

Dark Energy Proposal Evaluation

This document is an evaluation of the reviewing process for my Dark Energy proposal. Unfortunately, it met with mixed reviews, mostly because it was held to an unrealistic standard that, in view of events which occurred *months* before the review process (see Tom Siegfried's take on the April 2007 SN conference in the April 13th edition of Science – volume 315, pp 194-5), should not have been applied. This is, namely, that any such proposal to vigorously test the validity of, and correct for the systematics involved with, the Ia Cosmology determination of Dark Energy, must first explain how it could be *both* invalid, while at the same time, data sets with extremely low dispersion in magnitude could still exist. After the April SN conference, there simply is no justification for such a standard

Nevertheless, it is easily done (and was easily ignored by the 2nd reviewer, as well as by the panel). The local sample is still unrepresentative because of the extreme complexity of these objects. After the April conference, the burden of proof of having Ia Cosmology overturn the Standard Model *must* shift back to the Ia Cosmologists. They have to explain, by what miracle they have managed to obtain a valid result, in spite of the now clearly recognized extreme complexity of Type Ia SNe.

My great fear is, that by holding out false hopes for new physics associated with Dark Energy (and with Dark Matter as well), we will go on for another decade without taking the firm steps necessary to prevent planet-wide climatic catastrophe. We are quickly running out of time. Some astronomers may still believe in Dark Energy (and even more in Dark Matter – these should see Erica Nelson's and K. Petrillo's BAAS abstract 2007, 39, 1, 184), but there is a steady attrition of their numbers with each new Ia discovered which does not fit the pattern.

Nevertheless, there is some hope after all (at least for astrophysics), as the first review indicates (shown on the two following pages).

#24 LANL-Middleditch (#105529) \$348k

Scientific and technical merits

Clear scientific motivation?

This proposal presents an alternative to the commonly accepted paradigm for the precursor to type Ia supernovae (WD accreting from the non-compact star in a binary system). Middleditch cites evidence for the DD paradigm, the merger of two degenerate stars. This paradigm is now commonly accepted for short GRBs. Because the observations and assumptions about SN Ia have led to the inference that the universe is dominated by some unknown factor, labeled dark energy, any indications that the basic SN Ia paradigm is incorrect, even for a minority of objects, is vitally important. This proposal is for the continuation of theoretical work related to the DD paradigm for SN Ia and short GRBs.

*Would this work advance the design of Stage III or Stage IV ?
Directly or Indirectly ?*

This work is of great importance to space-based and ground-based studies of SN Ia.

Are results transferable to Stage III/IV concept?

The implications for Stage IV concepts involving SN Ia observations could be extremely important.

How imperative is the work for advancing stage III/IV concept

The assessment of the importance of the DD paradigm is imperative.

Appropriateness of proposed approach

Relative merit?

This proposal should have high relative merit – it questions the basic paradigm which has been invoked to draw conclusions about the method which is currently the most robust concerning the existence of dark energy.

How does this work fit in to the needed mix of theorv. current work and

Reviewer 1, page 2

Competency of Personnel and Adequacy of resources

Right skills and infrastructure support?

The PI has done reliable work, and is well supported by the infrastructure at LANL

Reasonableness and appropriateness of proposed budget

Is this work feasible in one year?

The proposal is for a three-year budget

Could the same work be spread over more years with the same amount of funding ?

Would additional funding in future years greatly add to the results of these studies ?

Would the study still benefit from a reduced amount of funding?

The proposal cannot be cut significantly – if it is funded, it should be funded in full

Are there superfluous budget items?

None

Rational. Refreshing. Recognizes what must be done.

The 2nd review is an out-of-hand rejection. None of the points made in the proposal were addressed:

Reviewer 2, page 1

#24 LANL-Middleditch (#105529) \$348k

Science	3.5
Technical	3.5
Appropriateness	4.0
People/competency	PROPOSAL 2.0
Budget	3.0

This proposal notes recent close SN-Ia observations that seem to bring into question the foundations of the DE measurement plan. In my opinion the proposal is written in a way that, while perhaps correct in detail, obscures the scale of the problem and does not seem to be a balanced appraisal of the potential problems in the SN-Ia. The narrative seems to declare the invalidity of the current paradigm while also declaring the correctness of another paradigm, which references two publications of the proponent. While the proponent may well be correct, the tone of the text gives one pause. I would say these ideas need to be developed a bit more, and the larger picture should be presented, with fair weight given to other ideas. I would not fund this proposal as written. The budget is fairly simple, but I did not find any budget discussion or justification.

