We thank the anonymous referees for their constructive comments. We will provide a revised manuscript addressing as much as possible all the concerns and questions that have been raised. In the following we describe how we will account for the recommendations to improve the clarity and the writing of the manuscript.

Reply to anonymous reviewer 1

This paper examines the response of an atmosphere-only GCM to gradually increasing the elevation of the Laurentide ice sheet. The main conclusions are that increasing elevation shifts the jet southwards, causing a southward shift in precipitation over Europe; and that the albedo and topography of the ice sheet have opposite effects on mass balance over the Barents-Kara (B-K) region. ***The conclusions appear well substantiated by the evidence provided. The paper is similar to Pausata et al 2011, which also examines the separate effects of albedo and topography. It has the added novelty of gradually increasing the topography, but not much use is made of this novelty (see below). As with all such studies, there is the question of model dependence****; but the study does a good job of documenting the behaviour of one particular model and can be of interest to the community. I would therefore recommend publication subject to some revision.*

We would like to precise that, besides gradually increasing the topography of the Laurentide ice sheet, the originality of the paper also relies on ice-sheet model simulations allowing to directly infer the response of the Fennoscandian ice sheet to various forcings of the North American ice sheet. This will be more clearly specified in the revised manuscript.

Major comments:

- The main novelty of the paper is in the gradually increasing topography, but in fact little use is made of this aspect. ***How much would the paper in general (and the conclusions in particular) change if you only examined the noIS, 00dhL and 100 dhL cases? What do we learn from the intermediate cases?*** *If the answer is "not much", then I suggest simply removing most of the figures for the intermediate cases, which will streamline the paper and let you show bigger, clearer figures. Otherwise, introduce new text (particularly in the discussion/conclusions sections) to highlight the new knowledge added by the intermediate cases.*

This is a very good comment and we acknowledge that the way we have presented our results does not highlight the necessity of showing figure panels corresponding to intermediate cases between 00dhL and 100dhL. However, with the exception of some specific climatic fields (e.g. temperature in Fig. 3), the response of the other fields is far from being linear. For example, the North Atlantic jet stream displacement (see Fig. 9) or the Fennoscandian ice sheet response (e.g. accumulation in Fig. 12 or surface elevation in Fig. 13) are not linear w.r.t the height of the imposed Laurentide ice sheet. In the revised manuscript detailed comments will be added to better highlight the behavior of each variable w.r.t. the increase of the LIS topography and to better highlight what can we learn from the intermediate cases. Moreover, as outlined by Reviewer 1, LIS and FIS co-evolved during the last glacial cycle. In this study, we have only considered idealized configurations of the Laurentide ice sheet. Therefore, we believe that making use of intermediate cases is of relevance for the purpose of our paper since this allows to take into account a larger range of configurations and to better assess the response of the Fennoscandian ice sheet within the framework of the last glacial cycle. In the revised manuscript we will pay a particular attention to clarify this and to extend the discussion section on the implications of our simulations and analyses.

- An important conclusion is that ablation rates increase so much over the B-K in the high-LIS cases that they prevent the formation of the FIS. The relevance of this conclusion to the real system is difficult to evaluate, though: the LIS and FIS in fact co-evolved, so the problem of FIS inception in the presence of a full LIS is obviously artificial ***. It's OK as a first step, but the interest of the paper would increase considerably if a new GCM simulation were performed in which the FIS has the elevation computed by the ice-sheet model in the 00dhL run while the LIS has its full elevation. The GCM outputs could then be fed back into the ice-sheet model to test for self consistency; it's possible that the FIS will be maintained in that case.***

This is a good point. The recommended simulation is currently running and the results will be presented in an additional section.

Minor comments:

Sec 3.2: Temperature changes over the B-K are explained exclusively through changes in advection. While this is reasonably convincing in the summer case, when there is a clear north-south temperature gradient across the B-K, but less so in winter, when there seems to be no gradient at all. I can't tell if this is just because the temperature goes off the scale across the whole B-K region in Fig 3 top right – ***if so, then adjust the scale so that the temperature gradient can be appreciated. If there really is no gradient, then you need an alternative explanation for the winter cooling – try looking at cloud radiative forcing.***

The problem comes from the colour scale. This will be corrected in the revised manuscript.

