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

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

November 2024

We would like to thank you for your valuable comments and advice to enhance the quality of our manuscript. We have carefully considered the feedback provided and revised our manuscript accordingly.

In the revised version of the manuscript we have focused on the following main aspects raised by you and 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 editor's 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 the editor

First of all, I would like to thank the reviewers for their careful and overall constructive assessment of the manuscript. The authors have already provided a fairly extensive reply, commenting on both editorial and scientific issues.

There appeared to be consensus about the manuscript being already fairly long, and on the hand lacking figures describing changes in ocean circulation. Reviewer 2 has suggested splitting the manuscript in two parts. I respect the decision of the authors to keep a one-part format, while shortening the analysis of the atmospheric response. Now that this option is taken, it is up to the authors to provide a revised version with the suggestions for added material about the ocean circulation and sea ice, while keeping the manuscript size within reasonable bounds.

We are pleased to be able to maintain the general structure of manuscript, analyzing both the atmospheric and oceanic responses to a disintegrated GrIS. To address the reviewers concerns and to improve clarity and conciseness, we shortened the manuscript substantially.

l 24 – 27: I agree with reviewer 1 that one collective citation of 15 studies is not so good practice. While I concede that it is not the place for a full review, perhaps splitting the citation into three groups describing more what they have brought would be better.

Thanks for this suggestion. We agree and have decided to split the references into multiple groups to improve clarity. Additionally, we have expanded the information content, including specific examples that highlight the outcome of previous studies:
"A number of studies have investigated potential climatic effects of a completely or almost completely melted GrIS on the Northern Hemisphere Earth system under various climates. These studies found considerable climatic changes in response to a reduced GrIS volume, including thermodynamic changes in the climate over and in the vicinity of Greenland, such as warming (Crowley and Baum, 1995; Davini et al., 2015; Dethloff et al., 2004; Hakuba et al., 2012; Junge et al., 2005; Lunt et al., 2004; Merz et al., 2014a; Ridley et al., 2005; Solgaard and Langen, 2012; Stone and Lunt, 2013; Toniazzo et al., 2004; Vizcaíno et al., 2008) and a redistribution in precipitation over Greenland (Davini et al., 2015; Dethloff et al., 2004; Lunt et al., 2004; Merz et al., 2014b; Solgaard and Langen, 2012; Stone and Lunt, 2013; Toniazzo et al., 2004; Petersen et al., 2004). Further studies found dynamic changes, such as an increase in cyclonic activity over Greenland (Toniazzo et al., 2004; Petersen et al., 2004; Junge et al., 2005; Davini et al., 2015) and a weakening of the Atlantic Meridional Overturning Circulation (AMOC) (Davini et al., 2015)."

Regarding the list of experiments: it still not entirely clear (to me) why the nudging experiments will provide different results than equivalent atmosphere-only experiments. I understand from the response of the authors that the parameter configuration of the atmosphere in the model is not the same as in the coupled version and perhaps that, in this case, the nudging approach was a more straightforward and secure option to isolate the atmospheric contribution (even though a much computationally-expensive). But other than this technical aspect, I don't see how the atmosphere would see something different in a nudging experiment than a an experiment were sea-ice and SST would both be prescribed (if changes in sea-ice are allowed with respect to the control experiments, then we have an experiments considering both atmosphere and sea ice responses and they should be clarified; the only other point I can think of is a change in ocean variability at a period smaller than the nudging timescale). Can you comment on this?

This is correct. We decided to run more expensive nudging experiments instead of atmosphere-only simulations for technical reasons. The nudging experiments, using the same model version and parameters, ensure that the climate response in the simulations is indeed a climate signal and not due to changes in the technical setup or parameter tuning.
As we only prescribed SST and SSS, sea ice is allowed to change in our nudging simulations. We have revised the respective part in the method section and have clarified that sea ice is not prescribed:
"The first three experiments resemble CTRL, noGrIS and noGrIS_elev, but SST and sea-surface salinity (SSS) are nudged towards the climatology of the CTRL simulation. Hence, these experiments exclusively consider interaction of the GrIS with the atmosphere *and sea ice*, suppressing the ocean response. We interpret them as the atmospheric contribution to the full climate response."

The CTRL_Wind experiment is also a little bit mysterious to me. It would be useful to be more specific about what is meant when speaking about flux correction. It doesn't seem to be a classical heat or freshwater flux correction, but rather, seemingly, a wind correction. Is that correct? Can you provide a little bit more detail?

Yes, you are correct. Our flux correction is not a traditional heat or freshwater flux correction; rather it is a wind correction. To prevent any confusion for the reader we, have decided to revise this section in the manuscript:
"[...] For this experiment, we maintained the same conditions as in CTRL, but *imposed the wind-stress anomaly* of the simulation in which we only consider atmospheric changes in response to the elevation reduction (noGrIS_elev_atm) and CTRL_atm."

Both reviewers raised some doubts about the relevance of the discussion about the regrowth of Greenland ice sheet and the related discussion of the surface mass balance. The authors have provided a rebuttal showing that they have considered the comments and their final decision should be respected.

We appreciate your feedback on the section of the evolution of the GrIS and would like to keep it as a part of the manuscript, as we believe that our study offers a significant advancement in this field. To clarify the purpose of the section, we have revised the beginning of the section to clearly highlight the benefits and new insights provided by these experiments (see response to reviewer 2).

