# Response report # 1

We thank the referee for their careful reading and thoughtful comments about the paper. We have addressed these comments and have modified the paper accordingly.  All the changes we made in the paper are highlighted in bold font. Below we attached our response to the referee.

(1) Small sample size of simulations

Generally, the authors should be more cautious of interpreting a few, rather specific cosmological zoom simulation as MW analogs. It is not clear how applicable these are to the observed case (as the manuscript itself alludes to in parts), and as such generalizing from the obtained findings for these specific cases is a major step that is not necessarily fully justified.
To provide an example: from the presented figures, it could also be concluded that the orbital pole concentration of the 11 brightest satellites is generally tighter in the case of no massive satellite infall (comparing m12b with m12i, but also m12m has no massive satellites and relatively clustered poles). While the pole concentration changes less than in case of an infalling massive satellite, the clustering is much tighter to begin with and thus closer to the observed situation. One could thus use the presented results to argue that having a massive satellite fall into a halo may be detrimental to obtaining very tightly clustered poles. I encourage the authors to be more careful to balance their interpretations and be cautious of such alternative ones, given the very limited dataset available.

## Response:

We agree that our sample of 7 galaxies is small. As the referee mentioned this is a trade-off between resolution and sample size. Yet our point is also that specific mergers can enhance co-rotation. Out of the 7 galaxies 5 host massive satellites, and we see that in all of the 5 satellites, the orbital poles change after when the satellites are at pericenter See Figures 14-17. The magnitude of the effects change depending on the properties of the satellite, mass, pericentric distance, and orbit. In particular, figure 17 shows that the spherical standard distance of the orbital poles is minimal through the evolution of the galaxy when the satellite is at the pericentric passage. In addition the dispersion of the spherical standard distance is greater for galaxies hosting massive satellites, highlighting that satellites do perturb the

distribution of orbital poles. Yet, this doesn't mean that among the 7 galaxies, the spherical standard distance has to be minimal for the galaxies with massive satellites. In fact, we find that m12i (massive satellite), m12m (massive satellite), and m12f (massive satellite) present the minimal spherical standard distance among the 7 galaxies. This implies that there are another mechanisms, such as filamentary accretion, that can induce co-rotation, however, a massive satellite can enhance the co-rotation. We have made these points clearer in the text. In particular in Sections:

- At the end of Section 5.2: we added a discussion highlighting that in m12i and m12m (no massive satellites) the distribution of poles is more anisotropic.
- In the conclusions: We added a paragraph where we discuss that m12i and m12m exhibit anisotropic distributions of poles, and therefore other effects such as a filamentary accretion are also important.

(2) Significances

Related to the small sample size is the question of the significance of the results. In several places throughout the manuscript, the word "significant" or "significant changes" or similar are used. However, no measures of significance are provided. This needs to be rectified, as the statements otherwise remain unsupported.

## Response:

We clarified what we mean by significant, these changes are shown in bold through the text. Specifically, we changed it in 5 appearances where we now quantified the changes in terms of the correlation function or in terms of the orbital poles mean, depending on the context of the discussion.

**(3) Mass ratio of m12b:**

Simulation m12b is presented as the closest MW-LMC analog and claimed in the text to have a satellite-to-host mass ratio of 1:5.7. Yet the mass ratio at infall stated in table 1 is 0.35, or 1:2.9. The number in the text can be reproduced as the ratio of satellite mass at infall with the total host mass at z=0, but this compares masses almost half the age of the universe apart in time. The host mass has increased by 70% since then, which presumably includes the mass of the accreted satellite which by then will have fully merged with it. The text thus needs to be updated to state the more meaningful mass ratio at infall.
A 1:3 mass ratio in turn places m12b above the current estimated MW-LMC mass ratios, well into the range of major mergers. This makes it a less convincing analog. This is also supported by

## Response:

We have revisited the numbers reported in table 1. We realized that the masses at z=0 for the hosts were incorrect as these were excluding baryons and therefore consistently lower to those reported in table 1 of the FIRE public release (Wetzel+23). For m12b in particular we also checked that the reported infall mass of the host was taken at t=6.57 and not at 8 Gyr which is the infall time. As such the corresponding mass of the host is M200= 0.9 x10^12 Msun. With these new values the mass ratio is 1:4.28. This is comparable with the MW-LMC mass ratio for the high mass LMC, which is the most common value in the recent literature e.g., (Vasiliev+23). We have adjusted the text accordingly. We have also fixed the Host-Satellite ratios.

