Response to reviewers

Dear reviewers,

To start with, we would like to thank you for your useful and constructive comments.
First, we address the comments raised by both reviewers.

Reply to both reviewers:

As suggested by both reviewers, we adapted the manuscript as following:

- **a)** we changed the paper title to stress that this study describes SIC-SST impact on modelled SMB, based on the regional climate model MAR outputs. The new title is now: "*Impact of anomalies in the surrounding oceanic conditions on 2007-2012 Greenland Ice Sheet surface mass balance using the regional climate model MAR*"
- **b)** we stressed the fact that the conducted sensitivity experiments assess the "direct" impact of SIC-SST on modelled SMB. This means that SIC-SST feedbacks on the general circulation are not considered since lateral atmospheric forcing is not allowed to change. However, it should be noted that the local circulation might be changed by the SIC-SST anomalies within MAR domain.
- **c)** we underlined the fact that these experiments suggest that SIC-SST anomalies only locally impact GrIS SMB, allowing a clearer distinction with Day et al. (2013) results.
- **d)** we included the Supplementary Material figures in the article.
- **e)** we provided a clearer analogy between the "fictive" SIC-SST anomalies, prescribed in our sensitivity experiments, and the recorded pre-2007 SIC-SST conditions. Table 1 has been modified to exhibit JJA mean anomalies between SIC-SST from the sensitivity experiments for the 2007-2012 period and JJA mean SIC-SST from ERA-Interim reanalysis covering the 1979-2000 period.
- **f)** an actual significance analysis is supplied regarding cumulated GrIS SMB anomalies obtained for every sensitivity experiment. The significance was evaluated using a one-sided Student's t-test with a 95% degree of confidence, based on the annual cumulated SMB components deviations between each sensitivity experiment and the reference simulations for 2007-2012. The Results Section was modified according to these new analyses.
- **g)** we decided not to include a comparison between AWS wind intensity observations along the K-transect and outputs from our experiments. It is beyond the scope of this (already long) paper to perform such analysis. Moreover:
  - a validation of MAR modelled wind speed has already been performed in Lefebre et al. (2005).
  - the relatively coarse horizontal resolution (40 km) used for these simulations precludes any accurate comparison between AWS observations and MAR grid-cells outputs.
  - we did not aim to validate MAR modelled katabatic wind, induced by oceanic conditions anomalies prescribed in MAR, against observations covering periods representative of high (resp. low) sea ice extent (SIC) and sea surface temperature (SST). This paper rather aims to suggest a general explanation of "direct" oceanic forcing impact on the modelled katabatic wind simulated by MAR.
- **h)** we improved the overall structure and organisation of the manuscript.

Below, our responses to the individual reviewers comments are displayed in blue to facilitate readability.

Referee review #1: *Anonymous*

1) First, it is not clear why the authors focus strictly on the 2007 - 2012 period when the MAR outputs are available for the period 1958 - 2013. I understand that they want to focus on the recent records but addressing previous periods would allow them a more complete analysis of those circumstances when sea ice was not as "damaged" as after 2007. Moreover, it would make the paper more complete.

⇒ The analysis was limited to a short time period (2007-2012) in order to reduce the length of these time-consuming simulations. The number of years selected is a trade-off between number of sensitivity tests and simulation period. Indeed, 13 simulations, 1 reference run + 12 sensitivity experiments covering a five-year period, were carried out, which we judged would be suitable for this goal. Of course, we selected a period with clear SIC anomalies to enhance the strength of any potentially available signal.

2) The other point is that throughout the entire paper the authors fail to mention that the results reported here are purely the outputs of a sensitivity experiment. This should be made clear when results and discussions are presented. In this regard, the paper is nothing more than a simple analysis of the outputs of the model and it would be interesting to have more discussion about the points that the model is missing (e.g. changes in atmospheric forcing not included).

⇒ Please see our reply **b)** to both reviewers.

3) The hypothesis of the effects of the katabatic winds is surely appropriate and supported by the model outputs. Nevertheless, the discussion concerning the strengthening or weakening of such winds and some of the authors? statements could have been accompanied by an analysis of AWS measurements along the coast of Greenland. This would have made the study more interesting.

⇒ Please see our reply **g)** to both reviewers.

