Reply to reviewer comments on

"Downscaled surface mass balance in Antarctica: impacts of subsurface processes and large-scale atmospheric circulation"

by

Nicolaj Hansen, Peter L. Langen, Fredrik Boberg, Rene Forsberg, Sebastian B. Simonsen, Peter Thejll, Baptiste Vandecrux, and Ruth Mottram

Dear Editor Alexander Robinson,
On behalf of my co-authors and myself, I would like to thank the two reviewers for their comments on our manuscript. The reviewers have made an extensive review of the manuscript language and we have followed their suggestions to our best efforts. In the following, we provide a point-by-point answer to all the issues raised by the reviewers. We have gathered and numbered all issues raised by the reviewers (Anonymous Referee #1: 1-29 and Referee #2 Christoph Kittel: 30-56), all issues will be followed by our suggestions for improvement to the manuscript highlighted in red.

We have added a figure in section 3.1, this figure is the new figure 3. This means that the old figure 3 is now figure 4 ect. Furthermore, we discovered a small bug in our firn density calculations so the numbers in table 3 and the text around it have been updated.

Best regards,
Nicolaj Hansen

Anonymous Referee #1

1. First, the manuscript and the reader's ability to understand and assess the data in them, would be improved if some more attention was given to the figures' presentation. Much of this can be summarised as a making a better choice of colour scales and markers. In particular figures 1 and 4 rely on colour scales that do not steadily increase in their darkness, but instead jump around somewhat. This makes assessing more negative and more positive values difficult. Following on from this I would suggest the authors ensure the colours used for the Fixed, Dyn03, and Dyn15 results are consistent across figures (and ideally different from the colours used in the diverging scales). Likewise improvements can be made in the maps in figures 3 and 4: a clearer indication of the zoomed areas, which will be aided by a better choice of background colour and non-biased colour scales. Throughout the figures I recommend that the authors use larger text and labels and better and more consistent labelling of subfigures.

   That is a good idea, the colour scales/choices have been changed, as well as the text and labels size. The new figures are shown under the specific comments.

2. Second, and more scientifically interesting, is the question of resolution. Much effort has gone into considering different layer schemes and how mass is transferred between them down to a scale of 0.065 m w eq. In the horizontal direction however the resolution is 12.5km. Clearly there are computational demands that limit this resolution, but it does raise a question: namely, given that some features that indicate or result from localised melt and mass loss can be of this same length scale or less, how is this handled in the model? Is it simple averaging per pixel? Is a higher resolution used in some geographic areas? Or are the smallest of these features simply not seen / modelled?
   The model has been tuned to mimic the average behavior of the ice sheet surface at 5-12km scale. It cannot resolve subpixel processes. However, the small scale features caused by surface melt (as lakes and streams) translate into an increase of water content in the model.
   To clarify this in the manuscript we suggest adding this in line 100
   "…. removed from the results. Furthermore, the model has been tuned to mimic the average behavior of the ice sheet surface at a 5-12 km scale. It cannot resolve subpixel processes. However, the small-scale features caused

by surface melt translate into an increase of water content in the model. Despite the forcing….”.

3. A final general point: given that the manuscript focuses on firn and surface processes for which surface radiation fluxes are important, the treatment of the albedo seems a little simplistic. Perhaps the authors could expand on this. I understand that computationally it is probably hard to go beyond a broadband albedo, but are the values stated extremes, or are they allocated for each class of surface i.e. all fresh snow is designated as having an albedo of 0.85, or is this dependent on grain size, density etc?
   Yes, indeed it is simplistic, we are planning to do more work on the albedo scheme later this year. However, it could be described better in the manuscript, this will be added starting in L87:
   “Following Langen et al. (2015) the shortwave albedo is computed internally and uses a linear ramping of snow albedo between 0.85 below -5 C and 0.65 at 0C for the upper-level temperature. The albedo of bare ice is constant at 0.4. Furthermore, a transition albedo is calculated for thin snow layers on ice, based on Oerlemans and Knap (1998) with an e-folding depth of 3.2 cm for snow.”

4. I think adding some comments on both the horizontal resolution point and some further details of how albedo is treated in the model would assist the reader with a general glaciology background, but who is less-versed in the details of such models.

   Yes, that is a good idea, we have done that, see answers to comment 2 and 3

5. l29 "as such might" -> "due to their role in"
   Changed

6. l57 "Acknowledging that the in-situ observations might be challenged judging the performance of the SMB model" -> "Acknowledging that it might be challenging to judge the performance of the SMB model against in-situ observations"
   Changed

7. l71-72 Regarding "The SAM is an atmospheric phenomenon..." this seems to be a more introductory descriptive sentence and better placed a few lines earlier when the SAM is introduced
   That is a good point, we suggest to move it up to L69:
   “…. for this reason we concentrate on its effects in this study. The SAM is an atmospheric phenomenon found across the extratropical southern

hemisphere that influences the climate over and around Antarctica (Fogt and Marshall, 2020). Marshall et al. (2017) found ……"

8. l88 Are these values for short-wave albedos? If so, add "shortwave" for clarity.
   Yes it is, from Van de Wal and Oelremans 1994 and we have added "shortwave" and the reference at line 88.

9. l89-90 "Specific, for the HIRHAM5 Antarctic simulations, was that we used the Antarctic domain defined in the Coordinated Regional Climate Downscaling Experiment" -> "Specifically (or finally), for the HIRHAM5 Antarctic simulations, we used the Antarctic domain defined in the Coordinated Regional Climate Downscaling Experiment"
   Changed

10. l90 12.5km resolution. This comparable size to small features often related to negative SMB: melt ponds, some glacier streams, blue ice areas. How are these handled?

    See answers to comment 2

11. l100 " Despite the forcing is based on 6 hourly ..." -> " Despite the forcing being based on 6 hourly...". Also could the forcing be interpolated to 1 hr time steps, or alternatively what would the impact be of simulating the subsurface at the 6 hourly interval of the forcing?
    First part: Changed,
    Second part: this sentence is badly explained in the manuscript, we suggest to rephrase the sentence :
    "The subsurface scheme is updated hourly by interpolating the 6 hourly forcing files to 1 hourly time steps. To ensure a smooth transition between two 6 hourly files, a linear interpolation in time between the two nearest 6 hourly files is used."