Regarding the work by Kanehisha, it is not straightforward to compare their results with ours for two reasons. 1) Their analysis is carried out at present-day z=0 while results we present here are time-dependent. Thus transient signatures like the ones presented in Figures 8-10 won't be captured at z=0.  2) The results presented here also argue that to induce the orbital poles clustering, satellites need to have pericenter distances similar to that of the LMC. For example in mergers like the one in m12c, in a very eccentric orbit with a pericentric distance of 20 kpc, the clustering of poles is not strong (Figures 7, 15). As such when comparing a statistical sample at z=0 as the ones in Kanehisha+23 the effect that we are reporting gets washed out. We have added this in the discussion see (lines 1044-1047)

expected to be strongest for the most distant tracers. Note also that the upper distance limit of 300 kpc extends well beyond the virial radii of the considered simulated hosts at their satellite infall times.

When discussing Pawlowski et al. (2022) in Sect. 2, the focus is on idealizations of the numerical model. However, their work also looked at the actual observed MW satellites and found the proposed reflex-motion and centroid-shift induced orbital pole clustering to be an untenable explanation for the degree of observed clustering (especially in light of the velocities of the observed satellites). The expected shift in center of mass and the expected shift in velocity do not appear sufficient to substantially change the orbital pole directions for observed the on-plane satellites. This should be mentioned as a caveat for applying the simulations' results to the observed system.

Another difference with the analysis of the observed MW satellite system is that the present study uses dark matter or star particles as tracers. This heavily weights the orbital pole analyses by mass (a massive satellite will have more particles). In contrast, orbital poles of MW satellites are typically not weighted, but each considered equally. Computing the clustering using individual particles will also introduce a lot of correlated particle pairs, since those residing in the same subhalo will have very similar orbital poles. Thus the results based on particle tracers are not easily applicable to the study of observed satellite galaxies. The manuscript should more carefully differentiate these approaches, as it currently jumps from inferences based on dark matter particles to statements about satellite galaxies, e.g., in several places in the conclusions. For example in the first bullet point, significant changes to the DM particle orbital poles due to a massive satellite infall are discussed, and then used to argue that the dynamical state of a galaxy needs to be considered when interpreting the clustering or satellite galaxy orbital poles. The latter leap is not fully justified by the presented evidence and limited number of simulated systems.

## Response:

**Results within 50-150 kpc:**
The maps below show the orbital poles distribution of distances up to 150. Qualitatively, the results are consistent with those presented in Figure 5 of the paper.

