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Interactive comment on "Impact of coastal East Antarctic ice rises on surface mass balance: insights from observations and modeling" *by* Thore Kausch et al.

Thore Kausch et al.

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TC Response to referee 1

Dear referee,
First of all thank you for carefully reading our manuscript and for your positive review and constructive criticism. The points you are raising are all very much justified and we tried our best to address them as good as possible.

Below you can find your original comments with our answers in blue as well as updated sections from the text in *italic*.

Yours sincerly,
Thore Kausch and co-authors

1 Referee 1 comments:

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)

1.1 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.

1.2 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.
For the roughness length and the threshold shear velocity we indeed used forward modelling to try several different values and in the end chose the ones, which would fit the observations best. This was not clear in the text before. We added a few sentence to clarify this in the text.
L182-184: For both, the roughness length and the threshold shear velocity we tried different values using forward modelling. We varied the roughness length between 0.05 and 0.00005 m and the threshold shear velocity between 0.3 and 0.8 m/s, and chose the values which qualitatively fit the reconstructed SMB values from the GPR best. In the end we chose a roughness length of 0.005 m and 0.6 m/s for the threshold shear velocity.
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?
Regarding the AWS, the reason that we made so little use out of the data is that we only had four months of continuous data available at the time. However we did use the AWS data to justify the "appropriate representation of erosion frequency" but failed to mention this in the text. (There are more details below in the part about the erosion frequency)
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?

We indeed observed large sastrugi in the field.

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.

We used the AWS regarding the "appropriate representation of erosion frequency" but failed to mention this in the text. We added a few sentences to clarify this.

L188-193: Based on the frequency with which RACMO2 simulated wind speeds exceed 11.4 m/s for the ice rise and the frequency with which wind speeds observed by the AWS exceed 11.4 m/s, we estimated that with those settings, we have an appropriate representation of erosion frequency in SnowModel. Between January and April 2018 the AWS on the windward side measured daily wind speeds exceeding 11.4 m/s 50 % of the days. This is in agreement with observations from (Amory, 2020) who observe similar erosion frequencies between January and April for a cite in coastal Adélie Land with a comparable elevation of 450 m.

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?

This is certainly an important question to ask. While both processes are always oppositional, it is likely coincidental that the two are approximately equal. This is because both, the erosion at the peak of the ice rise and the orographic enhancement of precipitation do not only depend on the curvature and the slope of the ice rise, but also on a large number of other factors like wind speed, atmospheric moisture content and the height of the ice rise. It would be possible to imagine scenarios in which one process happens in the absence of the other, for example a very dry and extremely windy day, where erosion would occur, while there is no enhanced precipitation because there would simple be none. However, a more sophisticated model, which for example includes the possibility to locally decrease the threshold shear velocity with snowfall, in combination with observations from multiple ice rises would probably be needed to say this for sure. We added a few lines in the discussion to address this.

L293-299: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. This is likely a generic feature for ice rises of all shapes as the orographic enhanced precipitation scales with the slope of the ice rise and the erosion at the peak of the ice rise scales with the curvature of the ice rise and both processes are generally opposed to each other. However, the magnitude of each process individually might vary strongly between ice rises as both processes also scale with other factors like wind speed and atmospheric moisture content, in addition to the topography.

1.3 Minor points and typographical corrections

1. Line 27: “interannual” (one word)

Done.

2. Line 40: Insert a comma after “ice rise”

Done.

3. Line 62: Maybe spell out the full names of the TIR and FKIR when you first mention them?

FKIR and TIR were project internal names, named after leading scientist in the Mass2Ant field expedition to the ice rises. However since there are official norwegian names for both ice rises we decided to rename them. Therefore we changed FKIR to Lokeryggen ice rise (LIR) and TIR to Hammarryggen ice rise (HIR). But we kept the name Derwael ice rise (DIR) since it was already named like this in earlier publications we are citing. We added a note though mentioning the norwegian name of DIR (Kilekollen).

4. Line 91: “sticks” (plural)

Done.

5. Figure 2: The black diamonds are not that easy to see. Maybe use markers above the colour bar instead?

Done

6. Line 154: IFS , not ISF

Done.

7. Line 170-171: What does the RACMO2 orography look like when compared to TanDEM-X?

RACMO uses Bamber et al.'s 2009 1km resolution DEM, whereas TanDEM-X has a 90m resolution for our study area, which naturally makes it significantly more detailed. While the overall topography of both data sets is comparable, the reduced resolution of Bamber et al.'s 2009 DEM compared to TanDEM-X can result in difference of up to 50 - 100 m on the flanks of the ice rises.

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.

It is true that there are a lot of numbers given in the text, however they are also already displayed in figures 3 and 4, which I personally think should make the numbers easier to grasp than in a table.

9. Line 234: “linearly”

Done.

