Science with 100m telescopes

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ABSTRACT

The next generation of ground based telescopes will break the 20th century paradigm of the “factor of two” diameter increase. Taking advantage of the enormous advances in technology that the present generation of 8-10m telescopes has fostered, they will be fully adaptive, fully steerable behemoths of up to 100m diameter performing at the diffraction limit in the optical and near infrared. At ten times the collecting area of every telescope ever built put together, they will have limiting magnitudes of 37-38, angular resolutions of 1-2 milliarcseconds, and a price tag that does *not* follow the historical $D^{2.6}$ cost law. In this paper I discuss some of the possible science cases for a telescope of 100m (based on the requirements for ESO's 100m concept, called *OWL* for its sharp night vision and for *OverWhelmingly Large* telescope). Among them the determination of H [not Ho] unencumbered by local effects, the study of every SN ever exploded at any z<10, the spectroscopy of extra-solar planets, studies of ultrahigh frequency phenomena, imaging of stellar surfaces, detection of brown dwarfs in external galaxies. The advent of the next generation of Extremely Large Telescopes (ELTs) will probably change substantially the operational paradigm of astronomical observations, expanding on the present trend towards Large Programs, much in the way particle physics has gone with the large accelerators.

Keywords: OWL, science with extremely large telescopes

1. INTRODUCTION

*“High angular resolution **belongs** to the ground”* (!)

The history of the telescope (figure 1) shows that the diameter of the “next” telescope has increased slowly with time (reaching a slope for glass based reflectors of a factor-of-two increase every ~30 years in the last century: *e.g.* Mt Wilson → Mt Palomar → Keck).

The main reason for this trend can be identified in the difficulty of producing the optics (both in terms of casting the primary mirror substrate and of polishing it). The advances in material production and in new control and polishing technologies of the last few decades, fostered in part by the requirements set by the present generation of 8-10 m telescopes, offer now the exciting possibility of considering factors much larger than two for the next generation of telescopes. And unlike in the past, they also offer the promise of achieving this *without* implying a lengthy (and costly) program of R&D.

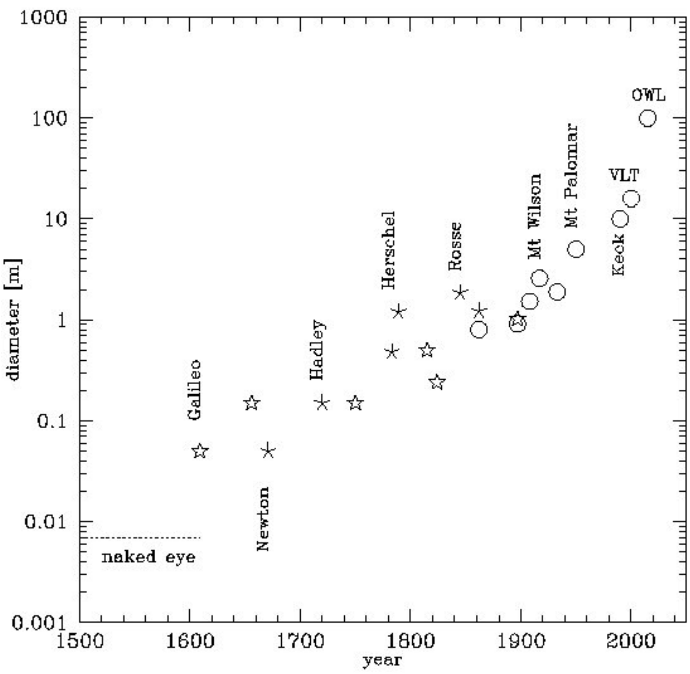

Figure 1. Brief history of the telescope. Stars refer to refractors, asterisks to speculum reflectors, circles to modern glass reflectors. Some telescopes are identified.