## m12b 50-150 kpc; t=6.59 Gyr



## m12b 50-150 kpc; t=8.81 Gyr



## m12b 50-150 kpc; t=10.22 Gyr
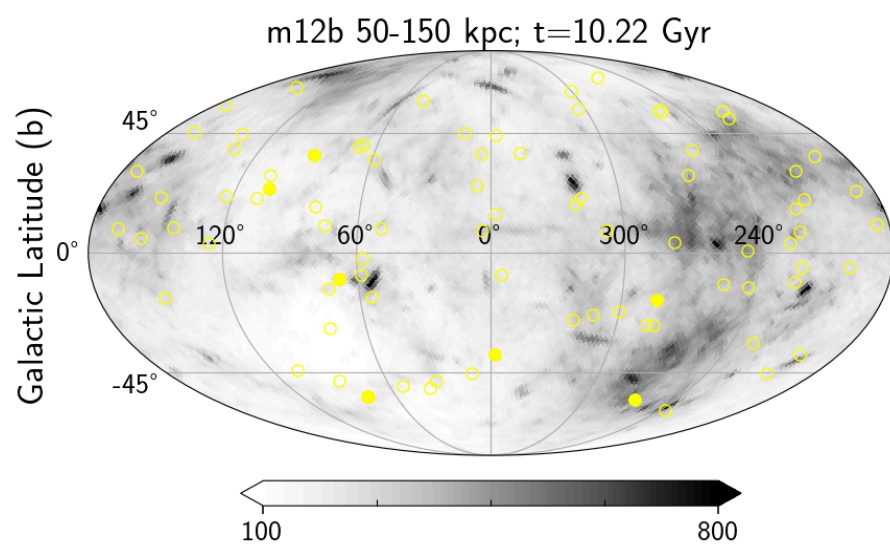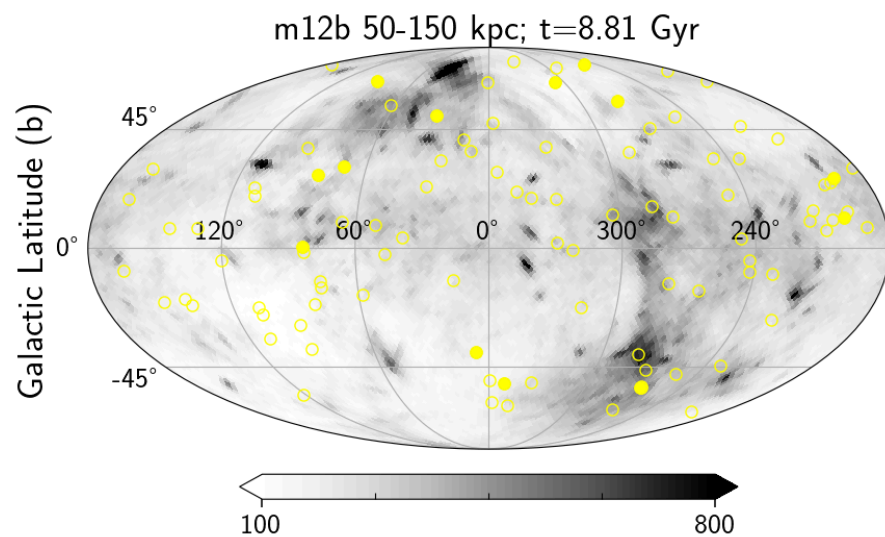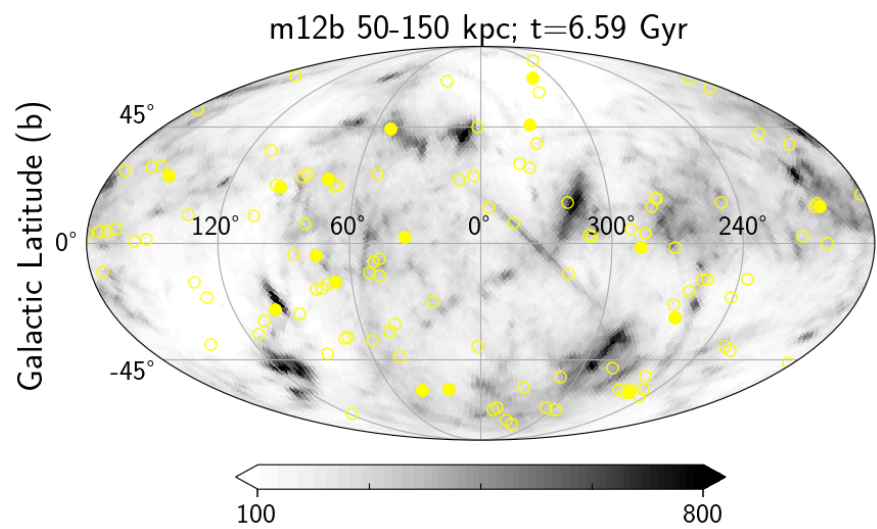
**The comparison made in Pawlowski et al. (2022)** is not a self-consistent comparison, in the sense that one needs to fully integrate the orbits of the satellites in a time-dependent potential to properly study the effects of the LMC. Subtracting the reflex motion and distances from N-body simulations in a spherical average way as in Pawlowski+22 does not fully capture the effect of the LMC. In a work in progress from Patel, Garavito-Camargo, et al, in prep. We are showing that when one integrates the satellites in these potentials the clustering of orbital poles do change as a function of time. We won't discuss this in this paper, since this is work in prep.

