Science and technology drivers for future giant telescopes

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ABSTRACT

The second decade of the third millennium AD will hopefully see a new generation of ground-based telescopes, from 20- to 100-m in diameter, that will open a completely new window on the Universe. Here I review the scientific as well as technological drivers that underlie the new projects, looking at how they interact in pushing the limits of the parameter space and in driving the design requirements, and at some of the challenges they bring. As one may expect, much of the preparatory work, both design and industrial, is largely "concept independent", indicating that synergy rather than competition is the way forward (as it is already seen from the various collaborations that have been forming in the past year). While one should not underestimate the technical challenges, the promising result of many studies so far is that the only clearly identified show stopper seems to be funding.

INTRODUCTION: THE CONTEXT

The decade 2010-2020 will see the maturity of the current generation of telescopes (VLT, Keck, Gemini, Subaru, LBT, GTC, HET, SALT, Magellan etc) equipped with a second generation of instruments often performing at the diffraction limit through advanced Adaptive Optics (AO) systems. Interferometry will have come out of its infancy to operate in the faint object regime (K~20) and to produce astrometric result in the μas range. ALMA will provide mm and sub-mm astronomers with a facility "equivalent" to optical ones (both in terms of service offered to the community and of resolution and sensitivity). And a new generation of ground based optical/NIR 30 to 100m telescopes now on the drawing board (CELT+GSMT+VLOT=TMT, GMT, Euro-50, OWL etc)[1] may open a completely new window on the Universe and produce unprecedented results (with resolution ~ mas and sensitivity hundreds or even thousands of times beyond what is available today).

Evolution of existing facilities: Adaptive Optics. AO, now in its "puberty", will soon outgrow the current limitations (single natural (N) or laser (L) guide star (GS), limited field of view, small sky coverage) through the development of Multi-Conjugated AO (or other forms of atmospheric tomography). MCAO uses multiple NGS/LGS systems to provide a wider corrected field of view, and is now being developed at several existing observatories: for example, Gemini is building an MCAO system for its instrumentation. ESO is building MAD (McAo Demonstrator) to see first light at the VLT in early 2005 as an enabling experiment for the new VLT instruments and for OWL. AO is one of the most critical developments for astronomical instrumentation, and is regarded as a GO/NOGO milestone for future giant telescopes.

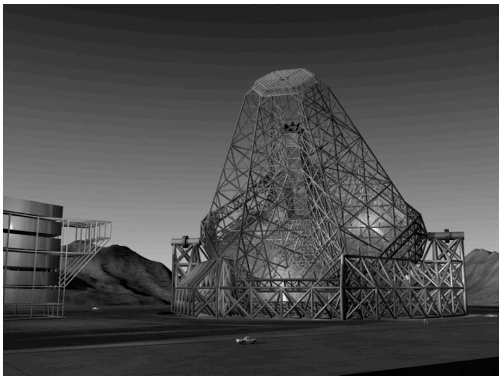


Figure 1. The 100-m OWL telescope being designed by ESO

Evolution of existing facilities: Second generation instruments. Among the second generation instrumentation considered by ESO (but similar ones are under study at many other observatories) are a multi micro-mirror, distributed classical AO system instrument (FALCON) to study in detail many individual objects in the telescope's FoV at the same time; AO-fed planet finders using nulling interferometry coronagraphs; NIR multiobject wide-field spectro-imagers; image slicer-based multi integral field spectrographs; very wide wavelength coverage "fast" shooters, able to do simultaneous spectroscopy from 0.3 to 3μm. The underlying philosophy is one of sampling the instrumentation parameter space (wavelength, resolution, FoV, image quality, multiplex, synergy with other space or ground facilities, etc) based on clear science requirements. The proceedings of conference 5492 (Ground-based instrumentation) contain many papers

[1] Some acronyms: CELT: California Extremely Large Telescope; GSMT; Giant Segmented Mirror Telescope; VLOT: Very Large Optical Telescope; TMT=Thirty Meter Telescope; GMT=Giant Magellan Telescope; OWL= OverWhelmingly Large telescope

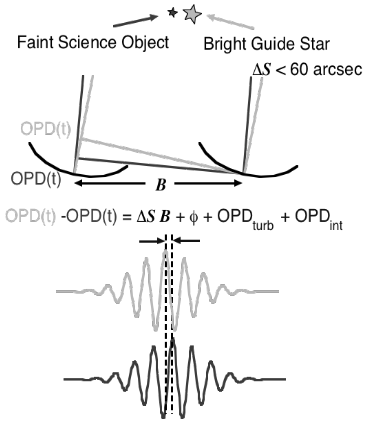


Figure 2. PRIMA's priciple

on future instruments, both for the current and future telescopes.

Evolution of existing facilities: Interferometry. Both Keck-I and VLTI have achieved fringes in 2001. VLTI, both with test siderostats and with the VLT 8m telescopes, is currently in continuous science operations with its instruments VINCI and MIDI. It will soon evolve towards imaging, both with the present generation of instruments (e.g. AMBER, which has already demonstrated 3-telescope measurements of phase closure) and with PRIMA (Phase Referenced IMAging, a dual feed facility providing stabilization of the fringes of a faint object by tracking the fringes of a bright reference star within one arcminute). These instruments will be used to image planetary systems, the inner regions of AGNs, and objects as faint as K~20. It will also provide astrometric measurements down to a few μas, thus enabling the possibility of direct detection of extrasolar planets and their orbits.

