The ESO-ESA Working Group on Fundamental Cosmology

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In September 2003, the executives of ESO and ESA agreed to establish a number of working groups to explore possible synergies between these two major European astronomical institutions on key scientific issues. The first two working group reports (on Extrasolar Planets and the Herschel-ALMA Synergies) were released in 2005 (Perryman and Hainaut) and 2006 (Wilson and Elbaz), and the report on Fundamental Cosmology has recently been completed (Peacock and Schneider 2006). In this article, we present the major findings and recommendations of this working group (hereafter WG) whose members are John Peacock (Chair, ROE Edinburgh), Peter Schneider (Co-Chair, AIfA Bonn), George Efstathiou (IoA Cambridge), Jonathan R. Ellis (CERN), Bruno Leibundgut (ESO), Simon Lilly (ETH Zürich) and Yannick Mellier (IAP Paris). A number of colleagues made further essential contributions to the report. Support for several face-to-face meetings in Garching was provided by the ST-ECF, particularly Bob Fosbury and Wolfram Freudling.

The WG's mandate was to concentrate on fundamental issues in cosmology. We have thus excluded direct consideration of the exciting recent progress in astrophysical cosmology, such as the formation and evolution of galaxies, the processes of reionisation and the first stars, etc. However, many of our recommended actions will produce vast datasets of general applicability; these will also have a tremendous impact on these broader areas of astronomy.

This is an appropriate time to take stock of the field. The past 10–15 years have seen huge advances in our cosmological understanding, to the point where there is a well-defined standard model that accounts in detail for (nearly) all cosmologically relevant observations. Very substantial observational resources have already been invested, so the next generation of experiments is likely to be expensive. Indeed, the scale of future cosmological projects will approach that of particle physics, both in financial and in human terms. We therefore need to identify the problems that are both the most fundamental, and which offer the best prospects for solution. In doing this, it is hard to look too far ahead, as our views on priorities will doubtless evolve; but planning and executing large new experiments will take time. We intend our report to cover the period up to about 2020, so that ESA's Cosmic Vision 2015–2025 document has been an essential part of our discussion.

Considerations

The recommendations of the WG are based on a number of considerations, given here in approximately decreasing order of relative weight:

What are the essential questions in fundamental cosmology?

The standard model consists of a Universe described by Einstein's theory of general relativity, with a critical energy density dominated by a component that is neither matter nor radiation, but a new entity termed 'dark energy', which corresponds to endowing the vacuum with energy. The remaining energy consists of collisionless 'cold dark matter' (about 22 %) and ordinary 'baryonic' material (about 4 %), plus trace amounts of radiation and light neutrinos. The Universe is accurately homogeneous on the largest scales, but displays a spectrum of inhomogeneities whose gravitationally-driven growth is presumed to account for the formation of galaxies and large-scale structure. The simplest consistent theory for the origin of these features is that the Universe underwent an early phase of 'inflation', at which time the density in dark energy was very much higher than at present. Given this background, there follows a natural set of key questions: (1) What generated the baryon asymmetry? Why is there negligible antimatter, and what set the ratio of baryons to photons? (2) What is the dark matter? Is it a relic massive supersymmetric particle, or something (even) more exotic? (3) What is the dark energy? Is it Einstein's cosmological constant, or is it a dynamical phenomenon with an observable degree of evolution? (4) Did inflation happen? Can we find observational relics of an early vacuum-dominated phase? (5) Is standard cosmology based on the correct physics? Are features such as dark energy artefacts of a different law of gravity, perhaps associated with extra dimensions? Could fundamental constants actually vary?

Which of these questions can be tackled, perhaps exclusively, with astronomical techniques?

It seems unlikely that astronomical observations can currently contribute any insight into baryogenesis. Furthermore, the nature of dark matter may well be best clarified by experiments at particle accelerators, in particular the Large Hadron Collider, or by direct dark matter searches in deep underground laboratories. This particle astrophysics approach may also tell us much about other fundamental issues, such as the law of gravity, extra dimensions, etc.

However, astronomical tools will also make essential contributions to these problems. They can constrain the dark matter constituents via their spatial clustering properties and/or their possible annihilation signals. Astronomy is also probably the best way to measure any time variability of the fundamental 'constants'. Finally, the nature of dark energy and the physics of inflation can be empirically probed, according to our current knowledge, only in the largest laboratory available – the Universe itself.

