been developed at the Westerbork Observatory and which is already producing maps of the small scale polarisation structure in the radio background emission (Brentjens and de Bruyn 2005).

The background intensity and polarisation due to interstellar dust grains was very nicely surveyed by *Pontieu*. He raised the important issue of the high percentage polarisations observed in the 353 GHz waveband by the Archeops experiment. According to these results, high percentage polarisations due to diffuse dust emission can extend to high galactic latitudes. These are particularly important observations in that they will strongly influence the ability of polarisation studies to detect the elusive B-modes, to which we will return below.

Equally intriguing is the problem of the *anomalous dust component*. According to *Davies*, this emission, which has greatest intensity in the most favorable wavebands for observations of the fluctuations in the Cosmic Microwave Background, seems to follow rather closely the 25 μm dust maps observed in the IRAS survey. The anomalous component has also been observed in compact dust clouds and has been attributed to the emission of spinning dust grains. *Verstraete* presented a very good discussion of the physics of the rotation of small dust grains and how rotational transitions between their high rotational states could account for the observed continuum emission. This is yet one more foreground component which needs to be stripped out of the intensity and polarisation maps.

There is no question but that one of the most important contributions of the Planck satellite will be to make optimum use of the nine frequency bands of the High and Low Frequency Instruments to make really thorough studies of the magnitude of the foreground problem. We should then have a very much better understanding of whether or not any of the apparent anomalies in the WMAP results are due to foreground problems. Of course, we should not ignore the fact that the determinations of the foregrounds themselves are matters of real astrophysical and cosmological interest and importance.

Next, there is astrophysical cosmology at large wavenumbers $l \geq 1000$ which is another area in which Planck will make further unique contributions. *Silk* emphasised the new types of astrophysi-

PoS(CMB2006)025

cal phenomena which are expected to come in on these small angular scales. Pinning down exactly the role of Silk damping and of the Sunyaev-Zeldovich effect at large values of l are important new areas of study and provide new ways of tacking some of the difficult problems of understanding the cosmological dark ages. But, as he suggested, perhaps we should be more ambitious. I liked, for example, his suggestion that we might look for clues about the topological structure of the Universe in the vast data sets which are now available.

Above all, there is the intriguing question of how well the ‘standard’ six-parameter concordance model can cope with fitting in detail the power spectra and polarisation properties of the first six maxima in the CMB power spectrum. The success of six-parameter concordance model makes it very testable indeed and clearly subject to disproof if the model cannot account for the details of maxima 4, 5 and 6. I use the testability of the theory and its falsification in the very best Popperian sense. For me, this is one of the key areas in which Planck may force us to introduce new astrophysical and cosmological ideas to account for what should quite superb data-sets.

I am sure it has not escaped anyone’s notice that it has taken a really huge effort on the part of the WMAP team to analyse the WMAP data and that it has taken some time to establish convincing results. The Planck data sets will be orders of magnitude greater and so the challenge is correspondingly greater. The data analysis centres for Planck have a real challenge on their hands.

4. The Cosmological Problems

The reviews by *Balbi* and *Silk* can be thoroughly recommended as excellent summaries of the current state of play on the determination of global cosmological parameters from the Cosmic Microwave Background Radiation and other cosmic probes. The success of the concordance six-parameter family was reaffirmed, but with small but significant deviations from the standard Harrison-Zeldovich power-spectrum for which $n = 1$. The best fit values seem to be $n \approx 0.96 - 0.97$ which leaves room for a significant contribution from primordial gravitational waves.

There remain, however, the very deep problems associated with the concordance values. Namely,

- Why is the dark energy density parameter between about $10^{60} - 10^{120}$ times smaller than the best theoretical estimates?

- Why are Ω_Λ and $\Omega_{\rm m}$ of the same order at the present epoch when they evolve quite differently with scale factor?

There really are very few compelling ideas about how these problems are to be solved. There seems to be a continuing lack of clues from particle physics - indeed, my impression is that the particle physicists are looking to the cosmological experiments to provide further insights.

The problem of studying the physics of Ω_Λ is that its effects only come into play at relatively small redshifts, $z \leq 0.5$, as was emphasised by *Baccigalupi*. Since we know so little about Ω_Λ, it would be interesting to analyse separately the data in large regions of the sky and find out if there are any correlations with the distribution of galaxies which is now available over the redshift range at which the dark energy term dominates the dynamics of the Universe. Since the statistical errors change quite slowly with the size of the data-sets, we could ask whether the same values of Ω_Λ and all the other parameters are found in different regions of the Universe. It really is a major

PoS(CMB2006)025

challenge to devise large-scale tests throughout the local Universe at z < 0.5 to find out if there are any detectable effects of Ω_Λ which might provide clues to its physical nature.