ALMA. The Atacama Large Millimeter Array, an example of big project collaboration between Europe and the US, is a variable configuration array of 64 12m antennas working in the 0.3 to 10 mm wavelength range to be put at 5000m in Chajnantor in the Desert of Atacama. ALMA is a 50/50 partnership, with ESO managing the European side and AUI the American side. It will have high angular resolution (to below 0.01" with baseline >10 km) and high sensitivity (area ~ 7,200m^2). ALMA will be able to study galaxy formation in the very early Universe, resolve the far infrared background, study star formation deep in dark clouds, search for protostars, analyze star and planet formation processes, and study the bodies of our solar system. The project is in its Phase 2 (construction, 2003-2010), and will start interim operations with a reduced number of antennas as early as 2006.

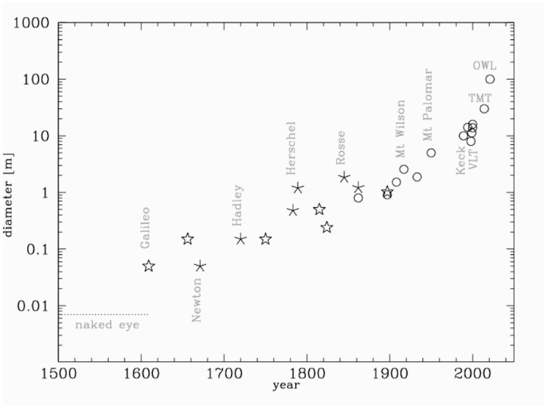
Figure 3. The ALMA array

Space missions. JWST, XEUS, TPF/Darwin precursor missions and others will explore the heavens from above the atmosphere, exploiting the freedom from turbulence, sky absorption and gravity (see the plenary talk by Eric Smith for a more in depth review). In view of the possibilities opened by adaptive optics, I believe that the optical/NIR capabilities of a "small" (5 to 10m) telescope in space may not be competitive with those of 30 to 100m telescopes on the ground. It is not inconceivable that 10 years from now it may make more sense to go to space only for those wavelengths for which the advantage is overwhelming (x-ray, UV, thermal IR etc, which is the case of the projects mentioned above), leaving the optical and NIR to adaptive ground based telescopes that for similar costs could provide much higher angular resolution and sensitivity. I think it is not premature to consider such a possibility, even acknowledging how much it depends on very demanding developments in adaptive optics. In the long run, however, it may be that putting matter into orbit will cost substantially less than today: in this case, not having to contend with air and gravity may become attractive enough that we consider moving *all* our telescopes to space. Be that as it may, it is probably a choice for the generation of telescopes after the next…

ELTs, THE NEXT GENERATION OF TELESCOPES[2]

Since a few years there are several ongoing design studies of the telescopes the astronomers believe they will need in the 2010s. They range between 20 and 100 meter in diameter and, to a more or less critical extent, they all try to break one or both of the traditional laws of the art of telescope making: the *cost law* ($\propto D^{2.6}$) and the *growth law* (the next generation telescope is twice as large as the previous one). The rationale for having larger than two increases in diameter

[2] Nomenclature: we all call the future telescopes ELTs, for Extremely Large Telescopes. As far as I remember, the original term came from Tom Sebring as the name of the successor of HET, but we have appropriated it for the whole class. Alternatives are GODs (Giant Optical Devices, coined by Jerry Nelson as a warning, I believe, about our collective *hubris*) and FGTs (Future Giant Telescopes, the title of one of the conferences at the 2002 SPIE), but these were never adopted unanimously, the title of this paper notwithstanding.



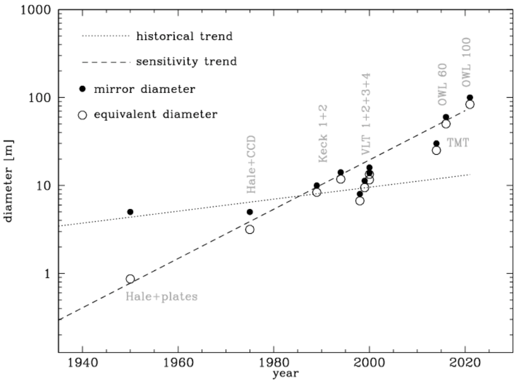


Figure 4. (*top*) Brief history of telescope. *Stars*: refractors, *asterisks*: speculum reflectors, *circles*: glass reflectors. Some specific telescopes are named. The trend to the present is a doubling in size every ~ 50 years (35 during the 20th century). The quantum jump between a 10m and a 100m telescope is equivalent to the one between the naked eye and the first telescope by Galileo. **(*bottom*) Recent history** of improvement in sensitivity of telescopes expressed in "equivalent diameter of a perfect telescope" = $\sqrt{(\eta D^2)}$, with η the telescope overall efficiency (the dashed line is an aid to the eye, not a fit). Over the last 50 years the increase in sensitivity has been mostly due to increase in detector efficiency. Now that detectors approach 100% efficiencies, large improvements require large increases in diameter (i.e. larger than a factor of two).

comes from the science cases; the one for reducing costs to "reasonable" totals is of course a key to the hope of ever getting one of these funded.