4) Overall, the paper sounds more like a section of a Master or PhD thesis, with little critical discussion and does not add too much to previous knowledge. I strongly encourage the authors to change the title of the paper. The way is proposed now makes it sound as if they had found a general solution to the analysis of the impact of the sea ice and SST on SMB in Greenland when, in reality, the paper is purely a sensitivity analysis of the MAR model to sea ice concentration and SST. The new title should reflect this.

⇒ We reformulated the title accordingly, also see our reply **a)** to both reviewers.

5) The figures presented in the supplementary material are heavily discussed in the paper and they represent the core of the discussion of the results of the sensitivity analysis. Supplementary material should be used only to show auxiliary information. I strongly encourage the authors to include the figures currently in the Supplement in the main body of the text. The current supplement is used as a placeholder for these figures and does not contain any text.

⇒ These figures are now included in the MS.

Detailed comments:

P 1456 line 13: the index does not have a "phase" but a value. Please change accordingly.

⇒ Thank you for pointing this out, we reformulated as: "Anomalous atmospheric forcing, attributed to the persistent 2007-2012 negative phase of the North Atlantic Oscillation (NAO) in summer, has favoured warmer and drier conditions over the ice sheet, enhancing surfacemelting (Fettweis et al., 2013b)".

Line 14: remove "the" before melting and specify that you are referring to "surface" melting.

⇒ We obviously meant "surface melting", it is now included.

Line 17: "30% is then" should be replaced with "30% might be explained" or "can be explained"

⇒ We replaced "30% is then explained" by "30% might be explained".

Line 21: it is not clear what the author means with "indirectly impacting SMB". There is not, to my knowledge, an established link for a "direct" impact between sea ice and/or SST and SMB in Greenland. I assume the authors are referring to potential feedback mechanisms at different spatial and temporal scales. If so, this should be explained either here and in the introduction. Line 22: again what is the "direct" impact of ocean on SMB ?

⇒ In the revised paper, we clarified what we meant by "direct" and "indirect" impacts:

- **Direct impact:** defined as local (i.e. around Greenland) SIC and/or SST anomalies impact on near-surface air temperature and moisture, without considering feedbacks on the general circulation.
- **Indirect impact:** takes into account the SIC and/or SST-induced general circulation variations (Overland et al., 2012) and their potential influence on the atmospheric conditions above Greenland.

Line 24: Can you provide a measure of how large this domain was and how close to Greenland and to the domain boundaries ?

⇒ In our study, the integration domain covers a ≈3200 km x 4120 km area, using a 40 km spatial resolution. This domain extends ≈800 km away from the GrIS towards the North, South and East while it extends ≈1200 km westwards, since most air reaches the GrIS from this direction.

⇒ In Hanna et al. (2009, 2013a), the integration domain covers a ≈1800 km x 3500 km area, using a 25 km spatial resolution. In this study, the domain extended ≈500 km away from the GrIS towards the North and South while it extended ≈250 km towards the East and West, possibly underestimating oceanic forcing since oceanic grid-cells are closer to the lateral boundaries. Fig.7 in Hanna et al. (2009) depicts this domain.

⇒ These dimensions are now included in the MS.

P 1457 Line 1: "is known" is a vague expression if not supported by a reference. Please, add.

⇒ This was removed from the MS.

Lines 1-2: What would be the consequence of this ?

⇒ By prescribing fixed monthly mean SIC and SST to force the oceanic conditions in HadRM3, Day et al. (2013) do not resolve the interdiurnal dynamics of oceanic forcing, allowing no consideration of the actual oceanic events and their influence on the atmospheric conditions. This is avoided by prescribing 6-hourly oceanic conditions. This explanation has now been included in the revised MS.

Lines 3-4: What does it mean that the forcing might be too weak ? And, what is this statement based on ?

⇒ By "too weak", we meant that prescribed SIC-SST anomalies in previous studies might induce anomalies with a too low signal-to-noise ratio. Therefore, instead of prescribing pre-2007 oceanic conditions to replace the current ones (Day et al., 2013; Hanna et al., 2013), our approach, similar to Hanna et al. (2009), attempts to assess the modelled SMB sensitivity to larger "fictive" SIC-SST anomalies. The magnitude of these SIC-SST perturbations relative to observations covering 1979-2000 is listed in Table 1. We replaced "might be too weak" by "might not provide sufficiently large oceanic forcing".