Authors have supplied new figures to answer the questions of reviewer #2 about jet stream indices (Figures 1 and 2). Technically, these figures are now published material (since the review is public) but I understand that adding them in the manuscript would not help the objective to shorten the paper. A brief note about the analysis could nevertheless be added somewhere in the final version of the paper.

We fully agree with your suggestion. As we aim for shortening our manuscript, we decided to not include the figures into the manuscript. However, we have added to the result section that we do not find significant changes in these indices in our simulations:
"*While the changes in the atmospheric circulation are substantial, we do not find statistically significant changes in atmospheric indices, such as the North Atlantic Oscillation (NAO), the Greenland Blocking Index (GBI) and the Northern Hemisphere Jet Stream position (not shown).* "

Finally, the authors mentioned here and there statistical significances. It is good practice to mention what is the associated null hypotheses (independent samples, equal distribution?)

Thank you for your comments. We have added the null hypothesis (equal distributions) to the figure captions where applicable.

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

November 2024

We would like to thank the reviewer for dedicating their time and effort to review our manuscript and for providing constructive feedback. We have carefully considered the feedback provided and revised our manuscript accordingly.
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 to provide valuable comments to help improve it.
In the revised version of the manuscript we have focused on the following main aspects raised by the reviewer:

- Shortening the presentation of our results
- Reorganizing certain parts of the results

- Strengthening the ocean analysis by incorporating additional figures

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 have removed 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 have removed 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 have verified 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 have added the analyzed remote changes explicitly to the text to clarify our point. The revised sentence reads 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 have revised 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.

We agree and have decided to split the references into multiple groups to improve clarity. Additionally, we have included specific examples that highlight the outcome of previous studies:
"A number of studies have investigated potential climatic effects of a completely or almost completely melted GrIS on the Northern Hemisphere Earth system under various climates. These studies found considerable climatic changes in response to a reduced GrIS volume, including thermodynamic changes in the climate over and in the vicinity of Greenland, such as warming (Crowley and Baum, 1995; Davini et al., 2015; Dethloff et al., 2004; Hakuba et al., 2012; Junge et al., 2005; Lunt et al., 2004; Merz et al., 2014a; Ridley et al., 2005; Solgaard and Langen, 2012; Stone and Lunt, 2013; Toniazzo et al., 2004; Vizcaíno et al., 2008) and a redistribution in precipitation over Greenland (Davini et al., 2015; Dethloff et al., 2004; Lunt et al., 2004; Merz et al., 2014b; Solgaard and Langen, 2012; Stone and Lunt, 2013; Toniazzo et al., 2004; Petersen et al., 2004). Further studies found dynamic changes, such as an increase in cyclonic activity over Greenland (Toniazzo et al., 2004; Petersen et al., 2004; Junge et al., 2005; Davini et al., 2015) and a weakening of the Atlantic Meridional Overturning Circulation (AMOC) (Davini et al., 2015)."

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 region (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 have modified 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 have added 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.

l 57: remove comma

We have removed 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 have added 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 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 manual of the land model JSBACH, 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 has been 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 have added 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 decided to run more expensive nudging experiments instead of atmosphere-only simulations for technical reasons. The nudging experiments, using the same model version and parameters, ensure that the climate response in the simulations is indeed a climate signal and not due to changes in the technical setup or parameter tuning. 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 have shortened this part.

l 141: replace -6K by 6K

That is correct, we have removed it and all further instances.

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 have added the March and September sea-ice margins from both the CTRL and noGrIS experiments to the air temperature figures. Additionally, we 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 and JJA with the March and September sea-ice extents of CTRL (green) and noGrIS (magenta).