The history of telescope growth. Figure 4 (top) shows the history of the telescope diameter, with a few future telescopes (TMT and OWL) added for reference. There are two aspects that are immediately evident: (1) "local" scatter notwithstanding, the trend of diameter increase has remained substantially constant since Galileo (doubling every 50 years or so) and (2) the *quantum jump* between a 10 and a 100m telescope is similar to that between the night-adapted naked eye and the first telescope, which certainly bodes well for the potential for new discoveries. During the 20th century there has been some acceleration, with the doubling happening every ~35 years, (see e.g. the "California progression" with the Hooker [2.5m, 1917], Hale [5m, 1948], and Keck [10m, 1992] telescopes).

One point that perhaps is not immediately evident, though, is that in the last 50 years there has been a larger increase in telescope sensitivity due to improvements in detectors than to increases in diameter (figure 4, bottom). Now that detectors are at efficiencies close to 100%, *large improvements can be obtained only through large increases in diameter*. For example, at the times of photographic plates, with efficiency of a few percent, even the 5-meter Hale telescope was only equivalent to a 1-meter "perfect" telescope (i.e. one with 100% efficiency).

ELT performance. Before looking into the science cases that determine the requirements of the future giant telescopes there are some general aspects of the scientific performance of ELTs that deserve some comments:

Confusion about confusion. There is a widespread concern that ELTs may hit the confusion limit, thereby voiding their very *raison d'être.* Much of this concern comes from past observations at poor angular resolution (e.g. X-ray data or deep optical images in 2" seeing of the '80s). Recent results with better resolution lead to resolving the "confusion" into individual objects (e.g. the X-ray background, now mostly if not completely resolved, or the HDF images showing 20 times more empty space than objects). Ultimately, some confusion level will be reached, but the 3-dimensional nature of astronomical objects (position and velocity) virtually ensures it will not be a limiting factor with OWL. In fact, a lack of confusion may offer information on the covering factor of galaxies, and seems tantalizingly connected to Olbers' paradox.

Étendue, or the AΩ product. The AΩ product is often used to compare the capabilities of telescopes of different sizes. This is very dangerous, as it may lead to surprising (and wrong) conclusions. For example, nobody would claim from $A\Omega_{\text{human eye}} \approx A\Omega_{\text{FORS@VLT}} \approx A\Omega_{30''\text{@OWL}}$ that these three "telescopes" are interchangeable in performance! Instead, it would be perfectly correct to deduce from $A\Omega_{\text{LSST}} \approx 120\ A\Omega_{\text{VIMOS@VLT}}$ that the 8-m LSST is a much better wide field instrument than one 8-m unit of the VLT. The point here is that when comparing telescopes of different sizes one cannot leave sensitivity out, and therefore AΩ-based comparisons make sense only for telescopes of similar size. A better estimator of relative performance is the time needed to achieve a given scientific goal (see next paragraph).

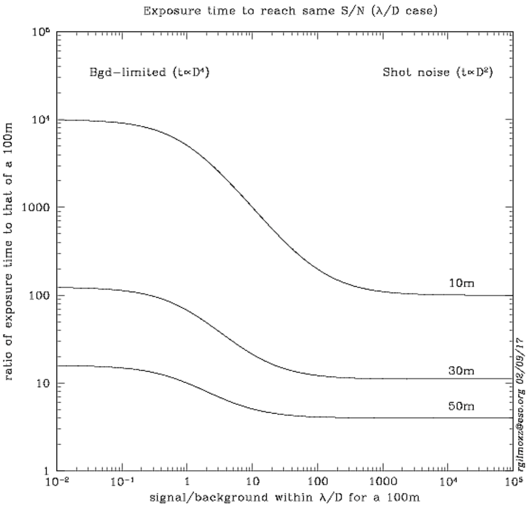


Figure 5. Time needed to achieve the same S/N on diffraction-limited telescopes as a ratio to the time needed on a 100m, i.e. t/t_{100}. The two regimes (background limited, $t/t_{100} \propto D^4$ and shot noise limited, $t/t_{100} \propto D^2$) can be seen.

Signal-to-noise vs diameter D. A too common misapprehension regards the dependence on D of signal S and signal-to-noise

$$S/N = S / \sqrt{(S + Bgd \times n_{pix}\, \Omega_{pix} + n_{pix} \times RN^2)},$$

where Bgd is the background flux per unit surface, Ω_{pix} is the pixel angular area, n_{pix} the number of pixels involved, and RN the readout noise. Too often one finds an $S \propto D^4$ assumption when the telescope works at the diffraction limit which is (alas!) not true: while the *peak* of the PSF indeed increases as D^4, its *integral* within a typical λ/D pixel increases as D^2. This means that the S/N is proportional to D^2 in the background-limited regime ($S \propto D^2$, $Bgd \propto D^2$, $\Omega_{pix} \propto D^{-2}$, S/N $\propto D^2/\sqrt{\text{const}}$), and to D in the shot noise regime (S/N $\propto D^2/\sqrt{D^2}$).
The time to achieve the same S/N for telescope of different sizes is proportional to $(S/N)^2$ and is a better estimator of the relative performance of different telescope diameters (see fig 5; this of course makes sense only when comparing a given science case). The relative merits of different telescopes are therefore a function not only of diameter but also of the science case.

