# Seeing stars as they explode: young core-collapse supernova observations with ZTF

## Impact of strawman surveys

I have read the the feedback with interest and thanks my colleagues for their candid and supportive criticism.

The main point, which remains even in view of the feedback, is whether we can push the study of supernovae (and other transients) beyond what was done by PTF/iPTF in quality and not just quantity. The main question is: do we go after the science returns of pushing towards a cadence of 1hr (rather 1d) or not. Obviously some of these returns are unknown, since we have no real data in this regime, but the little we have (from Kepler), as well as theory, promise a lot. ZTF can do something similar to iPTF (finding SNe at typical age of 1d, say) but much more of it (large numbers of such events) – this would be interesting but not a breakthrough. ZTF could (and I hope would) find really new things on 1-3h timescales (in SNe and in other transients) that would revolutionize (again) this field (and keep us ahead of our competition). We could find real shock breakout signals. We could see flash spectroscopy signals from more compact systems. I implore the committee to facilitate this by choosing the highest cadence possible, which mean (among the strawman options), the "fast" one. This is a clear question of cadence, not of sample sizes and simulated light curves. The original text shows we are likely to find such new things, and with the reduced cadence imposed by even the fastest strawman survey, the likelihood for breakthrough results is even better.

In details:

Strawman – wide: this is a terrible cadence for our science. It will be similar to the final months of iPTF (both g+R and g+I experiments) that did not produce a single high-quality early SN to study. Given larger area and better weather it would likely eventually yield events that are as good as the ones we have in hand from (i)PTF, but not really much better ones. With just two points per nights, we will not be able to undertake serious follow-up until both points are taken, which means that the idea brought by one of the referees (relying on massive external follow-up) is very hard to carry out, and likely not viable at least initially (since we will not be able to trust a single point without confirmation).

Strawman – combo: with 3/night cadence this is somewhat better than above, but still not optimal. For phenomena with timescales of ~3h (such as the Kepler "shock breakout" signal and as expected from theory) we will likely have only a single deviant point to mark the signal with respect to a trend set by two other points (best case). This will be hard to interpret and hard to get convinced by. The broad i-band survey is not useful for this particular science case at all.

Strawman – fast: 4/night cadence means on a typical night a visit every 1.5-2 hours. This would allow in some cases to get some structural information about signal that have timescales of 3 hours (i.e., detect the signal in more than a single point). While gggg is the best option for us, ggRg would be acceptable, since we can hope to detect a very young event by detecting it in the second (but not the first) g-band image. Have non-identical two first exposures would not allow this since the color difference may mimic a new events. While this cadence will not recover the full potential of the 8/night cadence we proposed, it would be the best one by far.

With regard to event numbers: we have recalculated the number of SNe expected to be detected at day 1 for the three scenarios presented. The numbers we get are decent 40 per year for the “fast” and “combo” and 80 for the “wide” options. These numbers are much larger than the numbers we aimed for with the 8/night cadence we proposed due the reduced cadence and usage of a larger part of the collaboration time (9/12 months). The issue here is **not quality but quantity.** Having 80 events in the “wide” option will increase the number of events we study by a factor of up to 10 – but these will all be of similar quality to the ones we already studied – we will not learn something fundamentally new. The “fast” cadence will allow to select about 12 events per year that actually happen during the Palomar night and have non-detections in the first 1-2 visits – these will provide access to the science of the first hours after explosion, which is new (including shock breakout signatures that last a few hours). The amount of events will be much reduced in the “combo” option since the chances for an event happening between the first and second visits is much reduced (for events happening between the second and third points we will have no verification since they will be detected in a single frame), and the number of these interesting events goes to zero for the wide option (we will never know if an events happened during the night or not).

Number of same-night events per year:

| | |
|---|---|
| Fast | 7 |
| Combo | 3 |
| Wide | 0 |

Let me end by stressing that by choosing a slow cadence ZTF is missing out not only on the very early core-collapse SN science: it is missing out on **all transients** with timescales <3h. This may include new and super-interesting events we do not know anything about. I hope our collaboration with not neglect its aspiration to go where we have not been before, and not sacrifice this for doing “more of the same” of PTF. The competition (e.g., ATLAS and PESSTO) have realized the potential in first-day observations and are there; if we want to keep ahead, we need a faster cadence.

With best regards

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