Tone!??? Oh my. I was unaware that Ia Cosmology had taken the status of Church dogma! Clearly this reviewer considers it such, unlikely to be among the more cynical of this group who are now thinking “How much longer can we milk this?”

This is a good place to quote a few snippets from the April 13th Science article: “We put the theory in the textbooks because it sounds right. But we Don’t really know if it’s right, and I think people are beginning to worry,” says Robert Kirshner, ...

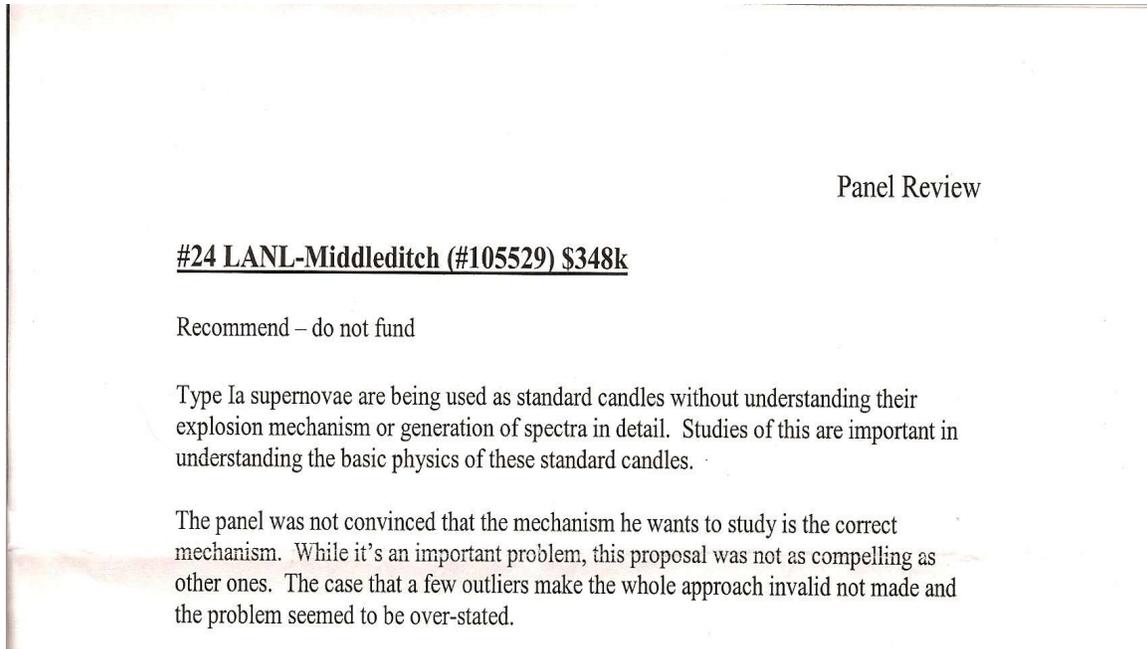
General agreement emerged that the textbook story “is a little bit of ‘the emperor has no clothes,’” as Lars Bildsten, ...

“We do not know the details,” says Alex Filippenko ... “There is still a lot of controversy about what exactly is going on in a Ia.” [Some of us have better ideas.]

“I think this lack of hydrogen is a very, very serious issue,” said Filippenko. [There are at least 10 very, very serious issues – see arXiv:0708.2263, my submission to ApJ Letters.]

“I wouldn’t say it’s a crisis,” he said. [Kirshner – I would say it *was* clearly such] “But if you ask, ‘Are the pieces falling into place?’ I’d say the answer is no.” That’s OK Bob, my pieces are all falling into place just fine (again, see arXiv:0708.2263).

The Panel is no better than the 2nd reviewer:



Not the correct mechanism???? Double degenerate is the *only* alternative to the now discredited single degenerate paradigm. If there's no effect, then nothing is compelling, it's all BS!

At the SN 1987A – 20 Years After and Gamma-Ray Burst conference in Aspen, the question was posed: “Is there any alternative left to double degenerate?” Someone tried an answer, but Nino Panagia reminded the questioner that it had already been shot down. One begins to wonder which planet the Panel and the 2nd reviewer were on during the last six months ...

Nino just got an \$125,000 award for HST observations about Dark Energy, and I doubt that he is a believer in Dark Energy, though I suspect he'll keep the cash.

John Middleditch, 11Sep07