Executive Summary

This book documents the work done at ESO in the past several years towards a conceptual design of an extremely large telescope. It was named OWL for the eponymous bird's keen night vision and for being OverWhelmingly Large.

At the outset, the goals of the design work were to seek answers to two key questions:

1. Is there an underlying physical reason for the fact that optical telescopes have been increasing in size by a factor of only two, approximately every 50 years since they were invented?
2. Is there a way to contain the cost law of telescopes, traditionally proportional to the diameter to the 2.6th power?

The underlying motivations for the first question were both scientific and technical:

- The increase in sensitivity of optical-infrared telescopes in the last 50 years has been due mostly to the improvements of detectors (they represent 80% of the total enhancement) rather than increases in diameter. Since detectors have achieved near-perfect efficiency, maintaining the present trend in scientific productivity requires a leap in aperture size – to 100-m by 2020.
- Strong science cases, like the imaging and characterization of Earth-like planets around other stars or the spectroscopy of the faint galaxies and other objects (e.g. gamma ray bursters and supernovae) at the edge of the Universe – to be discovered by future space missions like JWST or by OWL itself, require diameters of 100-m or more.
- The theoretically unlimited scalability of telescopes introduced by Keck with segmentation made thinking of a quantum jump in mirror diameter at least a reasonable possibility, and moved the principal challenges of a conceptual study towards other telescope subsystems (e.g. mechanics, kinematics). The existence of astronomical equipment (radio telescopes) of sizes up to 100 meters was an inspiring precedent, even taking into account their more relaxed requirements due to the much longer wavelengths.
- With the coming to maturity of wavefront control techniques, in particular adaptive optics, high angular resolution is no longer the domain of space astronomy. Future projects and missions would advantageously capitalize on healthy and cost-effective complementarities with extremely large ground-based telescopes performing at the diffraction limit. Instruments working at the diffraction limit do not necessarily increase in size with telescope diameter – depending on the field of view required.

The fundamental objective of the OWL study became trying to find an answer to the question "is a 100m filled aperture telescope feasible for about one billion Euro?". One billion is the limit that was thought "reasonable" for a large endeavor like this one since it represents an increase of a factor ≤ 2 with respect to projects already accomplished (VLT) or in development (ALMA), and would be comparable to, if not smaller than, an average, shorter lived space-borne astronomical experiment.

It should be noted however, that the financing of such a project was not explored during this design phase. Perhaps naively, it was felt that if the case were strong enough, new sources of income could be sought during the following phases of the design. Nevertheless, we are aware of the realities of the world and to the extent possible, the solutions explored were chosen for their (downsize) scalability. In fact, different designs for smaller apertures have also been analyzed and are presented in the report. If the conceptual design work were to be judged satisfactory, the following phase would concentrate on addressing the impact of financial issues and include, if appropriate, substantial re-design activities.

This report is not a complete picture of how one would build a 100-m telescope, but rather a set of approaches to, and investigations of, the challenges that we would face in building such an enormous telescope in a manageable timescale and at the lowest possible cost.

The challenge facing ESO engineers and their collaborators in academia and industry, has been to develop our 100-m concept to the point of delivering a believable budget. Clearly smaller, but still enormous, telescopes are feasible. What has been learnt from the specific developments described here is that a telescope as large as 100, or even 120-m, can be constructed using the technology available to us today. This report aims to convince the reader that such a project can potentially be done within the human and technical resources available to our community. There is still scope and need, however, for an overall harmonization of the telescope with its instrumentation in order to optimize its scientific performance.

This report is a description of research and development. The reader will find chapters that are much more advanced than others. In some cases, this is a result of our prioritization of areas we felt were more prone to potential “showstoppers”. In other cases, the developments just led us to delve deeper. Other areas are less well developed. We are fully aware of this and we actively and openly invite readers to assist us to develop these areas further.

This report has been structured as if it were a Phase A study. A requirements section is followed by detailed examination of the issues generated by the requirements and a discussion of the solutions. However, in some areas, this study process has not been closely followed nor completed and this is evident in the associated text.