Sec 3.3: Does "precipitation" here refer only to liquid precipitation, or to the total liquid+frozen precipitation?

It refers to total precipitation. This will be clarified.

Sec 5, l20: Seems to me that Lofverstrom et al (2014) attribute warm temperatures over Siberia to the Fennoscandian ice sheet (see their Fig 8), not the Laurentide as claimed here.

Yes, this is right. The sentence will be corrected and the paragraph will be modified accordingly.

Reply to anonymous reviewer 2

General comments: Based on an atmosphere GCM, this manuscript by Beghin and colleagues investigates the role played by the atmospheric changes associated with different Laurentide ice sheet (LIS) configurations on Eurasian climate, especially on Northwestern Europe. Via gradually increasing the LIS heights (similar approach as Zhang et al. 2014 Nature), authors propose that the atmospheric responses over Europe are characterized by seasonal and spatial heterogeneity. The results are interesting but might not be robust enough. In addition, the experimental design possesses weak relationship with real climate. Thus, I would rather recommend a major revision on this stage.

Major comments:

1. Lack of results/comprehensive discussion about potential effects of ocean circulation response on their conclusions. The core results of this study are based on AGCM simulations, in which the sea surface properties (e.g. SST) are fixed to the LGM outputs. ***This approach is able to well evaluate the initial responses of atmosphere circulation to the changed boundary conditions (here is LIS), but cannot provide in-depth information on the real climate (incl. atmosphere-ocean interaction).*** *In the model setup of this study, prescribed LIS changes encompass two extreme cases (e.g. the white and flat LIS and the LGM LIS) and the cases in between. This large spread of LIS heights will significantly affect ocean circulation, for instance, the Atlantic Meridional Overturning Circulation (AMOC) (e.g. Ullman et al 2013 CP, Zhang et al 2014 Nature), potentially leading to different patterns of the temperature and precipitation over Europe in comparison to the fixed ocean boundary****. I would recommend to additionally performing another suit of sensitivity experiment in which a different ocean boundary is used to force the atmosphere. For instance, the ocean boundaries from the fully coupled 00dhL and noIS simulations. If performing additional simulations were not possible, however, the authors would have to carefully discuss this issue in the revised version (which is not at all considered in this version).***

The reviewer is right. Our experimental setup does not allow us to provide an "in-depth information" on the real climate since the feedbacks of the ocean are not accounted for. Moreover, other approximations have been made since the Fennoscandian and Laurentide ice sheets co-evolved throughout the last glacial cycle (see remark below and comment of reviewer 1). This latter point will be addressed in the revised manuscript through an additional atmospheric-ice-sheet simulation. However, as outlined by the reviewer, our aim was to investigate the atmospheric response to changes in boundary conditions. Our approach must be therefore considered as a first-step before including the analysis of more complex processes including feedbacks between the different components of the Earth system. In the revised manuscript we will pay a particular attention to present more clearly our objectives. Nevertheless, we acknowledge that it would have been necessary to discuss in more details the limitations of our approach. Running coupled atmosphere-ocean circulations takes much more time than doing atmosphere-only simulations. It seems to be impossible for many technical reasons to do such simulations in a reasonable time span. However, we will provide an extensive discussion on how accounting for the ocean may change the atmospheric response.

2. The authors show plenty of anomaly fields from different LIS simulations to support their arguments. But without any significance test, it is hard to evaluate whether the contrasts associated with different LIS configurations are robust as well as the proposed mechanisms. ***Thus I would suggest here to include the corresponding t-test at least amongst simulations of noIS, 00dhL, 50dhL and 100dhL. In addition, it would be better to provide the ice sheet mask in all corresponding figures.***

The statistical significance will be indicated for the main figures of the paper. We have already included statistical test in Figure 10 where a boostrapping method has been applied to evaluate the variability of the latitudinal displacement of the North Atlantic jet stream and of the anomaly (xxdhL – nosIS) of winter precipitation. These tests strengthen our conclusions about the relationship between these variables.

