Scientific Requirements for Extremely Large Telescopes
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Scientific requirements for ELTs: exoplanets & star formation parallel session summary

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Abstract. I present a brief summary of the parallel session on exoplanets and star formation, and how their study drives the science requirements for extremely large ground-based telescopes. I also offer a few thoughts on the development of these highly ambitious and highly expensive new astronomical facilities, as viewed from the perspective of someone also involved in the James Webb Space Telescope.

Keywords. Planetary systems; Stars: formation; Instrumentation: high angular resolution; Telescopes

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1. Introduction

The next generation of extremely large ground-based optical-infrared telescopes (ELTs) should prove very important for the study of the birth and early evolution of stars and their planetary systems, as well as the characterisation of mature exoplanets. The current 8–10 m telescopes such as the VLT, Keck, Gemini, and Subaru have all made major contributions in this field, not only because of the increase in collecting area over the 4 m telescopes, but also through a combination of improved instrumentation, infrared optimisation, and better control of local seeing via good dome design, and active and adaptive optics techniques. Similarly, the step up to 30–40 m diameter telescopes will not only yield many more photons, but also much higher spatial resolution, more massive multiplexing, higher contrast and dynamic range: the potential for breakthrough observations is considerable.

However, the detailed requirements for exoplanet and star formation (E&SF) research are not necessarily the same as for other core science areas to be addressed by ELTs, with certain key aspects of E&SF research, in particular exoplanets, being driven more by the need for very high spatial resolution and contrast than by pure collecting area. Indeed, as a consequence, some have argued that the next cornerstone ground-based facility for E&SF research at optical/infrared wavelengths should be a very long-baseline (kilometric) interferometer with large individual elements, a super-VLTI, rather than a 'small' 30–100 m filled-aperture telescope (see, e.g. Labeyrie *et al.* 1988; Mountain 1996). However, at IAU Symposium 232 at least, it was taken as a given that any such interferometric facility is in the development queue well behind filled-aperture ELTs, and talks presented at the Symposium demonstrated that these ELTs will indeed enable a very wide range of key E&SF science.

2. Contributions

E&SF research was represented in the plenary session at IAU Symposium 232 via an excellent introductory overview by Karl Stapelfeldt, as well as a summary of the recent

IAU Colloquium 200 on the direct imaging of exoplanets held in Nice, presented by Andreas Quirrenbach. The one-day parallel session then included 11 contributed talks by Hans Zinnecker, Derek Homeier, Rainer Lenzen, Céline Cavarroc, Alessandro Berton, Matt Burleigh, Jim Hough, Penny Sackett, Christophe Vérinaud, Ulli Käufl, and Jean-Luc Beuzit.

These talks are written up elsewhere in this volume, but it is worth making one important comment about the spectrum of scientific interest covered. Specifically, the contributions were very heavily concentrated on exoplanet science, with relatively little said about the formation and evolution of stars and planets. However, it would be wrong to infer that people working in the latter field are uninterested in ELTs or that they will not be useful for such research. The real reason for this limited participation of 'star formation astronomers' in IAU Symposium 232 was that the *Protostars & Planets V* meeting was held in Hawaii just three weeks earlier, with over 800 astronomers and planetary scientists taking part. This numbers underlines the great community interested in the field, but the long haul to South Africa shortly afterwards was probably just too much for more than a few hardy souls. In addition, the geographical location of South Africa was likely responsible for the relatively limited participation of North American and Asian astronomers in the E&SF parallel session compared to Europeans: the science cases of all major ELT projects include strong E&SF components, independent of continental origin.

3. Synthesised requirements

It is beyond the scope of the present summary to be too specific about the requirements for ELTs to be used for E&SF research, but more quantitative information can be found elsewhere in this volume and in the various science case documents developed for the major proposed ELT facilities. Here, however, it is worth looking at the general requirements for star and planet formation on one hand, and exoplanet discovery and characterisation on the other. This split between the two cases is justified on the grounds that the former needs a broad range of 'workhorse' instruments very often operated in classical 'few nights per project mode', while the latter will need highly optimised, dedicated instrumentation and may often require long 'campaign mode' observations.