For example, in the exo-earths science case (see below), which is in the background-limited regime for any realistic scenario (unless AO delivers exactly 100% Strehl, the 10^{10} contrast between star and planet makes any residual from the AO correction much brighter than the planet), a 30m telescope would need ~ 120 times longer exposures than a 100m to observe star/planet systems that both can resolve.

SCIENCE DRIVERS

Some of the science cases for the individual projects have been presented during the conference and are reported elsewhere in this volume. Table 1 gives an overview of some of them, developed in the framework of the OPTICON working group on ELT science. Although projecting our scientific expectations to 10-15 years from now is of course prone to error (so that at times it is the opening of the parameter space that may be the dominant factor), there are a few cases that in my opinion are particularly significant in determining the requirements for the designs[3], and in pushing the instrumental parameter space (instruments are an integral part of the telescopes, and sometimes as complex): the quest for terrestrial planets (possibly also for exo-biospheres) in extra-solar systems; the study of stellar populations in a large sample of the Universe (including in elliptical galaxies, missing today – often referred to as "Virgo or bust!"); the still mysterious relation between matter, dark matter and dark energy (with their link to particle physics); the star formation history of the Universe and the evolution of the cosmos from big bang to today; the first objects and the epoch of re-ionization (primordial stars and their role); the *direct* measurement of the deceleration of the Universe (with no assumptions, no extrapolations, no models). In the following I discuss three of them.

Terrestrial planets in extra-solar systems. The habitable zone around a star depends on its luminosity. The place in a stellar system where water exists in liquid form is a pre-requisite for life as we know it. The search for planets within that narrow circle around a star requires both extreme light gathering power to detect the faint planet and extreme telescope size to separate the planet from the bright star light. The challenge is to observe an object that is about 10^{10} times fainter than its parent star. Not all stars have planets and few will have planets in the habitable zone, so the largest possible sample has to be surveyed. The number of stars that can be studied is proportional to the spatial resolution to

[3] One has also to take into account that a large fraction of the astronomical discoveries of the past 50 years, from QSOs to pulsars to gamma ray bursts etc, have been serendipitous, and therefore that the ELTs' unparalleled potential for new discoveries is in itself a design driver not to be underestimated.

the cube (i.e. to D^3). As we saw, the time to achieve the same signal to noise in the background-dominated regime is proportional to D^4. A 100m telescope can in principle detect an earth-like planet around a solar-type star out to a distance of 100 light years, which means that there are about 1000 stars of this type to be observed (or about 200 stars

Table 1. Summary of some selected ELT science cases.

Terrestrial planets orbiting other stars	Direct detection of earth-like planets in extra-solar systems and a first search for bio-markers (e.g water and oxygen) is feasible with a 80-100m ELT.
Planetary environments of other stars	Mapping orbits of gas giants, determining their composition, albedos and temperatures will be a first step on the way to the more challenging exo-earth observations described above. Study of the formation of planetary systems and Protoplanetary disks will also become possible
Solar system: planetary weather	Resolution at planets (excluding Mars!) will match in-situ spacecrafts and provide longer time baselines. Weather-satellite-like resolution achieved out to Neptune. (Resolution at moon: 2-4 meters)
Solar system: complete census of small bodies	With typical resolution of a few km, a 50-100m ELT will be able to map most asteroids, determine their composition. Trans Neptunian Objects and Pluto can be resolved to 50-100 km features.
Resolved stellar populations	Extend studies of individual stars so far possible only in our galaxy and nearest neighbours to a representative section of the Universe, including elliptical galaxies and reaching at least the Virgo cluster of galaxies. Will provide clues on how galaxies form (ages and composition of stars reflect past history). Need 100m ELT.
Massive Black Holes demography	Through dynamical analysis of circum-nuclear regions of galaxies, resolved out to Virgo, establish whether properties seen in AGNs hold also for dwarf galaxies, providing clues to BH formation
Star formation history across the Universe	When did stars form? Using the fact that stars eventually die in supernova explosions, it is possible to deduce the number of stars that have formed, and when. The observations will also provide critical information on SNe as physical entities. A 100m ELT can trace star formation back to re-ionization.
Dark Matter	The dynamics and kinematics of galaxies and their sub-galactic “satellites” within large dark matter haloes can be traced with an ELT out to redshifts of about 5. Thus we can observe the build-up of such dark-matter structures in the process of formation
Dark Energy	The “same” supernova observations used to determine the star formation history can be used to probe on empirical grounds cosmological models for the nature of dark energy.
First objects and the re-ionization of the Universe ($7 < z < 17$)	A first generation of objects providing the necessary UV photons to re-ionize the hydrogen in the Universe must have existed. An ELT will distinguish between candidates: QSOs, primordial stars, SNe.
High redshift intergalactic medium	The brightest earliest sources (GRBs, SNe, QSOs) are ideal to probe the high redshift interstellar and intergalactic medium, which is key to understand re-ionization and how the first stars, galaxies and AGNs formed