**Tracers:**

We agree that quantifying the poles clustering in DM particles is not the same as in satellites or subhalos. However, we use the correlation function to quantify relative changes (not absolute), so our results should be independent of the additional small-scale power induced by particle correlations *within* subhalos (see, e.g., Figure 6 and 7, which shows the *relative* correlation function for DM particles). But we also compute statistics for the subhalo kinematics themselves, not just the individual DM particles (see Figures 8–10; any plot titled "subhalos" shows results for individual subhalos not their DM particles). In the 5th bullet point of the conclusion, we also discuss the differences in halo tracers and how the results change depending on the metric and tracer used.

(5) Induced co-rotation:

The description of the co-rotation signature is not always complete, which could confuse the reader and leave them with an incorrect interpretation. See for example Sect: 5.2. Yes, the differential motion causes a clustering of orbital poles, but it also requires a shift in position of the central galaxy relative to the halo. As such, the outer halo does not appear to be co-rotating. To first order all of the outer halo moves in the same direction relative to the central galaxy. Yet the outer halo's center is shifted relative to the central galaxy in such a way that at a given distance from the central galaxy, one side of the halo has a somewhat higher density. Without this shift, one would have an enhancement of orbital poles along a great circle, not a rotational signature. The shift causes the orbital poles on one side of the host to be more numerous due to the enhanced density at given radius. This produces an overdensity and apparent rotation signature, but only in the orbital pole distribution. As such, the text should be revised to make clear that the halo is not rotating, and that apparent rotation is inferred from the orbital poles only if this density effect is in place. This also applies in other parts of the manuscript, e.g. the sentence around line 988: "A key signature of the dipole mode is the co-rotation in the outer halo as shown in this paper ...".

## Response:

Yes, we agree with the referee. This has now been addressed in different parts of the manuscript:
- Sect 5.2: First paragraph now explains in more detail how the co-rotation can be induced
- We fixed the language in line 988 (now line 1023)

(6) Figure 4 and related discussion:

It is not sufficiently clear what velocities are shown in the upper panels of Fig. 4. If this is the absolute velocity relative to the simulation box, how can it be negative? If it is the difference in velocity between the host at a given time and its velocity at a later time, again, how can it be negative? If it is the difference in absolute velocity relative to the simulation box at a given time and at a later time, I see how this could get negative, but in that case, the total velocity change could be considerably higher than this value because the direction of motion can change without affecting the absolute value. Please be more specific in what the underlying calculations are, what is plotted, and what this provides meaningful information on.
Why does m12b start with a 30 km/s reflex motion already before the massive satellite is accreted? This appears to contradict the statement on line 576 that the first pericenter induces a reflex motion of 60 km/s. Only half of that seems related to the first pericenter, the other half has been in place for a long time before.
Why is the outer halo velocity measured with subhalos, not the particles? Doesn't this hinder comparability with the idealized simulations which do not contain any subhalos?

## Response:

Yes, the velocity is relative to a point in time, specifically with respect to the infall time (t=6 Gyr). However, we did find a bug in our calculation and now we have done the relative change properly and that's why there are no negative values anymore.