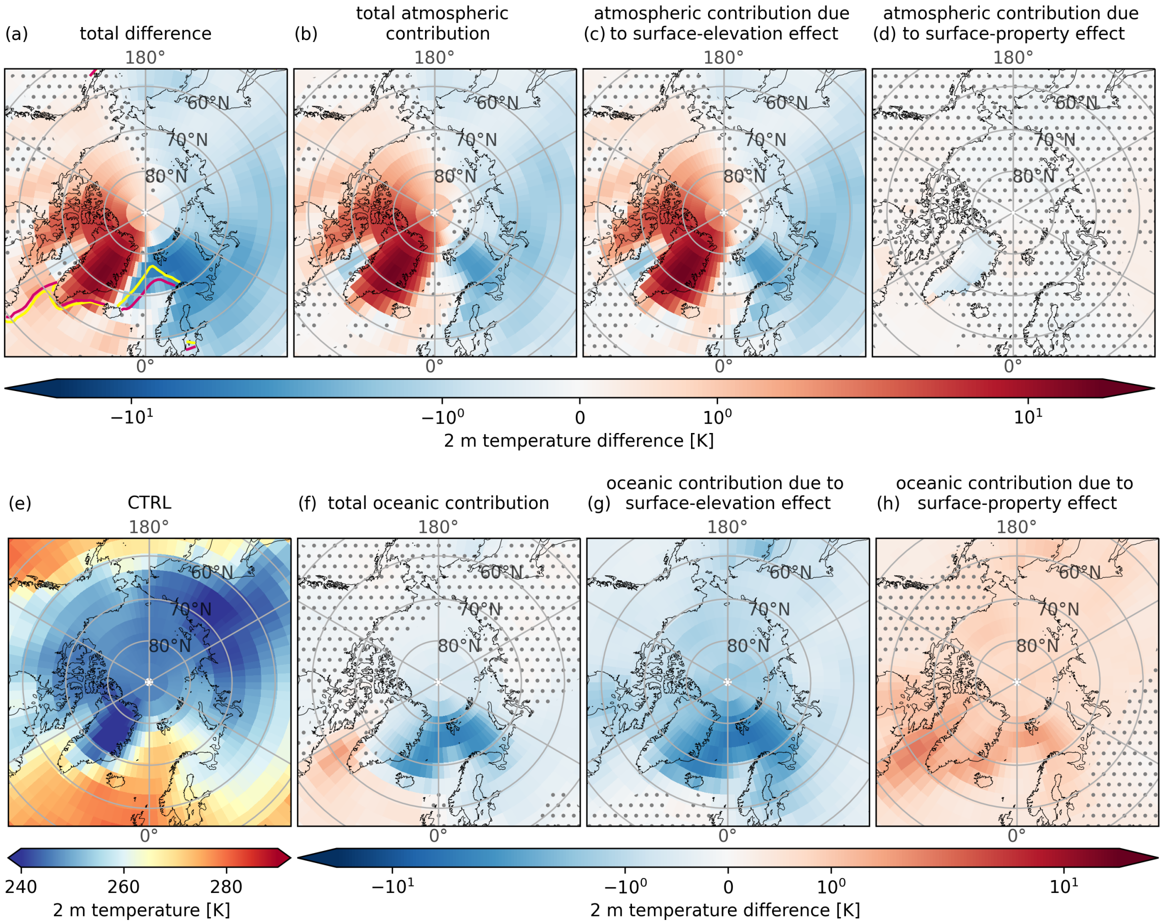

Figure 1: 2 m air temperature change over the Arctic in DJF with the March sea ice extent of CTRL (yellow) and noGrIS (magenta).

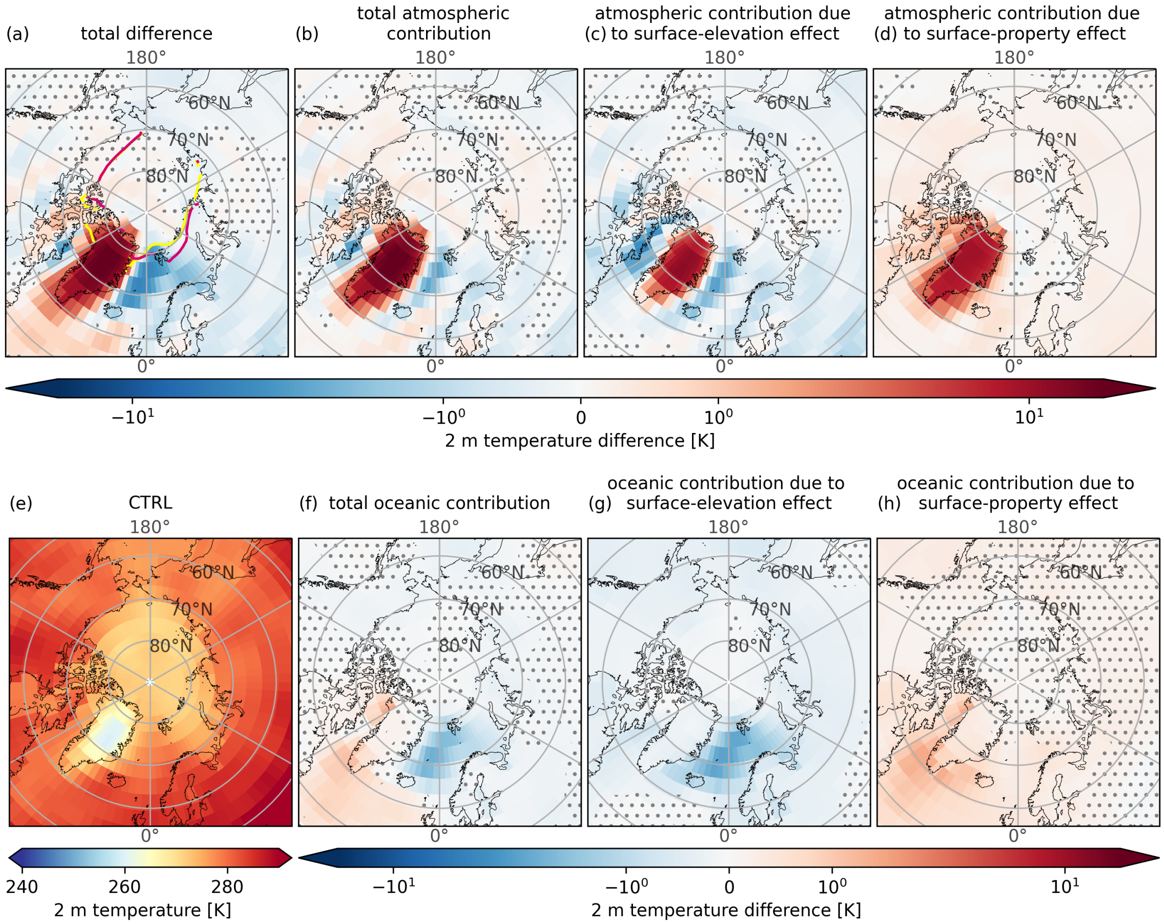

Figure 2: Similar to Figure 1 but for JJA and with the September sea ice extent of CTRL (yellow) and noGrIS (magenta).