At the same time, advances also in adaptive optics (AO) bring the promise of being able to achieve diffraction-limited performance. Though still in its infancy, AO is growing very fast, pushed in part also by customer oriented applications. New low-cost technologies with possible application to adaptive mirrors (MEMs), together with methods like multi-conjugated adaptive optics (MCAO), new wave-front sensors and techniques like turbulence tomography are already being applied to AO modules for the present generation of telescopes. Although the requirements to expand AO technology to correct the wave front of a 100m telescope are clearly very challenging (500,000 active elements, enormous requirements on computing power), there is room for cautious optimism. This would allow a spatial resolution of the order of one milliarcsecond, prompting the claim at the beginning of this section. Of course, this is valid only at wavelengths that make sense (*i.e.* $0.3 < \lambda < 2.5$ μm for imaging, $\lambda < 5$ μm for spectroscopy).

1.1 Can we afford it (in terms of time and cost)

Another consequence of the recent advances in technology is the fact that we can consider building a next generation telescope within a reasonable time. Since a large R&D phase is not required (with the exclusion of AO, which is however being performed right now under the requirements set by the current generation of telescopes), 10 to 15 year timelines are appearing reasonable.

The cost issue is evidently one that needs to be addressed (even if a 50 or 100m telescope is demonstrably feasible from the technical point of view, it will be impossible to build one unless the $D^{2.6}$ cost law can be broken). A “demonstration” that cost can be kept at low values has been put into practice by HET (admittedly accepting reduced performance). Based on this and on extrapolating the experience of the Keck (segmentation) and of the VLT (active control), the cost estimates range nowadays between 0.3 to 1 $billion (respectively 30m CELT and 100m OWL). These costs are large (though not as large as, say, a space experiment), but possibly within what some large international collaboration can achieve.

From the point of view of “astronomical strategy”, therefore, all this would also allow perhaps to optimize the space and ground facilities according to their *natural* location (*e.g.* optical/NIR astronomy from the ground, UV or thermal IR astronomy from space, etc), stressing their complementary rather than competitive roles. And this with the possibility of a reduction in “global” costs (the cost of HST would allow to build and operate at least *three* OWLs…)

1.2 Why 100m

ESO is developing a concept of a 100m ground based telescope, called OWL (*OverWhelmingly Large*) in honor of the eponymous bird’s keen night vision. Various aspects of the current design are described elsewhere in this SPIE conference (4004-51, 4003-53, 4007-25, 4004-40, 4004-42). The question “why 100m” deserves some comments (more in the Science case section).

The original starting point for the development of the OWL concept (at the time called the WTT, alternatively for Wide Terrestrial Telescope or Wishful Thinking Telescope) was twofold. On one side a preliminary and naive science case (what is the telescope size needed to do spectroscopy of the faintest sources that will be discovered by NGST). On the other side the interest in exploring the technological limitations in view of the recent advances, especially to what limit one could push angular resolution. In other terms: could the factor-of-two become an order-of-magnitude?

The progress both of the science case and of the design concept since the early days allows us to give some answers (albeit incomplete) to the question:

i. The HST “lesson” has shown that angular resolution is a key to advance in many areas of astronomy, both in the local and in the far Universe. Achieving the diffraction limit is a key requirement of any design.
ii. Milliarcsecond resolution will be achieved by interferometry (*e.g.* VLTI) for relatively bright objects and very small fields of view. The science case (including the original ‘complementarity with NGST’ one) requirements are now, for the same resolution, field (~ arcminutes) and depth (S 35th magnitude), *i.e.* filled aperture diameters S 100m.
iii. For diffraction limited performance, the ‘detectivity’ for point sources goes as D^4 (both flux and contrast gain as D^2). One could say that a 100m telescope would be able to do in 10 years the science a 50m would take 100 years to do!
iv. Last but not least, technology allows it: the current technological limitation on diameter of the (fully scalable) OWL design is ~140m)

1.3 Feasibility issues: do we need an intermediate step

Another question that arises often is whether we need an intermediate step to whatever size we think we should achieve for scientific reasons (in other words, whether we wish to maintain the ‘factor-of-two’ paradigm even if its technological *raison d’être* has been overcome). The debate has vocal supporters on both sides (we OWLers are obviously for going directly to the maximum size required by the science and allowed by the technology). “Accusations” of respectively excessive conservatism or excessive ambition are exchanged in a friendly way at each meeting about ELTs – including the present SPIE conference. The interpretation of where exactly technology stands and how much can be extrapolated is at the core of the issue. I think this (very healthy) debate will go on for some time yet, and will be the main topic of the OWL Phase A study which is underway (goal for completion: early 2003).