While a lot of work has gone on the science case for Extremely Large Telescopes (ELTs), and a brief summary is given in the report, this is not the report’s main focus. For the purposes of this report, we assume that a 100-m telescope is a scientific goal. What then would such a telescope look like? Telescopes have been around for a long time and there has been an evolution of their design towards a set of successful (engineers can build them, astronomers can use them) Ritchey-Chrétien Cassegrain telescopes (e.g. the VLT) and a few Gregorian ones (e.g. Magellan). The mounts have changed from equatorial to alt-az, making the Nasmyth focus available as a perfect platform for heavier instruments, without the light loss of a coudé train. Most importantly, the concept of active optics control, pioneered by ESO for the NTT, has made the present 8-10 m size scale feasible. Larger telescopes with spherical primaries such as HET and SALT have also been built, but the former is just now overcoming the limitations imposed by the construction and the latter has just had first light. Moreover, although they collect light and track objects, the strict limitation of a fixed altitude axis makes them very special cases of what the average astronomer would regard as a telescope. Other ideas floated for this class of telescope include making the two mirrors move on different structures i.e., pointing the primary and then moving the secondary to point towards the primary with another system. We also looked into these approaches at a very early stage of our OWL concept discussions, but none came to fruition.

One natural approach to ELTs is to continue the evolution along the Ritchey-Chrétien path. This is exactly what both the TMT and GMT projects are doing, to create larger (still challenging!) versions of respectively Keck and MMT. However, somewhere near 30-m diameter, the underlying concept in most telescope designs ceases to be realistic in terms of timescale and cost. Retaining the NTT/VLT active optics and the Keck segmented mirrors approaches, the OWL concept borrows from the serialized production common in industry to make a 100-m feasible. In particular, choosing a spherical primary brings colossal advantages in timescale, risk and cost because it is amenable to mass production. In fact, serialized production is applied across the board to the mechanical structure, the supports and the actuators, to provide not only huge time and cost advantages but also excellent technical performance and manageable operation (maintenance) solutions. These gains will percolate to the labor-intensive erection stage of the facility. Not only would these approaches provide much needed time and cost gains, they will additionally enable the possibility of an early start of scientific use of the telescope via a partially filled aperture at night while proceeding with the completion of the primary mirror.

What is the price to pay? Why has this not been done in the past?

There is a rather large price to pay by requiring the addition of a very large 4-mirror corrector, including two 8-m class mirrors, to the light path in order to cancel the spherical aberration and to provide a wide – by 100-m standards – diffraction limited field of view. This however has to be put into perspective. A 100-m telescope is bound to operate most of the time with Adaptive Optics correction, mostly utilizing Multi-Conjugate Adaptive Optics (MCAO). Until mass-produced AO actuator unitary cost (including control and integration costs) drop to a very small fraction of their present value, the ideal approach to ELT AO of paving the primary with hundred of thousands of actuators is not viable. This implies a corrector consisting of at least two manageable size mirrors placed at strategic locations, conjugated to the main atmospheric turbulence layers. The proposed OWL corrector will follow this approach and it should be noted that the total number of reflections prior to the detector of CONICA on the VLT is actually (slightly) larger than those projected for the ONIRICA MCAO camera on OWL.

The OWL design brings the wavefront sensing and the correcting optics into the telescope train and uses them as an integral part of the facility. This is a natural extrapolation of the active telescope pioneered by ESO. At the NTT, deforming the primary was nice to have (it corrected the polishing errors more than anything else) but the true novelty was that, for the first time, a telescope could keep itself actively collimated. A third of an arcsecond images could be obtained by visiting astronomers to a common user facility. The VLT, the next logical step in the evolution, not only keeps itself aligned but also optimizes the shape of the primary to correct for gravity deformations and removes the shakes that affect any structure exposed to the elements. Hundreds of hours of 0.3 arcsecond imaging and spectroscopy are provided to VLT users. OWL, by design, keeps itself aligned, fixes gravity deformations, removes the shakes and corrects for the atmosphere, in addition to relaxing fabrication tolerances. The design proposed is an evolutionary one in both control and optics.