12. l101 "... model is following the ..." -> "... model follows the ..."

    Changed

13. l122 "... which is fixed ..." -> "... which are fixed ..." OR "... the number of which are fixed ..."

    Changed to: the number of which are fixed

14. l120-136 How is the bottom boundary handled? Or equivalently, how is the remainder of the mass between the lowest layer and the base of the ice sheet handled?

    To clarify this, we suggest to add this, at the end of line 136:
    "The bottom of the lowest model layer is assumed to exchange mass and energy with an infinite layer of ice with a temperature, like in the Fixed model (Langen et al., 2015), calculated from climatological mean of the HIRHAM5 2 m temperature."

15. l157 Diagnosed snow depth? What does diagnosed refer to here?

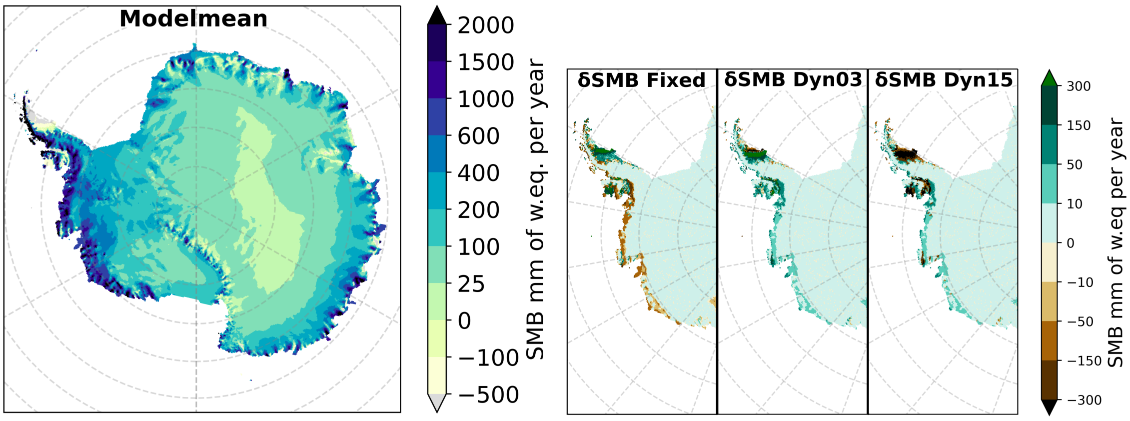
    Diagnosed snow depth refers to an estimate based on the snow concentration in each layer, calculated from the top down. It includes only snow above the first perched ice layer.
    To clarify this, this could be added in line 157:
    ".....refreezing, diagnosed snow depth (which is an estimate based on the snow concentration in each layer), net short wave…."

16. Table 1 "Fist snow then ice" -> "First snow then ice", I think?

    Yes, you are correct

17. Fig 1: The colour scale here is a little strange in the left hand panel refered to as (a) in the caption. Specifically it does not steadily get darker with a single hue, but changes part way through. This gives some bias and odd visual affects and probably makes it harder for those with colour blindness. I would suggest that the authors convert to using a standard diverging colour scheme such as Cynthia Brewer's Red-Blue scheme that can be found here: colorbrewer2.org

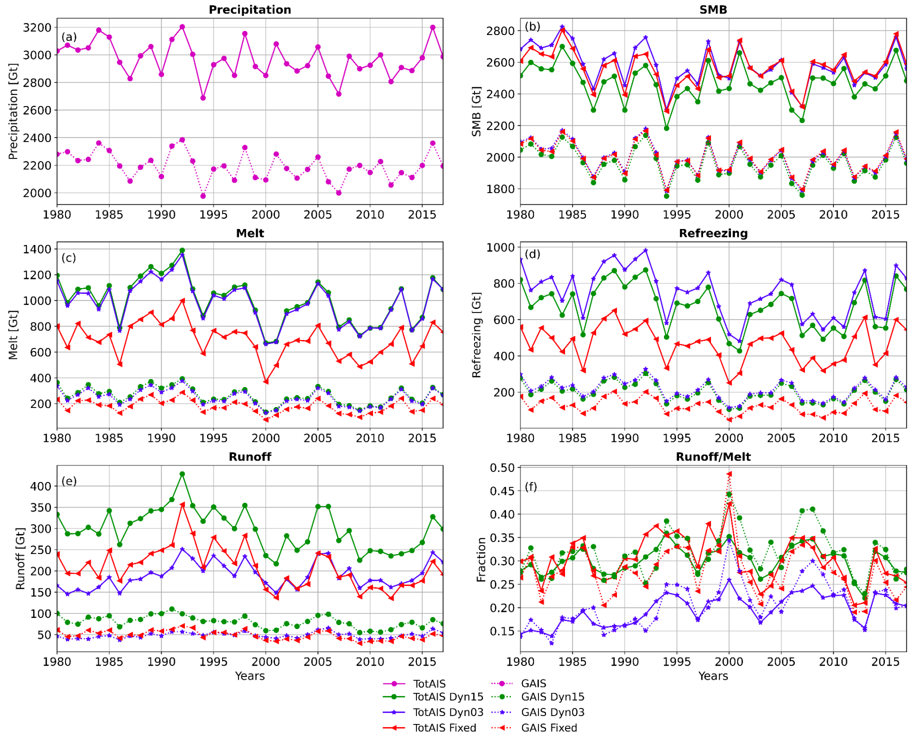
    We have changed the color scheme accordingly (Larger figure in the paper)



18. Also please check journal guidelines for placement of caption labels (a) and (b); in their current location, they were less obvious than placed outside top-left for example.

    We have checked that and make sure that the caption labels are placed consistently.