1.4 Diffraction limit *vs.* seeing limit

Why make the diffraction limit such a strong requirement for ELTs is yet another subject of debate. On this our position is very strong: we consider a seeing limited ELT (deprecatingly named a “light bucket”) as a goal not worth pursuing. While it is clear that the atmosphere will not always be “AO-friendly” and that, therefore, concepts of instrumentation to be used in such circumstances should be developed, there are scientific as well as technical reasons to justify our position.

Typically the seeing limit designs go together with wide field (here wide is many arcminutes) and/or high spectral resolution (ℜS 50,000) requirements. Apart from the *overwhelming* role of the background for seeing-limited imaging (sky counts of thousands of photons per second per pixel for a 50m telescope), source confusion is a major scientific issue (see also §2.1). From the technical point of view, building incredibly fast focal reducers, or high-resolution spectrographs with collimators the size of present day telescopes, may pose technical challenges more extreme than building the telescope itself.

On the opposite side, imagers for diffraction limited telescopes need very slow *f*-numbers (50 or so, although admittedly here the challenge is to have enough detector area to cover a reasonable field, and how to avoid severe saturation from ‘bright’ sources). Milliracsecond(s) slits would make the beam size of a high-resolution spectrograph comparable to that of UVES or HIRES (*i.e.* instrumentation could be considered “comparatively” easy in the diffraction limited case).

In the seeing limited case, a spectroscopic telescope (of say 25-30m and 5,000-20,000 resolution) could occupy an interesting scientific niche. Such a design is being considered as the natural evolution of the HET (Sebring et al), and is the first one to have actually been called ELT (in other words, we have stolen the generic name from them. Another possibility for generic name is Jerry Nelson’s suggestion of calling the future behemoths Giant Optical Devices or GODs. The hint about *hubris* is quite clear…).

1.5 ELT performance

At ten times the combined collecting area of every telescope ever built, a 100m filled aperture telescope would open completely new horizons in observational astronomy – going from 10m to 100m represents a “quantum” jump similar to that of going from the naked eye to Galileo’s telescope (see figure 1).

We have built a simulator of the performance of the OWL, which can be also used for different size telescopes (and compared with similar calculations presented at this conference or at the Bäckaskog 1999 Workshop on Extremely Large Telescopes, *e.g.* Mountain et al). The simulator uses the PSF produced by the most recent optical design iteration, and includes the typical ingredients (diffusion, sky and telescope background, detector properties, and as complete as possible a list of noise sources). The output is a simulated image or spectrum (see figure 2).

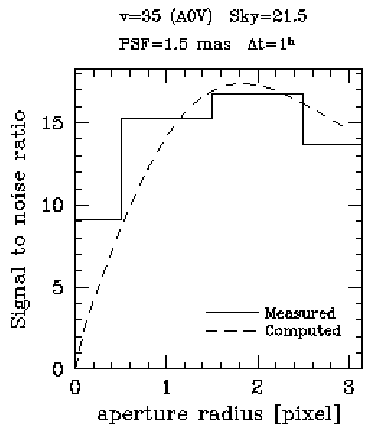

Figure 2. Output from the simulator. S/N for a 35th magnitude star in a 1-hour exposure measured on simulated image.

A magnitude limit for isolated point sources of V=38 in 10 hours can be achieved assuming diffraction limited performance (whether there *are* such isolated sources is a different question, see §2.1). Comparing this performance with the predicted one for NGST shows that the two instruments would be highly complementary. The NGST would have unmatched performance in the thermal IR, while a ground based 100m would be a better imager at $\lambda < 2.5$ μm and a better spectrograph (ℜS 5,000) at $\lambda < 5$ μm. Sensitivity-wise, the 100m would not compete in the thermal IR, although it would have much higher spatial resolution.

In terms of complementarity, OWL would also have a synergetic role with ALMA (*e.g.* in finding and/or studying proto-planets) and with VLBI (the radio astronomers have been waiting for us optical/IR people to catch up in spatial resolution for decades!)

