REVIEWER #1

General comments:

Díaz-Astudillo et al. use a 26 hours of acoustic data over a submarine canyon to provide another example of the importance of these topographical features in marine ecosystems. I commend the authors on their work. I think that is an interesting contribution and I have no specific comments on the validity of the methods or results. Overall, I think the manuscript is good, but I have two major comments that I would like the authors to address.

R: We are thankful for the positive opinion on our work.

Specific/Technical Comments:

1) A majority of the text is verbose, making the text difficult to get through. I would encourage the authors to read the text aloud, or move any extraneous text to the supplement (while important, the extensive description of the wind/upwelling conditions and accompanying figures at the start of the results comes to mind) to consolidate the text to make it easier for the reader to understand your main points. I would also suggest that the authors reduce the number of figures and should take the time to thoroughly proofread the manuscript (ie make sure all abbreviations are consistently used throughout and verbs are in the correct tense).

R: We appreciate this comment and agree on the text needing revision. We re-read the whole text and modified the sections that were too verbose. We also moved the description of the atmospheric conditions and river discharge to the Supplementary Material to shorten the Results section. We checked all abbreviations (with emphasis on NS, SS, ET and WT) to assure their consistent use.

2) The authors put a lot of weight in their results, suggesting that they have discovered a potential fourth mechanism for submarine canyons facilitating food web focusing using just over a day's worth of data. I think both mechanisms 3 (interaction of DVM and abrupt topographies) and 4 (canyon mediated retention) are valid, but the fourth mechanism is not new. The Hudson et al 2022a, b papers (in MEPS [cited already] and JGR Oceans) illustrate both of these mechanisms. These mechanisms differ slightly than those described here (ie, the Antarctic krill are not being 'blocked' from migrating, or if they are, it was not considered as a focusing mechanism), but the overall concepts are the same. I would encourage the authors to combine these two mechanisms into a single section in which they describe how BBC retains zooplankton and then describe the ecological implications in a different section.

R: We appreciate this comment which has made us reflect on the Discussion and delve deeper into these ideas. The papers by Hudson et al. 2022 does present a retention mechanism that involves the topography of a submarine canyon, however, the retention mechanism is directly associated with the formation of a subsurface eddy inside the canyon. The interaction of krill DVM with the retentive eddy is the mechanism that increases particle residence time within the Palmer Deep Canyon (Hudson et al. a and b). We did not find evidence in the hydrographic profiles nor in the currents of the formation of and eddy during our study, so the mechanism we describe is fundamentally different that the one in Hudson et al. 2022 a and b. We did find currents with opposed direction in one wall with respect to the other, but the flows changed in direction several times within the 26 h of our study, which is not consistent with the presence of an eddy. Additionally, we didn't see evidence of isopycnal doming which is another indicator of the presence of and eddy (we have included this in the Discussion). We have improved the Discussion section to support the hypothesis of the interaction of DMV with topography and alternating currents.

Furthermore, there is a lot of weight put on these results (ie, suggesting a "new" mechanism) on 26 hours worth of data, which included only 2 zooplankton samples. We always want more data, and I still think that the study is strong with this limited dataset, but I think this needs to be addressed in the discussion and should include a brief (~1 paragraph) discussion of the limitations of this study.

R: We appreciate the reviewer's acknowledgment that our study is strong even with limited data. We agree with the reviewer that maybe too much emphasis was put in the suggestion of a potential new mechanism. We toned down the emphasis put in a potential new mechanism, and we now propose a variation of a previously known mechanism involving currents, DVM, and topography, following the suggestions of both Reviewer 1 and 2. We also included several lines about the limitations of our study throughout the Discussion section. Finally, we wish to clarify that we presented results on 8 zooplankton samples collected on 2 strata during day and night. To make this clearer, we included a Supplementary Table 2.

You performed this sampling study after a period of upwelling winds; discuss how you might expect this mechanism to change under different conditions. What if there hadn't been upwelling winds prior to your sampling event? If this mechanism is tied to upwelling winds, is it truly a different mechanism?

R: Thank you for the suggestion. We have included the potential change in the Results under downwelling conditions in the Discussion (lines 294-298).