ESO realizes that it has no direct experience with segmentation although the proposed OWL design has two segmented mirrors. The reader will find frequent reference in this report to the experience of others. Of course, we have gained a huge amount of confidence regarding issues such as phasing from the experience of our Californian colleagues who built and operate the segmented Keck telescopes and must be regarded as the experts in segmentation. The Californian design for an ELT has a much smaller diameter primary mirror than our 100-m concept, but – due to their smaller unit size – not a significantly different number of segments. The phasing problem clearly scales as the number of interfaces that each segment will have. That depends on their shape rather than number. The complexity of the control does however depend linearly on the number of segments. We therefore will argue that even though we do not yet have direct, hands-on experience with segmented mirrors, this is a problem we believe has an affordable solution. As the report shows, not only are we working in-house to gain expertise through the Active Phasing Experiment, but also an excellent collaboration with the GTC team is already in place through FP6. In fact, technical time at the GTC, specifically to gain experience with segmentation, is foreseen as part of the in-kind contribution of Spain to join ESO.

A lot of thought, both by ESO and industry, has been put into the challenge of manufacturing, testing and shipping the thousands of segments needed for OWL. The confidence of industry is of course reassuring, although one must not be blinded by the enthusiasm of optical firms to supply thousands of tons of precision glass. In the report you will find that more than one supplier would be keen to bid. Moreover, the manufacturing experience for a serialized production of mirror segments is already in place within Europe. The cost, in time and money, of climbing the learning curve of segmentation has already been paid. We are ideally placed to take advantage of this.

The development of the mechanics within the report is probably the most advanced aspect described. Ingenious solutions to the challenges of constructing, transporting and erecting an enormous telescope have been developed over the years. The evolution presented in the report shows a progressively stiffer and cheaper design. The present concept can be retroactively understood as a ‘fractal’ design. This comes from the search for a solution that would provide minimal weight (60 times less dense that a VLT Unit telescope) with extremely rigid and a fast thermal exchange, i.e. have minimal volume and maximal area. The optical design evolved to match what could be built mechanically. It is clear that moving structures of the size proposed can be fabricated and erected. They already exist, even in astronomy.

The design presented is a clear step in the evolution of the control of telescopes. When moving weights were used to drive telescopes, feedback was non-existent. The wonders of guiding (whether by eye or automatically) brought higher accuracy requirements on the control of the telescope mounts. Even today, most telescopes will move hundreds of tons of metal and glass to keep the object within a slit. The requirements on the telescopes have become progressively stricter. The VLT, although built to exacting requirements (azimuth tracks to 10 milli-arc seconds rms), has revealed a better way of working. Control of the focal plane is better than control of the mount. On OWL, we will take this philosophy to the natural next stage. We will not try to track thousands of tons to astronomical accuracies. The design, with its multiple mirrors ahead of the focus, allows us to control the focal plane to keep the objects in the 'slits'. The mechanics of OWL are thereby made easier to build and move. The complexity has moved to where the scales are manageable with our current engineering skills. You will see this approach in the adapter-rotator chapters where innovative designs for the multiple guide probes are presented. While the terminology has stuck, these 'guide' probes no longer guide the telescope but rather sense aberrations from tip and tilt, focus, astigmatism, coma etc. They no longer move the telescope but rather the optics. Other control loops feed downwards to the mount. Together with its fractal mechanical design, the OWL control system concept provides us with some optimism for achieving operationally the exacting wavefront tolerance – 10 nm rms – even when a sizable wind (at least 12 m/s) is blowing on the telescope structure. in open-air.

Obviously the adaptive optics challenge we face is enormous. However, we take solace in the fact that even a gradual implementation would allow some solid science to be done early. Exo-planet detection is a special case as it needs none of the multiple lasers and exceedingly complex wavefront sensors and adaptive mirrors associated with full Multi-Conjugate Adaptive Optics systems. Rather, it requires just a baseline single conjugate AO with a bright natural star right in the center. On the other hand, achieving good light concentration in the H-band diffraction peak – not necessarily extreme AO with 85% plus Strehl – calls for a deformable mirror with close to 10^5 actuators and an associated fast Real-Time Computer. This feat is provisionally planned only for the 2^{nd} phase of AO deployment on the OWL facility.