for a 50m telescope and 30 stars for a 30m telescope). Key to the achievement of this challenging goal is the light gathering that will allow improving the contrast between planet and star through the detection of in situ spectroscopic features. As a huge bonus, it would then be possible to characterize planetary surfaces and atmospheres. The search for biomarkers in the planet atmosphere has the potential to provide first indications of extraterrestrial life. It is clear that larger planets and planets with larger separation from their star would easily be detected by a 50 or 100m telescope and open up the field of planet demographics down to low-mass planets. Such statistics will provide the clues for the detailed understanding of the formation of stars and their planetary systems, for example which stars have planets, what is required to form planets, what is the chemical composition of the parent stars and are there planets around special stars (e.g. white dwarfs, very old halo stars). The quest for high contrast imaging sets stringent requirements on the development of adaptive optics. Various methods are under investigation, *e.g.* coronagraphy, nulling interferometry,

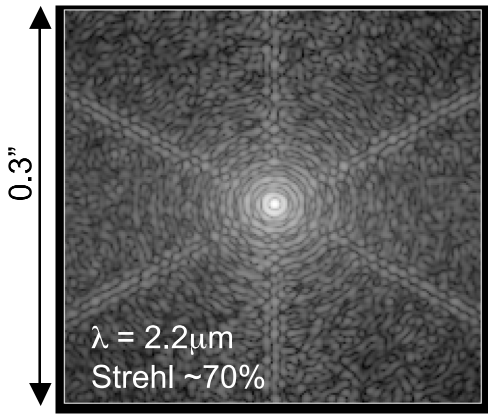


Figure 6. Simulation of OWL’s PSF for the exo-earths science case, using the parent star as guide star and ~10^4 sub-pupils/actuators. The simulation includes pupil and co-phasing errors.

eXtreme Adaptive Optics (XAO), simultaneous differential imaging (SDI), and have already shown promises of high contrast (e.g. NACO/SDI at the VLT has achieved ~ 5 10^4).

Dark matter and dark energy. Observations imply that dark matter exists on the scale of galaxies and beyond, and that dark energy is pervading the universe. This means that only observations of distant, and hence faint, objects can tell us more about their nature. Particle physics has been unable to date to identify the dark matter particles and clues about their nature are still coming solely from astrophysics. (It is interesting in this context that constraints set by astronomy on the mass of the neutrino are as stringent as the best upper limits from experiment.) Similarly, through a detailed study of the growth of structure in the universe it should be possible to derive further constraints on the dark matter nature and identify the most likely dark matter particle candidates.

Since an ELT will be able to observe regular HII regions to very high redshifts (z ~ 5), it will be able to map the dark matter content of individual galaxies throughout the observable universe. This will provide mass measurements of galaxies independently of the brightness of the galaxies themselves. A 100m ELT will not only resolve the distant galaxies into their luminous components, but also be able to characterize these individual components which will then be used to trace the kinematics within the galaxies (and in their extended dark-matter haloes) and determine the amount of dark matter required to build them. This will provide astronomers with a detailed evolutionary history of the clumping of dark matter throughout the observable universe.

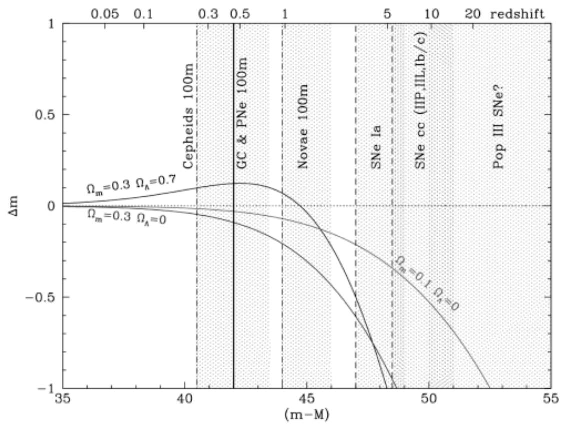


Figure 7. Using primary distance indicators to disentangle cosmological models. Regions of application for various methods with OWL are indicated.

The nature of dark energy is even more mysterious. The combination of the current matter density with the prediction of Einstein’s theory that the geometry of the universe is tied to its energy content shows that two thirds of the global energy comes from this dark (or vacuum) component. The direct measurement of the dynamical expansion history of the universe by supernovae has shown that the dark energy exerts a negative pressure and hence accelerates the universal expansion. An ELT can test the expansion history of the universe with several different astrophysical objects thus decreasing the dependency on possibly unknown systematic effects. Pulsating Cepheids, globular clusters, planetary nebulae and novae could be observed to distances where the effect of dark energy can be measured (see fig 7). A 100m ELT will be able to detect supernovae possibly all the way to the time when the universe became transparent to light. By accurately determining the potential variations of the strength of dark energy in early times, one can answer the fundamental question of whether dark energy corresponds to Einstein’s cosmological constant or to some “quintessence field” as suggested by modern versions of quantum field theories. The need for these observations is critical. In the words of the Astronomer Royal, Sir Martin Rees, “Cosmologists can now proclaim with confidence (but with some surprise too) that in round numbers, our universe consists of 5% baryons, 25% dark matter, and 70% dark energy. It is indeed embarrassing that 95% of the universe is unaccounted for: even the dark matter is of quite uncertain nature, and the dark energy is a complete mystery”