10. Line 288: “downwind” instead of “upwind”?

Changed to downwind.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-66, 2020.

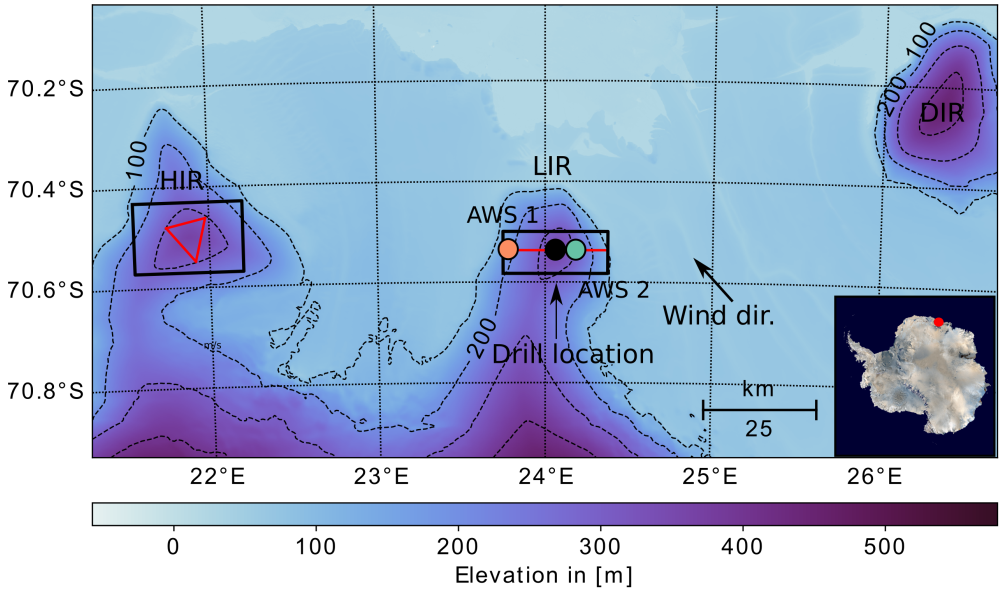

Fig. 1.

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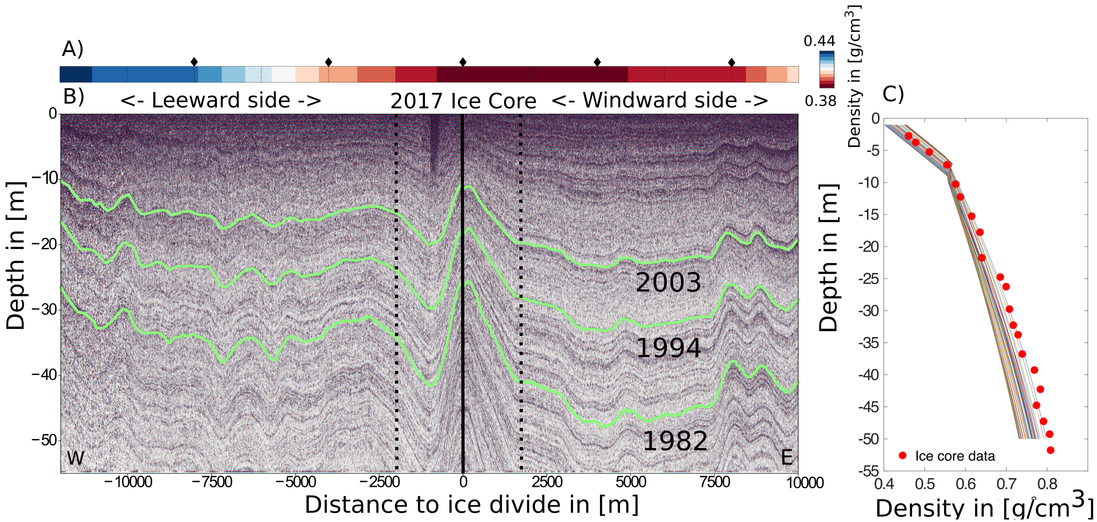

Fig. 2.