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 have replaced '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 have revised the section and have added 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 in response to the lowered GrIS surface elevation (Fig. 2b & c). *Figure 2f shows that ocean feedbacks lead to 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. Hence, they cannot be the driver of the temperature dipole pattern. In Sect. 3.1.2 we will further show that changes in the atmospheric circulation are the driver of the dipole.*"

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

We have changed 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 have replaced feedback by effect.

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

We have revised 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 have revised the sentence as suggested: “*In Sect. 3.1.2 we will further show that changes in the atmospheric circulation are the driver of the dipole.*.”

l 166: remove the comma.

We have removed 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 have revised the first sentence of the subsection to: ”In summer, contributions *over land and ocean* are comparable, albeit weaker than in winter [...].”

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

Good idea. We have included a figure of the annual-mean changes.

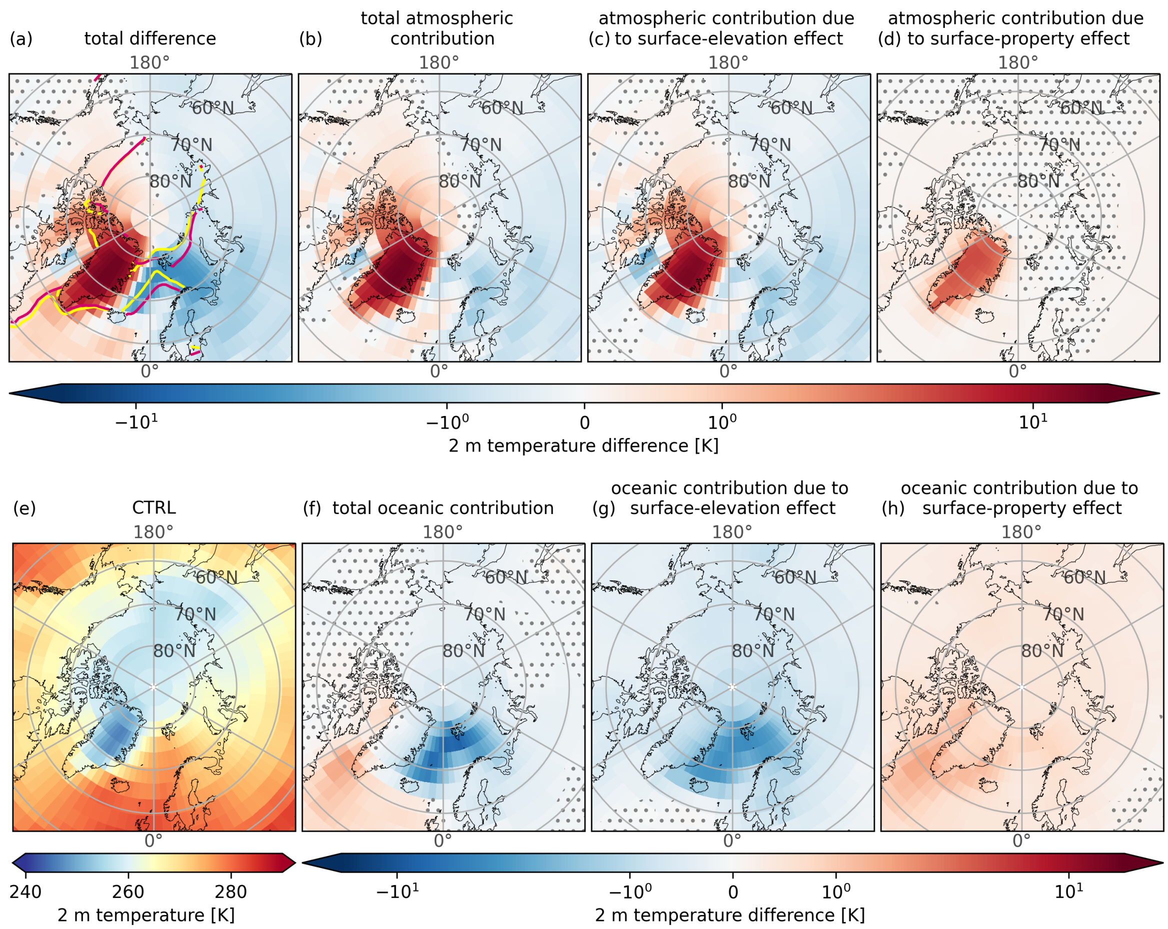

Figure 3: 2 m annual-mean air temperature changes with the March and September sea ice extents of CTRL (yellow) and noGrIS (magenta).

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

Thanks for the comment. We have changed the part accordingly, saying that the winds are weaker and change direction only over Greenland.
"*Around Greenland, annual-mean 10 m winds are weaker in the absence of the GrIS[...]. In noGrIS, 10 m winds also take a stronger easterly direction over Greenland as compared to CTRL [...].*"

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 have added the sea-ice extent into the 2 m air temperature figures for summer and winter and have included relevant references for changes in the sea-ice extent. Furthermore, we have added a figure illustrating the 2 m annual-mean temperature changes including the sea-ice extent. This figure is 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 have revised the text to reflect this.

l 190: Remove comma.