1.6 Interferometry

Is interferometry an alternative to filled aperture? The consensus seems to be that this is not the case. Interferometry has a clearly separate scientific niche – for similar baselines its field of view (few arcseconds) and (bright) magnitude limits are definitely not competitive with the predicted performance of a filled aperture telescope. On the other side, baselines of hundreds of meters, if not of kilometers (in space even hundreds of km, as in the NASA plans), might well be the future of interferometry. Looking for the details of comparatively bright objects at the micro-arcsecond level, looking for and discovering earth-like planets, studying the surface of stars even further away are a domain where interferometry will always be first. In a sense, it is a "brighter object" precursor for any filled aperture telescope of the same size that may come in the future.

1.7 ESO's 100m OWL concept: requirements

The top-level requirements for the OWL conceptual design can be summarized as follows:

- Pupil size: 100-m diameter, collecting area > 6,000 m^2
- Multi-conjugate AO with NGS or LGS
- Diffraction-limited resolution over field of view:
  Visible > 30 arc seconds
  IR (2μm) > 2 arc minutes
- Strehl ratio > 20% in the visible, goal 30%.
- Wavelength range 0.32 - 2.5 (12) μm
- Fully steerable alt-az mount
- First light:12 years after project funding, fully operational within 15 years

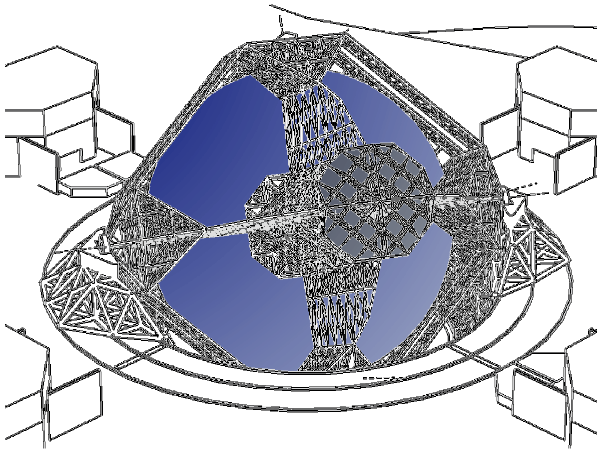

Figure 3. Current mechanical design. Seen pointing at 60^o from zenith and including maintenance buildings.

2. ITEMS FOR THE SCIENCE CASE

The science case for the extremely large telescopes of the future is not fully developed yet. Some meetings have taken place on the subject, and more are planned (there will be at least one Workshop on this in 2000). However, it is difficult to think of a branch of astronomy that would not be deeply affected by the availability of a 50 or 100m telescope with the characteristics outlined earlier.

In any event, there are a number of questions that the Science Case should pose, and find answers to, which will affect the final set of requirements for telescopes like the OWL. Do we need the angular resolution? Is 1 milliarcsecond too much, too little, enough? Is investing in AO research justified? Could we live with seeing limited? Can we not? Do we need 100m? Are 50m enough? Are 30m? Are 20m? Should we push even further? What is a sensible magnitude limit? Is interferometry a better alternative or a precursor? Do we need the optical and its tighter design tolerances and extremely more complex AO (especially since the faint/far Universe is very redshifted)? Do we have a compelling science case? Is "spectroscopy of the faintest NGST sources" enough? Is "unmatched potential for new discoveries" relevant? Is "search for biospheres" too public-oriented? Indeed, do we need an ELT?

In the following I will discuss some areas where OWL could give unprecedented contributions. This is by no means supposed to be a complete panorama, but rather reflects my own personal biases. In one case (SNe at $z < 10$) I will develop the case a little more.

2.1 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 is tied to observations obtained in the past, either from the ground or from space, with instrumentation whose angular resolution was very limited (*e.g.* the first X-ray satellites or the very deep optical images in 2″ seeing of the '80s). Recent developments have shown that whenever a better resolution is achieved, what looked like the confusion limit resolves itself in individual objects (*e.g.* the X-ray background, now known to consist mostly of resolved sources, or the HDF images, which show more empty space than objects).

