Thank you for helpful comments that have improved the manuscript. The full content of comments and responses for Anonymous Reviewer #2 are below.

Reviewer Comments in **black** and responses in **red**

Anonymous Reviewer #2

The author presents a comprehensive study on the Southeast Greenland (SEG) fjord systems, focusing on surface ice conditions and freshwater flux, especially differentiating the glacier-derived and Landfast ice from Landsat-8 and MODIS. The manuscript is well-structured, providing a significant reference for understanding the possible dynamic habitats in the context of ongoing climate change. But, I do have some concerns about the satellite algorithm, uncertainty quantification, and interpretation of the data in the following:

(1) When I saw the title, I thought the authors would include sections connecting the surface ice and freshwater flux with the biological system. But I didn't find that part, only some future implications, which made the title a little disconnected without hitting the main point of the paper.

The manuscript intent is to provide a physical science basis for ongoing and future biological applications. The paper metrics and methods were created via collaboration between physical and biological scientists and we note previously published research that uses a subset of these data to gain further knowledge of Southeast Greenland polar bears. The intent of this paper is not make further connections between physical and biological conclusions within this publication, but rather to create and share a physical science fjord ice analysis that was designed for biological applications. We feel the paper title reflects that intent, but are open to suggestions for alternative titles.

(2) Line 88-93. I don't know why you chose Skioldungen and Kangerluluk since, from Figure 1, those don't have the glacier location upstream, which also compromises the robustness of the characteristics of the SEG fjord. (I knew they are heavily occupied by polar bears, but since you want to link the physical and biological fjord system, doesn't it make sense to consider the glacier and polar bear activities as well?)

These fjords are included precisely because they help to create a wide range of fjord environments with varying levels of glacial ice input and areas that include varying polar bear use.

(3) Line 157: Can I have more details on why you chose 15% as your threshold? Does it make a large difference in differentiating the transition period in Figure 6?

Figure 9a is a time series of the offshore sea-ice area at the mouth of fjord #15. Our objective is to detect the start of the sea-ice season in the fall and the end of the sea-ice season in the spring by choosing a sea-ice area threshold and finding the dates when the time series of sea-ice area crosses that threshold. We want to choose a relatively low threshold to closely detect the timing of sea-ice appearance (fall) and disappearance (spring), but not too low that noise or small fluctuations affect the timing. The threshold needs to be scaled or normalized to something, and we've chosen to scale it to the mean March-April sea-ice area, which is set at 100%. The choice of 15% of that value for the threshold is somewhat arbitrary, but it meets the conditions of being relatively low while still excluding noise and small fluctuations from detection. The resulting interval of time between the disappearance of sea-ice in the spring and the re-appearance of sea-ice in the fall, marked by the vertical dashed orange lines in all panels of Figure 9, matches reasonably well with the period of time when the sea surface temperature (SST) is above the freezing point of -1.8 degrees C (panel e). That gives us confidence that the 15% threshold is a reasonable choice. A reader can also view the full time series within our figures if they would like to consider different thresholds.

Regarding the effect of the threshold on the spring and fall transition dates shown in Figure 6, choosing a threshold larger than 15% would cause the spring dates to shift to the left (earlier) and the fall dates to shift to the right (later), but the shapes of the curves would not change much. Also, since the slope of the sea-ice area time series is relatively steep at the times when it crosses the threshold, a small change in the threshold would lead to a very small change in the crossing dates.

(4) Line 163-170: After reading the paper, I hardly found an explanation for why you also analyze the SST and sea ice coverage in Figure 9 (e) since you didn’t mention their changes and connection with the Fjord system. If you want to show the MAR performance in those parameters, I would suggest you put them in the Appendix.

The reviewer is correct that didn't mention the MAR SST or sea-ice coverage in Figure 9e outside of the description at lines 163-170. We have added the following text to the revised manuscript in section 4.2: "Finally, we compared the spring and fall sea ice transition dates as calculated from AMSR2 sea-ice coverage (e.g. Fig 9a, vertical dashed orange lines) vs. MAR sea-ice coverage (e.g. Fig 9e, vertical dashed blue lines) for the eight focus fjords. For the three northern fjords (#15, 18, 31), which are all north of 64N, the agreement is quite good: the mean dates (across 5 years) are within 3 days of each other. These fjords have relatively well-defined annual cycles of sea-ice coverage, so there is little ambiguity in identifying the transition dates. For the five southern fjords, which are all south of 64N, the

agreement is less good: mean differences can be as high as +/-2 weeks, with larger variability than for the northern fjords. These fjords have relatively choppy annual cycles of sea-ice coverage, with lots of spikes, so the detection of the transition dates is noisier."