Line 4: The way the authors put it (they are going to "determine" the impact) makes it sound as they would have a final unique solution when, in effect, this study is another model sensitivity analysis. The sentence should be re-written to show this.

⇒ We reformulated this sentence accordingly: "This study aims to evaluate whether isolated or coupled SIC and SST anomalies could account for major GrIS SMB perturbations by prescribing modified oceanic conditions within MAR domain".

Line 5: is "significanlty"here used in a statistical way ? if not, please re-phrase

⇒ Reformulated as: " [...] could generate major GrIS SMB perturbations by prescribing modified oceanic conditions within MAR domain". See also our reply **f)** to both reviewers.

Line 5: what is "more extreme" forcings ?

⇒ Here we meant to indicate larger oceanic forcing than those prescribed in Day et al. (2013) and Hanna et al. (2009, 2013). Indeed, this was somewhat confusing and has been reformulated as: " [...] by prescribing modified oceanic conditions within the MAR domain."

P 1458 Line 21: "small" compared to what ? Please address

⇒ The appropriate term would have been "negligible" instead of "small". However, we decided to remove this sentence altogether and to modify the previous sentence as: "The ice sheet topography, based on Bamber et al. (2013), is kept fixed."

P 1459 Line 6: Can the authors explain better that the changes in SIC and SST do not actually impact atmospheric fields. This implies that the authors are actually testing only the sensitivity of the model to different SIC and SST conditions (given the same atmospheric conditions of the original SIC and SST) and without accounting for the potential impact of those changes on atmospheric fields. This should be made clear by the authors. In other words, the results are showing the sensitivity of the model to the SIC and SST and the authors should made this very clear.

⇒ Please see our reply **b)** to both reviewers. This was clarified in the MS.

Line 18: Can you add a reference for the freezing point of salt water ?

⇒ The actual SST under the sea ice is about -1.8°C (Stark et al. 2007). In our experiment, we "assumed" that the SST has to drop to -3°C to be completely covered by sea ice (SIC = 100%). Even though our threshold is lower than the actual freezing temperature, it allows to be certain that the sea ice-free to sea ice-covered pixels conversion, induced by our sensitivity experiments, would at least be observed in reality for similar oceanic conditions. Moreover, the selected threshold has not notably affected the results of our experiments but would only have a negligible impact on the resulting sea ice extent.

P1461 Line 3: you mean down to -10C ? What does it mean a "decrease up to 10 C" ?

⇒ Here, we meant that the wintertime near-surface air temperature decreased by about 10°C over the newly sea ice-covered areas. We corrected accordingly.

P 1462 Line 18: "significant" is again here used without a statistical meaning. Please change

⇒ Please see our reply **f)** to both reviewers.

Line 20: Any suggestion on why for SIC and SST the variation of SMB is linear ? It is an amazing R2 of 0.99! the authors do not discuss that such linear variability might be the simple effect of purely switching values without considering the changes in atmospheric fields coupled with the changes in sea conditions.

⇒ In this study, we only considered the "direct" impact of SIC and/or SST on the GrIS SMB without taking into account oceanic forcing feedback on the general circulation. As a result of unchanged ERA-Interim atmospheric conditions, MAR suggests a linear relationship between SIC or SST and the GrIS SMB. Different results might be obtained when considering changes in general circulation induced by the prescribed SIC or SST anomalies. But, as explained above, this is beyond the purpose of this paper.

Line 21: what are "combined" effects ?

⇒ The "combined" SIC-SST effects reflect the response to a combination of SIC and SST forcing, as is now more clearly explained in the paper.

How the authors can explain that in one case there is a linear effect on SMB and when both are combined there is a non-linear effect ?

⇒ MAR suggests a compensating effect between ablation and accumulation processes for the combined-forcing experiments. For instance, both the increase in SST and decrease in SIC favour enhanced evaporation at the sea surface, leading to a larger accumulation increase over the GrIS than simulated for non combined-forcing. On the other hand, runoff, depending mainly on summer near-surface temperature, is almost only driven by SST anomalies (Section 3.1). Therefore, ablation and accumulation anomalies compensate each other when integrated over the whole ice sheet, resulting in a non-linear relationship between coupled SIC-SST experiments and integrated GrIS SMB. This has now been clarified in the revised MS.