Admittedly, there *may* be a confusion limit somewhere. However, the back-of-the-envelope argument that "all far galaxies are 1″ across, there are about 10^{11} galaxies and 10^{11} arcseconds, therefore there must be a point where everything overlaps" fails when one resolves a square arcsecond in > 10^6 pixels (crowding may still be an issue, though). The topic however is fascinating (and tightly connected with Olber's paradox), and will be the subject of a future paper. For the purpose of this discussion, however, the only thing confusing about confusion is whether it is an issue or not. There is a clear tendency in the community to think that it is not.

2.2 Star formation history of the Universe

This is an example of a possible science case which shows very well what the potentiality of a 100m telescope could be, although by the time we may have one the scientific problem will most likely have been already solved.

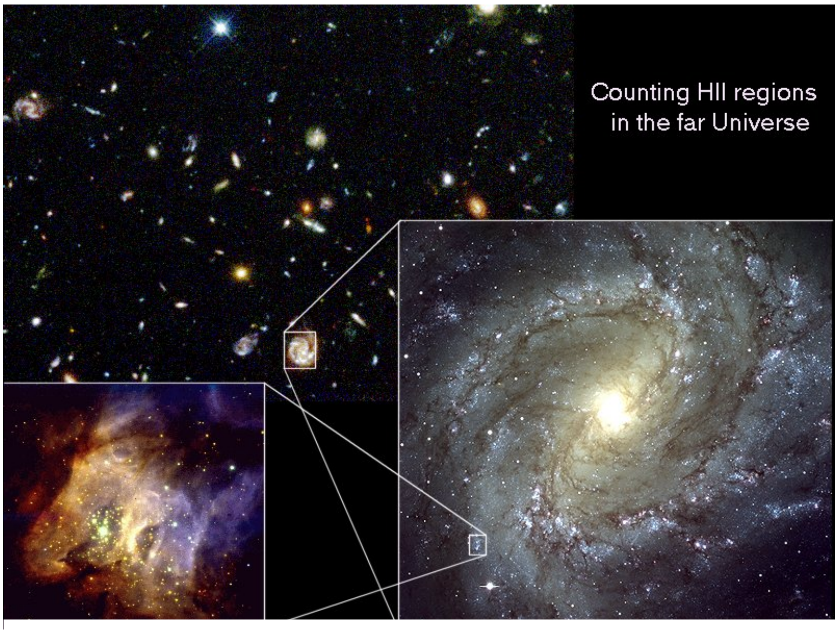

Figure 4. OWL's view of a galaxy in the HDF

The history of stellar formation in the Universe is today one of the 'hot topics' in astrophysics. Its goal is to determine which kind of evolution has taken place from the epoch of formation of the first stars to today. To do so, "measurements" of star formation rates are obtained in objects at a variety of look back times, and used to determine a global trend. These measurements are usually obtained by comparing some observed integral quantities of unresolved objects (typically an emission line flux) with predictions made by evolution models. Although the method is crude, results are being obtained and a comprehensive picture is starting to emerge.

With a telescope like OWL, what are today "unresolved objects" would be resolved in their stellar components. For example, one could see O stars out to a redshift $z \sim 2$, detect individual HII regions at $z \sim 3$, measure SNe out to $z \sim 10$ (see below). Determining the star formation rates in individual galaxies would go from relying on the assumptions of theoretical models and their comparison with integrated measurements, to the study of individual stellar components, much in the way it is done for the "nearby" Universe.

2.3 Symbiosis with NGST

This was the "original" science case for a 100m telescope, and runs much in the same vein as the case made by Matt Mountain for a 50m telescope to observe the faintest galaxies in the HDFs (SPIE *2871*, 597, 1996). The symbiosis with NGST would however not only be of the "finder/spectrometer" variety (though much science would be obtained in this way), but as explained above also in terms of complementarity in the space of parameters (wavelength coverage, angular resolution, spectral resolution, sensitivity, etc). My feeling is that a science case to complement the NGST is a strong one, but cannot be the main case for a 100m telescope.

2.4 Measure of H

Cepheids could be measured with OWL out to a distance modulus (m-M) ~ 43 (*i.e.* $z \sim 0.8$). This would allow the measurement of H and its dependence on redshift (*not* Ho) unencumbered by local effects (*e.g.* the exact distance to Virgo).

