Response to the comments of reviewer 1 for the manuscript "Impact of Greenland Ice Sheet Disintegration on Atmosphere and Ocean Disentangled"

by M. Andernach, M.-L. Kapsch and U. Mikolajewicz

October 2024

We would like to thank the reviewers for their time and effort in reviewing our manuscript and providing constructive feedback. Their valuable comments and advice will help us to improve the content and to enhance the quality of our manuscript. We have carefully considered the feedback provided by the reviewers and propose changes for the revision of our manuscript.

In the revised version of the manuscript we will focus on the following main aspects raised by the reviewers:

- Shortening the presentation of our results
- Reorganizing certain parts of the results
- Strengthening the ocean analysis by incorporating additional figures

We provide a detailed point-by-point reply to all comments below. The reviewers' comments are presented in regular font, the authors' replies in green font, and changes to the text in italic green font.

All authors have read and approved the suggested changes. We appreciate the opportunity to enhance our manuscript and are looking forward to your feedback.

Kind regards,

Malena Andernach, Marie-Luise Kapsch and Uwe Mikolajewicz

Response to reviewer 1

This manuscript investigates the climatic impact of a disappearance of the Greenland ice sheet, mainly at high northern latitudes. Sensitivity experiments are performed to disentangle the drivers of these impacts: on one hand, the atmospheric response and the oceanic response and, on the other, the effects of the reduction in surface elevation and the change in surface properties. I believe such a study should be interesting for a broad sector of the community. However I have major issues with the text that need reworking. These have to do mainly with the Results section. First, it is extremely lengthy and descriptive, which makes the reading a bit tiring, so shortening this section would improve the readability. I provide specific suggestions below. In this line, there are many sections that repeat information that was already given before. Second, some reorganization is also needed. For instance, changes in sea ice have a very large impact on surface temperatures but appear very late in the paper. Finally, and importantly, many results are mentioned and described but not shown, especially in relation with ocean changes which seems to be the focus of the paper, e.g. the AMOC. There is also a brief section focusing on remote impacts which are actually not shown. Having said that, the text is well written and the figures are excellent. I therefore provide below a list of specific comments which I think need to be addressed before the manuscript can be accepted for publication. As you can see they are quite detailed at the beginning because I think the first sections read well but more general

towards the end, where I think the problems I mentioned before are more severe.

We are grateful for the overall positive feedback of our analysis of the impact of a disintegrated Greenland Ice Sheet (GrIS) on the atmosphere and ocean. We thank the reviewer for taking the time to carefully read our manuscript and provide valuable comments to help improve it.

Abstract, line 1: you do not really investigate the impact on the global climate but rather mainly focus on high northern latitudes.

Thank you for pointing this out. As our simulations are global, they allow us to analyze also remote effects. However, we mainly focus on the impacts on the (sub-)Arctic as the simulated climate response is most pronounced in this region. As we would like to distinguish ourselves from previous studies that used regional climate models, we will remove the "global" but add that our simulations are performed with a global ESM: "We analyze the impact of a disintegrated Greenland Ice Sheet (GrIS) on the climate through steady-state simulations with the *global* MPI-ESM (Max Planck Institute for Meteorology Earth System Model)."

l 10: "whereas altered Greenland surface properties mostly amplify but also counteract few of the changes": this could be removed.

We will remove this part of the sentence.

l 11-12: "Only in the Labrador Sea, altered Greenland surface properties dominate the ocean response": I am not sure whether this is grammatically fully correct, please check.

We will verify the grammar.

l 14: "Despite the confinement of most responses to the Arctic, a disintegrated GrIS also influences remote climates": this is not really investigated. See below.

Thanks for your comment. As we show changes that happen in the subtropical gyre and Europe, we believe that it is justified to say that we investigate remote changes. We will add the analyzed remote changes explicitly to the text to clarify our point. The revised sentence will read as follows: "Despite the confinement of most responses to the Arctic, a disintegrated GrIS also influences remote climates, *such as air temperatures in Europe, the Atlantic Meridional Overturning Circulation (AMOC) and the subtropical gyre.*"

l 22: "the interplay between GrIS characteristics and the broader climate system is imperative to understand": I think this is not grammatically correct.

We will revise the grammar of this sentence: "[...], *it is imperative to understand the interplay between GrIS characteristics and the broader climate system.* "

l 24-27: you provide a very long list of references here just saying "These studies found considerable climatic changes". I suggest summarizing briefly the insight provided by those studies.