3.1. *Requirements for star and planet formation*

Embedded protostars, young stars, binaries, clusters, circumstellar disks, jets, and outflows in the Galaxy span a wide range in properties such as brightness, size, spectral energy distribution, clustering and crowding. Correspondingly, they cover a broad range in observational space, including limiting sensitivity, wavelength coverage, spatial resolution, field-of-view, contrast, and so on. For much the same reason, there are no 'killer applications' which set specific, concrete requirements on the capabilities of an ELT used in this field, in contrast to some cosmology or exoplanet experiments, as seen below. In particular, there appears to be no hard limit to the minimum diameter of an ELT below which some absolutely vital observations become impossible.

A number of general points relating to star and planet formation observations, some more obvious than others, were discussed in the various presentations and in and around the parallel session, as follows:

• Large filled-aperture telescopes are highly desirable, enabling high sensitivity to a mixture of faint point and diffuse sources along with high angular resolution imaging over a full range of spatial frequencies.

• The great majority of star and planet formation observations will be made in the near- and mid-infrared due to the relatively low effective temperatures of young protostars, stars, brown dwarfs, and planetary-mass objects, and the considerable dust extinction often associated with them. Thus infrared optimisation of ELTs is important, but at the same time, care should be taken not to place onerous requirements on an ELT at wavelengths (e.g. 20–30μm) where an extremely high and dry site would immediately be implied: JWST will be highly competitive with moderate-sized (30–50 m) ELTs at these wavelengths and such observations may best be left to it.

• There are good arguments for extending the instrumental capabilities out into the sub-millimetre for wide-field surveys of star-forming regions, strongly complementing ALMA's smaller field but higher spatial resolution (e.g. the SCOWL instrument studied for OWL).

• Star and planet formation projects would likely include a mix of narrow-field, single object observations (e.g. of an embedded proto-massive star) and wide-field, multi-object observations (e.g. of planetary-mass objects in young clusters), with fully-sampled imaging photometry, multi-object spectroscopy, integral field spectroscopy, and coronography techniques being employed as appropriate.

• Wide-field observations with (very good) seeing-limited pixels would be useful in some instances (e.g. surveys for free-floating planetary-mass objects in the field), although the corresponding instrument sizes may be prohibitive. In general, however, as spatial resolution is important in more crowded regions, the instruments used for star formation observations should be fed by 'normal' facility adaptive optics (AO) systems, ranging from ground-layer AO, single object natural and laser guide star AO, to multi-conjugate and multi-object AO, as required. One unknown with many of these newer AO techniques is the accuracy with which photometry and astrometry can be achieved.

Overall, it is clear that this mix of observations would best be served by 'general-purpose' type instrumentation, similar to that found on today's 8–10 m telescopes. It is also very likely that many projects could be completed in a few nights of observing time, thus pointing towards semi-classical observing, in either visitor or service mode.

Finally, it is important to note that the greatly enhanced sensitivity and spatial resolution of ELTs would make it possible to extend these studies to coverage of the whole of the Milky Way and out into the Local Group. In particular, resolved imaging and spectroscopy of stars in extremely massive young clusters such as Westerlund 1 in our galaxy, 30 Doradus in the LMC, and those in the giant OB association, NGC 206, in M 31, will help develop a much more detailed picture of how the physical processes of star formation scale from low-mass T-associations through OB associations to starbursts, as a function of density, metallicity, and so on. As the nearest large external galaxies such as M 31 lie at irreducible distances, a careful assessment of the sensitivity and spatial resolution requirements implied by extragalactic star formation projects is needed.