In fact, the distance to Virgo, and the value of Ho, would be determined as "plot intercept" at *t*=0! There is an interesting parallel to be done here with HST to get a "feeling" of what crowding problems we could have. Crowding would start affecting the photometry of individual Cepheids at about this distance in much the same way it does for HST images of Virgo galaxies. In fact, we would be about 100 times further than Virgo with a resolution about 100 times better than HST (Cepheids are observed with HST mainly in the undersampled Wide Field chips).

2.5 Supernovae at z ~ 10

An "isolated", *underluminous* Type II supernova like SN 1987A would be visible at (m-M) ~ 53. Assuming that crowding and/or increased background would bring the limit to 50 (*i.e.* z ~ 10, the exact value depending on one's favorite cosmology), we would still be able to detect *any* SN ever exploded out to that redshift (!).

Figure 5 shows model calculations of supernova rates assuming a 10^{12} M elliptical galaxy beginning star formation at z = 10. The rates are several dozen per year (*i.e.* ~ 0.3 per day!). Even for much less massive galaxies the rates are a few per year. This means that any deep exposure in a field ≤ 1 arcmin^2 will contain *several* new supernovae.

Since these SNe will be at high redshift, the observed light curves will be in the rest UV. This actually makes their identification easier, since Type II light curves last typically 12-24 hours in the UV: time dilation will lengthen the curves by (1+z) making them ideal to discover. (Note that the optical light curves, intrinsically some months long, would last years due to dilation).

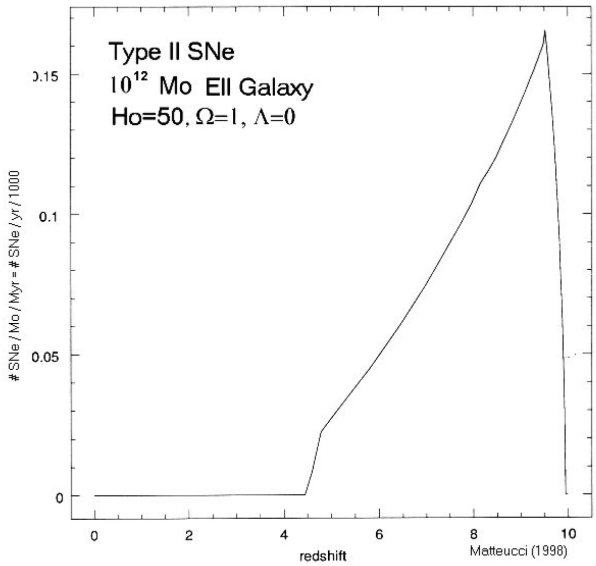

Figure 5. Type II SN rate at high redshift for a 10^{12} M elliptical galaxy (Matteucci 1998)

The study of SNe out to z ~ 10 (if indeed stars started forming at or before that redshift, which is not certain by any means) would allow to access ~ 30% of the co-moving volume (*i.e.* mass) of the Universe (at present, through SNe we can access less than 3%). Star formation rates at such early ages would be a natural byproduct of these studies. Nearer SNe would be bright enough to provide "light bulbs" to study the intergalactic medium on many more lines of sight than those provided by other bright but less common objects, *e.g.* QSOs. And of course, although with lower rates and at "nearer" distances (their rate peaks at $z_I \sim z_{II} - 2.5$), the brighter Type I SNe will also contribute to the study.

2.6 Other high redshift Universe studies

A telescope with the resolution and sensitivity of OWL's would find some of the most important applications in the study of the furthest and faintest objects in the Universe. Among many others, studies of the proto-galactic building blocks and the dynamics of their merging into higher hierarchical structures. The possibility of probing even higher redshifts with Gamma Ray Bursts (if they exist at earlier epochs) is also very exciting, as they are intrinsically orders of magnitude brighter than even SNe.

2.7 High frequency phenomena

Rapid variability is an area where the improvements brought by larger collecting areas can be truly enormous. The power spectrum of such phenomena is in fact proportional to the square of the flux, *i.e.* $P \sim D^4$. Dainis Dravins showed at the Bäckaskog Workshop that extremely large telescopes open a window on the study of quantum phenomena in the Universe which were till now only observed in the laboratory.