Thanks for this suggestion, we agree that providing examples would be beneficial. Therefore, we will add two examples that are mentioned in the referenced papers: "These studies found considerable climatic changes, both thermodynamic and dynamic, in response to a reduced GrIS volume, *including air temperature changes over and in the vicinity of Greenland and changes in the storm tracks.*"

l 50: "Here, we extend those studies by examining the interactions of the GrIS with the entire climate system, including the deep ocean": I do not think this is true, you mainly focus on the Arctic. You do comment on changes in the deep ocean and in the AMOC but you do not show the latter

Thanks for your comment. As we also analyze the response that occurs outside of the Arctic re-

gion (e.g., subtropical gyre, European temperatures), we think that it is justified to say that we examine interactions of the GrIS with the entire climate system. Also, in this sentence we rather refer to the different components of the climate system (e.g., atmosphere, ocean, biosphere), which are all included in our model set-up. To clarify what we mean by the entire climate system, we will modify the sentence slightly: "*Here, we expand upon those studies by examining the interactions of the GrIS with both the atmosphere and the ocean, including the deep ocean.*" Having said that, we agree that we should not only describe but also show the changes in the deep ocean. Therefore, we will add figures of changes in the NADW and the AMOC. We will display the respective figures at the relevant section of this document when we discuss the NADW and AMOC specifically.

l 57: remove comma

We will remove the comma, thanks.

l 95: Could you be more explicit about the surface parameters that you changed? Eventually a figure to illustrate these would be good.

We will add specific examples: "[...] allowing for the vegetation to dynamically regrow and surface parameters to change to those of a non-glaciated surface, *including for example changes in the albedo, ground roughness, soil moisture and the ground heat flux.*". As the removal of the glacier mask affects a multitude of surface parameters, we will add a reference to the JSBACH manual, which describes all the variables.

l 101: Since you are using a coupled atmosphere-ocean model which is supposed to be one of the main steps forward of this work, the fact that you do not take into account freshwater associated with the removal of the Greenland ice sheet (GrIS) should be mentioned more explicitly, in particular in the Discussion as a caveat.

This is a good point, but we believe that not adding the freshwater equivalent is as a strength of our study rather than a caveat. Adding the freshwater equivalent is important for analyzing transient simulations. However, we analyze equilibrium runs. Moreover, we point out that the impact of the 7 m sea-level equivalent from the GrIS on ocean salinity is less than 0.2% and is thus negligible. This approach provides the advantage of not needing to correct for the differences in global mean salinity when analyzing density changes, as it does not represent a climate change signal. This explanation will be incorporated into the Section Experimental Design.

l 105: This might be a bit subtle but do you interpret the *_atm experiments as the response of the atmosphere (alone) to the disappearance of the GrIS or the contribution of the atmosphere (only) to the climate response to the disappearance of the GrIS? I would tend to see it as the latter case and I think this is how you frame it below.

This is an important point. The *atm experiments simulate the response of the atmosphere to the disappearance of the GrIS and we interpret it as the atmospheric contribution to the full climate response (without the feedbacks from the ocean). To prevent confusion by the reader, we will add this explanation to the manuscript.

l 105-110: I am not sure how the *_atm experiments are done: do they really include the full coupled atmosphere-ocean model with SST and SSS nudging? Why not just doing an atmosphere-only run with prescribed SSTs? Do you really need 3000 model years in this case?

We opted to run the fully coupled model with SST and SSS nudging to maintain a consistent model set-up, using exactly the same parameters as our main simulations. This consistency would not have been possible with atmosphere-only runs, as the parameter settings in an atmosphere-only model would be different. In regard to the spin-up length, significant changes occur in the first decades of the simulation. However, a slight drift persists in the simulations beyond this period. Therefore, we extended the simulation length to ensure statistical equilibrium, which is reached approximately after

2000 simulation years. To analyze 1000 years of model simulations in full equilibrium, we had to run the simulations until year 3000.

l 139-141: This description is very detailed, I don't think you need to go so much into how much every region warms. You could spare a few lines here.

Thanks for your suggestion. We will shorten this part.

l 141: replace -6K by 6K

That is correct, we will remove it.

l 143: here and in many places in this section you refer to sea-ice changes without showing them. They only appear much later, in Fig. 10. I think these should appear right away and be discussed already here since they are crucial to understanding the temperature response.

To maintain a logical structure, we have chosen to separate atmospheric and oceanic effects. However, to acknowledge your concerns and to improve clarity in this section, we will add the March and September sea-ice margins from both the CTRL and noGrIS experiments to the air temperature figures. Additionally, we will direct readers to the dedicated sea-ice section for further details. The two figures below depict the updated 2 m air temperature change figures for DJF (upper row) and JJA (bottom row) with the March and September sea-ice extents of CTRL (green) and noGrIS (magenta).