Direct measurement of the cosmic acceleration/deceleration. Enormous collecting areas together with extreme instrumental stability open also the very exciting prospect of measuring the acceleration (or the deceleration) of the Universe in a direct way. The results mentioned above indicate that our universe has undergone a phase of acceleration following one of deceleration. So far most of the results in this area have come from the *interpretation* of the astronomical data (a *very* simplified example: SNe at $z < 1$ are fainter than expected and so apparently further away, while those at $z > 1$ are brighter and therefore were closer, when their light started on our way; this indicates that the universe must have been accelerating according to the curve labeled Ω_m=0.3, Ω_Λ=0.7 in fig 7). In some cases this interpretative process has been based on the very models the data were supposed to support (or disprove…). Not that there were alternatives: a direct measurement of the change in recession velocity of cosmological objects has always been considered impossible for the present generation of telescopes. However, it has been shown that with a sufficient flux of photons, and with a spectrograph stability of the order of 10 cm s^{-1} over 10 years, changes in the recession velocity of absorption lines in the Lyman-α forest of bright quasars out to $z \sim 5$ can be detected. This would allow a real *physics experiment* to be carried out with an ELT, whose results would be unequivocal, model-independent and assumptions-free. ESO is designing an instrument, called CODE (for COsmic Deceleration Experiment) to carry out these observations with OWL. (A stability of 10 cm s^{-1} is achieved already today, e.g. the HARPS instrument at the ESO 3.6m telescope; maintaining this stability over a long period of time is the challenge here: see the paper by Monnet and D'Odorico in conference 5492 for more information.)

THE CHALLENGES

Sensitivity. Most science cases aim at observing faint and distant targets in the universe, or resolve faint companions of bright objects. For example, spectroscopy of the faintest galaxies in the Hubble Deep Fields needs at least a 30m telescope. Spectroscopy of the faintest galaxies that will be discovered by JWST will need at least a 100m telescope. Spectroscopy of candidate earth-like planets within 30 light years requires at least an 80m telescope. All this drives to *maximize the diameter* of the future ELTs.

Atmosphere. The atmosphere is the most serious enemy of ground-based telescopes: it absorbs the light (completely in some regions of the spectrum); it degrades the image quality (so that uncorrected telescopes have the same image quality independently of the size); it has a strong, and varying, background (so that faint objects are swamped by it), and of course it has weather (see below). There is obviously nothing that can be done apart going to space to solve the first problem. The second is a challenge being addressed by Adaptive Optics (see introduction, and fig 6), and cautious optimism can be derived from the excellent results obtained by the present generation of telescopes. The problem of scaling up the existing AO systems to telescopes of up to 100m is daunting, but a staged approach will give us more R&D time to tackle extreme contrast and shorter wavelengths. The simulation in fig 6 is based on a factor of 10 increase on current technology within the next 10 years and would represent the initial (IR) stage of the AO system for OWL. The problem of the background, especially in the IR, is offset by the size of the ELTs so that, at least for wavelengths < 2.5 μm, they can be competitive with smaller telescopes in space (and are better up to ~ 10 μm for high resolution spectroscopy).

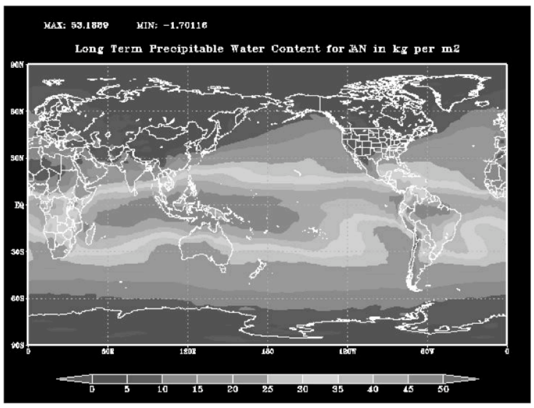


Figure 8. Global search for possible OWL sites (University of Fribourg)

Site selection. The search for sites for future ELTs is ongoing in most projects, in a cooperative way. The choice will be based not only on the meteorological characteristics or on the seeing statistics but will also include air aerosol content, seismicity, soil properties, logistic access etc. The final choice for each project will have to be made before the design is finalized, as all these parameters affect in many ways the technical choices to be made (stiffness requirements, foundations, optical design if conjugation to specific turbulence layers is needed, road development, transport etc). Both *in situ* tests of known or new sites and global searches based on all available data are under way (see fig 8).

Wind. Wind is a major concern for ELTs, given their size. The main issues are how it affects the overall structure and how it affects the mirror segments. To address this challenge, all projects are using multiple approaches. Critical are the mechanical design and its stiffness, but also the provisions for control loops that can compensate in an active way the vibrations or displacements induced by the wind. "Brute force" solutions, like protecting enclosures, windscreens or combinations are considered by some projects. A lot of work is being done using CFD (computational fluid dynamics) calculations, wind tunnel tests, and specific experiments on existing telescopes or on specially designed breadboards. For example, in the case of OWL, the CFD calculations were felt too optimistic in their indication that open air operations would pose no problem up to wind speeds of ~ 12 m s^{-1}. For this reason ESO together with Jodrell Bank have started a campaign of wind measurements on the 76-m Lowell radio telescope against which we can test the CFD calculations (with the advantage that the "experiment" is of the appropriate size for OWL, and so does not have to rely on any kind of extrapolations or scaling up of the results as e.g. a wind tunnel test would).