Line 23: So this means that the sensitivity of the model to the changes considered for SIC and SST are within the SMB model uncertainty. This does not "prove" that there is no impact but rather that the model cannot simulate changes because they are smaller than the models uncertainty.

⇒ Our sensitivity experiments suggest that, when integrated over the whole ice sheet, SMB anomalies ($\pm$7%) are within MAR SMB uncertainty ($\approx$ 10%). Nevertheless, this 10% uncertainty corresponds to the systematic MAR SMB outputs uncertainty. Every sensitivity experiments were run using the same model version and are thus not independent of this systematic uncertainty. Therefore, we calculated the statistical significance of each change in SMB components using the standard deviation of the inter annual variability anomaly. As a result, a statistical significant change does not imply that the latter change is considerable.

P 1463 Line 1: Can the authors provide references for this ?

⇒ We now cite Rennermalm et al. (2009) at the end of this sentence.

Line 2: Also, the reference to katabatic winds should be accompanied by more "historical" references describing the nature of these winds.

⇒ Thank you for the suggestion, we included a short explanation accompanied by references of the physical processes involved in the forcing of katabatic winds.

Line 8: What does it mean "allowing some oceanic influence" ?

⇒ We reformulated as: "allowing small oceanic influence" instead of "some".

Line 10: the authors here imply the katabatic winds are the "only" mechanism explaining the impact and should re-word this section.

⇒ Thank you for the suggestion, we reformulated accordingly: "Moreover, the western Greenland coast is more sensitive to oceanic forcing than the eastern coast (Fig. 6) partly due to its gentler slopes, leading to weaker katabatic winds".

Line 13: "Significantly" again should be replaced.

⇒ Please see our reply **f)** to both reviewers. This was clarified in the MS.

Line 19: What do the authors mean in the first part of their sentence ?

⇒ We referred to the explanation provided in Section 3.1. In summer, when the sea ice surface temperature is close to the ice melting point, it does not strongly impact near-surface air temperature which are similar.

Line 21: "insignificant" should be changed (maybe "negligible")

⇒ Thank you for the suggestion, we reformulated accordingly.

Line 15: Thw work by Rennermalm should be cited at the beginning as it represents an early work looking into the linkages between sea ice and SMB in Greenland.

⇒ We now cite Rennermalm et al. (2009) on Line 1.

P 1464 Line 10-12: This sentence is purely speculative as it derives from a sensitivity analysis of a single model which does not account for the impact of changes of SIC and SST on atmosphere. Therefore, it should be removed.

⇒ We have removed this sentence.

P 1465 Line 2: The sentence concerning the "efficiency" of previous studies is purely speculative and should be removed.

⇒ We have removed this sentence.

Figure 1: What are the minor ticks on the figures ? The numbers on the bars are too small and should be enlarged.

⇒ The bars were enlarged accordingly.

What are the units (is it $\%$ normalized to 1) ?

⇒ Fig.1a shows SIC expressed in $\%$ normalized to 1 while Fig.1b (resp. Fig.1c) depicts SIC anomalies expressed in $\%$ normalized to 1 (resp. -1). Now clarified in revised MS.

What are the units of the figures 1e and 1f ?

⇒ Fig.1d shows SST expressed in °C while Fig.1e and f depict SST anomalies expressed in °C. Now clarified in revised MS.

Figure 3: The caption is confusing. The authors should first introduce the background values and then explain the meaning of the arrows.

⇒ Thank you for the suggestion.

Also, it is not clear why they are using the temperature for JJA and specific humidity for annual mean. They should either report one parameter or increase the subplots to 4, showing both temperature and humidity for summer and annual means.