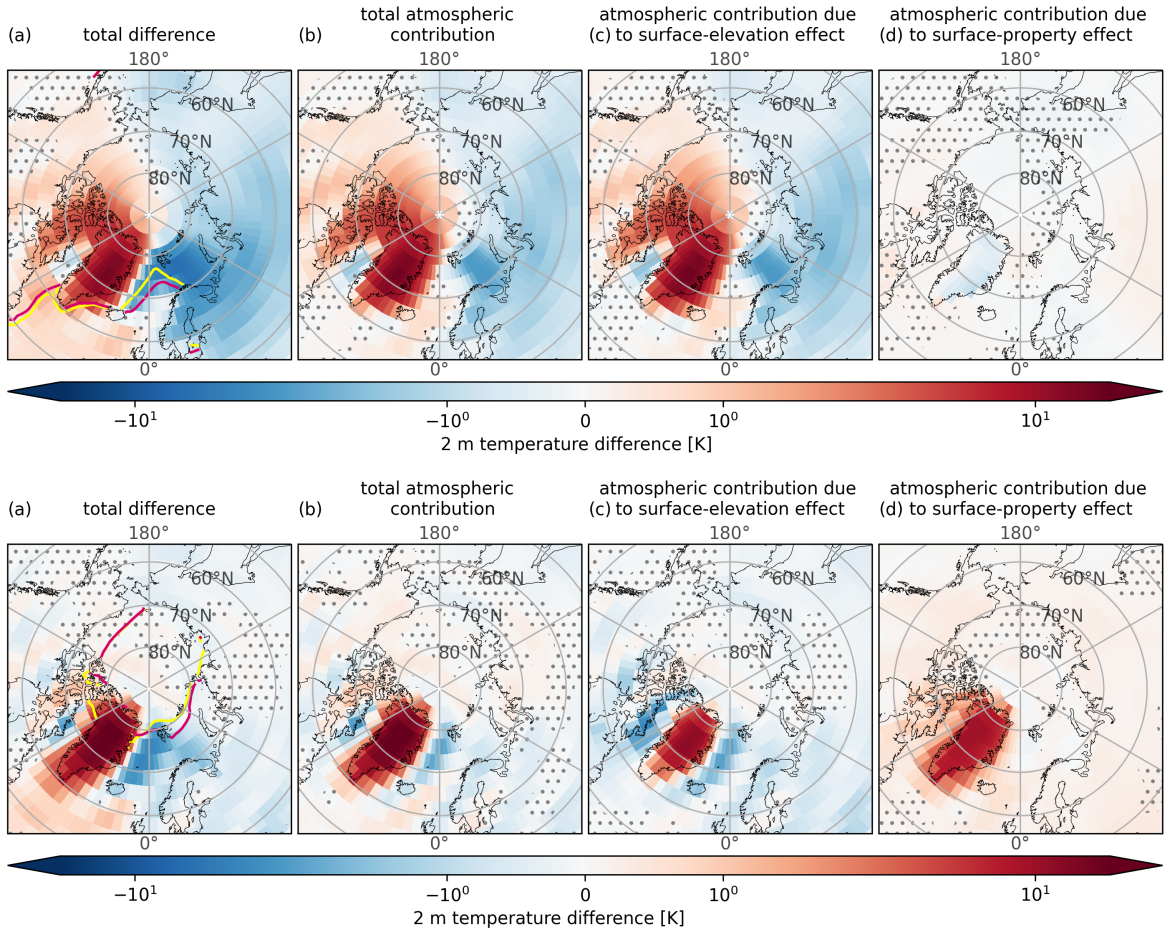

Figure 1: 2 m air temperature change over the Arctic in DJF (upper row) and JJA (bottom row) with the March and September sea ice extents of CTRL (yellow) and noGrIS (magenta).

l 145-146: Also, I would replace "atmospheric circulation changes" by "the atmosphere". And I would say the dipole pattern is caused in part by the atmospheric response, and in part by the ocean. In relation to my comment above, I see these changes as contributed to by the atmosphere and/or the ocean.

We will replace 'atmospheric circulation changes' with 'the atmosphere' as suggested. However, we disagree that the dipole pattern is partly caused by the ocean. Figure 2f shows an exclusively negative temperature response in the Arctic. Positive temperature anomalies are observed only south of the Arctic Circle, specifically over the Labrador Sea, but not over the Canadian Archipelago and northern Canada. This implies that the warming pattern is a response to the atmospheric changes. To clarify this point, we will revise the section and will add an explanation of the ocean response: "Experiments with a nudged ocean (noGrIS_atm and noGrIS_elev_atm) reveal that the temperature dipole results from changes in the atmospheric circulation due to differences in GrIS surface elevation (Fig. 2b & c). *Figure 2f shows an exclusively negative temperature response in the Arctic. Positive temperature anomalies in the ocean are observed only south of the Arctic Circle, specifically over the Labrador Sea, but not over the Canadian Archipelago and northern Canada.*"

l 148: I would write "limits temperatures to values below the freezing point"

We will change this accordingly: "[...] *the presence of a seasonal snow cover during winter limits surface temperatures to at and below the freezing point.*"

l 152 and below: sea-ice changes are mentioned here again without referring to figures.