What are the appropriate methods with which these key questions can be answered?

Studies of the dark energy equation of state can profit from four different methods: the large-scale structure of the three-dimensional galaxy distribution, the abundance of galaxy clusters, weak gravitational lensing, and the distance-redshift relation as measured from distant supernovae. An attempt was made to judge the relative strengths of these methods, where we took into account the detailed quantitative investigation of the USA's Dark Energy Task Force (*http://www-astro-theory.fnal.gov/events/detf.pdf*). All methods will require a substantial improvement of measurement accuracies, so that that unanticipated systematic limits may become a problem. Given the central importance of this key question, pursuing only a single method therefore bears an unacceptable risk.

The physics of inflation can be studied by three main methods: the direct detection of gravitational waves from the inflationary epoch; the B-mode polarisation signal of the cosmic microwave background (CMB) generated by gravity waves; and a precise measurement of the slope and curvature of the density fluctuation power spectrum. These are bounded by CMB measurements at the largest scales, and weak lensing and Lyα forest studies at the smallest scales.

Which of these methods appear promising for realisation within Europe, or with strong European participation, over the next ~ 15 years?

This issue is subject to considerable uncertainty, as it depends on the funding situation as much as on international developments, in particular when it comes to cooperation with partners outside Europe. Nevertheless, much work has been invested in planning for potential future projects, so in many cases there is a strong basis on which to pick the best future prospects. Certainly, there is no shortage of input, and it is a sign of the scientific vitality of European cosmology that there are unfortunately more attractive ideas than can feasibly be funded. Given the interagency nature of this WG, we have naturally chosen to emphasise particularly timely opportunities for collaboration between these two major players in European astronomy.

Which of these methods has a broad range of applications and a high degree of versatility even outside the field of fundamental cosmology?

Given that the next major steps towards answering the key cosmological questions will in any case require substantial resources, it is desirable that the projects to be pursued should lead to datasets of general applicability. Whereas the cosmological issues are the prime science drivers of these projects, and determine their specifications, a broad range of applications will increase the scientific value of the investments, and boost their level of support in the community.

Recommendations

Based on these considerations, our recommendation are as follows:

1. ESA and ESO have the opportunity to collaborate in executing an imaging survey across a major fraction of the sky by constructing a space-borne high-resolution wide-field optical and near-IR imager and providing the essential optical multi-colour photometry from the ground. The ESO Public Surveys VST/KIDS and VISTA/VIKING will be essential pathfinders for this sort of data, but substantial increases in grasp and improvements in image quality will be needed in order to match or exceed global efforts in this area. Near-IR photometry is essential for obtaining reliable photometric redshifts, in particular for galaxies beyond redshift unity, but also to minimise the fraction of outliers at lower redshifts. VISTA will be able to perform this role to some extent with regard to KIDS. However, imaging in space offers huge advantages in the near-IR via the low background, and this is the only feasible route to quasi all-sky surveys in this band that match the depth of optical surveys. Therefore,
   - ESA should give the highest immediate priority in its astronomy programme to a satellite that combines this near-IR photometry with high-resolution optical imaging, and in parallel,
   - ESO should give high priority to expanding its wide-field optical imaging capabilities to provide the required multi-band photometric data.
   - Furthermore, since the calibration of photo-z's is key to the success of this plan, ESO should aim to conduct large spectroscopic surveys spread sparsely over ~ 10 000 deg^2, involving > 100 000 redshifts. This will require the initiation of a large key programme with the VLT, integrated with the imaging survey.

   This project will be an invaluable asset for several of the methods mentioned before. It will provide the necessary data for weak lensing and large-scale structure studies of the dark energy component in the Universe. Furthermore, it will provide an indispensable dataset for statistical studies of dark energy using galaxy clusters, yielding the means to determine redshifts and optical luminosity of X-ray and SZ-selected clusters, as provided by, e.g., eROSITA and Planck. In addition, such a project (essentially 2MASS with a 7 magnitude increase in depth plus an SDSS imaging survey 4 magnitudes deeper and with ~ 3 times larger area), together with highly accurate photometric redshifts for galaxies and quasars, would be a profound resource for astronomy in general, a legacy comparable in value to the Palomar surveys some 50 years ago. Among the numerous applications of such a dataset, we mention the selection of targets for deep spectroscopic studies, either for the VLT, the JWST and finally an ELT.