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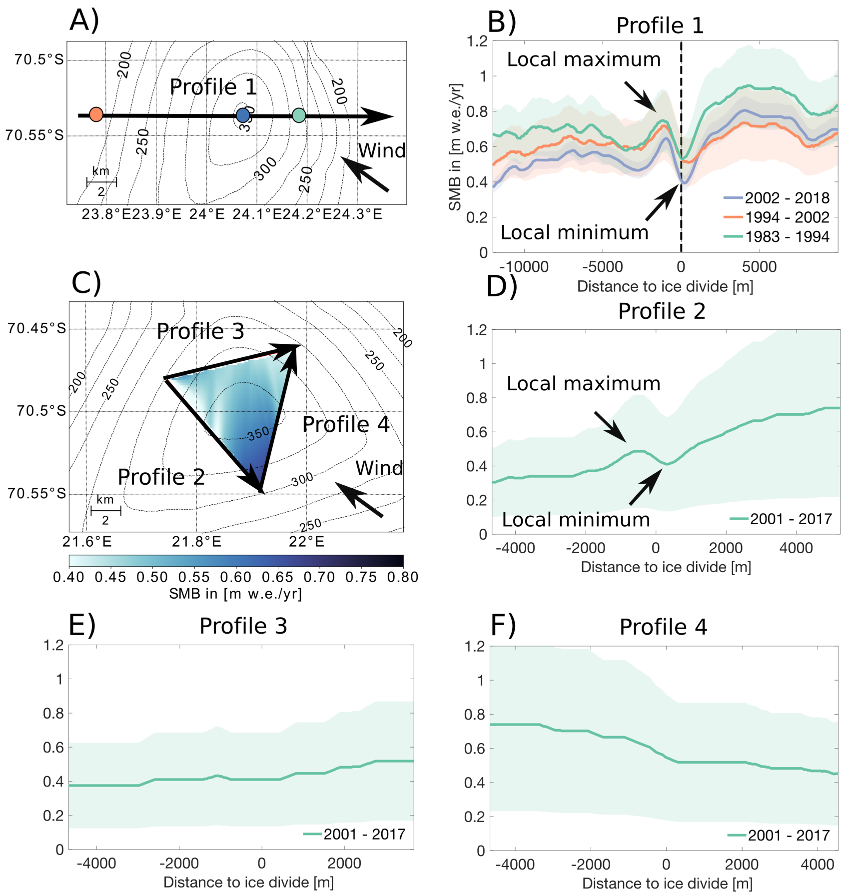

Fig. 3.

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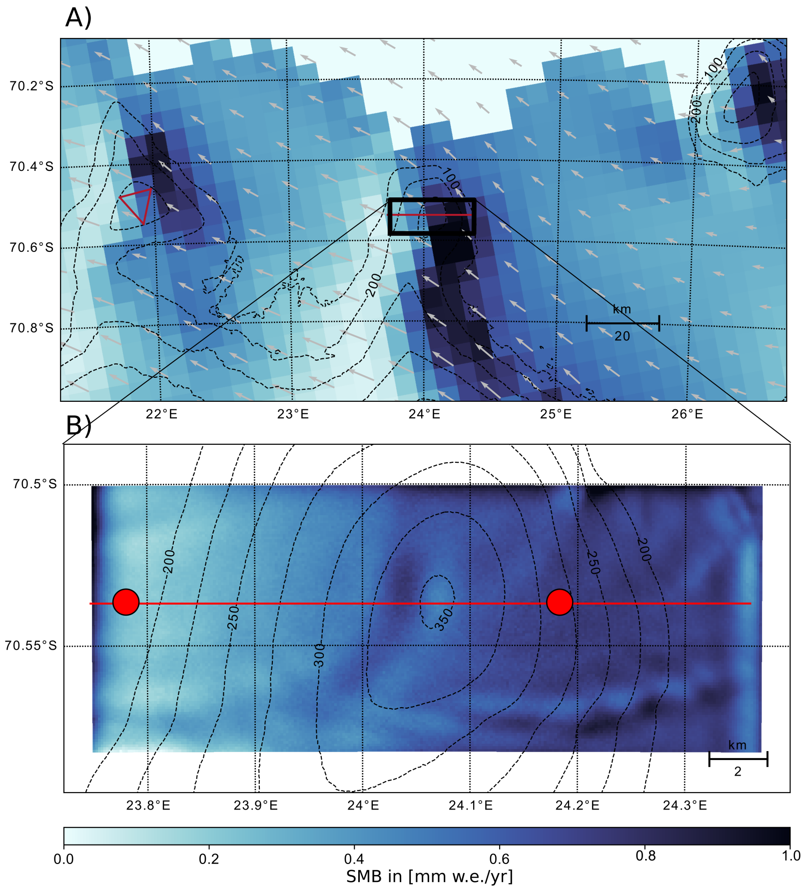
A)
70.2°S
70.4°S
70.6°S
70.8°S
22°E
23°E
24°E
25°E
26°E
100
200
km
20
B)
70.5°S
70.55°S
23.8°E
23.9°E
24°E
24.1°E
24.2°E
24.3°E
200
250
300
350
km
2
0.0
0.2
0.4
0.6
0.8
1.0
SMB in [mm w.e./yr]

Fig. 4.