Please see our reply to your previous comment on sea-ice changes in the atmosphere section.

l 155: I think here where you write "feedback" you mean "effect". The effect is opposite to what you describe in lines 153-154 but the feedback is the same.

Thanks for spotting this. We will replace feedback by effect.

l 157: I think here again you mean negative temperature response rather than feedback.

We will revise the sentence as suggested.

l 160: I think it is premature to say the atmosphere changes are associated with circulation changes. You can nevertheless say you will show in Section 3.1.2 that is the case.

We agree and will revise the sentence as suggested: "*In Sect. 3.1.2 we will show that changes in the atmospheric circulation are the driver of the dipole in the 2-meter air temperatures.*"

l 166: remove the comma.

We will remove it, thanks.

l 165-168: Why not comment on the ocean response briefly?

We appreciate the suggestion. As the response patterns over land and ocean are similar to the DJF response (as stated in line 163 of the first submission), we will revise the first sentence of the subsection to: "In summer, contributions *over land and ocean* are comparable, albeit significantly weaker than in winter [...]."

l 170-175: Please show the annual mean changes too.

Good idea. We will include a figure of the annual-mean changes.

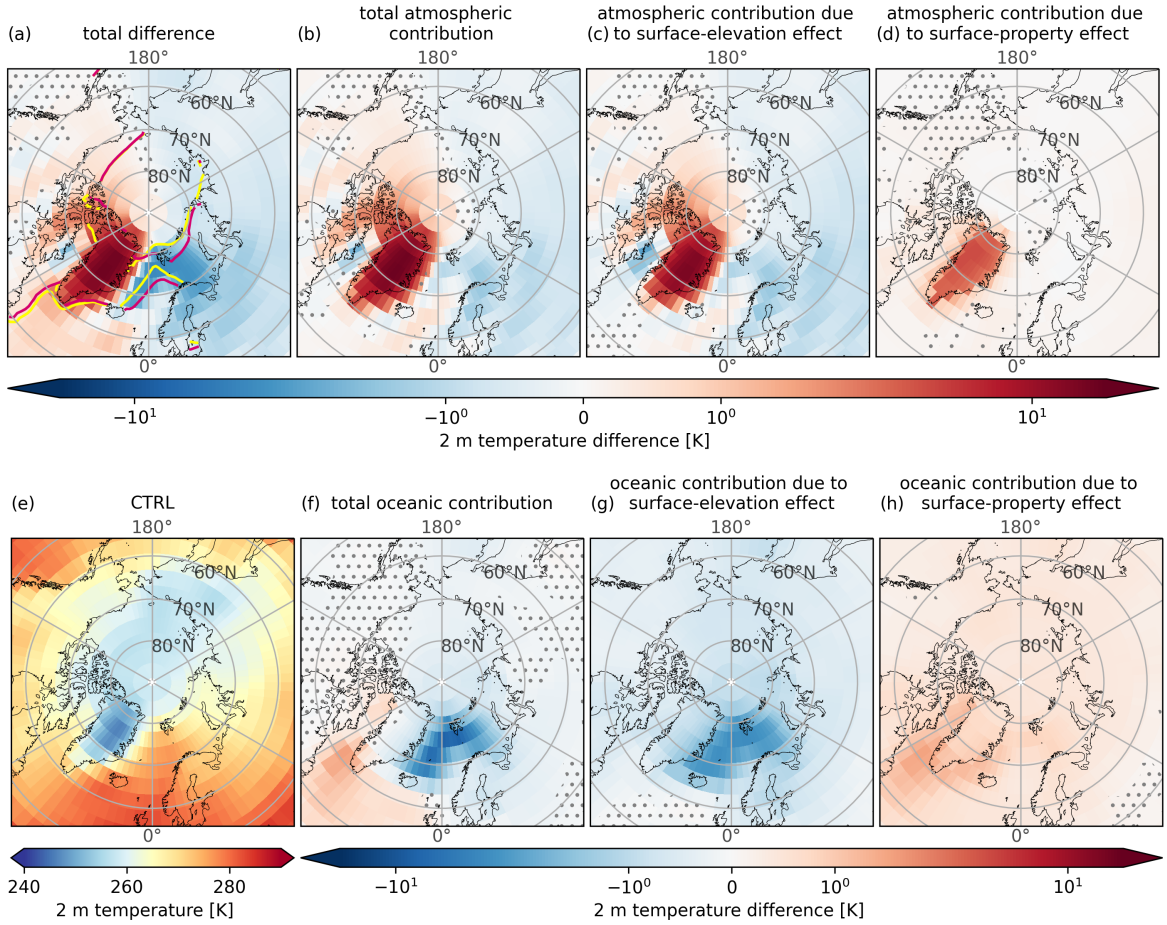

Figure 2: 2 m annual-mean air temperature changes with the March and September sea ice extents of CTRL (yellow) and noGrIS (magenta).

l 185: 10-m winds are (in general) not reversed but decreased.