I liked *Baccigalupi's* approach to the study of the equation of state of the dark energy by looking at gravitational weak lensing effects. His objective was to study the dark energy just at the point where the Ω_Λ term starts to dominate and cause the present acceleration of the Universe. The attraction of this approach is that the lensed background objects should typically be located at $z \sim 1$ to result in significant lensing effects and so surveys to look for these effects are within the capabilities of current technology.

5. The Reionisation Problem

Puget's summary of current issues concerning the epoch reionisation was splendid, emphasising just how important the understanding of the end of the dark ages is for astrophysical cosmology and equally how difficult it is to pin down the exact sequence of events which must have taken place from existing observations.

An important point which he emphasised is the very strong sensitivity of the magnitude of the re-ionisation problem to redshift. When the reionisation redshift was believed to be as large as $z \sim 30$, there were real problems in understanding what the source of heating might be. The reason for this problem is that the cooling rate of primordial ionised plasma depends very strongly upon redshift, typically as $(1+z)^6$. On the other hand, if the heating and ionisation took place at much later epochs, say $z \sim 10$, there would be no need for exotic sources of heating or ionisation such as a population of hypothetical population III stars. If the reionisation epoch is close to $z \approx 10$, the heating and ionisation could well be associated with the first generation of stars in galaxies. Puget's analyses showed just how difficult it will be to distinguish different ionisation histories from observations of the Cosmic Microwave Radiation alone, but a imoportant point is that the optical depth τ for Thomson scattering out to the recombination epoch is one of the key global constraints on the thermal history of the Universe through the epoch of re-ionisation.

This is a tough problem and my hunch is that the determination of the sequence of events which led to the reheating and reionisation of the intergalactic gas will probably be determined by projects such as LOFAR which aims to detect directly the highly redshifted 21-cm line of neutral hydrogen through the reionisation epoch. The proponents of LOFAR and similar very low frequency radio arrays are growingly optimistic that they will detect the epoch of reheating by this technique. The discovery of global signatures of the reionisation epoch would then be followed up by detailed studies of the reionisation epoch by the Square Kilometre Aray (SKA).

6. The Physics of the Early Universe

We heard some outstanding surveys of the problems of relating laboratory physics to the physics of the early Universe. Let me first comment on *Trodden's* excellent and very helpful review about what particle physics can contribute to studies of the early Universe. We are in the curious situation that the cosmologists feel that there is a lot to be learned from the particle physicists, while at the same time the particle physicists are looking to the observational cosmologists to provide constraints on physics beyond the standard model of particle physics.

PoS(CMB2006)025

I liked very much his approach of looking very hard at physics in the TeV energy range which will become accessible with the Large Hadron Collider and the next generation International Linear Collider. He put strong emphasis upon generic arguments which mean that TeV physics will undoubtedly impact our understanding of the Universe back to epochs $t \sim 10^{-8}$ seconds. He emphasisied the difficulty of making more specific predictions of quantities like the mass of the lightest supersymmetric particle. But, to paraphrase the key points of his argument, since we now know that physics beyond the standard model of particles physics is necessary, almost any model involves new particles at the TeV scale, which are related to the particles of the standard model through new symmetries. To avoid proton decay and violating precision tests of electro-weak theory, an extra new symmetry is required. It is this new symmetry which leads to the expectation of some new stable particle at the weak energy scale. Particularly intriguing is the fact that the weak interaction cross-section is of exactly the right order of magnitude at the decoupling mass-scale to provide sufficient mass density in WIMPs to account for the dark matter. These are persuasive arguments of the communality of interests of the particle physicists and cosmologists - it is an ideal case where they can mutually supportive on the basis of common physical problems.

Equally striking was *Sadoulet's* brilliant survey of the experiments to detect directly dark matter particles in deep mine experiments. The innovative genius involved in these experiments is truly outstanding. The situation reminds me of the history of the gravitational wave experiments. 10-15 years ago, the gravitational wave experimenters predicted it would take this sort of time to reach the sensitivities at which there would be a reasonable probability of detecting gravitational waves. They have now reached these and are on the verge of producing really new science. The same remark applies to Sadoulet's experiments and those of his collaborating and competing groups. It is an extraordinary technical achievement that the Cyrogenic Dark Matter Search (CDMS) has been able to set upper limits to the WIMP-Nucleon interaction cross-sections of less that 10^{-42} cm^2 at an energy of about 100 GeV. This should be improved by an order of magnitude with the CDMSII experiment planned for 2007 and then by successive order of magnitude improvements through the different phases of the SuperCDMS proposal. As a community we must be fully supportive of these remarkable experiments.

7. The Really Tough Cosmological Problems

There remain the really tough problems of extracting more information from the observations about the physics of the very early Universe. *Gondolo* gave a compelling presentation in which he showed how precision determinations of the cosmological parameters beyond the standard concordance six-parameter family can help define the inflaton potential. Equally challenging was *Matarrese's* discussion of non-Gaussian features which must arise from a variety of different physical processes at some level. What is encouraging is that some of these effects may be measurable in the next generation experiments.