Instruments. The ELT instrumentation challenges were amply discussed at conference 5492. Here I just recall a few. Pixels: lots of them (at milliarcsec resolution, even a few square arcsec require an inordinate amount of pixels). Size: while "diffraction limited point source" instruments may be comparatively "easy" (e.g. a spectrograph, which in the diffraction limit is scale invariant since the beam size is proportional to diameter times the slit size, i.e. D × 1/D, so we can use one we already have), the field of view, multiplex and stability requirements of most instruments are certainly very difficult and may drive the instrument size dramatically (although an F/30 camera is "easier" to build than the F/0.5 we would need if we worked at the seeing limit). Active control to achieve optimal stability is an area only just under investigation. And active atmospheric dispersion compensators may be required if we want to observe away from zenith (unless we find a way to use the atmospheric dispersion as a "component" of our instruments…)

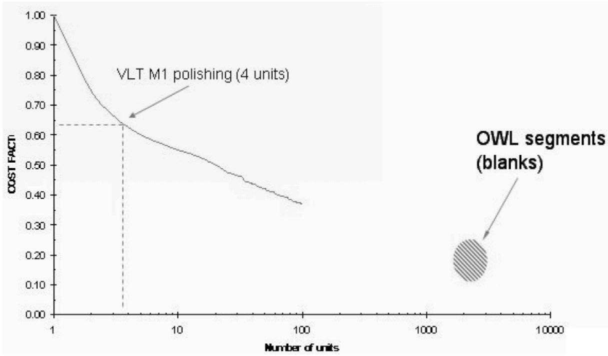


Figure 9. Advantages of serialized production: cost per element as a function of number of elements. The line is actual industrial data for conceptually "simple" elements. The locations of the VLT M1 polishing and of the OWL segment fabrication studies are indicated. Extrapolation to zero cost for $>10^4$ elements may be unadvisable…

Cost. Breaking the historical $D^{2.6}$ cost law is a critical goal of ELTs. There are several avenues that may be (and are) explored: innovative designs, whereby tradeoffs between scientific and technical requirements may allow simpler and/or cheaper solutions; early involvement of Industry (so that what is feasible is determined early in the project and the design properly adapted to it – ESO's OWL design has already benefited greatly from this); long term savings, e.g. "built-in" maintenance concepts to keep the running costs to a minimum (goal 3% of capital); and new concepts or paradigms. An example of this last is the adoption, in the OWL design, of the concept of serialized production (which is "new" only to the art of telescope making). As far as possible, all OWL's subsystems are based on a limited number of identical elements (e.g. spherical mirror segments, mechanics building blocks, supports, motors etc – these are also all sized as multiples of a single basic size): this brings in the advantages of mass production (cost per element goes down as a function of number of elements, see fig 9). The cost estimates for the various projects vary less than their sizes, and lie between 0.6 and 1.2 B€.

KEY TECHNOLOGICAL DEVELOPMENTS

Industrial readiness. We have seen that it is not easy to predict how the universe looks at milliarcsec resolution. Neither is it easy to predict how fast technology develops[4]: the evolution of industrial processes in the last decades has undergone a fantastic acceleration. For example, the main reason for the *factor-of-two* growth law of telescopes was mainly the technological difficulty of casting mirrors and of polishing them to the necessary shape. This is no longer a

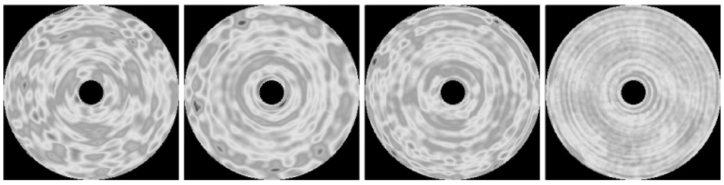


Figure 10. Surface quality of the four 8.2m primary mirrors of the VLT. Although the first (left) was already within specifications, the improvements in the following ones are enormous (the fourth, at 8.5 nm RMS, is 4 times better than specs).

[4] Two famous "predictions" (possibly apocryphal) in the swiftly evolving field of computers are by Thomas Watson, chairman of IBM, who in 1943 said, "I think there is a world for maybe five computers", and by Bill Gates, founder of Microsoft, who in 1981 apparently claimed that "640K ought to be enough for anybody". Anecdotes apart, industrial growth seems to defy prophesizing...

limitation: computer controlled polishing delivers now mirrors of unparalleled surface quality (e.g. the 4^{th} 8.2m primary mirror of the VLT, with a surface RMS of 8.5 nm, is one of the best mirrors ever made, see fig 10), much better than the specification (that are anyway set by the wavelength, not the size). Mirror segmentation, with its theoretically infinite scalability, has removed the problem of casting prohibitively large mirrors from the list of impossible tasks (of course, with problems of its own: but as the twin Keck telescopes demonstrate, these have been solved at least for the 10m class telescopes).