We have removed it, thanks.

l 193: Do you mean insulation rather than insolation?

Yes, you are right. We have changed 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 have edited the figure and have changed the scale and the vectors to logarithmic scales and have included this in the manuscript.

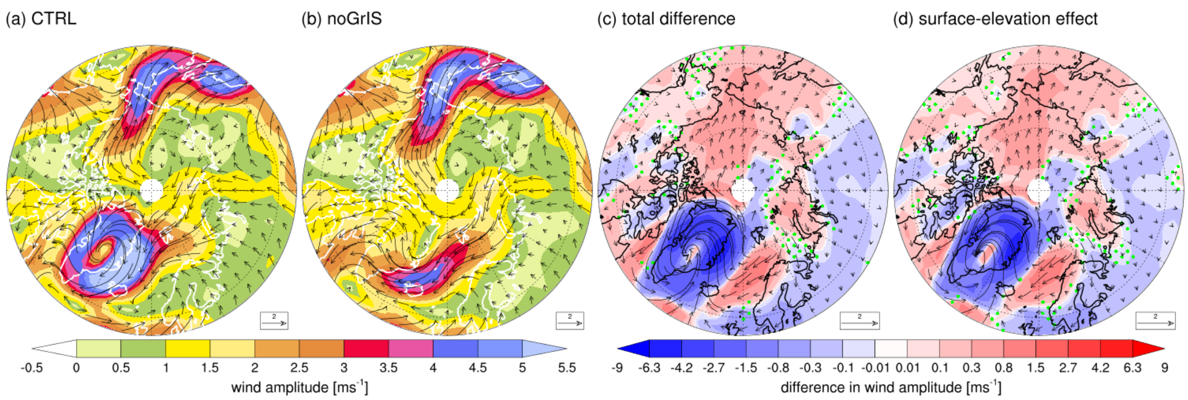

Figure 4: (a, b) Annual 10 m wind amplitude (contours) and direction (vectors, ms^{-1}), (c, d) total difference (noGrIS - CTRL) and surface-elevation effect contribution (noGrIS_elev - CTRL) to the difference in the 10 m wind amplitude. Stippling designates statistically non-significant regions (see Fig. 2). Please note the logarithmic scale in (c) and (d).

l 213: remove comma

Thanks for spotting this. We have checked the manuscript on commas again and made sure that they are placed correctly. We have also slightly revised and shortened 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. 2f).* [...]"

l 216: remove "both"

Yes, we did.

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 have added 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 have moved the part about the surface-property change into Section 3.1.1. Further, we have removed 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 have revised this section with a focus on creating an improved structure and conciseness. We have changed the structure from a basin-oriented to a process-oriented structure, which better highlights the physical processes and is easier to read. In doing so, we have removed duplicated information and have significantly shortened the entire section without compromising the content. Additionally, we have removed the introductory paragraph as suggested.
Furthermore, we agree with the comment that figures of deep-ocean processes could significantly aid the understanding. Therefore, we have added 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 (Figure 5c & d). These tracers are used to differentiate between the two major water masses: NADW and AABW.

