Reviewer 2

“This submission explores many aspects of fast ice around the periphery of Antarctica, including trends, links with bathymetric depth, monthly timings of minimum and maximum coverage, age, and persistence etc. It is based on the analysis of 18 years of record obtained via hires remote sensing.
The submission has the potential to make a significant contribution to the literature, but it is not quite there yet. Before I would be able to recommend acceptance, there are a number of issues which need to be addressed.”

- We thank Reviewer 2 for their careful reading of the manuscript, and constructive suggestions of ways to improve it.

- Lines 51-54: The authors should comment on how their dataset might different from one that used the improved scheme of Paul and Huntemann applied to MODIS to detect cloud cover over Antarctic sea ice as well as its discrimination from sea-ice cover and open-water areas. See Stephan Paul and Marcus Huntemann, 2021: Improved machine-learning-based open-water-sea-ice-cloud discrimination over wintertime Antarctic sea ice using MODIS thermal-infrared imagery. The Cryosphere, 15, 1551-1565, doi: 10.5194/tc-15-1551-2021.
    - Thank you for mentioning this manuscript, which is already of great interest to us as a way of providing an independent (to the widely-used MOD35 MODIS product) cloud mask. While the Paul and Huntemann dataset is focused on retrieval of polynya extent, the dataset used in this paper focuses on fast ice extent. These two often share a common boundary. We are already exploring overlap between our data processing and hope to progress this in the future. We will provide comment on this around lines 51-54.
- Lines 56-58, and various other places in paper: It makes sense that the five sectors that have been traditionally used for sea ice analyses might not be appropriate for fast ice (because of the very different dynamics and thermodynamics). The authors later go on to explain how they choose eight coherent sectors for the fast ice investigation. Then throughout the paper they refer to the ‘newly-defined regions’. This becomes a little tedious and is really unnecessary – I suggest they just refer to the ‘regions’, as the meaning will be quite clear.
    - A good suggestion - thank you. We will incorporate this.
- In the Appendices they describe the autocorrelation approach to identifying the eight regions, and their approach is reasonable. They also point out some of the caveats of how they have done this. An extra caveat that should be mentioned is that they have obtained these semi-coherent sectors in a ‘a posteriori’ fashion rather from, e.g., physical arguments. Hence, by its very nature coherent collections are identified, which

in turn will mean than any trends will have an enhanced level of statistical significance. I don't have a great problem with the approach, but the caveat should be made clear.
    - The reviewer is correct: these are certainly defined a posteriori (as with Raphael and Hobbs (2014)). A caveat along these lines will be added in Appendix B.
- Appendix A and B: As a point related to that raised immediately above, it seems that the 'traditional' five sectors are only used in these Appendices (and in Fig. A1). Given that, I strongly suggest that these five sectors (the red ones) be removed from Figure 2, as it makes that Fig. more complicated than it needs to be. (The longitude limits of these sectors can be presented in the text in the Appendices). As a separate idea I would suggest that an extra column be added to Table 1 to present the trends (and p values) for the eight regions; this would mean that the results of the trend analyses (which are perhaps the most interesting aspect of the paper) are presented 'front and center' to catch the reader's eye.
    - We agree, and are very much in favour of de-cluttering Fig. 2 in any way possible. This (removing the red lines/sectors from Fig 2) is a good suggestion. We propose moving the sector boundaries to the caption of Fig. A1. The column addition to Table 1 is a great suggestion - however, since the p-values for the regions are all similar (and very close to zero), we will report the confidence interval instead of the p-value.
- Also on Figure 2 (and also Figures 3 and 4) the sectors are labelled 1 thru 8. These numbers are not referred to, so they should be deleted.
    - Will be deleted on these three figures - well picked up, thank you.
- Line 76-77: Author names have been repeated.
    - Will be rectified.
- Lines 83-86: Why not simply say here 'the first four Fourier components'? Also, perhaps make clear why the standard method of calculating Fourier amplitudes and phases was not used. What was the advantage of the L-M approach here?
    - Will change to "first four components". The L-M implementation (mpfit in IDL) was used here due to prior experience with this package, and provides no advantage to the traditional approach other than speed of execution (important on this grid of ~11 billion points (5625*4700 spatial; 432 temporal)). This will be mentioned in lines 83-86.
- Line 95 (and elsewhere where relevant): Jodie Smith's paper now has a doi, so should now be referenced as 'Smith et al. (2021)'
    - Thank you for providing this update.
- Line 113: To avoid any possible confusion perhaps best to be explicit and write 'circumpolar extent time' as 'circumpolar fast ice extent time'.
    - Will be modified.
- Line 117: Also cite here the update of Simmonds et al., 2021: Trends and variability in polar sea ice, global atmospheric circulations and baroclinicity. Annals of NY Acad. Sciences, doi: 10.1111/nyas.14673.
    - Interesting reference - will be added, thank you.