3.2. *Exoplanet requirements*

While the range of scales, distances, and brightnesses associated with galactic star formation necessarily leads to a broad range of instrumental capabilities, the potential discovery and characterisation of exoplanets with ELTs is a more narrowly constrained problem and thus has more specific requirements. In particular, the nearest stars to the Earth lie at known distances, have known spectral types, brightnesses, and (to some extent) ages. Thus it is possible to predict the absolute brightnesses of exoplanets in orbit around these stars as a function of their masses and physical separations and, equally importantly, the brightnesses of such exoplanets relative to their parent stars.

Again, specific numbers can be found elsewhere in this volume, but some general considerations can be listed here.

- The most fundamental requirement is for extreme contrast, in order to be able to detect the exoplanet against the glare of its parent star. Detailed analyses suggest that this will require several stages of suppression, with extreme AO, followed by coronography, and then potentially multiple stages of differential imaging (e.g. in and out of specific planetary absorption features, via polarimetry, and so on). Even then, residual, slowly time-variable aberrations in the optical train may prove extremely difficult to overcome.
- Extreme AO systems will profit from the de facto presence of a very bright star very close to the optical axis, but even then, there will be very few photons available to each element of the wavefront sensors per time slice, given the very high sampling of the pupil and high frame rates that will be required to reach the necessary Strehl ratios and contrasts.
- The required fully AO-corrected field-of-view is very small in almost all cases (a few arcsec), as only single targets will be observed: this serves to alleviate the AO requirements to some extent.
- The choice of wavelength will depend largely on the types of planets under investigation, as the key spectral tracers will differ for gas giants and rocky planets. Broadly speaking, gas giants will be observed in the near- and mid-infrared, while terrestrial analogues will be searched for at optical to near-infrared wavelengths. Interesting new ideas for possible tracers include OH airglow in the upper atmosphere of Earth-like planets (see Käufl, this volume).
- Beyond direct imaging experiments, there are a number of potential exoplanet projects which would use an ELT purely as a light bucket to collect very large numbers of photons. These would include very high precision radial velocity studies searching for terrestrial planets via stellar reflex motions (e.g. following up distant transit discoveries made by the Kepler satellite), as well as the detection of reflected light via polarisation and atmospheric spectroscopy via transit monitoring.

By now, there have been a reasonable number of detailed studies looking at the plausibility of direct imaging of exoplanets using ELTs, including several in this volume. Some have been essentially theoretical and analytical, looking at the issue from first principles (e.g. Chelli 2005), while others have also included extrapolations of the results from current AO systems and other technologies developed for planet-hunting (e.g. Dekany, Stapelfeldt *et al.* personal communication).

There is an emerging consensus that the ultimate goal of imaging exo-Earths in the habitable zone around nearby stars would be very challenging indeed, even for a 100 m diameter OWL-like telescope and very optimistic extrapolation of current noise suppression and contrast enhancement technologies. In this volume, for example, Karl Stapelfeldt argues that it may be impossible to push to contrasts of better than 10^8 with ground-based ELTs working against the atmosphere, and thus that terrestrial exoplanet detections requiring contrasts of closer to 10^{10} would be ruled out.

At IAU Symposium 232, it was announced that ESO will no longer be looking at a 100 m OWL, but will now concentrate its efforts on a rebaselined ELT in the 30–60 m diameter range. Given this and the 25–30 m diameter facilities under discussion in North America (e.g. TMT, GMT) and elsewhere (e.g. the Japanese ELT), it appears likely that direct imaging of terrestrial planets will no longer be a headline science goal for ELTs. Nevertheless, it was quite clear from the presentations made at the Symposium that substantial and crucial exoplanet science will be carried out with ‘smaller’ telescopes, studying the physical and chemical properties of gas and ice giants, and so-called super-terrestrial rocky planets.