19. Fig 2: Would recommend consistent labelling of sub-figures. Here they are capitals, the last figure was lower case and the placement has changed between figures. They are refered in the text as lowercase. Also I would suggest adding a legend entry for precipitation, and chosing more distinct symbols for the TotAIS and GAIS cases.
    According to the journal "*Labels of panels must be included with brackets around letters being lower case (e.g. (a), (b), etc.)*" so we have maked sure that the labelling are all lower case letters, consistent and referred to correctly. Legend for precipitation has been added, and the GAIS symbols have been changed too. (Larger figure in the paper)



20. l217 Given the importance of albedo in driving subsurface processes I wonder if it is possible to use a more refined scheme here. The single broadband value for snow / bare ice mentioned earlier appears crude compared to the detail given in the handling of layers for example. Would a density-based albedo be an option, even if broadband? If this is the case, this should be clarified.

    As mentioned earlier, we are well aware that it is a simple albedo scheme. For present day/reanalysis runs we are working on adding observational data instead of a modelled albedo. For historical runs and projections, our aim is to start a phd-project later this year to develop a sophisticated albedo scheme. See comment #3 for more details on the albedo.

21. Table 3: which units are being quoted? I guess kgm-3, but it should be stated.

    Yes, it is kgm-3, it has been added

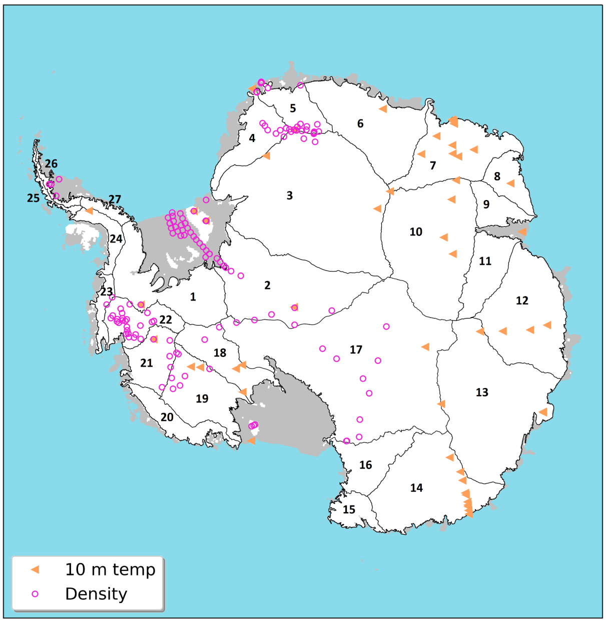
22. l230: "Statistical comparison of mean difference and one standard deviation between the firn cores and the modelled densities, for the three simulations are given in Table 3." -> "A statistical comparison of the mean difference and

one standard deviation between the firn cores and the modelled densities are given in Table 3 for the three simulations."

Changed

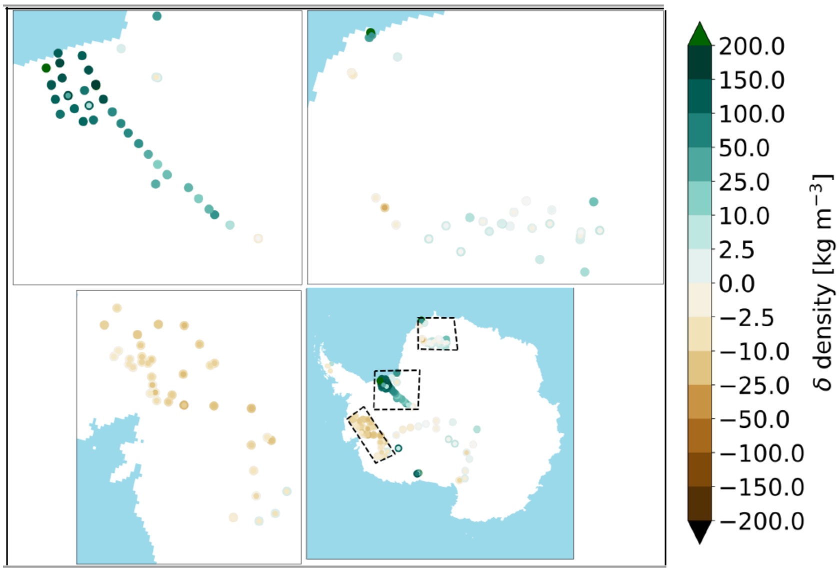
23. Fig 3: The choice of colours here has the potential to confuse when red and blue are used in previous and later maps to signify high and low deviations. The choice of colours for the AIS and ice shelves does not help. If a light blue were used for the Southern Ocean, a light grey could be used for ice shelves leaving the GAIS white, making the markers clearer, and allowing a better and wider choice of colours, for example purple and green to remove any ambiguity.

The colours/symbols for the observations have been changed to colours not used earlier/later. Furthermore we have changed the colours to the suggested ones, Southern ocean = light blue, GAIS = white and the ice shelves = light gray. (Larger figure in the paper)



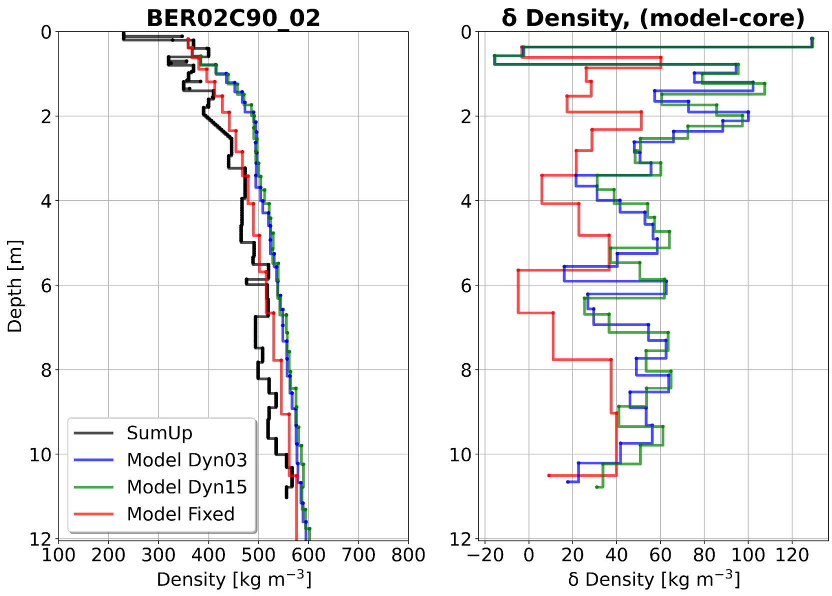
24. Fig 4: Again, this could be clearer. It is very hard to discern the zoomed in areas on the larger maps. A thicker line is needed to indicate these to the reader. The colour scale has the same issue as in figure 1. Again, I would recommend using a standard and well-tested red-blue diverging scale. Changing the colouration of the underlying map as in Fig 3 would also help the clarity of these figures.