Above all, however, it is clear that the 'crown jewels' of all the next generation experiments is the search for the signature of *primordial gravitational waves*. The search for the B-modes associated with primordial gravitational waves is a very tough challenge indeed. It involves challenges both technological, to make the experiments feasible at all, and also computational and interpretative in order to distinguish the real gravitational wave signature among other polarised emissions.

Kesden gave a very good review of the problem of estimating and eliminating the effects of gravitational lensing upon the polarised background signal which mimics exactly the signature of gravitational waves on small angular scales. He also indicated how a programme could be developed to measure first the EE component and then eliminating gravitational lensed BB signals to leave the primordial BB signal. Working very hard it may be possible to reach gravitational wave amplitudes relative to the scalar signal of $r = 10^{-3}$ for $l < 10$.

8. Future Missions

We heard excellent presentations of future possible missions and the technologies to support them from *Lawrence*, *Bock* and *Gaier* and also from the presenters of the strategic plans of national and international agencies. The technology continues to develop very impressively indeed.

The message was very clear that the next great challenge is the search for the B modes and concepts are being developed to meet it. *Favata* outlined the ESA Cosmic Vision programme for the period 2015 to 2025 and the forthcoming call for proposals for implementing it. There is a clear opportunity for the All Sky Cosmic Background Polarisation Mapper which is included as a potential mission. He emphasised the need to come up with bright and visionary ideas which will excite the ESA executive. *Salamon* carried out the same exercise for the NASA programme, describing the aftermath of the severe cuts imposed upon the Science programme. The list of zero-funded programmes makes depressing reading, but the *Beyond-Einstein* programme has been preserved as well as the long duration balloon flights facility, which is cause for some optimism.

The national representatives presented their programmes - *Moura* from France, *Rebolo* from Spain and *Gear* from the UK. We also heard of conceptual plans for next generation satellite projects from *Bouchet* and from *de Bernardis*. There is an enormous amount to be done before projects such as SAMPAN, B-POL, CMB-Pol and the ground and balloon-borne projects can become a reality, but the knowledge of what needs to be developed is already coming together. Of course, a great deal depends upon the outcomes of the the Planck mission, but these are major future programmes with long lead-times and it is definitely not too early to be developing concepts.

It was striking that there is essentially universal agreement that the next great challenge for Cosmic Microwave Background studies is the search for the B-modes of primordial gravitational waves. It was also plain that both ESA and NASA have tightly constrained budgets for such future projects. It does not need a master politician to suggest that the communality of international interest might be best served by a joint NASA-ESA mission. B-Pol and CMBPol have essentially identical goals and so why not plan the future mission as an international endeavour from the very beginning. Already the NASA and ESA specialists are collaborating in the development of the Planck data analysis procedures and, to me, the natural way ahead is to continue this fruitful dialogue and collaboration into the next great CMB experiment.

9. Conclusions

I much enjoyed *Silk's* sketch of the evolution of observational and theoretical studies of the Cosmic Microwave Background Radiation from its discovery in 1965 to the present day. Among my most cherished memories of this remarkable era was the year I spent in Moscow from 1968-9 working with Zeldovich and Sunyaev. I was present as they hammered out the physics of temperature fluctuations in the Cosmic Microwave Background Radiation. Over the next two decades, the observational upper limits to the amplitudes of the fluctuations improved steadily and the theory had to be modified to accommodate them. A key epoch was the early 1980s when the baryonic model of structure formation could no longer be sustained and hot or cold dark matter had to be added to ensure consistency with the upper limits which continued inexorably downward. I remember Yuri Parijskij's continued frustration that, as soon as he had produced a yet more powerful limit to the temperature fluctuations, the theorists ducked and swerved to make their theories compatible with the new limits. But, eventually the bedrock was reached when the fluctuations had to be discovered or something really fundamental was wrong with the standard cosmological model. COBE, WMAP and a host of ground and balloon-borne experiments discovered the fluctuations at exactly the predicted level and the rest is history.

The result of this joint observational, instrumental and theoretical effort is an unprecedented improvement in our understanding of the origin and evolution of the Universe about us. The quest continues with the ongoing tremendous success of the WMAP mission, the prospect of Planck in orbit within two years and the future generations of experiments which will probe what have turned out to be some of the greatest mysteries of modern science.

References

[1] Tegmark, M. et al. (2004), *Cosmological Parameters from SDSS and WMAP*, Physical Review D, **69**, 103501, (1–28).

[2] Brentjens, M. A. and de Bruyn, A. G. (2005), *Faraday Rotation Measure Synthesis*, Astronomy and Astrophysics, **441**, 1217-1228.