Thanks for the comment. We will change the sentence accordingly, saying that the winds are weaker and change direction only over Greenland.
"*In noGrIS, 10-meter winds are weaker and take a stronger easterly zonal direction over Greenland as compared to CTRL (Fig. 5b).*"

l 185 - 198: Here again you mention often changes in sea ice that should be shown. Also, you refer to temperature changes without referring to the figures, which would help the reader to follow the argument. Since the atmospheric circulation changes are only shown on an annual mean basis, showing the annual mean temperature changes would help too.

In response to your previous comments regarding sea-ice changes, we will add the sea-ice extent into the 2 m air temperature figures for summer and winter and include relevant references for changes in the sea-ice extent. Furthermore, we will add a figure illustrating the 2 m annual-mean temperature changes including the sea-ice extent. This figure will be referenced to throughout the entire manuscript.

l 189: You mention here as a cause of the warming the reduction in sea ice but Figure 3d shows (for summer) a larger warming role of the atmosphere-only compared to ocean-only. Again I think it would be better to compare this with the annual mean temperature change.

The figure illustrates the warming of the Labrador Sea exclusively in the ocean-only simulations (Figure 2f and 3f). This indicates that the warming is due to the ocean storing heat that is advected from

Greenland during the summer months by the atmosphere. Since this effect maintains warmer ocean temperatures throughout the year, it is important to reference the annual-mean temperature change in form of a figure. We will revise the text to reflect this.

l 190: Remove comma.

We will remove it, thanks.

l 193: Do you mean insulation rather than insolation?

Yes, you are right. We will change it.

l 199-216: Here I repeat the same arguments as above. You very often mention sea-ice changes, which need to be shown, and you relate changes with temperature changes, which I think need to be shown and discussed on an annual-mean basis.

Please see our response to your previous comments on sea ice and 2 m air temperature changes.

Figure 4c,d: a logarithmic scale would be better because the changes over Greenland are so large that the rest of the arrows and values are muted.

Thanks for your suggestion. We edited the figure and changed the scale and the vectors to logarithmic scales and will include this in the manuscript.

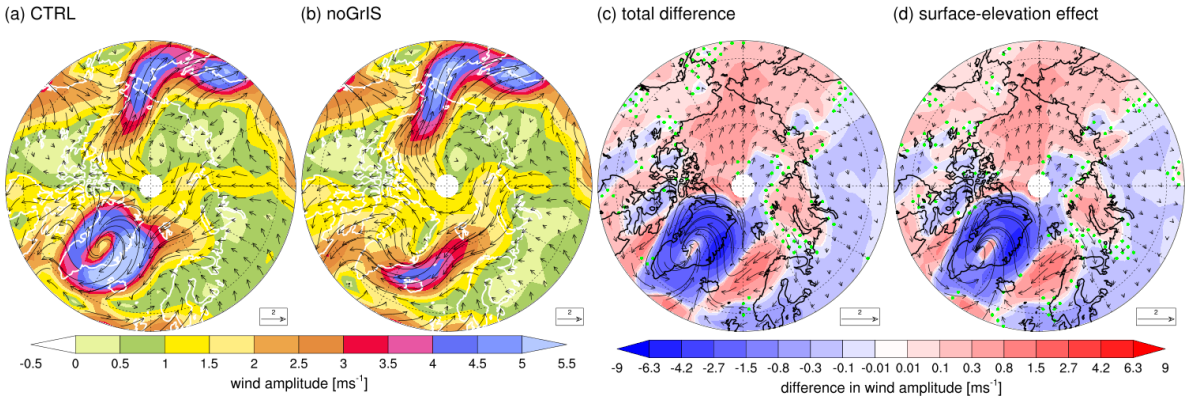

Figure 3: (a-b) Annual 10 m wind amplitude (contours) and direction (vectors, ms^{-1}), (c-d) total difference (noGrIS - CTRL) and surface-elevation effect contribution (noGrIS_elev - CTRL) to the difference in the 10 m wind amplitude. Stippling designates statistically non-significant regions (α>5 %). Please note the logarithmic scale in (c) and (d).

l 213: remove comma

Thanks for spotting this. We will check the manuscript on commas again and make sure that they are placed correctly. We will also slightly revise and shorten this section:
"[...] *This adds up to a total difference of up to -3.3 K in noGrIS relative to CTRL in the annual-mean (Fig. 4a) and -6.0 K in winter (Fig. 2a). The sea-ice expands also in other regions of the Northern Hemisphere, resulting in an overall negative temperature response of the ocean (Fig. 2g).* [...]"

l 216: remove "both"

Yes, we will.