A thicker line for the boxes has been added
Furthermore, we have added a better colour scheme like for Fig. 1 and done the same colouring of the underlying map as in Fig. 3 (Here is the figure for the Fixed version, the Figures for the Dyn03 and Dyn15 is in the pdf, but they have the same colors



25. Fig 5: The labels and other text needs to be made larger so that it is legible to the reader. Again labelling of the subfigures needs improving and to be made consistent. Here, the issue is that the labels (a) and (b) lie closer to the panels that are, in fact, (c) and (d)

The labels/text has been made larger, for the labelling see answer to comment #18 and #19, furthermore the labels have been moved closer to their subfigues. (Larger figure in the paper, the rest b,c,d is shown in paper)

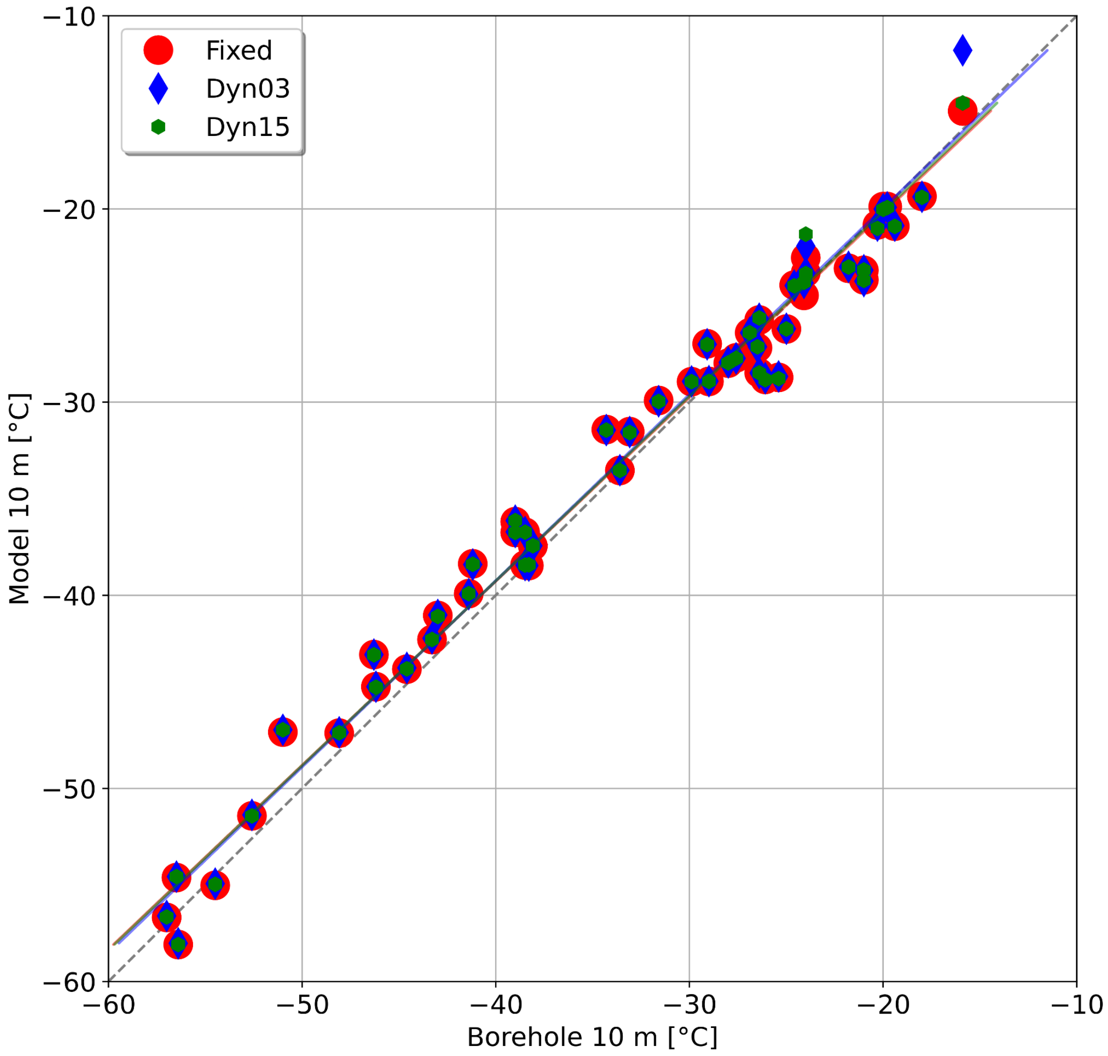


26. l271 "the agreement becomes smaller" -> "the agreement becomes worse".

