Wide Field Imager in Space for Dark Energy and Planets

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ABSTRACT

A wide-field imager in space could make remarkable progress in two very different frontiers of astronomy: dark energy and extra-solar planets. Embedding such an imager on a much larger and more complicated DE mission would be a poor science-approach under any circumstances and is a prescription for disaster in the present fiscal climate. The 2010 Decadal Committee must not lead the lemming stampede that is driving toward a DE mega-mission, but should stand clearly in its path.

1. WMAP Model for DE: Faster, Cheaper, "Better"

Dark energy (DE) is arguably the most important physics problem of the 21st century, with major implications for astronomy, fundamental physics, and perhaps even philosophy. Unfortunately, a variety of bureaucratic and sociological forces on several continents are now driving toward a dark energy mega-mission that would simultaneously attack this problem on 3 fronts: weak lensing (WL), baryon acoustic oscillations (BAO) and supernovae (SN). If adopted, this course of action will produce an utter disaster, delaying progress on a crucial frontier of science for many decades. While the science goals of these 3 experiments are complementary, the instrumentation is not, and hence the costs and engineering complexity are bound to spiral out of control. Moreover, we are entering an era of severe financial crisis when such exponentiating costs simply will not be tolerated.

The siren call leading to this disaster is that only by obtaining agreement among 3 independent DE measurements, each with its own systematics, will it be possible to solve the DE problem. This is nonsense: DE will not be "solved" by this mega-mission, nor 2 or 3 of them. It will dominate 21st century physics. The missions currently conceived will at best offer some initial clues.

WMAP offers a far better model for attacking such a scientifically compelling and technologically challenging problem: faster, cheaper, "better". I have put "better" in quotes because while *WMAP* was fast and cheap, *Planck* will obviously be better. But from the standpoint of advancing *CMB* science in the broadest sense, including practical development of the field, theoretical inquiry, and – very importantly – motivation and design of future projects, *WMAP*'s rapid launch and solid data actually did make it "better" than waiting a decade for a "better" satellite.

These lessons apply even more strongly to DE. We are struggling to measure 1 or 2 parameters, not refine a basically coherent model. Hence, what we learn at each stage is even more crucial to the design of the next. There is no doubt that any results from a single experiment will be called into question on account of systematics, but that is not reason to delay simple experiments in favor of more complicated, later ones. On the contrary, the doubt raised by early experiments will be the strongest driver to construct new, more decisive experiments.

This was true for the SN results that first put DE on solid footing. It was also true for the Bahcall-Davis solar-neutrino experiment. They didn't wait for the massive, technically challenging, and very expensive *p-p* experiments to be feasible: they tested the solar model with what was accessible with 1960s technology and funding. Yes, their results were doubted for 30 years, but these doubts are exactly what drove future experiments. Science is about doubt.

The 2010 Decadal Committee faces a choice: stand at the head of the DE lemmings and recommend a mission that will never be built, or stand in their path and recommend a mission that is faster, and cheaper, and, yes, "better".

2. DE and Planets: Convergent Evolution

The upshot of § 1 is that the first DE satellite should be simple and attack DE by one of the routes (WL, BAO, SN), not all three. No argument was made as to which one. And from the standpoint of DE alone, I do not think that a compelling choice can be made among these three. For example, on DE grounds alone, a reasonably good case could be made for ADEPT, which is a relatively simple mission aimed primarily at BAO.

But here I want to point out a very simple fact: The wide-field imaging satellite needed to do a weak-lensing survey is essentially identical to the one needed to do a microlensing planet search.

Here I do not mean that back-of-the-envelope calculations lead to the same sort of figures of merit. I mean literally that two *completely independent* efforts were made to design satellites that would achieve these very different goals, and the characteristics derived from fairly mature engineering work were almost identical: same aperture, same IR pixel scale, same IR camera size, same orbit. The only significant difference is that the WL satellite requires an optical camera in addition to IR. Even the fields are complementary: WL looks at high-latitude fields while microlensing looks at the Galactic bulge. So they could amicably share time on the same satellite.

When I say “completely independent”, I mean that there was absolutely no contact between the science/engineering teams that developed these designs. Indeed they did not even know of each others’ existence until a French astronomer in contact with both DUNE (WL) and MPF (microlensing) put them in contact with each other.

I will not go into detail about the designs. This is properly the subject of an RFI paper. Here I am just focusing on the proper scientific approach to two big problems.

3. Microlensing Planet Searches

Microlensing is potentially the most powerful method of finding planets. It is the only method sensitive to analogs of all $M > 0.1\,M_\oplus$ solar-system planets (no method is sensitive to Mercuries); the only method sensitive to Mars-mass planets in the habitable zone; the only method sensitive to old free-floating planets; the only method that is sensitive to planets independent of the mass of their hosts; and the only method that has good sensitivity to planets in two major Galaxy environments (disk and bulge). That is, while microlensing certainly does not supersede all other methods, it is the best single method for conducting a systematic survey of planets as a function of planet mass, host mass, host-planet separation, and position in the Galaxy.

Given the strong claims just made, why has microlensing so far discovered only 14 planets (8 published + 6 in prep), while transits have discovered dozens and RV has discovered hundreds? The short answer is that to fully achieve the above potential requires a wide-field imager in space.

Today, microlensing is already making some remarkable discoveries about planets: first detection of “cold Neptunes”, first Sun/Jupiter/Saturn analog, and lowest-mass planet around a “normal” star. But when considering these discoveries, it is important to keep in mind that they are being made by 1m class telescopes *and smaller*. Indeed, amateurs (equipped with 25–40cm telescopes) made major contributions to the detection of 8 out of 14 microlensing planets.

These early successes have led to funding of a second generation of microlensing planet searches. The first generation combines a wide-field search for microlensing events with followup of the most promising events by small telescopes to search for planets. This yields 2-3 planets per year. In the second stage, wide-field cameras will continuously monitor about 16 deg^2, which will yield dozens of planets per year. Note that while US astronomers played a major role in stage one, none (*zero*) of the roughly \$40M required to build the second generation is coming from US sources.

The third generation is a space-based wide-field imager. Detailed simulations show that it will improve both the mass limit and the number of detections by an order of magnitude relative to stage two. Again, it is not my purpose here to review these simulations or designs, but to try to push the thinking of the committee "out of the box".

4. Bottom Line

DE science will be best served by a mission that can be launched quickly and so can obtain early results, stimulate new theoretical work, as well as new scientific and engineering ideas on how to proceed to the next step.

No convincing argument can be made for attacking DE first by WL, BAO, or SN. All have merits and demerits that will be endlessly and pointless debated by their proponents.

However, a WL mission will simultaneously enable a search for microlensing planets that will revolutionize the field of extra-solar planets.

The 2010 Decadal Committee cannot tread in the path charted by 2000 Committee of trying to be "all things to all men". As bad as that path proved to be in the boom years of the present decade, it would lead to complete catastrophe in the next one, which will be subject to much more severe financial constraints.

In DE (and probably most other areas as well), the Committee must chart a course of faster, cheaper and "better".