⇒ We prefered to leave this unchanged, but we have improved our explanation of why we do so in the revised MS:

- For **Fig. 3a**, we only selected the JJA period since runoff is mainly driven by summer near-surface air temperature. Therefore, this plot suggests that mean JJA air temperature anomalies, induced by the SIC-6 / SST+4 experiment, don't propagate far inland because of the blocking effect of near-surface katabatic winds. As a result, anomalies in runoff are restricted to marginal regions where small positive anomalies in air temperature remain since katabatic winds dissipate over flat coastal regions away from the ice sheet.
- For **Fig. 3b**, we selected an annual mean specific humidity (g/kg) to better represent the total accumulation over the GrIS, consisting of both winter and summer precipitation. The resulting plot suggests that mean annual air moisture anomalies don't propagate far inland because of the katabatic winds blocking effect. Therefore, anomalies in precipitation are also restricted to marginal regions where katabatic winds dissipate.

Referee review #2: *E. Burgess*

In addition to the comments below, I suggest that overall improvement of sentence structure and organization is needed. Results should simply include the results of the sensitivity tests and and the analysis needs to be reorganized into a logical discussion of results. In many cases, the much more detail and discussion is also needed.

⇒ Please see our reply to both reviewers.

I agree with reviewer #1 that the title appears misleading. This is a simple sensitivity test of MAR to SIC and SST. My concern is also is that the title uses the word "small" while your abstract, for example simply says "does not affect". Consistency in your conclusion throughout the paper is needed.

⇒ We improved the paper accordingly, see also our reply **a)** to both reviewers.

Detailed comments:

P1454L26: "make that" awkward rewrite

⇒ We reformulated accordingly: "The GrIS mass loss has accelerated over the last decades (Rignot et al., 2011; Enderlin and Howat, 2013; Fettweis et al., 2013b; Wouters et al., 2013) as a result of enhanced GrIS surface melting and iceberg calving (Hanna et al., 2009; Van den Broeke et al., 2009). These ablation processes contribute to 25% of ongoing global sea level rise (Shepherd et al., 2012), affecting coastal regions worldwide".

P1455L12: "air temperature over and moisture" rewrite

⇒ Thank you for the suggestion, we removed "over".

P1455L13-17: Runon sentence, please clean up.

⇒ This has now been improved in the revised MS.

Intro: The authors need to make the text more clear wrt the relationship between SMB and events like the 97% melt event. To the reader it is not clear if you are trying to understand the decrease in SMB or the reason for the melt records like 97%. While the two are related they are not the same.

⇒ This study aims to provide an answer to both questions:

- On the one hand, we assess the "direct" impact of oceanic forcing on GrIS SMB.
- On the other hand, we stress that the surface melting records broken during 2007-2012 are not "directly" induced by SIC and SST anomalies. Enhanced melting is more likely generated by the recent general circulation shift observed over the GrIS, which might be partially explained by the "indirect" oceanic forcing effect.

P1456L4: ", and" is typo

⇒ Thank you for pointing that out.

Sentence needed describing the negative NAO pattern

⇒ This has now been included in the revised MS.

P1456L16: Need to explain what "anomalous" means here

⇒ Here, "anomalous" refers to the recent shift in the general circulation attributed to negative NAO pattern. We reformulated as: "According to Fettweis et al. (2013b), about 70% of the recent surface melt increase can be attributed to the NAO-induced southwesterly warm air advection towards the western GrIS (Box et al., 2012)."

P1456L16: Warm air advection from where? and as a consequence of what pattern of circulation?

⇒ As a result of negative NAO pattern, the Icelandic Low weakens. This favours more frequent warm southwesterly air advection towards the western GrIS coast, probably enhancing the surface melting. This is now explained in the MS.

P1459L1: Why is spin up needed in the snow model and why is 5 years the chosen spinup time?

⇒ The snow cover initialization is essential to accurately model GrIS SMB (Lefebre et al., 2005). Indeed, surface albedo strongly depends on the initial snow cover. Therefore, a five year spin-up (2002-2006) was carried out to allow the formation of a "realistic" snow cover in 2007.

P1459L12: Since this is a sensitivity study, you need to discuss both the scale and pattern of OBSERVED SIC changes in relation to how this method will reduce modeled SIC?

⇒ Please see our reply **e)** to both reviewers, see also the new Subsection 2.3.4 in the MS.

P1460L2: Similar to previous, need discussion to justify your choice of a constant 2,4 degree changes. How does this compare to the pattern and magnitude of observed and expected future SST changes?

⇒ Please see our reply **e)** to both reviewers, see also the new Subsection 2.3.4 in the MS.