From this point to where we would like ultimately to be, is a complex path. Multiple lasers, ground layer, multi-conjugate, multi-object and other flavors of adaptive optics are all discussed in the report. We have grown most in confidence in the area of AO from the evolution that has taken place before our very eyes – and hands – during the last decade. When the first unit telescope of the VLT was being erected, AO at ESO was ADONIS on the 3.6-m at La Silla. It was complex and far from a common user facility. AO at the VLT does not require specialist operators and is self-optimizing. In as much as down time can reflect robustness, AO at the VLT is just another tool. Lasers and the survivors from the pantheon of AO will evolve to similar levels of robustness. Clearly we do not wish OWL to be their test-bed and, for this reason, deformable mirrors are being constructed for the VLT. Furthermore, a laboratory/on-sky demonstrator of multi-conjugation techniques (MAD) is already partly integrated and has achieved closed-loop "first light" in the laboratory.

The project schedule to first light is defined mainly by the production of the 8-m mirrors of the corrector, with commitments for their procurement delayed until major subsystems such as enclosure and telescope structure have reached final design. The project schedule is also such that the longest possible amount of R&D time is foreseen for AO development. A progressive implementation of capabilities over at least another 15 years of development will be required before the most difficult requirements are met. It is clear however that, if by the time of the preliminary design review, the AO prospects of substantial advances are not convincingly on the right path, a thorough reassessment of the telescope size and capabilities should be made, quite independently of budget or other considerations.

All telescopes need a home. A global search, coordinated amongst all interested parties, is ongoing to find the right one. A home with, or near to infrastructure, not particularly seismic, with good weather and welcoming hosts would be very nice. As you will find in the report, options exist. They are being evaluated before a final recommendation is to be made. Detailed design will need to take into account the site specifics and therefore a decision will need to be made soon. We all recall the early plans for the VLT enclosures and the big changes that the move to Paranal entailed for the project.

For a telescope such as OWL, the instruments can no longer be regarded as add-ons designed and strapped onto the telescope at the last minute. Selecting and designing an instrumentation set that could fulfill the major science drivers selected for OWL should be seen as a last crucial step, “closing the loop” on the feasibility of the whole project. We have started to investigate this issue only about a year ago because we felt that a consolidated telescope design was necessary to define a set of preliminary interfaces. Quite naturally, initial results, presented in this report, point more to identified problems than to their solutions. What lies ahead in achieving more powerful facilities, is a need for identifying key enabling technologies – e.g. moving from static to active instruments. Clearly we need to make meaningful tradeoffs between the essentially unlimited astronomical appetite for field of view and target multiplexing, and associated technical and cost constraints. Ample time and resources will be allocated in the OWL Phase B to explore innovative solutions, in synergy with the activities of the FP6 ELT Design Study.

In parallel, a thorough iteration of the optical design to take into account requirements identified during the instrument conceptual design studies should also take place and is included in the proposed plan.

The instrument studies already included here consist of a healthy mix between potential ‘work horse’ instruments (e.g. ONIRICA, MOMFIS) and more focused or specialized instruments designed to answer specific questions (e.g. CODEX) or to open an entirely new window in astrophysics (e.g. QuantEye). They confirm that a larger telescope will open up the discovery space leading to exciting new science. While not embracing all the possibilities that can be imagined, they do suggest that designing and building such instruments for a 100m telescope will be a challenging exercise which may require tradeoffs on both the instrument and telescope sides in order to reach the best compromise in terms of overall scientific performance.

After reading the report, we hope that, as discussed earlier in this summary, the reader will be convinced that the question of ‘can we?’ is answerable with a qualified ‘yes’. Qualified by ‘but it will be quite some work’ for ESO, for its community and for the whole astrophysical community at large. The next crucial step ahead for us will be to determine the telescope size that we can afford, in terms of risks, financial and human resources, and timescales for the construction. And to start designing it.