International collaborations. The most advanced projects are now well into their Phase A stage. There are a large number of developments that are design independent. International collaborations to pursue these in a shared way have been established in the last year. In Europe, a technology development study has been submitted to the Framework Programme 6 of the European Commission with the aim to foster industrial readiness to build an ELT. This proposal, joining 39 institutes from 13 countries under ESO's leadership, has been partially funded. ESO and AURA (a partner in the TMT consortium) have signed a memorandum of understanding to collaborate on key technologies (segmented mirror fabrication, adaptive optics, instruments and detectors, site selection). The science cases working groups have established formal relationships and started regular joint meetings. Apart from these formal agreements, the exchange among all groups everywhere is very open and constructive (even if some level of healthy competition does exist).

Key developments. There are four key areas that have been identified as essential by various ELT programs: telescope systems, facility AO systems, site evaluation and science instruments. Some we have touched upon above. See Table 2 for an overview.

Investment needed by:	GMT	TMT	LAT	OWL
Adaptive Secondary				
Durable Coatings				
Alternate Segment (SiC)				
AO system studies & simulations				
Deformable Mirrors				
Na Lasers				
Detectors for Wavefront Sensors				
Site Evaluation				
Large format near-IR detectors				
Large format mid-IR detectors	?			
Image multiplexers			?	
VPH; immersed Si gratings	?		?	
Instrument Concept studies				

Table 2. Investment needed by different projects on specific key technologies, showing that much R&D is common to all (light grey: possibly needed). Adapted from Simmons, 2003

Active optics. Controlling the primary mirror's segment errors, the secondary mirror misalignments and the shape of corrector mirrors (in the case of OWL) is crucial to the performance of ELTs. Investment is needed in edge sensors, actuators, control system, wavefront sensors.

Adaptive secondaries/tertiaries. The technology of large adaptive mirrors, pioneered on MMT and LBT, is an integral part of several designs: two to four meter-class deformable mirrors with up to 10,000 actuators, 10-15 mm interspace, 5-10 micron stroke are needed. These are an integral part of the AO system and of wind-buffeting compensation. They will also be very efficient at ground-layer compensation.

Coatings. If Al coated, a telescope like OWL, with 3,000 segments, would need to recoat 10 segments per day just to keep the average age of the primary at one year old. Clearly this is a maintenance nightmare. All projects are investing in the development of high performance, durable coating with lifetimes around 10 years, high reflectivity from 0.4 to 20 μm, yielding potential major savings in operational costs.

Alternate mirror materials. The baseline material for the mirror blanks is glass. Studies are underway to determine whether alternative materials may offer better solutions. SiC is showing great promise in that at 40 kg m^{-2} it would substantially reduce the weight of the mirror (and therefore of all subsystems connected, including the overall mechanics). Several test blanks are being produced within the OWL industrial studies, and within the FP6 collaboration.

Adaptive Optics. It is not by chance that AO keeps coming up as a challenge, since it probably is the most critical component to achieve many of the most demanding scientific goals underlying the ELT designs. Developments in multi conjugated AO, in extreme AO and in Ground Layer AO are under way now. The good news is that a lot of applications (military, medical, even consumer electronics) are beginning to make use of AO so that we can expect even more thrust in R&D than from astronomy alone. Also positive is the fact that AO technology can be developed in parallel with the telescopes themselves and therefore does not need to be frozen early in the programs: this will give ample R&D time which, together with a progressive implementation plan, assures us at least another 10 years of development.

CONCLUSIONS

The science cases indicate that new extremely large telescopes are needed to explore the questions that the present generation of space and ground telescopes has opened. The required improvements in collecting area, now that improvements in detector efficiency can only be fractional, need to be well beyond the historical *factor-of-two* diameter increase. Industrial studies indicate that this goal is not unrealistic. At the same time, investment in key technologies is critical to all ELT programs. Enabling experiments, demonstrators and breadboards are planned. The overlap in technology development has fostered various collaborations to pursue common design-independent R&D. Industrial partners are showing a growing interest in this phase, and participate in a very constructive way to the design phase. Schedule estimate put the first light of the most advanced projects between 2013 and 2016.

Figure 11. Next generation astronomers looking at next generation telescope…

No showstoppers have been identified by any project so far, or by the industrial studies. Even AO, with all its difficult and necessary extrapolations (and while waiting for the first results of the MCAO tests on sky at ESO and Gemini), seems to generate some optimism. The most critical aspect seems to be funding (and this of course is the strongest possible showstopper). In the case of ESO, if its present funding and spending profiles remain as planned, it could consider building a 60m version of OWL after the completion of ALMA, between 2010 and 2020 (the “grow a telescope” concept will allow start of science with a 40m equivalent partially filled primary in 2016). Building the 100m version of OWL would need a doubling of available funds during the same period (science with a 50m equivalent partially filled primary would start in 2017 although earlier funding could accelerate the schedule by 2-3 years).

Although many technical challenges remain to be solved, and funds found, the possibility that the next generation of extremely large telescope may become a reality is far less unlikely today than it was when all the discussions started a few years ago. While we may not see all of these projects transformed in glass and steel, it begins to appear that we may see at least one, probably the result of some large collaborative effort. Ten year from now, turning such a scope to some nearby earth-like planet, or to the far reaches of the universe may not be as wild a dream as it was yesterday.

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