Section 2.3.3: Similar as previous, need more discussion of the relationship between SSTs and SICs.

⇒ Please see our reply **e)** to both reviewers, see also the new Subsection 2.3.4 in the MS.

On lines 14-17 you have begun to address questions of this comment as well as the previous two. However this discussion must be more thorough and placed appropriately.

⇒ See the new Subsection 2.3.4 in the MS.

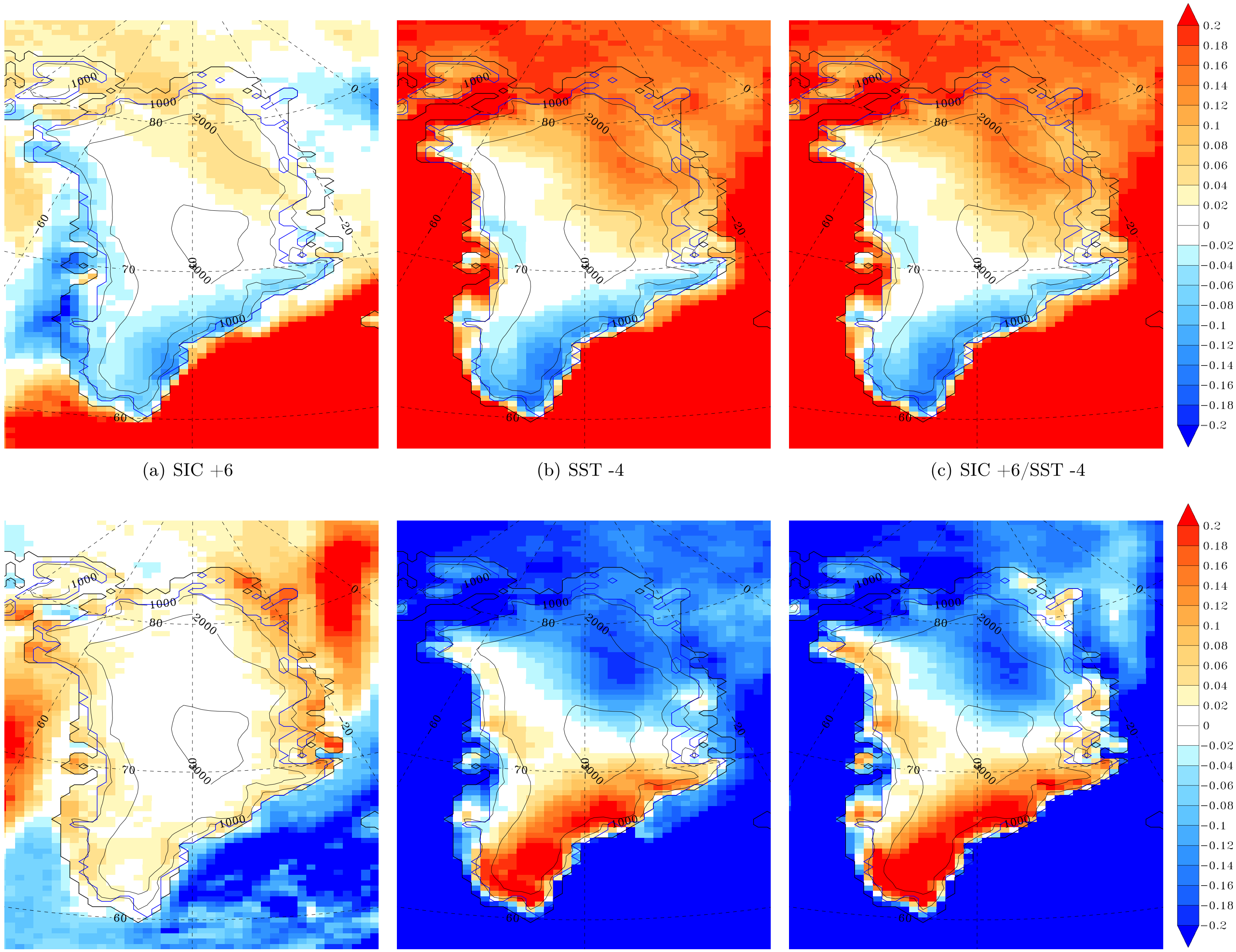
P1461L2-9: This is an important point but could be explained with far better clarity.