Achieving these goals will still require substantial investment in test-bed AO, coronography, and differential imaging systems, as well as exceptionally good optical performance from the ELT itself. Thus it seems likely that they will only be reached quite some time into the life of an ELT, through second generation instrumentation. A key strategic question to be considered in the mean time is how to phase the development of ELTs and their extreme AO systems appropriately so that they can take full advantage of experience gleaned from high contrast AO systems such as Planet Finder for the VLT and the GPI/ExAOC instrument for Gemini. Both are currently under development, but will likely not yield results until 2010, relatively late in the schedules of most ELTs if any major design revisions become identified.

4. Closing thoughts

The impetus towards the construction of a new generation of Extremely Large Telescopes has grown considerably over the past few years, to the point where it now seems almost inevitable that a small handful of facilities in the ∼25–40 m diameter range will be built. It is recognised that substantial technology development is required in order to implement such telescopes, their AO systems, and instrumentation, and that they will necessarily be very expensive. But the growing consensus in the community appears to be that such investments are worthwhile, as a broad (and also growing) range of key science topics have been identified which will benefit enormously from these ELTs.

Thus, to an extent, the various ELT projects are in a phase where expectations and enthusiasm are rising more rapidly than the corresponding cost in terms of money and engineering, as illustrated schematically on the left side of Figure 1. Interestingly, the same was true of the NASA/ESA/CSA James Webb Space Telescope some five years ago, when it was still the NGST, 8 m in diameter, and identified as a very high priority for implementation by astronomers and agencies in North America and Europe. Since then, JWST has turned into a real project, with large sums of money (currently ∼\$1M per day) being spent at industrial engineering contractors and government labs designing the mission and developing its advanced technologies.

Very significant progress has been made on JWST, with the delivery of extremely good new infrared detectors, the ongoing fabrication of ultralightweight beryllium mirror segments, the development of sophisticated wavefront sensing and control systems, new cryogenic systems, and so on. However, as the substantial technology challenges have become better identified and quantified, costs have risen considerably (now to ∼\$5G over the full lifetime of the project). Consequently, hard decisions have been made over the past few years in order to contain cost, risk, and schedule, including most notably descoping the primary mirror from 8 m to 6.5 m diameter and some relaxation of the short wavelength ($<1\mu$m) performance requirements.

Thus, while still an enormously capable observatory when compared with existing facilities on the ground (e.g. the 8–10 m telescopes) and in space (e.g. HST and Spitzer), JWST has reached a rather common phase in project life where the cost-benefit curves begin to look less rosy than they were at the peak of pre-implementation exuberance, as shown on the right side of Figure 1. ALMA is approaching a similar position, as its costs rise and the number of antennas is reduced.

In addition, cost growth in highly visible projects brings intense political scrutiny, as JWST is discovering with repeated questioning on its status in the US Congress, for

