

## ***Interactive comment on “Small impact of surrounding oceanic conditions on 2007–2012 Greenland Ice Sheet surface mass balance” by B. Noël et al.***

**Anonymous Referee #1**

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The paper by Noel et al. studies the impact of sea ice concentration and sea surface temperature on the surface mass balance of the Greenland ice sheet. A sensitivity analysis of the outputs of the MAR model is carried out to study the changes induced by replacing current conditions with simulated ones when SIC and SST range between prescribed values. Moreover, the authors provide an analysis of the katabatic winds to support their hypothesis of the minimal impact of the ocean and SIC on recent records of SMB over Greenland.

Overall, the study attempts in analyzing an important aspect of the Arctic system through a well tested and robust tool (the MAR model). Nevertheless, there are many issues that need to be addressed before the paper can be considered for publication.

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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 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). 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. 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.

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line 13 ... the index does not have a ‘phase’ but a value. Please change accordingly  
Line 14 remove ‘the’ before melting and specify that you are referring to ‘surface’ melting  
Line 17 ‘30 % is then’ should be replaced with ‘30 % might be explained’ or ‘can 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

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

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Line 1. 'is known' is a vague expression if not supported by a reference. Please, add. Lines 1-2. What would be the consequence of this? Lines 3 – 4 What does it mean that the forcing might be too weak? And, what is this statement based on? 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. Line 5 is 'significantly' here used in a statistical way? if not, please re-phrase Line 5 what is 'more extreme' forcings?

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Line 21 'small' compared to what? Please address

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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 make this very clear. Line 18 Can you add a reference for the freezing point of salt water?

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

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P 1462 Line 18 'significant' is again here used without a statistical meaning. Please change Line 20 Any suggestion on why for SIC and SST the variation of SMB is linear? It is an amazing  $R^2$  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 Line 21 what are 'combined' effects? 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? 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 model's uncertainty.

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Line 1 Can the authors provide references for this? Line 2 Also, the reference to katabatic winds should be accompanied by more 'historical' references describing the nature of these winds Line 8 What does it mean 'allowing some oceanic influence'? Line 10 the authors here imply the katabatic winds are the 'only' mechanism explaining the impact and should re-word this section. Line 13 'Significantly' again should be replaced Line 19 What do the authors mean in the first part of their sentence? Line 21 'insignificant' should be changed (maybe 'negligible') Line 15 The work by Rennerl-malm should be cited at the beginning as it represents an early work looking into the linkages between sea ice and SMB in Greenland.

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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.

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

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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.

Figure 1. what are the minor ticks on the figures ? What are the units (is it % normalized to 1) ? The numbers on the bars are too small and should be enlarged. What are the units of the figures 1e and 1f ?

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

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Interactive comment on The Cryosphere Discuss., 8, 1453, 2014.