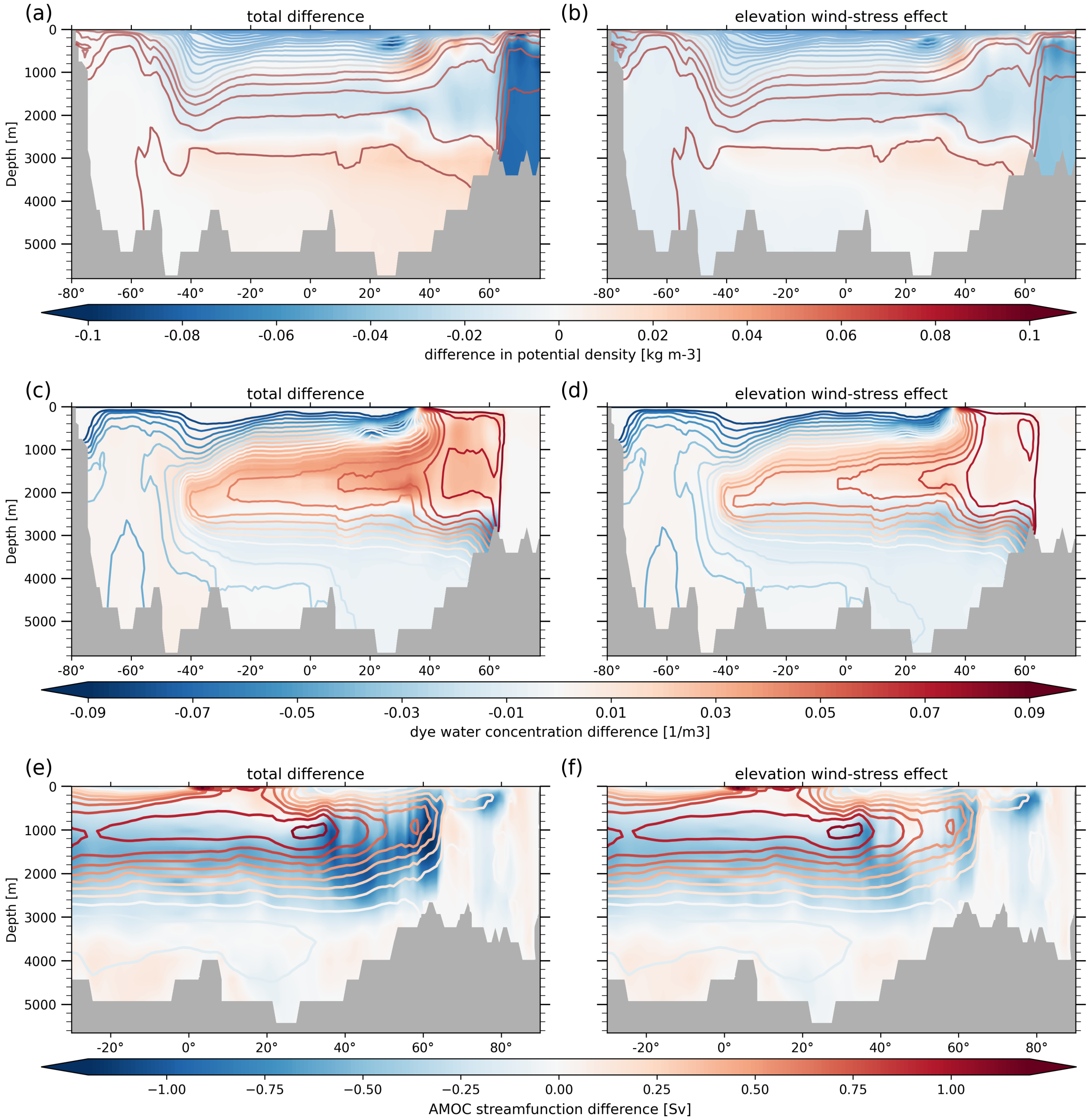

Figure 5: Cross-sections zonally averaged over the Atlantic Ocean. (a, b) Difference in potential density, (c, d) difference in dye water concentration as an indicator for the distribution of NADW and AABW and (e, f) difference in the AMOC streamfunction. The differences are overlayed with the contour lines of CTRL. Please note a change in the intervals from 0.1 to 0.2 kg m^{-3} between the contour lines in (a, b) above and below 1026.4 kg m^{-3}, respectively.

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 have added Figure 5 to our manuscript that shows (e) the total difference and (f) the elevation wind-stress effect. The CTRL AMOC is displayed as colored contour lines on top. We have described the figure in the text.

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 have added 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 for which we have added a figure to the manuscript (Figure 5a & b).

