EUCLID

Euclid: Post-Referee Inputs – Referee #6

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Reference Documents

1	*Euclid* Phase A Proposal to STFC, MSSL/*Euclid*/AD/001.03		11 May 2008
6	*Euclid* Referee Report #6	received	19 June 2008

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1. Scope

This document responds to the Referee #6 report [ref 6] in response to the *Euclid* Phase A proposal for Cosmic Vision (MSSL/Euclid/AD/001.03) [ref 1].

2. Response

We include the full text of the referee's report below, in blue, with our responses for each section. Some re-paragraphing has been carried out, but the order of the text remains unchanged.

2.1 Proposal details

Name of PI seems to be Prof Mark Cropper but the e-mail from STFC also says Prof John Peacock is PI.

The PI is M Cropper.

2.2 The Proposal:

This is very high quality science indeed if it can be accomplished.

Euclid is an ambitious mission: it would be surprising and disappointing if it were not. The feasibility of all such missions is a matter of judgment at the outset, requiring some adjustment of horizons. Looking at the track record of both ESA and NASA, in general the ambitious goals are met or exceeded.

It's easy to cast doubt on whether a mission can be accomplished. However, the situation is relatively clear in this case: ESA advisory group stucture believe it can be done, the ESA Concept Advisory Team (merging *DUNE* and *SPACE*, and composed of Dark Energy specialists) believe it can be done, the ESA engineering team carrying out the reference design at the Concurrent Design Facility believe it can be done, and the proposers for the missions at the time of the Cosmic Vision submission believe it can be done, and backed this conclusion with a huge simulation effort by many Dark Energy and instrumentation specialists. NASA have similar sorts of missions in their JDEM programme – they believe it can be done.

Technically speaking, *Euclid* is low risk, with all elements (except for the DMDs) at a good TRL. Much of the same ground has been covered by the much more ambitious *Gaia* mission. It is true that *Euclid* is complex, and the payload will be expensive. Shaking this down, while retaining the core science performance, is the aim of the phase A, and is why scientific institute groups such as those requesting support in this proposal are at the heart of this process.

The study of dark Energy and Dark Matter remain of the highest priority in cosmology. Unfortunately the case as presented does not make it clear whether EUCLID will be significantly better than either ground or space based supernova searches that can be expected to be completed by 2020, or any other Dark Energy missions that are under study by NASA and the DoE.

We believe we did do this, in Section 2.3. We provided in Table 2-1 a (largely complete) list of all of the other projects and missions, specifically including, for example NASA/DOE projects such as *SNAP* and *ADEPT*. Then we provided in Table 2-2 *Euclid's* Figure of Merit improvement over other Stage IV projects (those in the *Euclid* timeframe). We noted further that beyond the FoM, knowledge and control of systematic effects are critical for this work, and that, by combining the imaging and spectroscopy, *Euclid* is much better placed in controlling these than any other currently proposed Dark Energy project or mission.

We will provide at the presentation, an enhanced version of Table 2-2, the FoM for *Euclid*.

The route proposed is a sound one but depends strongly on being able to design an instrument which is extremely stable in its imaging performance so as to detect weak lensing signals unconfused with subtle instrumental effects. Whether this can be done as a medium mission is not clear. It may be much more expensive.

Yes, we agree with this. As noted above, this is the aim of a phase A, and why it is essential to have the deep involvement of groups from scientific institutes.

One cannot say that the approach is especially novel, but that is no great disadvantage as it builds on ground based experience at Edinburgh. There are a number of serious technical challenges which will have to be addressed by means of this phase A study before December 2009. The optical configuration is extremely complex and does not yet seem to have fully integrated the Dune and Space ideas. In particular the merging of the IR components with their cooling requirements with the optical side seems to need a great deal of work. Technical challenges (which are not always bad things) exist in the CCD development and the DMD's to bring them up to space standard and demonstrate TRL 4 by the end of next year.

We agree with this in general.

We remark additionally that a Medium mission is *required* to be as low risk as possible; the approach builds on experience based beyond Edinburgh, within the other UK institutes, and within the instrument consortia Europe-wide. It also has the endorsement of ESA.

The integrating of *DUNE* and *SPACE* was under ESA control, and we believe it has been undertaken in a sensible way so far, preserving the science performance. Also, sensible interfaces have been maintained, and the payload can be built in units. We are at the beginning of the project, and an important role of the phase A is rationalise and simplify the mission, and no doubt a better integration of the instrumentation will be an outcome.