Regarding whether Figure 9e belongs in the Appendix, we note that the MAR data for all the other fjords are already in the Appendix (Figures A2-A8, bottom panel). We don't think it's necessary or desirable to break up Figure 9 and put panel (e) in the Appendix by itself. The sea surface temperature (SST) and offshore sea ice coverage data are included in the multi-panel time series because we are endeavoring to provide more of a *systems* characterization of the Southeast Greenland fjord environment, so it is important that these metrics are viewable together.

(5) Section 3.4. I have the same concerns as Reviewer 1 since Landsat-8 and MODIS are primarily limited and impeded by clouds and water vapour. How many of the images are really affected by the cloud? The visual classification doesn't consider the cloud, right? Then, how much real MODIS and Landsat-8 data are used in the visual classification compared to the full-sky MODIS and Landsat-8 before cloud filtering? Can we take the filtered data as well-representation? Does it make the results have a systematic bias?

As noted in response to Reviewer #1, we have clarified the information in the Figure 3 caption to make clear that only imagery determined to have clear conditions over the fjord analysis area are included in the figure and in later analysis. This determination is made directly within our team; we do not depend on product cloud classifications to do this since those can run into problems distinguishing across clouds, ice, etc. In this way, clouds are removed before fjord surface digitization is undertaken. This does limit the amount of data that was usable for analysis and Figure 3 clearly shows the final distribution of data used. There may be a systematic bias related to data reduction due to polar night, but we do not identify a systematic bias due to cloud cover.

(6) Line 205-209. I know visual classification is quite time-consuming work, and you've analyzed a lot of MODIS and Landsat-8 data, but can I know the satellite passing by date between two satellites? Further questions on how to clarify the potential limitations or biases introduced by manual digitization and how they were mitigated would add to the methodological rigour. How can the uncertainties from MODIS and Landsat-8 be quantified since Table 2 seems quite dependent on individual experiences?

Satellite imagery dates are reflected in Figure 3 and also included in all archived datasets. We have detailed our methods for analyzing accuracy of the manual digitization process

and reported those results in sections 3.4 and 3.5, so we are uncertain what additional information the reviewer is seeking. It is correct that individual experience influences digitization, so we had a single person complete all manual digitization as another means to reduce bias and minimize uncertainty. We feel this is the best possible method given the inherent constraints surrounding manual digitization. We also provide both written (Table 2) and visual (Figures 4-5) references to demonstrate ice classification categories. Early reports from external collaborators (personal communication) suggest good correlation for similar output from other research groups.

(7) Figure 10. What's the spatial resolution of frequencies in fast ice presence and glacial ice presence?

Based on our understanding of this request, we believe that the reviewer will find the information of interest in the stacked time series plots that are included in the primary manuscript and the appendix. Panel (b) in these figures shows the area and the percent fjord area coverage for fast ice and for the different glacier-derived ice types.

(8) I am slightly lost when you show solid ice discharge, landfast ice, and Glacier ice values. Can you explain their possible implications? Since, for me, they are just shown here without any inner interpretation or further analysis. The integration of diverse datasets is a strength of this study. However, discussing the challenges and uncertainties involved in combining different data types (e.g., remote sensing data with climate model outputs) and how these might affect the interpretation of results would be beneficial. Moreover, a more detailed analysis of the variability observed across the fjords and its potential biological implications would add depth to the study.

This study endeavors to provide characterization of the Southeast Greenland fjord ice environment but does not endeavor to provide in-depth analysis of the processes connecting these metrics (e.g., the transformation of solid ice discharge into glacier-derived ice with varying character and spatiotemporal presence). We hope that future research both inside and outside of our research team will continue to develop process, physical system, and biological insights that use these multiple data streams and move beyond the results presented and discussed in this paper.

(9) As I mentioned in the first point. The manuscript misses a deeper discussion on the specific biological applications of the physical environment characterization provided. For instance, detailing potential impacts on the habitat preferences, migration patterns, or population dynamics of key species would directly link the physical and biological aspects of the fjord systems.

We have intentionally limited the extent to which we speculate on biological implications of this physical system characterization since this study is a focus on the physical system observations and not the combining of physical and biological. We do biological context regarding the motivation for the study and also limited comment on biological implications within the Discussion and Conclusion. We prefer not to introduce additional speculation on biological implications because we are not providing new analysis on these connections.