Dear authors,

I appreciate the very organised and objective answers to each of my previous points and all the analysis that has been verified and redone. The paper is much more robust and internally consistent now.

There is only one major point that was raised when I compared your previous analysis with the updated version. I would like this to be addressed at least internally before I recommend the paper for publication. Depending on the output, it could be also added to the paper. Apart from that, there are only a few minor details left. They should not take much more of your time.

For the sake of organisation, in this report I make references to the numbers used by the authors in their answers, with the acronym MA-NN for the previous major points and MI-NN for the previous minor points.

Best regards.

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The only major point:

MA-10: Thanks, I understand your explanation on how the fitting works. You confirmed in the answer MI-43 that the membership probability is indeed used as weight in the fit, but for my surprise, it has a smaller effect in the fit, whereas a large number of faint stars with higher photometric uncertainties and lower membership probability have a larger effect in the fit.

I also understand that the isochrone plot is only a sanity check to visually inspect whether the fit was reasonable. This is what called my attention in the first version of the paper for the case of NGC4349 and RUP87, in particular, where the isochrones did not seem to reasonably represent the stars with higher membership probability.

As you said in MA-11, your concern of avoid biasing the results by hand-picking stars was present in the updated analysis, but still you have shown that if you cut off the fainter stars from NGC4349 and if you select only high-probable members of RUP87 the isochrone plot seems to represent better the CMDs, with a significant change in the fitted parameters.

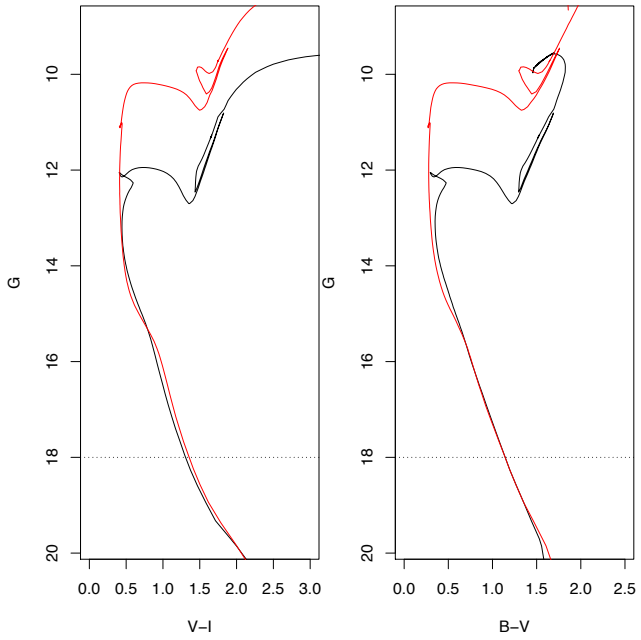
Even though the new results seem convincing, and even the average distance difference between corrected Gaia and ASTECA is shorter now, it still calls the attention that depending on whether you make a selection of stars before the fit or not, the parameters change by more than their error bars. And as far as I understand, the strength of ASTECA is exactly the attempt to be as blind and objective as possible during the fitting process, which is notable, and, in my opinion, should be kept to strengthen the results of the paper.

Could you please explain why ASTECA did not recover parameters (represented by an isochrone) that reasonably match the high-probable members stars in the first version of the paper, in particular for NGC4349 and RUP87?

If I may, I write some thoughts here, considering my limited view on the details of the code.

I send a plot of NGC4349 to support my argument here. I downloaded the same isochrones used by the authors with the parameters derived in the first (black line) and second version (red line). The authors removed stars with 20 > G(mag) > 18 and the final results changed from the

black isochrone to the red isochrone. But the differences between the isochrones in this magnitude limit is negligible, which leads to the conclusion that the fainter magnitudes and colours did not affect the fitting considerably. But briefly reading Perren et al. (2015,2017), it seems that the parameters estimation is done by fitting synthetic CMDs to the data, and the current improvement of including parallax and proper motion is done only for the step of assigning the membership probability. Moreover, on Sect. 4.2 you mention that the magnitudes are not used to estimate the memb. prob., only parallax and proper motion. Therefore I am tending to think that the memb. prob. do not necessarily generate a CMD with a sharp sequence of high-probable member stars (as a memb.prob. estimation based only on colour and magnitude would naturally do), and this may be the root of the answer to my question above. In other words, the apparent mismatch of the isochrones with the high-probable member stars would not be an issue with the isochrones (aka parameters), but with the membership probability.



Minor points:

MA-03.
- the coefficients of equation 2 have three to five significant figures, which seems too much. I wonder if you found that precision in the fit. Maybe adding the uncertainties and some quality factor of the fit would help clarifying.

- Please reduce the significant figures of the uncertainties to two figures and the parameter value accordingly also in Fig. 2.

- paragraph 6 of sect.3.1 discuss the differences between magnitudes without mentioning the r.m.s. of each mean. In all comparisons the offsets are compatible with zero within less than 1-sigma (incl. the figures sent only to the referee). Therefore it is pointless to discuss any offset. Maybe if only the brighter stars are considered on the mean, the r.m.s. would be smaller and the difference would or would not be consistent with zero.

MA-07:
- Please mention the mathematical artifact that you explained also in the paper for clarity.

- Sect.4.2: it is ok to fix manually the radius in this case, if the King profile (or other) does not converge. But please explain in the paper how you reach the precise values of 2.23, 2.89, 1.49 arcmin etc... manually.

- I did not realise until now that you combined data from different instruments with different pixel scales and FOV for the clusters NGC4349 and Lynga15. Combining flux of images with different pixel scales is not a trivial task, if you did it please explain it briefly in Sect.3. Also, if the images with different FOV sizes were combined to generate Figs. 9 and K.2, please add this information in the figures at least, as this could explain the steep decrease in the field density for these two cases that stand out from the other clusters.

MA-08:
- please add back more than 2 numbers in the colour bar. I know the scale is linear (and not logarithmic) because of the previous version of the paper, but the reader has no way to know that with only two numbers.

MI-01:
- still for aims (2): the 'results' say you found a variable level of disagreement (in other words, no systematic difference), and 'conclusions' say your results suggest that a systematic shift of 0.024 mas should be added to Gaia parallax.

MI-12:
- OK, but please add the unit information, i.e. "(degrees)" to both axis labels, or at least in the legend.

MI-15:
- OK, but the reference should be to Fig.7 not A.4 (now that you chose a different cluster as first example).

MI-17:
- OK, but please add reference to "King profile", is it King et al. 1962? 1966?.

MI-20:
- OK, but now that the sentence is clarified, please make a final update for the sake of clarity:
After -> During
is finally compared -> is compared

MI-29:
- OK, but please match the number of significant figures between the parameter and uncertainty. For example: 13.5±0.26 mag should be 13.50±0.26 mag, 5±0.6 kpc should be 5.0±0.6 kpc etc. Check the paper for that. The Gaia distance is correctly written 5.48±0.44 kpc.