    Changed

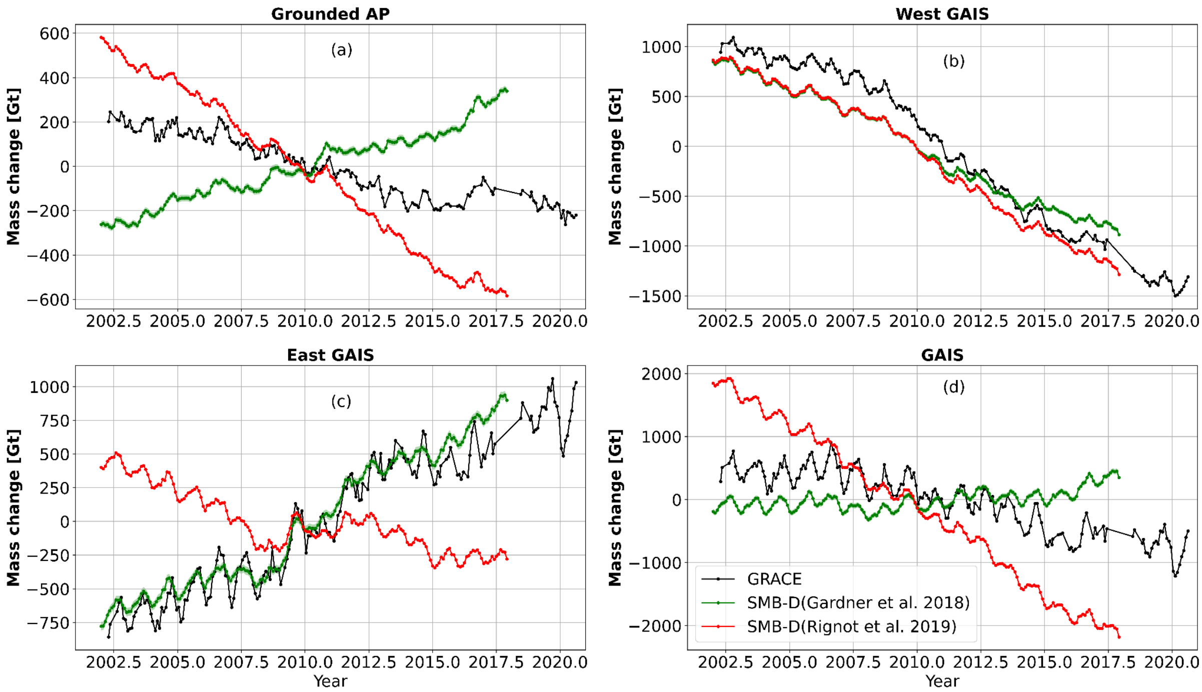
27. Fig 6: It appears that most of the points are from Dyn15 here, but I suspect it is a case of overplotting. I would suggest using different symbols so that it is clear when markers overlay one another and when they do not. Also why have the colours used for the different simulations changed? Earlier they were red, blue and green, now red, blue and yellow. It would make interpretation of the data easier if they were made consistent between figures, and better still if different colours than used for the diverging colour markers in Fig 1 and 4.

    Yes, many points are on top of each other. We have changed the symbols so it is clearer. The yellow colour for Dyn15 was a mistake, which has been changed for consistency. We keep the colours for Fixed, Dyn03 and Dyn15, but have changed the colours schemes in Fig. 1 and 4



28. Fig 7: Larger text would be helpful here

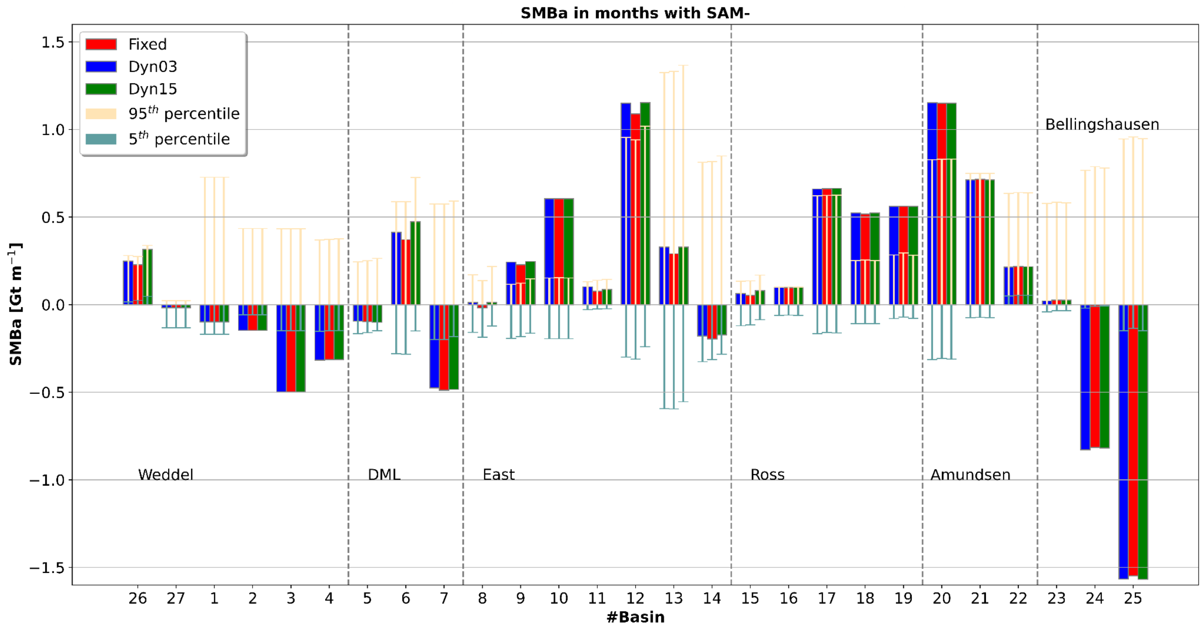
The text will be made larger, furthermore the labeling will be changed to match the other figures.



29. Fig 8: I find the narrow bars confusing in this figure. Could standard uncertainty markers be used instead? Why do the means for the three simulations fall outside of the 5-95 percentile range so often? Perhaps some more explanation is needed in the caption.

First part: We will change the way the uncertainty is visualized.
Second part: First of all, the three simulations are forced with the same precipitation field and these results are only over the grounded ice sheet so this is why the three simulations are so similar. Second, the Y-axis represents different basins, so by "often" I assume you mean many basins? They fall out of the percentiles when we have a robust signal between the SMB and SAM, if they did not fall outside these percentiles, there would not have been a robust relationship between the SMB and SAM. So, when our results fall outside the 5th and 95th percentiles in nearly half of the basins, we actually show that there is a robust relationship.
To clarify this, this will be added to the caption:
"... can be seen in Fig. 3. In basins where the SMBa values fall outside the percentiles, there is a robust relationship between the SMB and SAM."

Figure 8a is shown here with changed uncertainty markers, figure 8b has also been changed.