The management structure does not seem ideal for this ambitious task. MSSL have a fine track record in certain areas of space instrumentation but the crucial systems issue is the system performance needed to deliver the weak lensing signal with no instrumental biases. MSSL have no experience in this level of optics or data reduction and therefore their position in the systems role may suppress this central goal. The teams from Edinburgh and Durham have strong experience in these issues and the ATC has a good track record in producing astronomical instrumentation of high quality. Notwithstanding those remarks MSSL's experience in CCD work is strong and with Andew Holland's assistance they should be able to do a good job.

The referee may not be sufficiently familiar with the approach required by ESA for this project. We have explained this in Section 9 and in the organisation chart (Figure 9-1) in the proposal [ref 1]. MSSL is not undertaking the system role in this project: the system roles for each instrument are delivered at the instrument consortium level (*i.e.* European-wide). Each of the UK technical groups participate in the system activities in the different instruments, and the UK science groups support both the instrument consortium groups and the ESA *Euclid* Science Study Team. There is appropriate and unique expertise in the UK groups to provide these inputs. As the largest requestor to STFC for *Euclid* phase A funding in the proposal, MSSL's role is to provide a single interface to STFC (as required by STFC) for the coordination of *Euclid* activities: MSSL has the requisite experience to do this.

Having said this, the referee appears not to be aware of MSSL's optics and systems role in many optical instruments currently operating in orbit (as opposed to the ground) including *XMM*-OM, *Swift*-UVOT, *Hinode*-EIS, and planned, including *JWST* NIRSpec and *Gaia* RVS, in many of which the PI was personally involved. In data reduction, MSSL is active in many areas, including, in the astrophysics area, *XMM*-SSC, *Gaia*-DPAC, *Swift*; participation within Astrogrid, and several other Solar, space plasma and Earth remote sensing data analysis systems.

The role for industry in the final production of flight equipment is not clear. Given that this is now a major goal for STFC to demonstrate its Knowledge Exchange performance, more could be asked about who will do this in the UK. Who will build the optics and the spectrographs in UK industry as opposed to UK research groups?

We explained the role for industry quite extensively in Section 14 of the proposal [ref 1].

We accept that we didn't address the industrial options for the optics. Some of these optics will be fabricated by UK research groups (can only be fabricated by them) and some will go to optics industry within the UK. There will be some optics for which there will not be any UK industrial expertise, and these will be sourced abroad within Europe. It should be made

clear that not all of the spectrographs will be built in the UK, and the exact share will depend on the work done by UK groups in this phase, and on UK funding levels.

The outreach plan is fine as far as it goes but it concentrates mainly on the legacy science not the DE problem which the public is indeed very aware of

We believe we addressed both the Dark Energy (2 paragraphs) and legacy science (1 paragraph) in Section 15 of the proposal.

2.3 Area of Research

This proposal is consistent with world efforts to gain an understanding of Dark Energy although the comparison with the NASA/DoE projected mission is somewhat fudged. The international standing of Taylor and Peacock is excellent in this area and Durham's work on spectrographs is well recognised. Some of the hyperbole in the proposal (" Close-by, EUCLID will address all areas of galactic structure and low temperature objects…") is misplaced and unnecessary. We are not left with a very secure understanding of exactly where EUCLID will stand, especially in comparison to ground based efforts by 2020 and this is a pity.

We have addressed the comparison to NASA/DoE missions, and the comparison to other efforts in Section 2.2 above.

Perhaps we did slip into unnecessary hyperbole in the proposal in one or two places… Nevertheless, *Euclid* will in fact provide a unique and very substantial resource for many areas of astronomical research and will be widely used.

2.4 The Research Team

The research team is strong in all relevant areas , though the expertise is spread between different institutes. I believe that the applicants can deliver significant input to the Phase A process. My main concern is whether they can retain the missions objectives within the mass and cost constraints of a medium mission.

We appreciate the endorsement of the team's capabilities.

Regarding the maintenance of the mission objects within the mass and cost constraints, we note that this will be carried forwards by the instrument consortia *i.e.* Europe-wide, in which the UK will play a strong role if funded sufficiently. We are fully aware of the end-goal of this process: to fly a scientifically competitive mission within a competitive timeframe funded by resources that are actually likely to be available.

2.5 Resources

These seem appropriate in the main but they are inflated somewhat by the number of separate institutes involved.

The number of institutes involved is an indication of the scale of interest in the UK community in the potential of *Euclid*.

Given the low level of oversight we envisage of the science groups (Section 9 of [ref 1]), we believe the expertise is delivered more efficiently by using the best capabilities of different groups than requiring them to be concentrated in only a few groups. On the technical side, the number of groups is relatively small for the scale of the project.

Given that SN do not appear in the science case and we are given no indications what EUCLID will be able to do in respect of SN detection it seems bizarre to ask for travel money for Oxford to explore this aspect of the mission. Nonetheless it is a small sum.

We accept this is an omission. We will make this clear to the Visiting Panel.