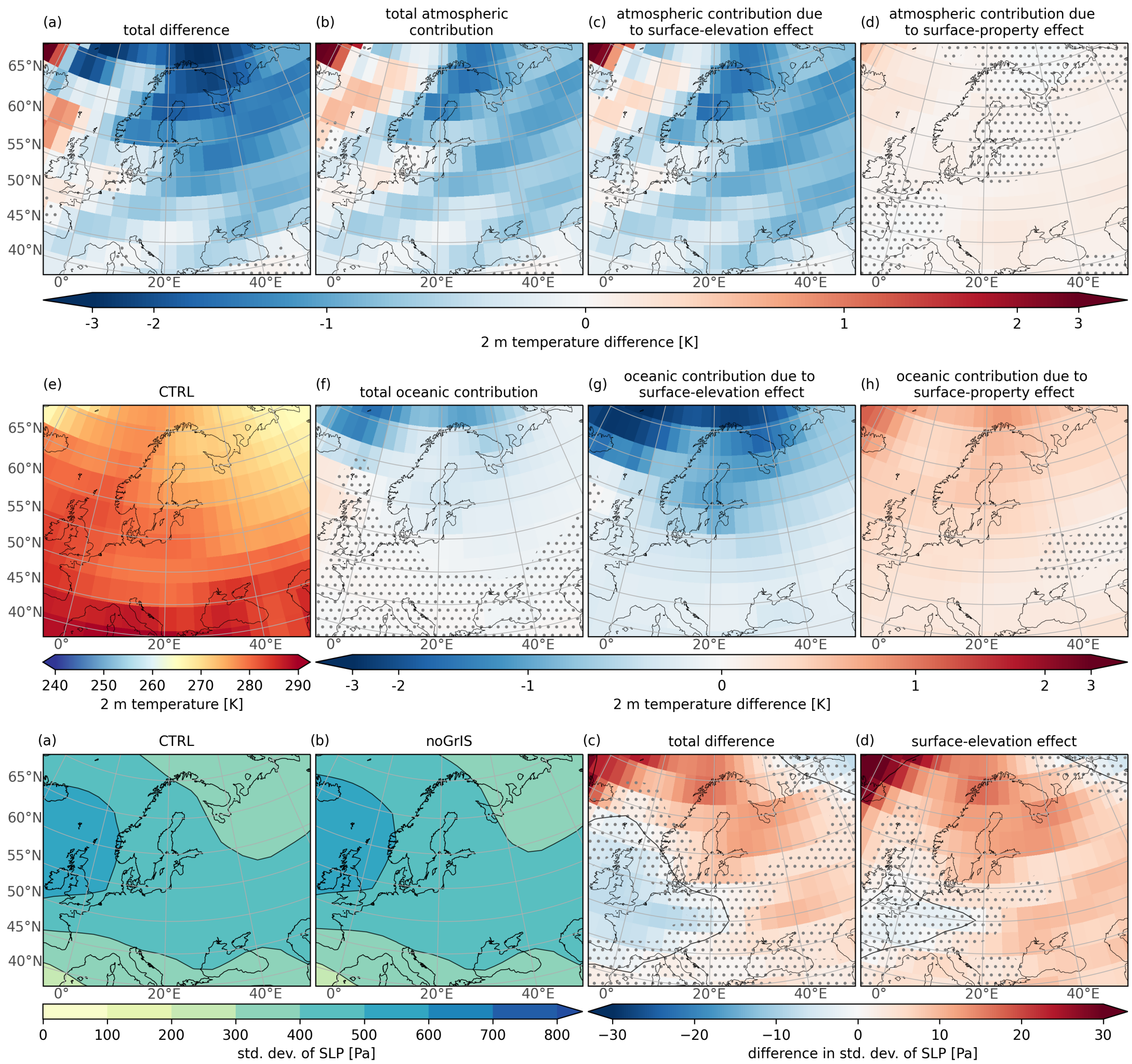

Figure 6: Annual-mean 2 m air-temperature over Northern and Central Europe (upper and middle row) and standard deviation of the DJF 2 to 5 days band-pass-filtered sea-level pressure (SLP) as a measure of cyclonic storm activity over Northern and Central Europe (bottom row).

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 have substantially shortened this section by summarizing 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 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 have revised 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 analysis 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 and 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 have emphasized the distinct impacts of the GrIS surface-elevation and property effect throughout the analysis of this section.

Response to the comments of reviewer 2 for the manuscript "Impact of Greenland Ice Sheet Disintegration on Atmosphere and Ocean Disentangled"

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

November 2024

We would like to thank the reviewer for his valuable comments. We have carefully considered the feedback provided and revised our manuscript accordingly.
In the revised version of the manuscript we have focused on the following main aspects raised by the reviewer:

- Shortening the presentation of our results
- Mention the response of Northern Hemisphere climate indices to a disintegrated GrIS
- Provide contextual understanding of the analysis of GrIS regrowth

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 2

This paper presents sensitivity experiments of the global model MPI-ESM with or without the Greenland ice sheet. It is interesting to read, mainly well written and figures are nice. It then deserves to be published in ESD.

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 review our manuscript and to provide valuable suggestions to improve our manuscript.

However, some discussion about the usual atmospheric indexes (NAO, AO and GBI) and their seasonal variability is missing for me as it will help the readers to put in a larger context the atmospheric impacts of noGrIS. Moreover, the impact on the Jet Stream and its variability should be also discussed for me as it is the main driver of the atmospheric dynamics. As this paper is already too long, I suggest to split this paper into 2 papers with a partim 1 about the atmosphere and associated climate indexes and with a partim 2 about the ocean (including its feedback on the atmosphere).

Thank you very much for your ideas on improving our manuscript. As suggested, we computed the atmospheric indices NAO (Fig. 1) and GBI (Fig. 2). Recognizing that various definitions of the NAO can yield different results (see Pokorná & Huth, 2015, https://link.springer.com/article/10.1007/s00704-014-1116-0), we applied three different methods to compute the NAO index: the difference of the normalized sea level pressure between Iceland and the Azores, an Empirical orthogonal function (EOF) and a principal component (PC) analysis. Further, we investigated the position of the Jet Stream over the North Atlantic region (Fig. 3). We found only small differences between the experiments that are not statistically significant. We assume that the horizontal resolution of our atmospheric model is not high enough to resolve such dynamics sufficiently. Therefore, we have decided to not include the full analysis of these indices and the Jet Stream to our manuscript and to only add a brief note about the analysis to the results section:

"*While the changes in the atmospheric circulation are substantial, we do not find statistically significant changes in atmospheric indices, such as the North Atlantic Oscillation (NAO), the Greenland Blocking Index (GBI) and the Northern Hemisphere Jet Stream position (not shown).* "