Christoph Kittel Referee #2

30. Since the subsurface model is forced by HIRHAM5, how does the use of HIRHAM5 affect the results? The subsurface model is forced by water (evaporation, sublimation) and energy (latent and sensible heat, downwelling longwave) fluxes that are strongly linked to the surface state as computed by HIRHAM5 (and in general for the method, by a model with a less sophisticated surface scheme than the one from the subsurface model). It looks like a vicious circle where results from the subsurface model might be not independent from the surface scheme of the forcing model.

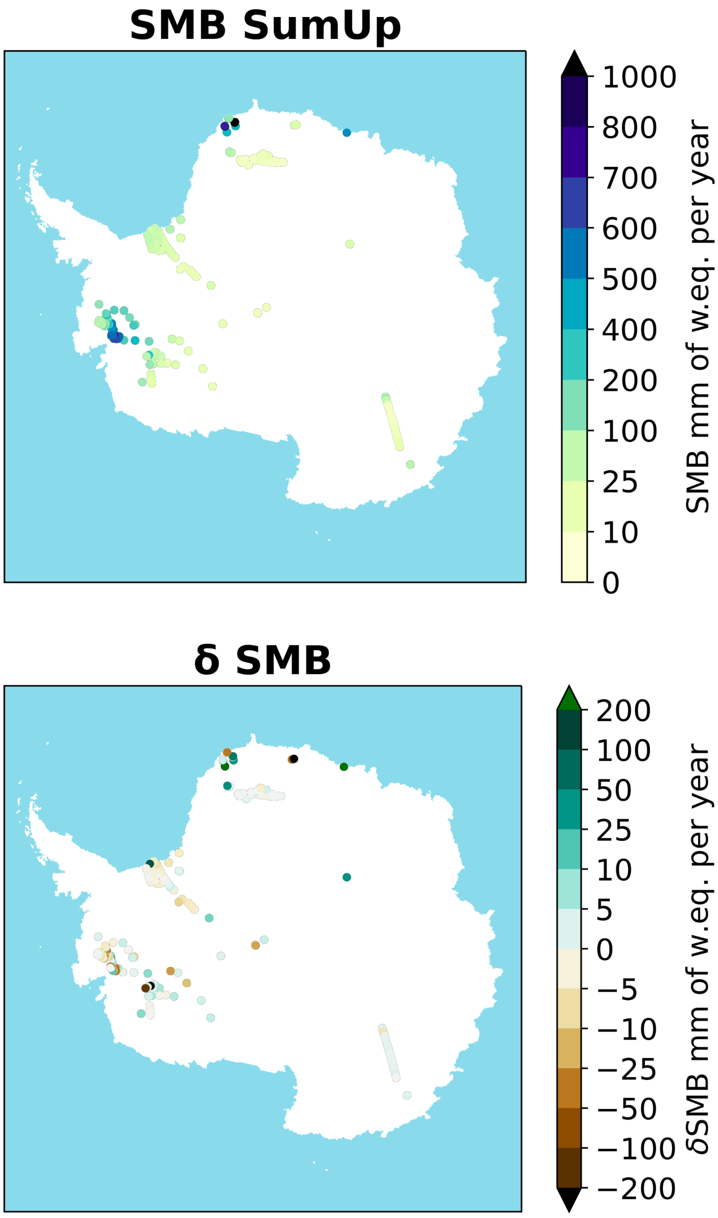
We cannot avoid non-independent results. And this is not the aim in developing this subsurface scheme.
It is absolutely true that both the turbulent fluxes (sensible and latent heat) and the downwelling LW come from the HIRHAM5, where the fluxes, in turn, depend both on the overlying (larger-scale) atmospheric state and the local temperature of the surface. The latter is, in turn, dependent on the surface scheme employed on-line in the HIRHAM5.
Given that: 1, the on-line subsurface scheme is an older (and coarser vertically resolved) version of the present off-line scheme. 2, the on-line scheme has the same w.eq. thickness of the top layer as in the off-line scheme. 3, the upwelling LW and SW are independent of the on-line scheme (and the downwelling SW likely also mostly independent of the on-line scheme) and 4, the ground heat fluxes (diffusive heat exchange between upper layer and the layer below) are generally small.

We expect the importance of this circular determination of fluxes to be less important than the impact of large-scale variability in the atmosphere and the local-scale variability in upwelling LW and SW fluxes.
The main gain in adding the off-line scheme is thus not expected to be in the surface energy balance but rather in the representation of the subsurface fate of the meltwater. (refreezing, ice layering, runoff etc).

31. The evaluation of the physical conditions (densities and temperature) of the snowpack is exhaustive however I wonder if the main product of the paper (the SMB) is sufficiently evaluated. Considering all uncertainties in GRACE and discharge estimations, the present evaluation should rather be a supplement comparison than the main part of the evaluation. I recommend the authors to evaluate their SMB against in-situ local observations. It might also help to assess the added value of using the subsurface model instead of the HIRHAM5 SMB by comparing their reconstruction over the observation (see first comment).

We agree that the SMB can be better evaluated, so we have compared it with the SumUp data set, since we already use parts of it. The comparison is added in section 3.1. Regarding the GRACE section, yes, we agree that it is not the main result. However, it still sheds light on the problem of just relying on one discharge data set or one model. Here is the text the figures that we have added (in the paper these new figures are figure 3a, b and c):
"Koenig and Montgomery (2019) have, in the SumUp data set, collected accumulation rates over Antarctica. Here we evaluated the modelled SMB values against the SumUp accumulations assuming that over most of the AIS, accumulation is nearly equivalent to SMB. The SumUp data set has yearly measurements for some locations, and mean values for longer periods for other locations. To make it consistent, we computed the yearly mean at each location, shown in Fig. 3a, and compared it with the nearest grid cell in the ensemble mean for the period from 1980 to 2017. If there was more than one measurement in one grid cell, an average was used (Fig. 3b). Lastly, we computed the change between the observations and the ensemble mean in percent (Fig. 3c). In total 2221 measurements have been used located in 251 different grid cells. The SumUp accumulation data set has areas with a high concentration of measurements, like Marie Byrd Land, Dronning Maud Land and Dome Charlie, however, in East Antarctica there are larger areas that are not represented in the SumUp data set. The accumulation ranges from near 0 to 100 mm w.eq. yr^{-1} at the South Pole, Dronning Maud Land and Dome Charlie, up towards a 1000 mm w.eq. yr^{-1} in Marie Byrd Land and the coast of Dronning Maud Land. Figure 3b shows the difference between the model ensemble mean and the in-situ observations where it is seen that there are

some large numerical differences in Marie Byrd Land and near the coast in Dronning Maud Land. Figure 3c displays the difference in percent for the δSMB, it shows the only three of the 251 grid cell comparisons have a difference on ±100%, furthermore half of the 251 comparison points fit within ±13%"



