# Post-Decadal White Paper: A Dual-Satellite Dark-Energy/Microlensing NASA-ESA Mission

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## ABSTRACT

A confluence of scientific, financial, and political factors imply that launching two simpler, more narrowly defined dark-energy/microlensing satellites will lead to faster, cheaper, better (and more secure) science than the present EUCLID and WFIRST designs. The two satellites, one led by ESA and the other by NASA, would be explicitly designed to perform complementary functions of a single, dual-satellite dark-energy/microlensing "mission". One would be a purely optical wide-field camera, with large format and small pixels, optimized for weak-lensing, which because of its simple design, could be launched by ESA on relatively short timescales. The second would be a purely infrared satellite with marginally-sampled or under-sampled pixels, launched by NASA. Because of budget constraints, this would be launched several years later. The two would complement one another in 3 dark energy experiments (weak lensing, baryon oscillations, supernovae) and also in microlensing planet searches. Signed international agreements would guarantee the later NASA launch, and on this basis equal access of both US and European scientists to both data sets.

## 1. Introduction: Faster, Cheaper, "Better"

In an earlier pre-Decadal-Survey white paper (Gould 2009), I advocated a dark energy (DE) space mission that would perform only one DE experiment, as opposed to a "mega-mission" trying to simultaneously perform 3 experiments (with fairly incommensurate optimal design specifications). Regarding DE per se, I expressed indifference as to whether this satellite should carry out a weak lensing (WL), baryon acoustic oscillation (BAO), or supernova (SN) experiment. I noted that all 3 have advantages and disadvantages. However, I pointed out that satellites designed for WL were actually optimal for also doing microlensing ($\mu$L) planet searches, and even the times of year when WL and $\mu$L targets are available were noted to be complementary.

The basic point was that a simple satellite would be launched sooner and, by providing earlier results, stimulate both theoretical ideas on DE and broader interest in (and so broader support for) new missions that could carry out additional DE experiments. By contrast, the more complex satellite designs that seemed to be taking shape on both sides of the Atlantic would lead to technical delays that would inevitably be compounded by the adverse financial environment. Analogy was made to WMAP, which was clearly both "faster and cheaper" and, as a result, "better" than a more complex mission because it produced earlier results that stimulated interest in later, more complex missions.

The recommendations of the Decadal Committee (Blandford et al. 2010) were in basic accord with the viewpoint advanced in my white paper. The WFIRST design is far simpler than the do-everything (and so, ultimately, do-nothing) mega-missions that were emerging. It is true that the WFIRST design still envisages 3 DE experiments, but it achieves this by somewhat restricting WFIRST's capabilities in favor of planned synergies with ground-based observations.

However, the purely space-based requirements on WFIRST are still somewhat contradictory. Most notably, the pixel scale must be a compromise between the high-resolution needed for WL and the large angular area needed for BAO and SN. $\mu$L is the main beneficiary of this compromise, since its pixel-scale requirements are intermediate. Moreover, as I will summarize in the next section, WFIRST does not fit into the immediate financial priorities of the US, which substantially diminishes its independent role in pushing forward DE science and so, indirectly, diminishes the chance that it will ever be launched.

## 2. New Requirements: Faster, Cheaper, "Better"

### 2.1. NASA Response to Astro2010: Germ of an Excellent Approach

NASA's basic response to Astro2010 has been
1) Great idea!
2) Budget does not permit immediate start on WFIRST ($1.5B)
3) Can implement Decadal Science by
  A) Modest (20–33%) share in EUCLID
  B) Begin work on WFIRST after JWST is launched

This is a reasonable response given NASA's fiscal constraints, but in its current form, makes no sense scientifically. For example, if EUCLID is launched in its current mega-mission form, the scientific drivers for launching WFIRST 5 years later will be muted at best. And this muting, combined with financial pressures and other competing scientific agendas, means that WFIRST would in fact never be launched.

Nevertheless, this response does contain the germ of an approach that can yield outstanding science, while taking account of the financial and political realities. As in the NASA response, there would be two satellites, but these would be explicitly designed to be complementary (constituting a single "mission") and would be linked by binding international agreements. The first satellite, undertaken by ESA, would be a purely optical wide-field version of EUCLID, optimized for WL, i.e., with excellent resolution. The second satellite, undertaken by NASA, would be a purely infrared telescope, basically in the model of WFIRST but with weaker constraints on image resolution (and so with larger field of view). In the following subsections, I outline how this "dual-satellite mission" is nearly optimal from a science standpoint, while meeting all the financial and political constraints.

### 2.2. Science

There are four major scientific objectives, WL, BAO, SN (for DE) and $\mu$L (for planets). I treat these in turn.

WL would benefit the most. If the ESA mission were simplified to a wide-field optical imager, it would be substantially cheaper and easier to build and so would be launched sooner. Freed from the burden of infrared (and other) add-ons, it could probably have a larger field or a larger mirror or a longer operational lifetime (or all three), but in any case would be launched faster and at lower risk. There would be immediate science return. It is true that the science would not be as strong as it would be with complementary infrared (IR) photometry for photometric redshifts, but these would eventually be forthcoming when WFIRST was launched. So there would be initial excellent returns and improvements after several years. The second stage of analysis based on IR data is not likely to be quickened (and may be further delayed) by having a single EUCLID mega-mission because it would be more complex, more expensive, and more subject to delays or even cancellation. Hence, there are substantial gains, reduced risk, and very little lost with this approach.