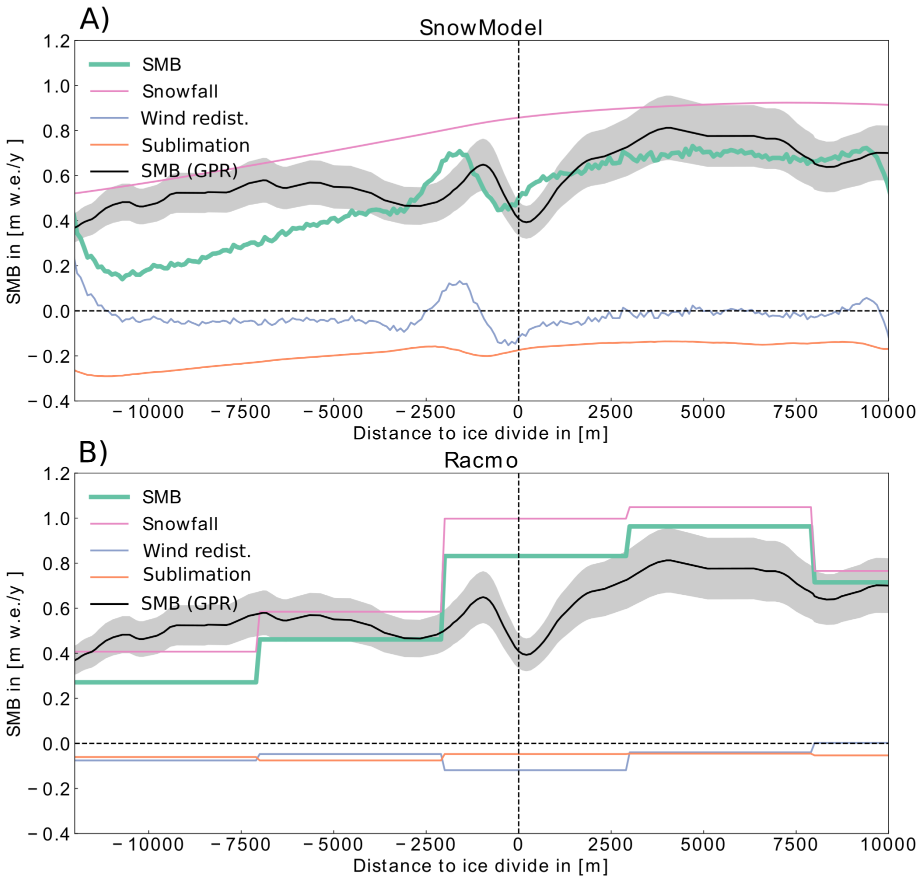
A)
SnowModel
SMB
Snowfall
Wind redist.
Sublimation
SMB (GPR)
SMB in [m w.e./y]
Distance to ice divide in [m]
B)
Racmo
SMB
Snowfall
Wind redist.
Sublimation
SMB (GPR)
SMB in [m w.e./y]
Distance to ice divide in [m]

Fig. 5.

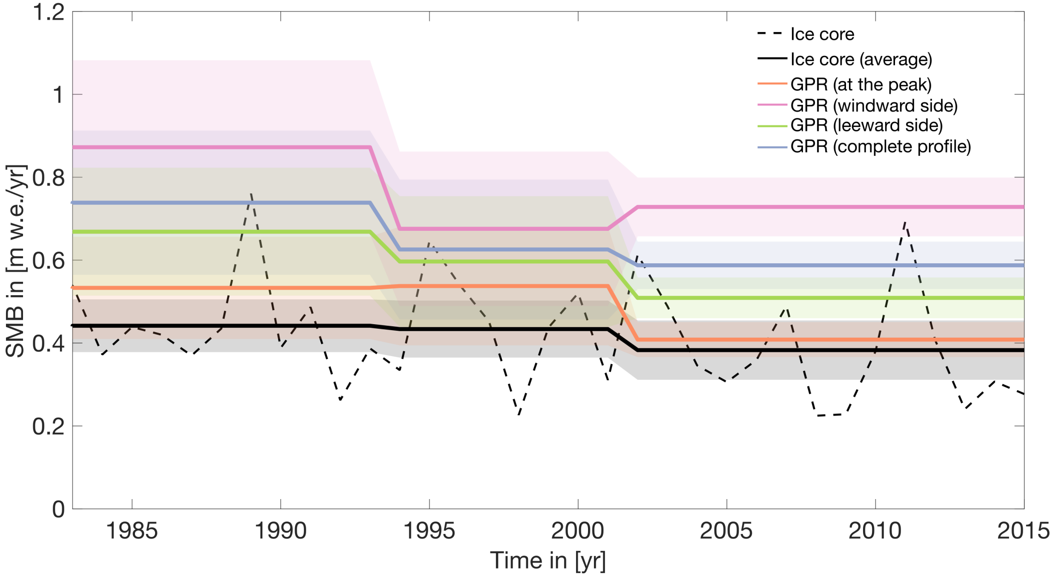

Fig. 6.

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