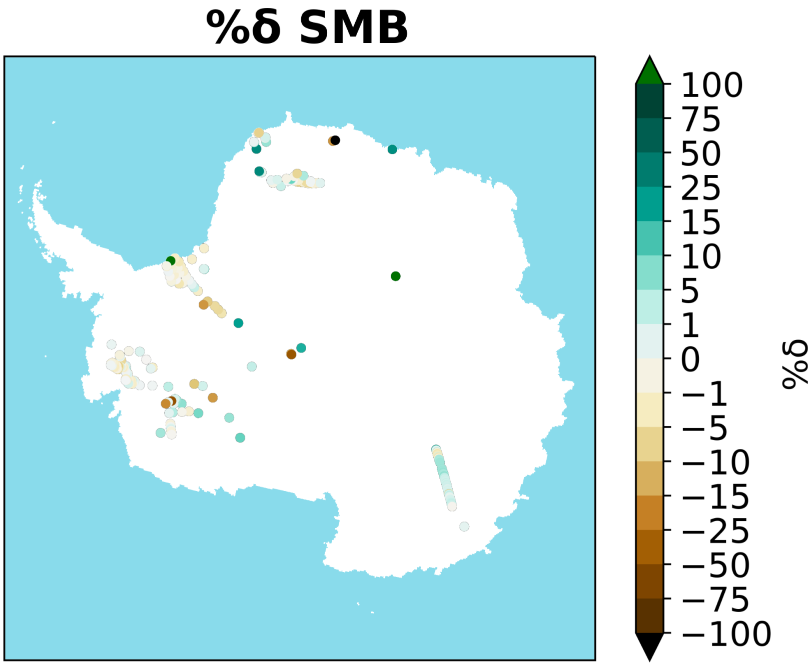


Figure 3.a) The SMB from SumUp. b) The δSMB (SumUp minus model ensemble mean) and, c) the charge in percent

32. Although SAM influences on SMB is still an open question, other studies (ignored in this manuscript) have attempted to contribute to answer this question. I suggest the authors to add some references in their introduction and discussion to better situate their work in the existing literature. Here is a list of some potentially interesting references on the subject (all may not have the same relevant level and probably do not need to be included, and the list is far for being exhaustive):

    We have added this at the end of the introduction:
    "Studies (Marshall et al., 2017; Dalaiden et al. 2020) have found that a positive SAM reduces precipitation over the Antarctic plateau and increases it over the western AP and in some coastal areas in East Antarctica. Finally Vannitsem et al. (2019) found that the Antarctic SMB is influenced by the SAM in most of the coastal areas of East Antarctica and large parts of West Antarctica. Therefore, we also investigate the spatial distribution of SMB over the grounded AIS (GAIS) in relation to the phase of the SAM."

    And we have added this in the discussion:
    "...SAM plays an important role. Comparing with Vannitsem et al. (2019), we see agreement in large parts of West Antarctica. However, it is difficult to compare in East Antarctica because we use basins and most of them go far

inland, whereas Vannitsem et al. (2019) defined narrow coastal regions and one large plateau region. From 1980….

33. P10 L216-217: This should be verified by comparing the albedos of the different simulations. How is the albedo prescribed in subsurface models? Is it the same parameterisation as in HIRHAM5? There is no information on this subject whereas the albedo is a determining parameter and will be even more so in a warmer climate.

    We have verified the albedo in the different runs and rewritten the sentence: "... Again this is focused largely over the ice shelves, especially over the Larsen and Amery ice shelf where Dyn03 and Dyn15 have more bare ice and thus a lower albedo."
    No, the described albedo scheme is in the subsurface model, so the description has been moved down to section 2.2.

34. All the experiments reveal particularly high melt values that are significantly different from other estimations based on RCMs (eg., Van Wessem et al., 2018; Agosta et al., 2019; Kittel et al., 2021) or satellites (eg., Trusel et al. 2013). It does not mean that these values are erroneous since there are by definition no observations of melting, but they deserve further discussion even if they have no impact on the SMB in the current climate.These large differences in the present climate might suggest that the model cannot be used in a warmer climate where melting and runoff would have much more impact. The authors could compare their estimates with SEB model estimates forced by AWS (Jakobs et al., 2020) or any other estimates.

    We have discussed this in section 4 (L324-329)
    "....toward west. In general all three simulations display a higher melt compared to other RCM studies e.g 71 Gt yr^{-1} in RACMO2.3p2 (van Wessem et al., 2018) or 40 Gt yr^{-1} in MARv3.6.4 (Agosta et al., 2019). These two numbers are without the AP, but are nevertheless very low compared to our melt rates. Trusel et al. (2013) derived satellite-based melt rate estimates from 1999 to 2009 and over that period, the Larsen ice shelf experienced the largest melt of around 400 mm w.eq. yr^{-1}. However, these estimates were derived using RACMO2.1, and the satellite detects some melt areas on the Larsen ice shelf that were not simulated in RACMO2.1, most likely due to coarse resolution, so the 400 mm w.eq. yr^{-1} might be on the low end. Nevertheless Trusel et al. (2013) estimates are still three to six times lower than our simulation. This suggests that the subsurface model may compute a melt rate that is too high in at least some locations.

35. Gt per year: to be consistent with kg m - 3, consider Gt yr -1 P1 L11-L14: consider to remove the section about the density and temperature biases from the abstract as it does not seem to be a particularly important information.
    We have changed to Gt y-1 and removed the density and temperature biases

36. P2 L32: Add blowing snow erosion/deposition in the SMB definition or specify that is naturally included in the local solid precipitation balance.