Regarding the reflex motion, we find that at larger distances it is rare to have a zero reflex motion (as also seen in m12i and consistent with the recent result by Salomon+23). We have fixed the quoted values for the reflex and disk velocity accordingly in Section 5.1

## (7) Coordinate system

Please provide details on the chosen coordinate system for Fig. 5. An orientation with the plane of the central galaxy only defines one axis, there is still freedom to rotate along the longitude direction. How was this fixed? In the discussion of Fig. 5 for m12i, it is suggested that the orbital pole evolution is related to the passage of this satellite. Could it simply be due to a re-orientation of the central disk? Since the latter defines the coordinate system, a disk reorientation would rotate the plotted pattern. Related to this, how much of the mean orbital pole orientation shown in Fig. 8 is caused by changes in the coordinate system and thus the orientation of the central disk?

## Response:

Indeed the coordinate system will have an effect on the shift in the orbital poles. We were aware of this when we carried out the analysis and indeed found that one needs to be careful when doing the rotations to fix the reference frame. We use the **pynbody** library where these rotations are performed and indeed the longitude is fixed by. ([faceon rotation in pynbody](#)78).

In short, the routine uses the angular momentum vector of disk particles (in the reference frame of the host) and apply a rotation with respect to a unit vector defined  n the box reference frame and the direction of angular momentum As such any residual motion in the halo's freame would be caused by peritbration to the halo, like those in satellites.

To demonstrate that there are not spurious rotations we plot the satellite's orbit by component in the figure below. There are no spurious rotations in longitude since the orbit is smooth at all times. Note, that this same rotation is always applied through our analysis. We have now defined the chosen coordinate system in more detail in lines: 611-616.
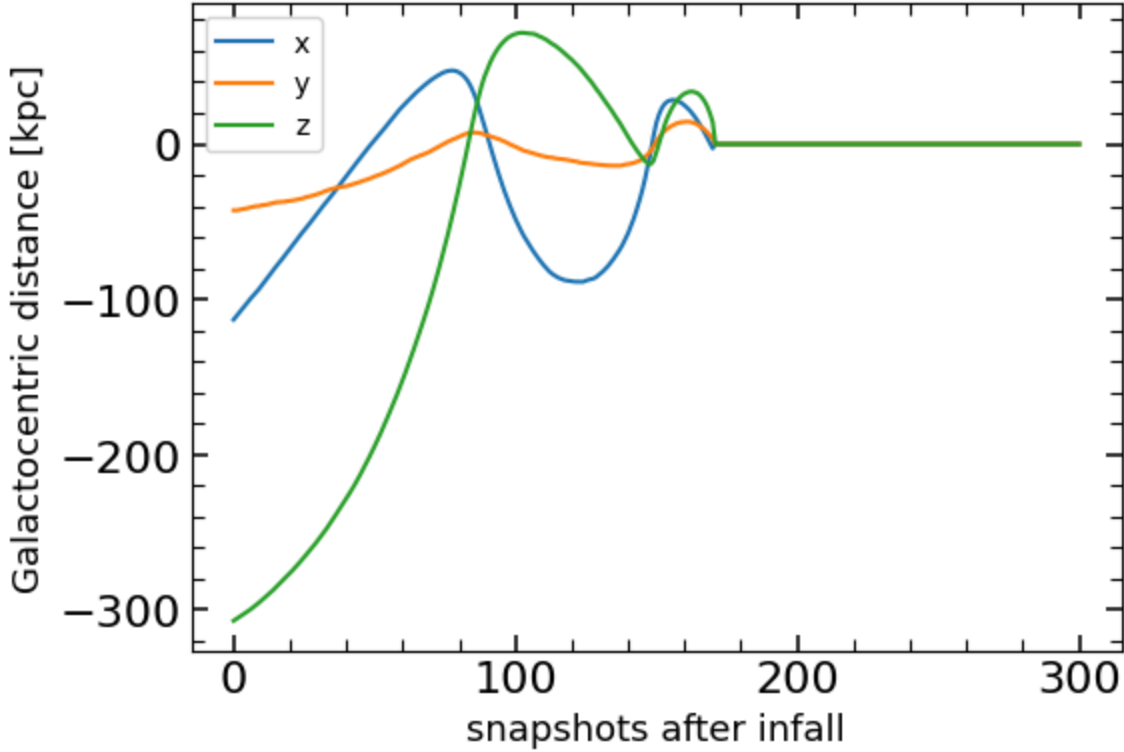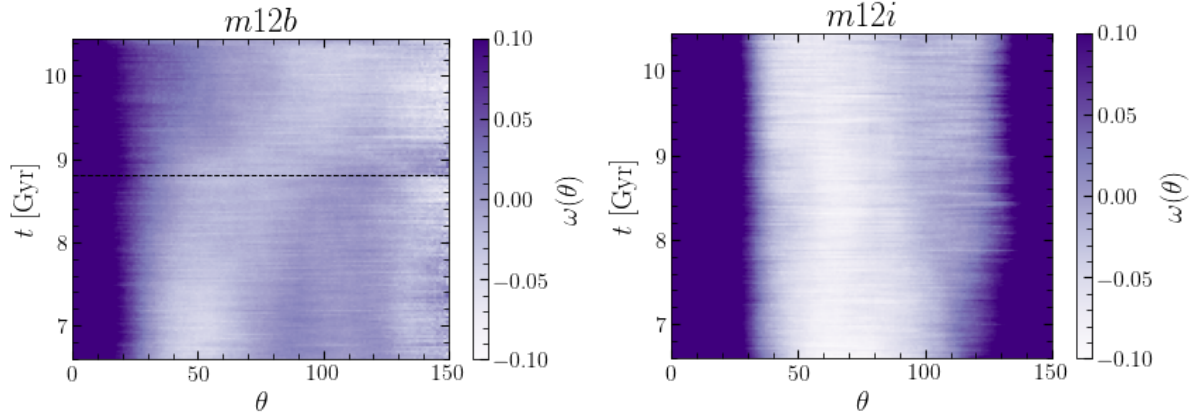
**(8) Figure 5 and related discussion:**