3. In the part associated with AGCM outputs, the authors carefully demonstrate the mechanisms accounting for different temperature and precipitation responses over different regions of Europe. From my point of view, there is no flaw on the logic but on the way to clearly present the results. ***As two main factors accounting for the ice sheet mass balance, I would recommend two sections associated with temperature and precipitation in this part, and putting the corresponding mechanisms as the subsections.***

The reviewer is right. We will follow this recommendation in the revised manuscript.

4. The ice sheet modeling part is the most novel part in the whole manuscript. In the present version, the authors only discussed the responses of Fennoscandian ice sheet to the atmosphere circulation changes associated with different LIS configurations. ***How are the responses of LIS per se? For instance, how would the LIS respond to the corresponding atmosphere forcing? Given the co-evolution of both LIS and FIS during glacials, it would also be interesting to evaluate the feedbacks of FIS on LIS mass balance via the atmosphere circulation.***

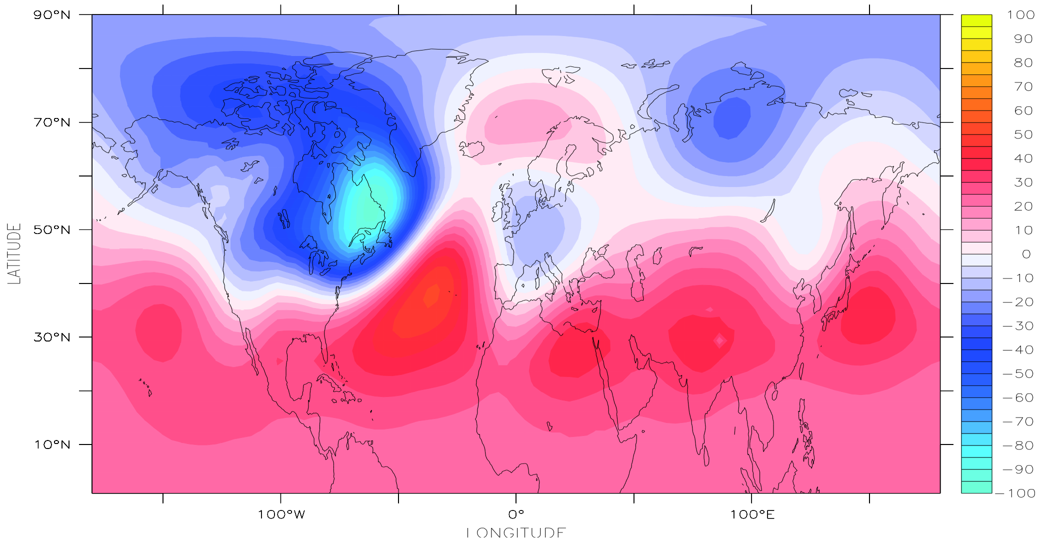
Raising the problem of the co-evolution of the both LIS and FIS is a very good point. In the revised version of the manuscript we will provide information on the response of the simulated Laurentide ice sheet. However to assess the feedbacks of FIS on LIS requires to conduct another set of experiments with varying heights of the Fennoscandian ice sheet. This will be achieved in the near future and will be the scope of another paper.

Minor comments:

P29 Line 19-22: In Ullman et al 2013, it is shown that the tsurf and p-e do not change significantly over Fennoscandian ice sheets under two extreme 21ka ice sheet configurations. ***Can you give a potential interpretation on this point, possibly based on your results?***

The experimental setup of Ullman et al. (2014) (referred to as ULL14) is fully different from our approach. Their objective was to compare the impact on the global climate of two different LGM reconstructions of the Laurentide ice sheet (LIS), namely ICE-5G (Peltier, 2004) and the second one from Licciardi et al. (1998), referred to as LICCI98 in the following. The topography of the other ice sheets was that provided by ICE-5G. The most striking differences between both LIS reconstructions rely on the LIS maximum altitude (4520 m in ICE-5G vs 3560 in LICCI98) and on its shape. In fact, the centre of mass is located over the Keewatin dome in ICE-5G and the ice sheet has a single dome. The LICCI98 reconstruction is characterized by three domes and a centre of mass located eastward compared to ICE-5G. The differences between both LIS reconstructions result in a6 to 9°C cooling in northeastern Asia, Beringia and the North Pacific, but almost no change in surface temperature is observed over the Fennoscandian region, except over the easternmost margin. In the same way, the main changes observed in the P-E climatic fields are far from the ice-sheet area and located in Pacific and southeastern North America. However, the LIS differences induce changes in the patterns of the 500 hPa geopotential height. In the present study, we do not test LIS ice-sheet elevations as high as the ICE-5G one. The highest altitude (i.e used in the 100dhL experiment) is ~3600 m, fully similar to LICCI98, but the centre of mass is rather located over the Keewatin dome, similarly to ICE-5G, although less extended. Moreover, in all of our experiments, the Fennoscandian ice sheet (FIS) has been removed. It has been previously shown that at the LGM the atmospheric circulation and the LGM climate are mainly controlled by the topography of the ice sheets (e.g. Pausata et al. 2011; Cook and Held, 1988). Therefore, the removal of the FIS likely results in a shift of the low and high pressure centres compared to the ULL14 study with ensuing consequences on the simulated climate.