    Done
    "...(RO) of meltwater and erosion of blowing snow. However, blowing snow is not implemented in this model configuration, so the SMB is defined here as: SMB=P-S-RO. Of these…"

37. P2 L40-43: RCMs also improve the physical representations of specific processes over polar areas (see for instance Lenaerts et al., 2019).

    This is added

38. P2 L43-44: "Mottram et al. (2020) evaluated the atmospheric output from five different RCM simulations of Antarctic SMB driven by ERA-Interim (1987-2017)." Atmospheric output vs SMB (= surface output) is confusing, please rewrite.

    Rewritten to: "Mottram et al. (2020) evaluated Antarctic SMB calculated from the outputs from five different RCM simulations driven by ERA-Interim (1987-2017)".

39. P2 L44-47: Indicating the original values of the models does not provide much more information since these are the models that were used in Mottram et al., 2020. It is more of a repetition with perhaps less relevant information because the masks are different (i.e. the SMB is also different, whereas this artifact is corrected in Mottram et al., 2020). I would remove the individual values, and if the authors still want to link this comparison study with original model publications, they could cite the name of the models (+reference) that were used in Mottram after the SMB ranges.

    The numbers from the individual models have been removed due to mask size bias

40. P3 L65-67: Please add some references here.
    We have added Turner, 2004, Irving and Simmonds, 2016, and Marshall and Thompson, 2016

41. P3 L66-70: Even is SAM has indeed a strong effect on precipitation patterns, Marshall et al. (2017) rather suggest that precipitation patterns result from a combination of the different modes. Consider add other references to better justify the selection (ie, Kim et al., 2020).
Have have added this sentence
"...circulation indices. Further, Kim et al 2020 found a multi-decadal relationship between the SAM and variations in the SMB, for these reasons we concentrate on its effects in this study…."

42. P4 L100-101: "Despite the forcing is based on 6 hourly values the subsurface scheme is used to simulate the subsurface at 1-hour time steps." Is there an interpolation between two 6-hourly inputs to produce a smooth transition between two forcing time steps?
Yes there is, the hourly values come from a linear interpolation in time between the two nearest 6-hourly files. To clarify this, a sentence has been added. See answer 11 for R1.

43. P4 L105: Please specify if this is vertically or also laterally transferred?
It is vertically, we will specify that

44. P5 L131: Replace weighed by weighted
Done

45. P5 L137: Could you be more specific? Does this mean that the melting is taking place on several layers in the vertical at the same time, i.e. that the energy is transmitted into the snowpack?
Surface energy fluxes do not penetrate through the top layer, so generally melt only occurs there. Nevertheless, if a time step presents more melt (as calculated from the surface energy balance) than what is contained in the top-layer, the underlying layers are successively melted. However, this scenario is very unlikely for our model vertical resolution and for typical Antarctic melt-rates.
The original sentence (L137) is indeed not very clear. Therefore we suggest to rewrite it to:
"Another difference between the two model versions is that the dynamic-layer model melts simultaneously the snow and ice content of the top layer while the Fixed-layer model melts the snow content first then the ice content of the top layer. This update aims at preventing the top layer from becoming only ice and a barrier to meltwater infiltration. Furthermore, the Dyn….."

46. P5 L141-142: What climatological means did you use? Is it based on Hirham5 inputs?
Yes, it is from HIRHAM5, we will specify that

47. P5 L144: Why did you initialized with a uniform density over the whole ice sheet, that is close to snow values given by Kasper et al. parameterisation? I guess that spinup time remove the dependency to the initialization but it could have been more consistent?
The 330 kg m^{-3} talked about in line 144 are only an initialization of the run, whereas Kaspers et al. is used for fresh surface snow throughout the run, whenever it snows over the ice sheet.

48. P12 L240-242: Could it also due to overestimated melt/refreezing (see minor comment #2). Overestimated precipitation could also result in more fresh snow with lower/uncompacted densities. It could also result from an overestimation of the “fresh” surface snow density (linked to the parameterisation itself or HIRHAM biases)
Yes, you are right, it has been added

49. P14 L54: Why did you select these specific cores? Are they the only ones with a high vertical resolution or are they representative of the region? It would be interesting to state the objectives of this comparison in order to extrapolate the conclusions that can be drawn from these few examples.

They are selected because they are located in different regions of the AIS, furthermore they show different examples of under/over estimations of modelled densities. We will add that in the text (line 256) so the selection is justified/explained
“...(Fig. 5d). These four cores are selected because they are located in different regions of the AIS and, furthermore, they show different examples of under/over estimations of modelled densities. The simulations…”

50. P16 L293: Melt is not a balance, there are either no melt or melt and then positive values.
Yes, you are correct, we have removed the minus sign

51. P17 L306: Do you mean that one layer in the pack can be ice and snow at the same time or that melt occurred at different vertical layers simultaneously? (see also P5L137)
Yes, one layer can contain snow and ice at the same time, described with a fraction/concentration. This has been added to line 306.

52. P20 L399-404: Check the SMBa units, shouldn't it be Gt m -1 (as monthly SAM values or as Figure 8) instead of Gt yr -1 ?
    Yes, it should be per month, it has been changed.

53. P6- Table 1: replace fist by first
    Done
54. P8- Figure 1: The colormap is confusing as it is non-continuous. Please select something with a linear transition that will allow the reader to easily identify SMB variations. (See for instance https://matplotlib.org/stable/tutorials/colors/colormaps.html, perceptually uniform colormaps or sequential colormaps). Consider to use the abbreviation you defined for water equivalent (and similar remark than the first specific one).
    The colour scheme has been changed, see also comment 17 (Reviewer 1)

55. P13 – Figure 4: similar remark than Figure 1 about the colormap + consider to make it bigger (maybe on a whole page with the elements one below the other?) to improve the reading.
    The colour scheme has been changed, see also comment 24 (Reviewer 1)

56. P21 Figure 8: add the time unit relative to the SMB accumulation (m -1)
    Done, see comment 29 (Reviewer 1)

Regarding the "Stylistic suggestions" we have implemented four of them the: AIS SMB, P2 L46-57, P2 L54 and P6 L172.

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