Please indicate the orbital pole and orbital path of the infalling massive satellite in these plots, so they are available for comparison in all panels. Thus far, only at the end of the caption for Fig. 5 the orbital pole direction is mentioned (but there the values for l and b appear to be swapped).
When discussing M12b, it is stated that the orbital pole distribution is more isotropic than for the other simulation. A visual impression alone is not necessarily convincing, can be subjective, and thus possibly differ among readers. Therefore, please support such statements quantitatively. It should also be mentioned that there appears to be no particular clustering of satellite orbital poles in the direction of the orbital pole overdensity oriented like the infalling satellite in the bottom panel for m12b.

Response:

We have now included the orbital pole of the satellite in (pink) and fixed the values of l and b that indeed were flipped.  Regarding the quantification of the distribution of poles, we now refer the reader to Figure 10 where the spherical distance is smaller in *m12i* than in *m12b* since

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the clustering is larger in *m12i* than in *m12b*. We also quantified the distribution with the two-point correlation function in $10^6$ randomly selected DM particles for both halos. The figure below shows that in m12b the correlation function w(\theta) is more homogeneous as a function of \theta than in m12i, where it is very concentrated at smaller and larger values of theta, which is a signature of an anisotropic distribution of poles. We do not include these figures in the main text, since the paper is quite long already, but we mentioned this in lines 690 of the main text. We also add a comment about the clustering of satellite galaxies in line 714.



(9) Figure 7 and related discussion:

I would like to caution against over-interpreting the shown temporal correlation as causal. No test of correlation or its significance has been provided. As such, this remains highly speculative. Yes, some of the enhancements in the figure seem to be close to the indicated satellite accretion events. However, there are also cases with considerable enhancement without an indicated satellite infall. The enhancements sometimes begin before the arrow's position and sometimes only some time later, and in case of m12w the satellite might even cause a reduction of orbital pole correlation.

## Response:

We have adjusted the language regarding the discussion of  Figure 7 and the end of Section 5.3.