l 225: include "not shown" at the end

In fact it is shown. The slightly counteractive effect of the altered GrIS surface properties can be derived from Figure 5. The anomaly in the normalized geopotential height is slightly less negative over Greenland when including the GrIS surface property effect (Fig. 5c) compared to the experiments without the altered surface properties (Fig. 5d). We will add the reference to the figure to the text: "The GrIS surface-property effect slightly counteracts the normalized geopotential height reduction over Greenland *as depicted by the slightly less negative anomaly in Figure 5c than in Figure 5d.*"

Section 3.1.3: I think this section very much repeats what has been said in the previous ones so I don't really think it is necessary. The only thing I think is really new is the description of how the surface properties change when the GrIS is removed, but since these are relevant from the beginning of the discussion they should probably be shown earlier. Figure 8, however, is a nice summary, so it could be kept but very much reducing its description.

Thank you for your suggestion. We agree and will move the part about the surface-property change into Section 3.1.1. Further, we will remove all doubled information to shorten the section.

Section 3.2.1: This section seems extremely long and descriptive to me. There are about six pages describing changes in the Arctic. I think there is no need to go in detail over each of the five basins as is done now, but rather try to synthesize the main results. In this line, the introductory paragraph (l 305-317) is not needed and the rest should not be arranged on the basis of individual basins but as a whole, much more succinctly. Therefore I do not go in detail over the text. Another important problem of this section is that many results which are mentioned are not shown. For instance, changes in deep water formation is mentioned in lines 376 or 443 but not illustrated in the figures. The same applies to the overflow in the Denmark Strait (l 433) and changes NADW and AABW (l 439).

Thanks for your comment. We will revise this section with a focus on creating an improved structure and conciseness. Our aim is to shift from a basin-oriented to a process-oriented structure, which better highlights the physical processes and is easier to read. In doing so, we will remove duplicated information and significantly shorten the entire section without compromising the content. Additionally, we will remove the introductory paragraph as suggested.
Furthermore, we agree with the comment that figures of deep-ocean processes could significantly aid the understanding. Therefore, we will add a figure together with a description showing the changes in the NADW and AABW. The figure presents a cross-section of dye water concentration, illustrating the distribution of two tracers in the ocean. These tracers are used to differentiate between the two major water masses: NADW and AABW.

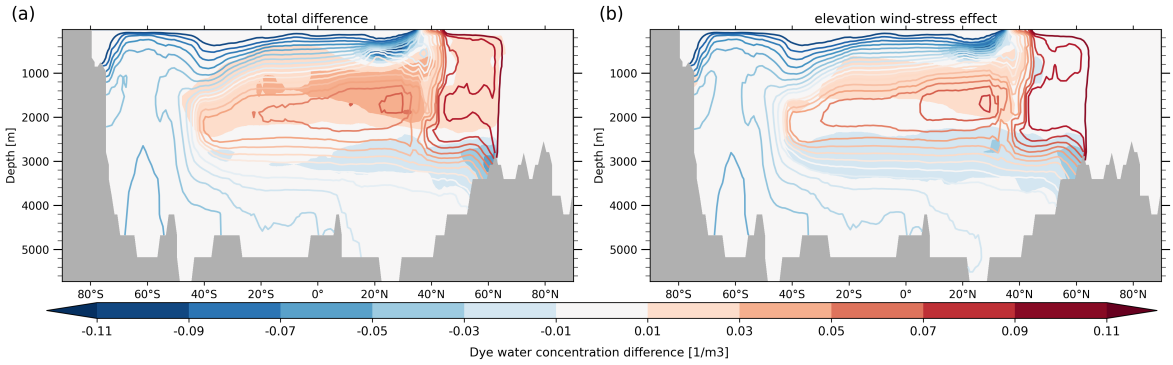

Figure 4: Cross-section of dye water concentration as an indicator for the distribution of NADW and AABW zonally averaged over the North Atlantic Ocean. (a) shows the total difference (noGrIS - CTRL) and (b) the elevation wind-stress effect (CTRL_wind - CTRL) both overlayed with the contour lines of CTRL.

Section 3.2.2: This whole section refers to AMOC changes but they are not shown. One wonders whether a figure has been forgotten but there is no reference to one. You should either focus on ocean circulation changes and show them or not.

Thanks for your comment. We agree that a figure showing the AMOC changes should be added to enhance the understanding. We will add the following figure to our manuscript that shows (a) the total difference and (b) the elevation wind-stress effect. The CTRL AMOC is displayed as colored contour lines on top. We will describe the figure in the text.