⇒ This has now been clarified in the revised MS.

P1460L1: We need an explanation of why is this anomaly only occurring in the SE when there are large changes in SIC to the west? This text should be incorporated into an organized discussion section

⇒ Anomalies in snowfall are not "only" restricted to the south-east of Greenland. However, at other locations, snowfall anomalies are not large enough to be depicted in Fig. S1. The panel below presents annual mean anomalies in cumulated snowfall (%) for each sensitivity experiment over 2007-2012. Anomalies in snowfall do exist along the western coast but are small relative to the south-east. This point has now been clarified in the MS.

Annual mean anomalies in cumulated snowfall (%) for each sensitivity experiment over 2007-2012



(a) SIC +6 (b) SST -4 (c) SIC +6/SST -4

(d) SIC -6 (e) SST +4 (f) SIC -6/SST +4

P1461 Section 3.2: Need to explain the relative contributions of higher near-surface temps and a rain induced albedo feedback to GrIS SMB.

⇒ For the SST +4 experiment, GrIS runoff is affected by higher summer near-surface temperature and reduced surface albedo:

- runoff is mainly driven by summer near-surface temperature. Therefore, higher SST and hence near-surface temperature favours larger runoff.
- the snowfall conversion into rainfall, resulting from a warmer ocean and near-surface temperature, wets the surface snow cover and hence reduces the surface albedo. This leads to an increase of surface melt energy, allowing enhanced surface melting and runoff. When combined, both these ablation processes exceed the accumulation, resulting in negative anomalies in SMB when integrated over the GrIS.

This is now included in the manuscript.

P1462L4-6: This runs on and confusing. Please improve clarity
⇒ This has now been clarified in the revised MS.

P1462L18: "suggest"
⇒ Thank you for pointing that out, we replaced "suggests" by "reveal".

P1462L18: "significant" sounds like it could be referring to statistical significance. Please clarify.
⇒ We replaced "significant" by "major".

P1462L21: Very passive voice, suggest moving to more active.
⇒ We reformulated accordingly.

P1462L24: Seems to me from figure 2 that the SMB anomalies are in all coastal regions not just west? On that note need text in the results CLEARLY describing the spatial pattern of mass loss on periphery and mass gain on high elevation SE ice sheet.

⇒ On P1462 line 23-25, we stressed that SMB anomalies are mostly restricted to western and southeastern regions where SMB anomalies are respectively driven by runoff and snowfall variations. This has now been clarified in the revised MS.

P1463L9: Seems somewhat contradictory to Figure 2 where precip changes are largest on the steep SE coast. Please clarify. Same for P1463L11-14. This is out of my expertise but are the katabatic winds not very shallow relative to the depth of the boundary layer over the ocean and thus the depth moist air advection?

⇒ This is due to the fact that air humidity anomalies, induced by oceanic forcing, extends higher than air temperature anomalies (see Fig. 3). Therefore, even though katabatic winds are strengthened along the eastern coast, the advection of humidity anomalies persist above the katabatic wind layer and affects precipitation further inland. However, most of the humidity anomalies are limited close to the surface and are then blocked by the near-surface katabatic flow. This has been clarified in the MS.

P1463L15: wrt "enhanced in summer", previous discussion mentions how summer SIC has little effect on SMB because the water is almost 0 if SIC is absent. This sentence has to be clarified wrt this point.

⇒ In summer, SIC anomalies have no major impact on SMB. However, the summer thermal and pressure gradients between the ice sheet interior and the ocean weaken, reducing the katabatic winds intensity. This allows SST anomalies to stronger affect GrIS SMB in summer than during winter.

P1463L24: Statistically insignificant?

⇒ We replaced "insignificant" by "negligible".

P1463L25: "The horizontal temp..."

⇒ Thank you for the correction.

P1464L15: Seems to me from figure 2 that the SMB anomalies are in all coastal regions not just west?

⇒ Please see our reply to your comment about P1462L24.

P1464L22: The combined impact is small but the spatial pattern of SMB change is worthy of more discussion.

