Review of "Impact of coastal East Antarctic ice rises on surface mass balance: insights from observations and modelling", by Kausch et al. (TC-2020-66)

General comments

In this well-written paper, the authors use a comprehensive set of measurements of surface mass balance (SMB) across two Antarctic ice rises together with meteorological observations, a regional atmospheric model and a SMB model to investigate the factors that control the spatiotemporal variation of SMB across these features. The authors conclude that variations in SMB across the ice rises is controlled by both orographic forcing of snowfall and wind-borne transport of snow, with the two effects partially cancelling each other. As a result, SMB at the ice rise summit is close to its value on the surrounding ice shelf. The results have important implications for the interpretation of ice core records of SMB obtained from ice rise summits.

The paper is very clearly written and the methodology is sound and is well-explained. The conclusions are soundly based on the data and the analysis. I recommend publication of the paper following minor revisions, as detailed below.

Main points

1. Section 2.5. It is not entirely clear to me whether the values used for the various SnowModel parameters were chosen *a priori* or were used to "tune" the model to obtain the best fit to the observations. Tuning is a perfectly valid approach, but you should clearly state if that is what was done. Did you carry out any validation of the SnowModel wind field against your two AWSs – very little use appears to have been made of this source of data? You justify using a rather large (5 mm) value for roughness length on the grounds that Amory and others have measured similar values over the large sastrugi fields found in a strong katabatic wind regime in Adélie Land. Did you actually observe similar large sastrugi on your ice rises? I'm not sure how you can be certain that your parameter choices give you an "appropriate representation of erosion frequency" (lines 187-188) unless you have observations to validate this.
2. Lines 284-286: "Therefore, in case of the FKIR, it seems like the erosion at the ice divide partially cancels out the higher SMB values due to orographic uplift and results in an overall lower SMB at the ice core location, which better resembles the surrounding shelf." Is this likely to be a universal result for ice rises? Orographic enhancement of precipitation on the upwind side probably scales with the broad-scale topographic slope on this side, while erosion at the ice divide will scale with curvature at the summit. Is it just coincidence that the two are approximately equal at FKIR or do the fundamentals of ice dynamics mean that this ratio should be the same for ice rises of any scale?

Minor points and typographical corrections

1. Line 27: "interannual" (one word)
2. Line 40: Insert a comma after "ice rise"
3. Line 62: Maybe spell out the full names of the TIR and FKIR when you first mention them?
4. Line 91: "sticks" (plural)
5. Figure 2: The black diamonds are not that easy to see. Maybe use markers above the colour bar instead?
6. Line 154: IFS , not ISF

7. Line 170-171: What does the RACMO2 orography look like when compared to TanDEM-X?
8. Section 3.1: There are a lot of numbers given in the text. It might be useful to summarise them in a table or bar chart.
9. Line 234: “linearly”
10. Line 288: “downwind” instead of “upwind”?