2.8 Nearby Universe

In the nearer Universe we have again a myriad of possible contributions. The detection of brown dwarfs in the Magellanic Clouds would enable to determine an accurate IMF for those galaxies. It would be possible to observe White Dwarfs in the Andromeda galaxy and solar like stars in galaxies in the Virgo cluster enabling detailed studies of stellar populations in a large variety of galaxies. The environment of several AGNs would be resolved, and the morphology and dynamics of the inner parts nearest to the central black hole could be tracked and understood. If the rings around SN 1987A are a common phenomenon, they could be detected as far as the Coma cluster. In our own galaxy, we could study regions like Orion at sub-AU scales, determining the interactions between stars being born and the parent gas. We would detect protoplanetary disks and determine whether planets are forming there, and image the surface of hundreds of stars, promoting them from points to objects. Unlike interferometry (which also can image stellar surfaces, but needs many observations along many baselines to reconstruct a "picture") these observations will be very short, allowing the detection of dynamic phenomena on the surfaces of stars other than the Sun.

2.9 Extra-solar planets

Finally, a critical contribution will be in the subject of extra-solar planets. Not so much in the discovery of them (I expect that interferometry will be quite successful in this), but rather in their spectroscopic study. Determining their chemical composition, looking for possible biospheres will be one of the great goals of the next generation of ELTs. Figure 6 shows a simulation of an observation of the Solar System at 10 parsecs (based on the PSF of an earlier optical design, and including the effect of micro-roughness and dust diffusion on the mirror) where Jupiter and Saturn would be detected readily. Several exposures would be necessary to detect the Earth in the glare of the Sun. Sophisticated coronographic techniques would actually make this observation "easier" (or possible at a larger distances).

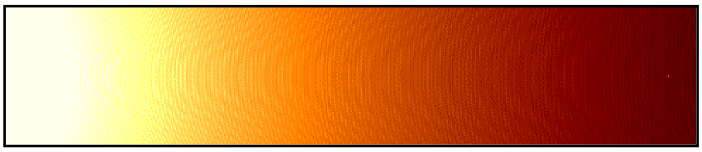

Figure 6. Simulation of the Solar System at 10 parsecs. Jupiter can be "clearly" seen on the right. Saturn would also be detected, about 10 cm on the right of this page.

3. OPERATIONAL ISSUES

The sheer size of a project like OWL, or any other ELT project, makes it unlikely that the operational scenario would be similar to that of the current generation of telescopes. I believe that the current (mild) trend towards Large Programs (where the need for deep – *i.e.* long – exposures is combined with the statistical requirement of a large number of measurements) will evolve towards some sort of "Large Project" approach, similarly to what happens in particle physics. In this sense, maybe even the instrumentation plan could be adapted to such an approach (*e.g.* a Project would develop the "best" instrument for the observation, and when it is over a new Project with possibly new instruments would take over). What I imagine is "seasons" in which OWL (or whatever) will image the surface of all 'imageable' stars, or study 10^5 SNe, or follow the dynamics of the disruption of a star by an AGN's black hole. In other words, a series of self-contained programs which tackle (and hopefully solve!) well defined problems, one at a time.

4. CONCLUSIONS

In this paper I have tried to give a 'flavor' of the kind of exciting science that could be performed with a telescope the size of a soccer field. I have kept the discussion at a fairly non-specialist level given the interdisciplinary nature of the SPIE conference. From the technical point of view, no obvious showstoppers to build a 100m telescope have been identified so far. The price tag of many ELTs remains below the cost of a medium space mission, so we could call it "reasonable". The timeline for construction is around 10 years. Industry is indicating that there is an interest in building one, and that they agree about its feasibility. The science case is exciting and stunning, and there is an unmatched potential for new discoveries. Let's do it!

Acknowledgements: The OWL concept is the brainchild of the OWL people. I am indebted in particular to P. Dierickx, B. Delabre, E. Brunetto, M. Quattri, F. Koch, G. Monnet, and N. Hubin for the many discussions and for their work. I also wish to thank M. Mountain, J. Nelson, J. Spyromilio and T. Sebring for useful (and lively!) discussions.