Some other points:

- The statement around line 658 that orbits of subhalos accreted along filaments are co-planar needs to be supported by evidence. It also contradicts the following sentences about a sinusoidal pattern. The latter occurs when subhalos are accreted from one direction but pass the center of mass on all sides, such that their orbital poles distribute

perpendicular to their infall direction. This is not at all co-planar, their orbital planes can be inclined by up to 90 degrees.

- Contrary to the statement in the text, at least to me it is obvious from Fig. 2 that the halo is embedded in more than two filaments. Please clarify and provide evidence (or refer to a previous work) supporting this statement.
- It is stated in the introduction that the M31 satellite distribution is lopsided rather than planar. These two are not mutually exclusive, M31's satellites appear to be both lopsided globally and contain a significant planar subset.

Literature:

The manuscript provides a thorough overview of the related literature. Most of the relevant publications are included, and mostly discussed appropriately, with only the following exceptions that should be addressed:

- When discussing the M31 satellite plane, it might be relevant to mention that Sohn et al. (2020) measured the first proper motions and found aligned orbital poles for two members of M31's satellite plane.

We have included this reference.

- The introduction suggests that when accounting for tidal disruption of satellites, one finds co-orbiting planes at a 2% probability. The two references provided for this statement seem to suggest a lower number. Requiring both planarity and co-rotation, Sawala et al. (2022) report only 1 out of 202 systems to match their MW-analog criteria, i.e. 0.5%. Pham et al. (2022) explicitly write that when accounting for disruption, the MW satellite system is a $\approx 3\sigma$ outlier, i.e. around 0.3%. The number given in the introduction thus appears too high.

We have corrected these values.

- When citing the recent paper by Li et al. (2022) reporting that satellite planes around hosts in Local Group like pairs are no more frequent than those round isolated hosts, it would be appropriate to also cite the earlier study by Pawlowski & McGaugh (2014) on this exact topic that was omitted in their publication.

Thanks for pointing out  this reference out

- To be fair and unbiased, the discussion of mechanisms proposed for co-rotation pattern in Sect. 6.2 should also mention the well known tidal dwarf galaxy scenario (some references are already cited in the introduction, but worthy of repeating here).

We included these and other relevant references in this section.

- Several of the provided references are to arXiv preprints. Please replace these with the references to the proper, peer-reviewed publications.

Clarification and typos

Please proofread and edit the manuscript carefully, as there are numerous typos, unclear sentences, and errors in referring to panels in figures. These include:
- In Sect. 2: "For example, halos with recent accretion events or halos accreting massive satellites show stronger dipoles then those predicted from LMC perturbations". Isn't the LMC a massive satellite being accreted, too?
- Also in Sect. 2: "For example, the dynamics of the MW satellite galaxies is well represented by a spherical dark matter." What is this supposed to mean? What is "spherical dark matter"? If a spherical dark matter halo is meant, it is not clear in what quantities this represents the satellite system. The observed spatial distribution is, due to the satellite system's flattening, not exactly well represented by something spherical.
- In Sect. 4.2, DD(T_infall) is defined as the number of pairs at times after z=0. Since the simulations end at z=0, this appears to be a typo? Please clarify.
- In the caption of Fig. 3, the sentence about the host's disk velocity appears to be misplaced, or at least it is unclear what this is supposed to refer to in those figures.
- In several places, panels are referred to incorrectly. For example line 546 refers to the left panels for the idealized sims but they are the right ones in the figure. Please go through the manuscript and correct these. Also, it would ease comparability and understanding for the reader if the different simulations would always be presented in the same order (particularly the case for the figures in the appendix, where the simulations appear to be shown in different random orders in each figure).
- Line 581: Something is wrong here, "Among all." is not a full sentence. Maybe a typo or accidentally deleted content?
- Title of Sect. 6.2: "Mater" should be "matter"
- The third bullet point in the conclusion mentions velocity dispersions. This seems to be a typo.

**Reponse:**
We have included all of these comments and literature.