Response to the ZTF-EFC

Reviewer #1 (MK)
This is fun project with the chance for breakthrough science. I don't have the experience in crowded field photometry but that seems a non-trivial matter to solve reliably. On the other hand, for lensing of cosmic strings the signal (=doubling flux) is large.

Q1.1) For the other cases the assumption for detectable variation where not stated. Please clarify.

A1.1) The assumption made in the plots was that amplification was >1.34 (for consistency with comparison with LMC-EROS results).

Q1.2) Not sure I follow the stated FoMs. Rather than number of exposures, etc, it should be the sensitivity for selected benchmark scenarios.

A1.2) The difficulty is that the parameter space is large (especially since we are considering very different lensing objects), and our sensitivity changes very rapidly. There is no guidance from theory as to which scenario is more likely occurring in nature. The only thing all of the scenarios have in common is that the shortest cadence is the least tested by other surveys, i.e., possibly the low hanging fruit for ZTF to grab.

Q1.3) The weakest part of the proposal appears it's competitiveness. While the results would be better than anything published (by far), it does not make use of ZTFs huge FoV, i.e. PTF could do it essentially equally well. Furthermore, Pan Starrs or DES with their much smaller pixel scale should be able to do it much better. Or why don't they attempt it?

A1.3) ZTF is not uniquely placed to do this science, this is correct, although there are reasons why ZTF (in particular) could do very well. One important aspect is the readout time. That is a limiting factor for the shortest cadences we can probe, which as stated above, are the most interesting. In that regard, the short ZTF readout time (10 sec) is better than we could do with iPTF (36 sec). DECam (20 sec) is also worse, while PanStarrs is about the same and would be a natural competitor in this science. Note also that DES, located in Chile, cannot monitor M31 (Dec +41). That brings me to another clarification: I am the first to propose to target M31 for cosmic string searches, earlier papers have focused on assessing the feasibility for stars in the Milky-Way, where the prospects are worse.
Probably the best instrument for this science is HSC on Subaru, in spite of a much smaller FoV (better than both ZTF and PanStarrs), and I am currently discussing doing a pilot study on cosmic string searches with Japanese colleagues. They had not heard about this possibility prior to me mentioning, and seem very eager to explore this science, especially since they have high-cadence observations of M31 already in

their archive. In the long run LSST (2 seconds read-out time) will be superbly suited to do this kind of science, although because of its southern location will not be able to target M31.

Q1.4) A thought: choose M31 for one of the fast cadence fields and target the low hanging fruits.

A1.4) Optimizing the exotic science requires obtaining series of consecutive images, possibly faster cadence than pursued for other transient science.

Comments on microlensing of stars in M31
T. Prince
11 March 2017

Because of time limitations, I am not able to verify the estimates of event rates shown in Fig. 2, nor the time estimates for cosmic strings shown in Fig. 1. Taking these as given, I have the following comments:

Q2.1) Under what assumptions was Fig 2 calculated, i.e. how many days of observations of M31?

A2.1) The plots show the maximum discover potential considered, that is 3 years worth of data, with 10 exposures/night.

Q2.2) What is the rationale for 2 sequences of 5 images each night? This clearly doubles the number of short-cadence observations, but given the "expected" number of microlensing events, it would seem sufficient to do a single sequence of 5 images together with a second image (or perhaps as sequence of 2 images, rather than a full 2nd sequence of 5 images). Given that 2 images per night are already likely part of the regular ZTF monitoring of M31, this would only require 4-5 additional images per night. If 5 additional images, then the total additional time is 5x45s or only about 4 minutes of additional observations. A discussion of the tradeoffs of alternatives to the proposed set of observations would be useful.

A2.2) The most interesting part of the parameter space is the shortest time scale, i.e., minutes or less (if that would be feasible). That is where we would have a discovery potential for the most exciting exotic phenomena described in the WP. Consecutive visits is the best (fastest) we can do. I was aiming at 10 obs/night and thought it would be good to combine it with the regular monitoring (as Tom suggests). Since the latter is likely going to aim at images separated in time to be able to tell variables from asteroids, the proposed data could be collected with 4 extra repetitions of each nominal visit. Simply put, the science reach scales linearly with the number of consecutive repetitions. Just the two images separated by an hour could be interesting for a small subset of the science presented. In particular, for cosmic strings, this would be of no value.

Q2.3) If the stars to be used are actually resolved, then the image analysis techniques developed by Soraisam are not needed. In any case, those methods rely on detection of a statistical excess in images, probably not as useful in this application. A discussion of how density of the regions for which 1 million stars will be monitored would be useful.

A2.3) The 1 million resolved stars in the Spitzer catalog are the basic set of stars, hopefully the lower limit of objects that can be used. In addition to those, some of the blended regions could be used. The most challenging (and interesting) science concerns digital lensing by cosmic strings: a doubling of the flux over short time. For a resolved star, we are thus looking for a 0.75 mag flat difference. On the other hand, if N (similarly bright) stars are blended, the microlensing variability becomes 0.75/N mag. It is not clear to me how large N can be for us still be able to detect a flat microlensing event with some significance. To answer that question, we would have to do experiments with fake signals injected in real images. I would appreciate any help from the M31 stellar group in carrying out such studies with iPTF data.

Q2.4) As mentioned, observations of M31 should yield interesting stellar astrophysics. Exploring variability on a number of time scales, not just minute time scales, should be quite productive. However, this project is likely quite time consuming if techniques other than those needed for the Galactic Plane are required.

A2.4) I hope we can use the same techniques, don't see why that would not be feasible.

Cadence: the proposed project, aiming at getting single pointing observations at the fastest possible cadence (limited by readout time) is largely insensitive to any of the strawman choices (wide/fast/combo). With any of these, the charting new territory for microlensing of stars in M31 would require adding 4 or more (extra) consecutive images of the M31 pointing, regardless of strawman option.