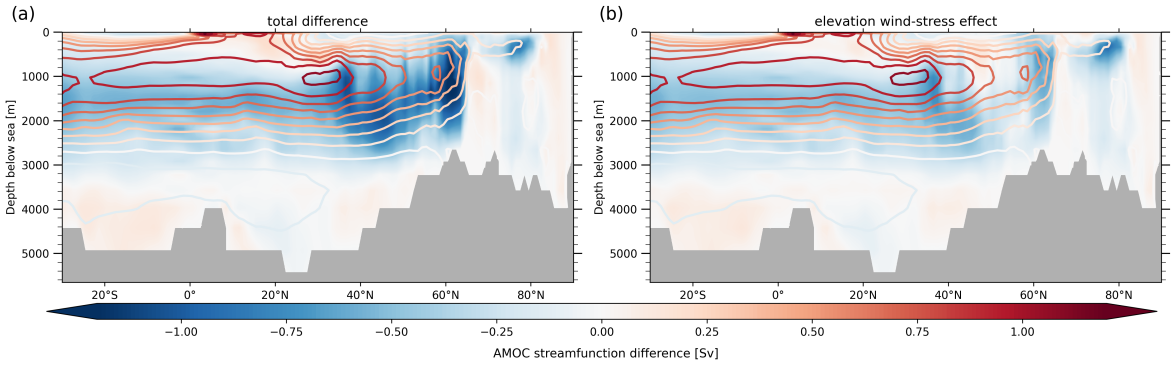

Figure 5: Cross-section of the AMOC streamfunction (a) total difference and (b) elevation wind-stress effect, both overlayed with the contour lines of CTRL.

Section 3.3: This section is supposed to focus on remote changes but these are not illustrated. A few remote changes are mentioned (cooling over Europe, a reduction in the storm tracks over NW Europe and a shift in the North Atlantic subtropical gyre) but these, again, are not shown. The authors have summarized these and other changes in a very nice figure but without showing the main changes many of the features shown just have to be believed. I would suggest removing the remote changes and focusing on the regional ones, and keeping a short description of the figure (Figure 13), which is a nice summary.

As we are able to simulate the global response due to the usage of a global comprehensive ESM, we aim to also highlight the impacts on remote regions four our readers. Consequently, we have decided to keep the section on remote changes. We agree that these changes should also be visually presented, and will add a figure illustrating temperature and storm track changes over Europe to the Appendix. The changes in the subtropical gyre can be inferred from the change in ocean potential density of which we will also add a figure to the Appendix.

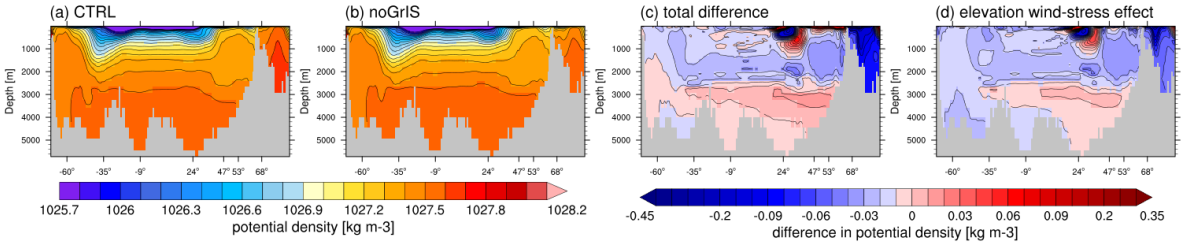

Figure 7: Cross-section of the potential density in the Atlantic Ocean. The two left columns show CTRL and noGrIS, the two right columns show the total difference (noGrIS - CTRL) and the wind-stress effect due to the lower GrIS surface elevation (CTRL_wind - CTRL). Please note the change intervals in the colorbar in (c) and (d) after -0.1/+0.1 kg m^{-3} that changes from a step of 0.01 to 0.05 kg m^{-3}.