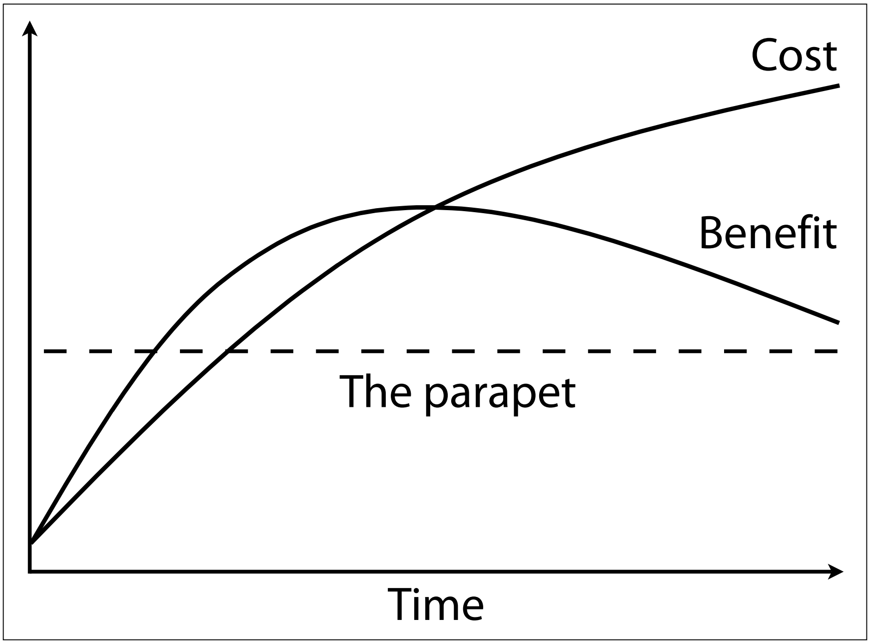

Figure 1. A purely schematic, non-quantitative illustration of the rise and fall in the cost-benefit ratio of a major project as it transitions from PowerPoint slides to real engineering solutions. Sticking your head above the parapet and into political visibility can often prove to be a risky business.

example. The worst case scenario in such situations is outright cancellation: the Super Conducting Super Collider is the seminal case of a scientific mega-project growing too far in cost and visibility (Riordan 2001), although to be fair, at its cancellation in 1993, the SSC had grown to roughly \$15G from its original 1982 cost of ~\$6G (both in 2005 dollars), enough to buy the JWST three times over.

Nevertheless, the analogy is clear: the proposed ELTs are also highly visible, very expensive science projects facing considerable technology challenges, and as they take real shape over the coming decade, costs will almost inevitably rise and compromises in the scientific returns will have to be made. Thus, it is my opinion that in order to minimise the dangers such events may bring, we need to be careful *now* how we manage the expectations and hopes of the astronomy community, politicians, and the general public. Our most ambitious and far-reaching headline science aims for big, fully diffraction-limited, high contrast ELTs, but must be pragmatically balanced by solid goals achievable even with smaller, less 'perfect' facilities, which will nevertheless dwarf any existing telescopes in terms of their size and grasp. As long as we are level-headed now, I am confident we will overcome the inevitable engineering, financial, and political hurdles, and that by 2015, operational ground-based ELTs will be working with an L2-stationed JWST in a complementary, synergistic fashion to answer some of the outstanding questions of modern astrophysics.

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Discussion

ZINNECKER: I would like to add an aspect that you perhaps did not have time to emphasize. There is a continuity of ELT science cases from star formation to stellar populations and galaxy evolution. Not only do we need to study star formation in local examples of obscured star forming regions, but also on a global scale (dusty galactic nuclei, high-z starburst, etc). Angular resolution and sensitivity in the near- and mid-infrared are as important in the latter is in the former.

MCCAUGHREAN: Agreed

DRAVINS: High time resolution is one (among several) obvious ELT applications that hasn't been much discussed at this meeting. For example, to study the internal structure of neutron stars, one would like to measure the temporal spectrum of their pressure-mode oscillations. Characteristic timescales are set by the high speed of sound, and the small dimensions, leading to frequencies on the order of many Kilohertz. Such, and numerous other high-speed applications would warrant a symposium on their own. Other, potentially large, fields largely left out include stellar physics. At present, almost all stars are observed as point sources, but ELTs will make hundreds of them appear as surface objects, opening up many new fields of study (imagine how poor extragalactic astronomy would be, were all galaxies seen merely as point sources...).

MCCAUGHREAN: Thank you for that clarification.

MOLARO: I would like to know if the radial velocity (RV) technique to detect planets has been discussed in your panel. The CODEX spectrograph has been inspired by HARPS and it will improve the HARPS performances from the 1 m/s to the 10 cm/s regime. Would CODEX be useful for the planetary research?

MCCAUGHREAN: Yes, the RV technique will clearly benefit from an ELT and has the advantage of allowing operation in seeing-limited 'light bucket' mode. Higher velocity sensitivity will enable the detection of lower-mass planets, while the larger light-gathering power means we can detect planets at larger distances, including perhaps those detected in transits by Kepler and other surveys. Of course RV surveys take a lot of observing time.