Review 1 of Antwerpen et al 2026, TC Discussion

The manuscript presents a Machine Learning approach to calculate the spatial and temporal evolution of albedo in the bare ice zone on the SW part of the Greenland Ice Sheet. PIXAL is trained on MODIS albedo and compared to the method for calculating albedo in MAR, after calibrating the MAR method to the same MODIS data. While I found the interpretation of the SHAP a little difficult to follow and I was missing some discussion on the incident angle/slope dependence on MODIS data, the manuscript is clear and well presented and I can recommend publishing after some minor revisions. I hope the authors will take this work further in the future and implement PIXAL or something similar in MAR.

Here are my line by line comments:

48: maybe add to the sentence something like: […] resulting in a net mass loss *and exposure of the bare ice surface every year*.
Thank you for the suggestion, I added "[...] and the potential exposure of the bare ice surface."

67: I think Cryoconite is a collective term for dust and algae, and not a type of particle in itself?
You're right, I removed cryoconite from the list.

88: Consider adding a *thus*: […] which can lead to underestimates of surface melting and *thus* sea level rise […]
Done.

225-227: Maybe I missed it somehow, but for the albedo prediction in the XGBoost you use all the MAR output listed in line 140-146, except Albedo and cloud cover. But what about: surface melt and shortwave upward radiation. These must also be dependent on the MAR albedo. But I am unsure how this affects the results, could you maybe add a sentence about this?
Shortwave upward radiation is included in the list in 140-146. We include runoff instead of surface melt.

Figure 2 c): Why do you think there is this spatial difference in the performance? I wonder if the difference you see is due to slope? There must be an issue with MODIS seeing albedo differently depending on slope. I think it would be easy to compare that here – although maybe out of the scope for this study.
That's an interesting point, it could be related to the slope, satellite viewing angle, and sun angle. But it's unfortunately out of scope for this study.

338: I am not sure I understand this: “In other words, the SHAP value shows how much the ice albedo prediction increases or decreases due to each individual feature relative to the mean ice albedo”. When you say ice albedo prediction increases do you then mean that it is the performance of the albedo prediction that gets better?
With an increase/decrease in ice albedo prediction I mean that the absolute value of the predicted albedo is higher/lower. In this sentence I do not relate it to the performance, just the actual albedo value. I changed the sentence to “[...] how much the predicted ice albedo value increases or decreases [...].”

340-342: I don’t understand why you determine the surface height and slope to be the primary and then the climatic to be the secondary. To me it looks like temperature, shortwave incoming and wind is a better predictor than slope and surface height. Temperature in particular looks unambiguous. But maybe it relates to the fact that I have have not really understood what you are comparing in the SHAP? I think a few sentences should be added to clarify this.
I adjusted the wording in this section, the abstract, and the conclusion to account for this. I removed the distinction between primary and secondary drivers.

367-374: This is all correct, but maybe add some discussion here on what does that mean for results from PIXAL if used for future runs?
This is addressed in L462-469.

380-386: As mentioned above, I think that MODIS albedo is likely affected by the slope, giving a somehow skewed picture of actual albedo e.g. Wang and Zender (2010) in “MODIS snow albedo bias at high solar zenith angles relative to theory and to *in situ* observations in Greenland” (https://doi.org/10.1016/j.rse.2009.10.014). I am missing bit of discussion on this.
I added this sentence: “Note that the MODIS-derived albedo may be affected by high solar zenith angles (SZA) >55° (Wang and Zender, 2010; Alexander et al., 2014). This may introduce a negative albedo bias, especially in areas with a high ice surface slope.”

426-230: I was looking forward to hearing your thoughts about the albedo bias as mentioned above. To me this seems like an obvious next step and I am looking forward to see your results! In this study it would be interesting to see how often MAR albedo based on PIXAL would have to be recalibrated.
We are excited about this next step too. However, it is outside of the scope of this manuscript unfortunately.

440-450: Again I am missing some discussion on MAR and incident angle / slope
This is addressed in the comment above.