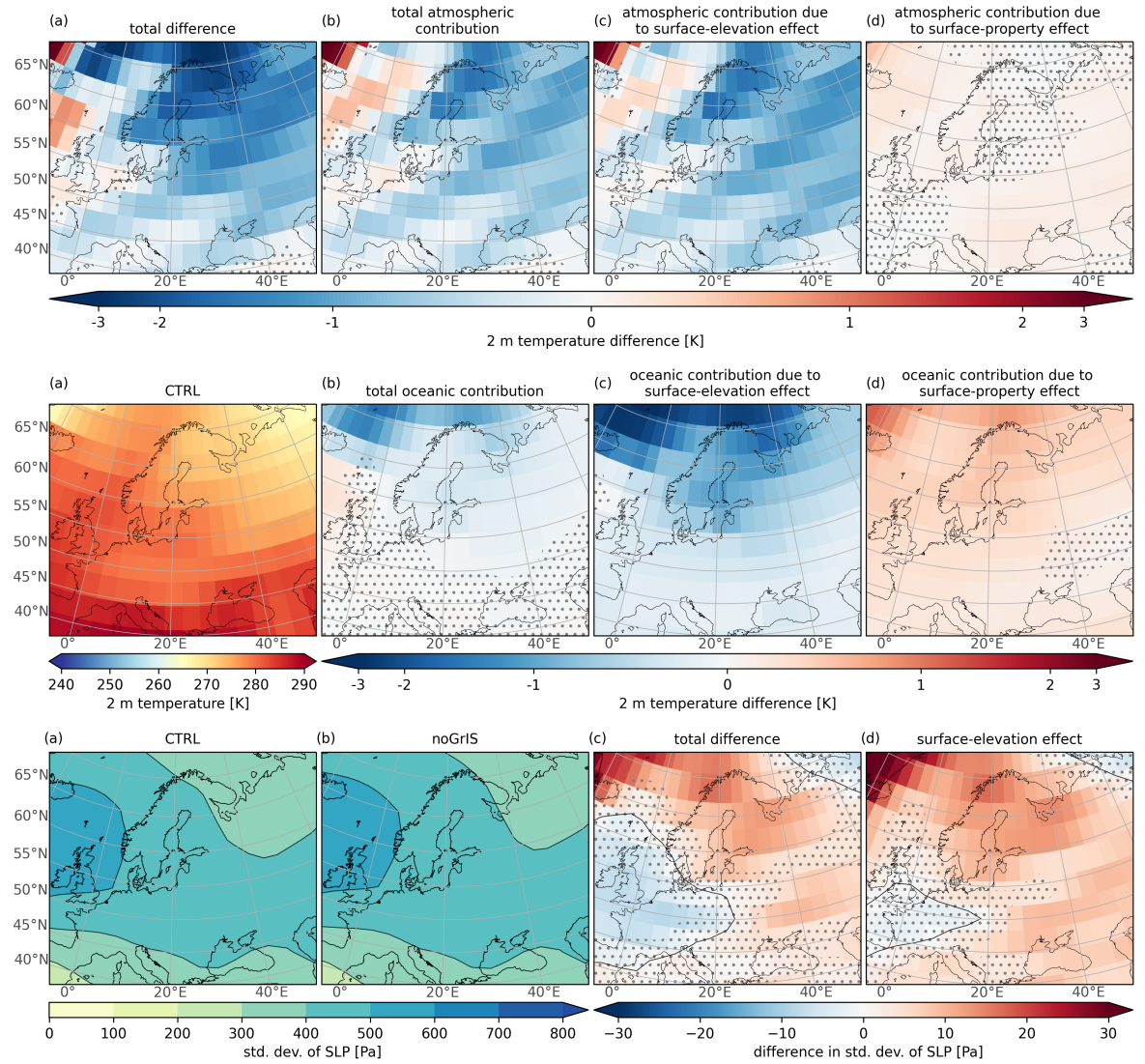

Figure 6: Annual-mean 2 m air-temperature over Northern and Central Europe (upper and middle row) and standard deviation of the DJF 2 to 5 days band-pass-filtered sea-level pressure (SLP) as a measure of cyclonic storm activity over Northern and Central Europe (bottom row).

Section 3.4: As before, this section is extremely long and repeats very much what has already been

learnt. As I said before, I would just keep a short description of figure 13, which is a nice summary.

Thanks for your idea. We will substantially shorten this section by focusing on our main points on the contributions of the GrIS surface-elevation and surface-property effect.

Section 3.5: This section is interesting but seems to be out of scope. I am not totally against it but given the length of the manuscript and the results that are omitted the authors could consider removing it. There are many studies with ice sheet models addressing the irreversibility of a Greenland ice sheet disappearance.

We fully agree with your point regarding previous studies that have addressed the irreversibility of a GrIS disintegration. However, the simulation designs of those studies do not allow for a clear distinction between the contributions of the GrIS surface-elevation and property effect on the regrowth of the GrIS. Our study offers a significant advancement in this respect. Our findings reveal that regrowth in certain regions of Greenland is not only hindered by the reduced elevation but is also critically suppressed by surface-property effects, such as a lower surface albedo and higher surface temperatures. Further, we demonstrate that changes in the background climate decisively constrain the regrowth. Therefore, we believe this section advances our understanding of the potential consequences of a GrIS disintegration and its potential (ir)reversibility. We will revise the beginning of the section to clearly highlight the benefits and new insights provided by these experiments:
"[...] To investigate this matter, we ask the question: Could a new ice sheet form under the different climate conditions in noGrIS? *This is an important step towards a better understanding of how ice-sheet induced climate changes would impact a potential regrowth of the GrIS and whether the GrIS would be stable under the altered climate conditions. Thereby, our sensitivity experiments enable us to investigate the individual impacts of altered GrIS surface height or surface properties on a potential regrowth. Using a complex ESM for such analysis is hereby novel. This allows us to attribute the potential regrowth of the GrIS or its lack thereof to the two main effects that a disintegrated GrIS has on the climate.*"
Additionally, we will emphasize the distinct impacts of the GrIS surface-elevation and property effect throughout the analysis of this section.