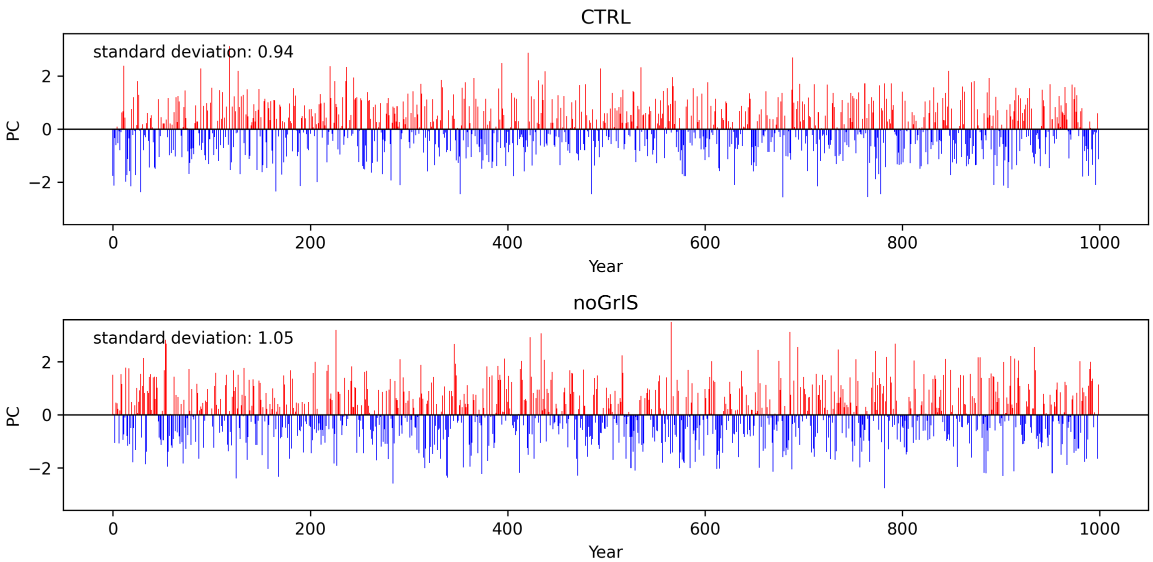

Figure 1: First (PC1) principal component time series associated with the leading EOF mode in sea-level pressure in DJF. The leading EOF modes explains 40% of the total variance.

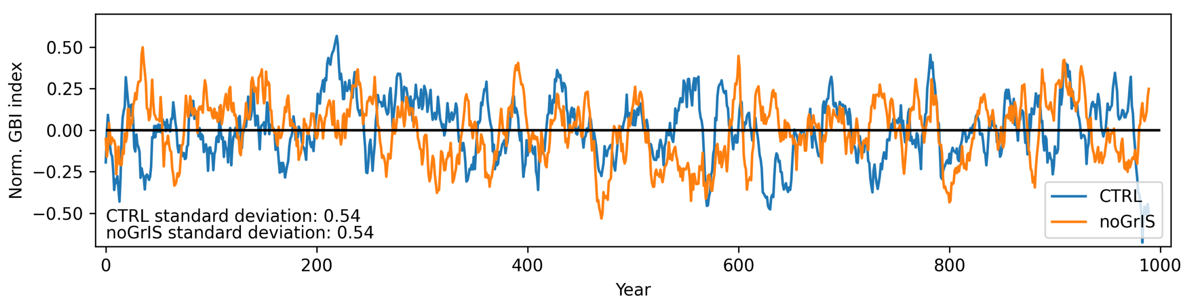

Figure 2: Time series of the GBI in DJF in CTRL and noGrIS using a 5-year running mean.

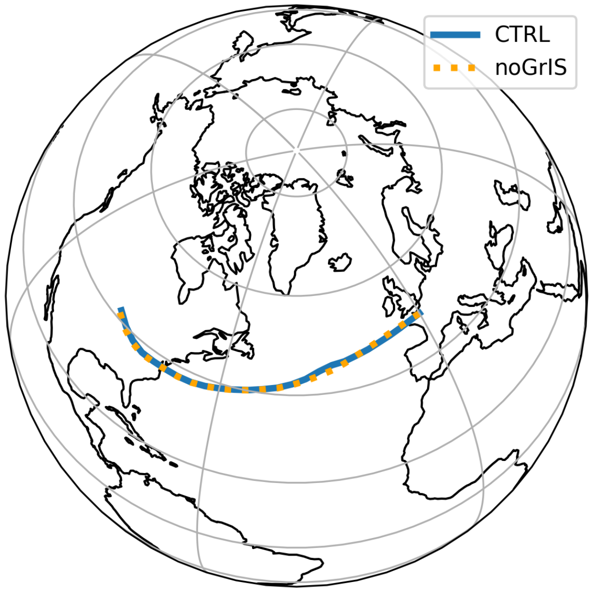

Figure 3: Mean location of the DJF Northern Hemisphere Jet Stream calculated from the last 1000 years of the simulations CTRL and noGrIS.

We agree that the manuscript is rather long. However, we believe that our manuscript is stronger when including the analysis of the atmosphere and the ocean in one single manuscript, as we found many interactions between the atmosphere and ocean in the climate with an absent GrIS. Additionally, the reader has to understand the changes in the atmosphere to be able to understand the oceanic response. As our focus is on the oceanic response, we decided to shorten the analysis of the atmospheric response and to keep both parts in one manuscript. We hope that this will overcome the concerns on the lengths of the manuscript by the reviewer.

Finally, the impact of noGrIS on the SMB is a bit out of context here and not enough scientifically robust with respect to other parts of this paper. This part should be leaved for another paper for me where a fully coupled ice sheet – atmosphere – ocean model should be used to evaluate this as a regrow of the ice sheet will impact the atmosphere which is not taken into here while discussed in depth in the other parts of this paper.

Thank you for your comment. Our analysis enhances our understanding of the factors that influence the formation and stability of the GrIS. This is crucial for comprehending the long-term implications of the simulated climatic changes in response to a complete GrIS disintegration on the Earth system. An important advancement of our study compared to previous studies is that our sensitivity experiments enable us to investigate the individual impacts of altered GrIS surface height and properties on a potential regrowth. Using a complex ESM for such analysis is hereby novel. We demonstrate that a removal of the GrIS would lead to such substantial alterations of the climate conditions in the Northern Hemisphere that these new conditions would inhibit the formation of a new GrIS. Not only do we observe an incomplete regrowth, but our sensitivity studies allow us to attribute this lack of a full recovery to its specific drivers: 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 have revised the beginning of the section to clearly highlight the benefits and new insights provided by our experiments. Additionally, we have emphasized the distinct impacts of the GrIS surface-elevation and property effect throughout the analysis of this section.