⇒ As a result of the katabatic winds blocking effect, cumulated GrIS SMB anomalies induced by combined SIC and SST changes are limited and restricted to coastal regions. Locally, SMB anomalies do exist along the western coast, driven by runoff anomalies, and the south-eastern coast, driven by precipitation anomalies. However, when integrated over the GrIS, ablation and accumulation changes compensate each other, resulting in almost unchanged SMB.

P1465L15-17: Confused by this conclusion. You just stated that katabatic winds "are strong enough to prevent near-surface oceanic air from penetrating far onto the ice sheet and hence affecting its SMB". Then you state that the melt events are due to "winds advecting warm air to the GrIS, resulting in enhanced surface melting". Given your positioning in the introduction, a more thorough discussion of what could be causing the melt events is needed.

⇒ In this study, we stress that oceanic forcing has no "direct" impact on GrIS SMB since anomalies in air temperature and humidity are mostly limited to the surface where katabatic winds exert their blocking effect. However, the recent shift in the general circulation observed over the GrIS, expressed as a more frequent occurrence of a negative NAO pattern (i.e. indirect impact of oceanic forcing), results in southwesterly warm air advection in the upper atmosphere. This would result in larger longwave radiation towards the ice sheet and hence enhanced melt energy available at the surface, possibly explaining the recent surface melt records broken over the GrIS since 2007. This has been clarified in the MS.

External review: *J. Day*

1. The question implies the hypothesis: "there was something different about the 2007- 2012 years" (i.e. low ice and high SST). Therefore I was expecting to see comparison to a reference run (pre 2007) and perturbations to this (i.e. 2007-2012 SSTs & pre-2007 SIC, 2007-2012 SIC & pre-2007 SSTs etc.). I suppose the SIC+6/SST-4 is the "pre- 2007" simulation in this context. But it is not clear to me what this represents. What observed years are the SIC+6 sea ice cover like? More ice than the 2000s? 1990s? It would be useful to make this analogy to reality so the reader can put the perturbations into context.

⇒ Please see our reply **f)** to the reviewers, see also the new 2.3.4 Subsection.

2. Both the perturbed SST and SIC simulations with MAR use the same lateral boundary conditions (LBCs). I am concerned that, even with the relatively large (900km) domain, the impact of the local surface conditions on large scale variables such as 500hpa geopotential height will be suppressed by the imposed large scale synoptic situation imposed by the LBCs. Is there some justification for why this would not be the case? Is the distance from the lateral boundary to the ice sheet longer than the horizontal correlation length scale of geopotential height or SLP? Could this explain the lack of any significant circulation change over the upper levels of the ice sheet in Fig. 3a? Is any nudging imposed on the RCM interior?

⇒ Contrary to Day et al. (2013), our study assesses the "direct" impact of oceanic forcing on the GrIS SMB. Therefore, SIC and/or SST feedbacks on the general circulation, referred to as the "indirect" oceanic forcing effect, are not considered since atmospheric boundaries forcing are not allowed to change in our experiments.

3. I think it is important to stress that two rather distinct types of RCM sensitivity experiment are compared in the manuscript:

a) Impact of local forcing: experiments that aim to determine the roll of changes in local surface condition by modifying the surface boundary conditions and leaving the boundary condition unchanged (Hanna et al., 2009, 2014).

b) Impact of global forcing: experiments that aim to determine the roll of changes in global or regional surface conditions by modifying both the surface and lateral boundary conditions (the latter coming from a GCM) (e.g. Day et al., 2013).

An important caveat of 1 is that the impact of any local changes on the general circulation will not have an effect in the RCM. Thus, the impacts of any changes in surface boundary forcing are likely to be underestimated. I think it is important to synthesise the results from both types of experiments, as is done here, but one must keep in mind these non-trivial differences.

⇒ Please see our reply **c)** to both reviewers.

4. In the conclusions: P1464L26: I have an alternative explanation for the apparent discrepancy between Noël et al. and Day et al. (2013). Due to the differences between experiment type 1 & 2 above, the perturbation in Day et al. (2013) is much larger, so I expect that JJA temperature anomalies to be higher, hence more summer melt. Note that the runoff in Day et al. (2013) is calculated from an ITM SMB model, so Vernon et al. is only relevant for precipitation, but not runoff.

⇒ Agreed, we reformulated accordingly to your comment.