BAO and SN would also benefit. The basic point here is that the WFIRST-like mission in this scenario is launched on the same timescale as within the NASA response. Its IR capability would be crucial to BAO photo-$z$'s for $1.5 < z < 2.5$ and for rest-frame-IR SN lightcurves at $z < 0.8$ (in addition to WL photo-$z$'s mentioned above). WFIRST's ability to meet BAO and SN objectives, by coordinating with ground-based observations is exactly the same as within the Decadal Committee's recommendation. In fact there are two improvements: first the field of view can be somewhat larger because the IR imager pixel size is not constrained by WL high-resolution requirements. Second, there will be high-resolution optical images of essentially all fields, which could help the analysis in some aspects. Again, if one is extremely optimistic about how fast a mega-mission EUCLID can be launched, then there will be some delay in achieving these objectives. But if NASA is bound by international agreements to build and launch WFIRST, these delays will be modest at worst compared to the delays likely from trying to build a mega-mission EUCLID.

$\mu$L has significant benefits but some costs. By streamlining EUCLID, a $\mu$L planet search gets on the sky much faster than it would otherwise. For fixed aperture, field of view, and duration, an optical $\mu$L planet search will monitor perhaps 2–3 times fewer microlensing events. However, first, the full power of the IR survey will follow when WFIRST is launched. Second, it is likely that the optical camera on EUCLID can actually be bigger than the IR camera on WFIRST. Third, the higher resolution in the optical, while not essential to microlensing, will be of some benefit. Fourth, detection of host-star light, which is the main way that host-star masses will be estimated, is easier if there is a longer time baseline, which will be a byproduct of having time-separated satellites looking at the same field.

There are, however, several drawbacks for $\mu$L. First, once the WL constraints on IR pixel size are relaxed, then $\mu$L no longer benefits from the "compromise" between high resolution and wide field. $\mu$L can tolerate marginally sampled (or even slightly under-sampled) images because the source shapes are known a priori ($\delta$-functions convolved with the PSF). But if the image quality is degraded too far, $\mu$L light curves will suffer. Further study will be required to determine if there are real conflicts between the BAO/SN and $\mu$L requirements on IR pixel size. If there are, some compromise will be in order. Second, the field choice of an IR-only or optical-only $\mu$L experiment would not be the same: IR can probe much closer to the densest star fields toward the Galactic center, which have a higher $\mu$L rate. Therefore, either some compromise fields would have to be chosen, or the wider optical telescope could concentrate on an area of lower reddening while doing "supplementary" observations of the designated IR fields. This is a complication, but not an insurmountable one.

### 2.3. Finance

Clearly the best financial aid that NASA can give ESA for EUCLID is not to supply IR arrays, but to relieve ESA of the requirement of incorporating them in the mission. This saves not only the fabrication costs, but also reduces the complexity of optics, spacecraft design, weight, communications, etc. The main financial concern on the NASA side is not absolute cost but timing. By committing itself to build WFIRST on a specified schedule, NASA rids itself of the 20% (or 33%) share of EUCLID, and wins equal access to EUCLID data for US scientists.

Better still would be $\sim 20\%$ NASA-ESA cross contributions for the two satellites. This would provide the basis for science cross participation, while retaining unitary management for each satellite. Moreover, the broad specifications of the two satellites should be jointly worked out on the basis of optimal science. If these lead to substantially different estimated costs, the cross contributions could be adjusted to make the financial burdens truly equal.

### 2.4. Politics

My original proposal of first launching a simpler satellite to carry out a single DE experiment has apparently been a non-starter on both sides of the Atlantic, for fundamentally political reasons: with the high cost and infrequent launching of major missions, it is natural to fear that ones "own" approach to DE will never get a mission unless it is on the first such mission. Hence, any single-experiment satellite is guaranteed to acquire more political enemies than friends. However, if there is a signed international agreement to launch a follow-up satellite, and if it is explicitly recognized by all parties that the two satellites constitute a single inseparable "mission" (in the larger sense), then this concern goes away, or at least is strongly diminished.

## 3. Conclusion

Dark energy is arguably the most important physics problem of the 21st century, with major implications for astronomy, fundamental physics, and perhaps even philosophy. Significant progress will almost certainly require unprecedented levels of international cooperation among scientists and the agencies that both fund and provide an organizational framework for their largest undertakings. Cooperation is required not only to mount experiments of sufficient scope, but also to rationally divide work into complementary components that enable a multi-faceted attack while minimizing the complexity of individual facilities.

Because the required level of cooperation is unprecedented, it will not be achieved easily. The structures and procedures of ESA and NASA do not lend themselves to coordinated missions. Nor is it the habit of astronomers to think in these terms. But the stakes are high. Without bold leadership that can draw on diverse efforts to build a coordinated attack on the problem, dark energy studies are likely to sputter for years or decades.

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## REFERENCES

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