- Lines 128-132: Authors should comment on the physics that may be responsible for these phase difference between the fast ice and the sea ice.
    - This comment is already given in the discussion. We feel that it fits better here (lines 198-207).
- Line160: Change ‘Fogwill et al. (2016),’ to ‘(Fogwill et al., 2016),’
    - Will be rectified
- Lines before lines 162 (caption of Figure 5): The caption text says ‘… trend p-value are indicated in the title of each sub-plot’ but the p values are not shown (only the standard error). Please add the p values.
    - Apologies - this mistake was also picked up by Reviewer 1 (Dr Greg Leonard). We will rectify this mistake as he suggested.
- Lines 162-164: Authors should present some ideas or speculation on what this environmental forcing over a large spatial scale’ might be.
    - Very happy to speculate on this - but it’s purely speculation (at this stage). Given the widespread distribution of the positive trend along the eastern part of the Weddell Gyre, we speculate that this environmental association is likely oceanic in nature. Following careful reading of Simmonds and Li (2021), we also note that this is a region of increasing summertime, springtime and wintertime sea ice concentration (their Fig. 2), which may favour formation of more extensive or longer duration of fast ice coverage, i.e., this fast ice trend may be associated with an oceanic trend which has atmospheric drivers. This will be added around lines 162-164.
- Line 183: Delete one of the ‘Fraser et al.’.
    - Will rectify.
- Lines 180-191: Some interesting points are raised here, and certainly warrant future work. For the moment however, some extra discussion is required here. The tentative link/association made here with sea ice extent should be reinforced with comparison with sea ice concentration. The four regions of fast ice decrease and four of increase for the most part closely follow the trends in sea ice concentration shown by Li et al. (2021) ‘Trends and variability in polar sea ice, global atmospheric circulations and baroclinicity. Ann. NY Acad. Sci., doi: 10.1111/nyas.14673’. Reference to that paper and some extra comments on this will make this part of the interpretation much stronger.
    - Thank you again for bringing this paper to our attention. It’s an important point (to link the observed fast ice trend distribution to that of sea ice concentration, in addition to the extent discussion presented here). While the time periods of the Simmonds and Li paper differ to that presented here, we can indeed see that the general distribution of trend coincides with the southern hemisphere trend in concentration from 1979 to 2020 in Sept-Nov (Fig. 2B, bottom row), but this link is somewhat less convincing for other seasons. That this relationship is strongest is Sept-Nov is not surprising, since this season coincides with maximum fast ice extent. We will add this discussion around lines 180-191.

- Lines 199-207: Also helpful to reference at this point in the text the more recent paper by Aoki, S., 2017: Breakup of land-fast sea ice in Lützow-Holm Bay, East Antarctica, and its teleconnection to tropical Pacific sea surface temperatures. Geophysical Research Letters, 44, 3219-3227, doi: 10.1002/2017GL072835.
    - A good suggestion. Although the precise mechanism was not discussed in the Aoki paper, if the mechanism involved an oceanic link, then this might also explain why fast ice lags sea ice. This discussion will be added around line 207.
- Lines 283-285: Relevant remote atmospheric teleconnections into the Peninsula from warming down in the Tasman Sea have recently been identified by Sato, K and coauthors 2021 - Antarctic Peninsula warm winters influenced by Tasman Sea temperatures. Nature Comms, 12, 1497, doi: 10.1038/s41467-021-21773-5. Valuable to also reference this here.
    - A good point: the Sato et al paper is a good example of a remote teleconnection which has demonstrable impacts on relevant (for fast ice) local climate parameters, even though it does not specifically mention fast ice. Will be incorporated around line 285.
- Line 317: Reinforce this statement by also referencing Sato et al., 2021 (‘Antarctic skin temperature warming related to enhanced downward longwave radiation associated with increased atmospheric advection of moisture and temperature. Env. Res. Lett., 16, 064059, doi: 10.1088/1748-9326/ac0211’) (his Fig. 4).
    - Reference (and Fig. 4 in particular) is highly relevant and will be added.
- Lines 395-396: Please to note missing details plus wrong order of authors - Kim, M., H.-C. Kim, J. Im, S. Lee and H. Han, 2020: Object-based landfast sea ice detection over West Antarctica using time series ALOS PALSAR data. Remote Sensing of Environment, 242, 111782, doi: 10.1016/j.rse.2020.111782.
    - Apologies - will be rectified
- Lines 450-451: Please note full details of this paper are Claire L. Parkinson, 2019: A 40-y record reveals gradual Antarctic sea ice increases followed by decreases at rates far exceeding the rates seen in the Arctic. Proceedings of the National Academy of Sciences of the United States of America, 116, 14414-14423, doi: 10.1073/pnas.1906556116.
    - Will be rectified.
- Lines 461-462: Updated details are: Smith J., Nogi Y., Spinoccia M., Dorschel B. and Leventer A. (2021) A bathymetric compilation of the Cape Darnley region, East Antarctica. Antarc. Sci., doi: 10.1017/S0954102021000298.
    - Thank you for the update - will be updated.
- Lines 466-467: Turner, John et al., 2016: Absence of 21st century warming on Antarctic Peninsula consistent with natural variability. Nature, 535, 411-415, doi: 10.1038/nature18645.
    - Will be completed.