2. The existence of major future imaging surveys presents a challenge for spectroscopic follow-up. For some applications, such as weak gravitational lensing, photometric redshifts with few per cent precision are sufficient. But some science questions need true spectroscopy, and this presents a problem of grasp. A capability for massive multiplexed deep spectroscopy (at the level of several thousand simultaneous spectra over a field of order one degree) is required for this. Such a facility would permit surveys of > 10^6 redshifts needed to probe dark energy using the galaxy power spectrum as a standard ruler, and there are a number of international plans for instruments of this sort. ESO should secure access to such an instrument, either through the development of such a facility for the VLT, or as a collaborative arrangement with an external project,

perhaps in conjunction with sharing some of Europe's proposed imaging data.

3. A powerful multi-colour imaging capability can also carry out a supernova survey extending existing samples of $z = 0.5$–1 SNe by an order of magnitude, although this requires the imager to be of 4-m class. In order to exploit the supernova technique fully, an improved local sample is also required. The VST could provide this, provided that time is not required for other cosmological surveys, in particular lensing.

4. Whereas the WG sees the main science drivers for a European Extremely Large Telescope (E-ELT) as lying in other fields of astronomy, we recommend that the following applications in fundamental cosmology should be regarded as forming an essential part of the E-ELT capability:
– Supernova surveys need to be backed up with spectroscopy to assure the classification for at least a significant subsample and to check for evolutionary effects. The spectroscopy requires access to the largest possible telescopes, and an E-ELT will be essential for the study of distant supernovae with redshifts $z > 1$.
– A European ELT will also be important in fundamental cosmology via the study of the intergalactic medium. Detailed quasar spectroscopy can limit the nature of dark matter by searching for a small-scale coherence length in the mass distribution. These studies can also measure directly the acceleration of the Universe, by looking at the time dependence of the cosmological redshift. Furthermore, by providing information of the density fluctuation power spectrum at the smallest scales, the Lyman-α forest provides the biggest lever arm on the shape of the power spectrum, and thus on its tilt and its potentially running spectral index.
– E-ELT quasar spectroscopy also offers the possibility of better constraints on any time variation of dimensionless atomic parameters such as the fine-structure constant α and the proton-to-electron mass ratio. There presently exist controversial claims of evidence for variations in α, which potentially relate to the dynamics of dark energy. It is essential to validate these claims with a wider range of targets and atomic tracers.

5. In the domain of CMB research, Europe is well positioned with the imminent arrival of Planck. The next steps are (1) to deal with the effects of foreground gravitational lensing of the CMB and (2) to measure the 'B-mode' polarisation signal, which is the prime indicator of primordial gravitational waves from inflation. The former effect is aided by the optical/near-IR imaging experiments discussed earlier. The latter effect is potentially detectable by Planck, since simple inflation models combined with data from the WMAP CMB satellite predict a tensor-to-scalar ratio of $r \simeq 0.15$. A next-generation polarisation experiment would offer the chance to probe this signature in detail, providing a direct test of the physics of inflation and thus of the fundamental physical laws at energies $\sim 10^{12}$ times higher than achievable in Earth-bound accelerators. For reasons of stability, such studies are best done from space; we thus recommend such a CMB satellite as a strong future priority for ESA and the support of corresponding technological developments.

6. An alternative means of probing the earliest phases of cosmology is to look for primordial gravity waves at much shorter wavelengths. LISA has the potential to detect this signature by direct observation of a background in some models, and even upper limits would be of extreme importance, given the vast lever arm in scales between direct studies and the information from the CMB. We thus endorse space-borne gravity-wave studies as an essential current and future priority for ESA.

7. A future Square Kilometre Array would provide colossal advances in the field of radio astronomy and, depending on its design, also in fundamental cosmology. It might be able to provide a spectroscopic (21 cm) redshift survey of $\sim 10^8$ galaxies and to study the large-scale structure and its baryonic oscillations with unprecedented accuracy out to redshifts $z \sim 11.5$. This project will operate on a longer time-scale, and is a natural successor to the studies described above. A strong European participation in the SKA is therefore essential.

References

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Image of the Sombrero Galaxy, from the VLT Photo Gallery.