Nevertheless, we tried to analyze more in-depth the differences between ULL14 and the present study. We plotted the difference of the 500 hPa geopotential height (with zonal mean removed) between the 60dhL and the 100dhL experiments (see figure below). This plot can be compared to the ULL14's Figure 2f. Note that contrary to ULL14 we did not mask out the LIS. We obtained high/low pressure patterns over the northern part of the Eurasian continent and the North which are quite similar to those simulated in ULL14 with higher pressures in 60dhL over the northern North Atlantic and northwestern Europe and lower pressures over Siberia, Beringia and North Pacific. However, the locations of the pressure centres are shifted compared to ULL14. In our study the highs are centered over the North Atlantic (against Scandinavia in ULL14) and the lows are centered over Siberia (against the North Pacific in ULL14). ULL14 attributed the cooling simulated in LICCI98 (w.r.t. ICE-5G) over northeastern Asia, Beringia and the North Pacific to the dominant stationary wave patterns, particularly over Siberia and Beringia. Almost no change in surface temperatures appears over the Fennoscandian area, except in the easternmost part. This seems consistent with our findings: in our study, the maximum cooling also occur over the eastern part of the ice sheet. However, the cooling is more pronounced and extends over the entire Fennoscandian area (see Fig. 3). The differences in surface temperature patterns may likely be explained by the westward location of the low pressure centre.



Differences between 60dhL and 100dL experiments for the annually averaged 500 hPa geopotential height with zonal mean removed.

As mentioned above, we have not tested the impact of LIS topographies higher than that of the 100dhL experiment, which is equivalent to the LIS LICCI98 topography. However, the occurrence of a threshold effect linked to the LIS topography is a plausible explanation of the ULL14 results. In other words, as the LIS altitude reaches an upper limit, changes in surface temperatures are no longer observed. This hypothesis is even more conceivable for the P-E climatic field and is, by the way, supported by our own results, at least for the Scandinavian region (see section 3.3 in the first version of the manuscript and Fig. 6). Actually, the positive anomaly of precipitation simulated over Scandinavia is shifted southward as the LIS gets higher, reaching French and Iberian Peninsula Atlantic coasts.

Finally, we cannot exclude a possible influence of the ocean in the ULL14 results. The ULL14 simulations have been conducted with GISS-E2-R Model which is a fully coupled atmosphere-ocean model. Since our aim was to only investigate the atmospheric response to increasing LIS topographies, both SST and sea-ice coverage were prescribed from the IPSL PMIP3 LGM run outputs.

We acknowledge that the differences between our results and ULL14's ones are worth being mentioned. This will be done in the revised manuscript along with the possible explanations at the origin of these different results.

P33 Line 21-22: Please show the 2-d absolute fields of the LGM forcing, as well as 2-d variance fields of the interannual variability.

In the revised manuscript we will provide an additional figure showing the SST and the sea-ice coverage. However, since we do not deal with the interannual variability in the paper, the 2D figure of the variance will only be provided in a more detailed response to the reviewer that will be sent with the revised version of the paper.

P36 Line 11 Does the precipitation in the main text always refer to the total precipition (incl solid and liquid)?

Yes, it does. This will be clarified in the revised manuscript.

P38 Line 28-P39 Line It would be more instructive to show the similar figure as your Figure 10 w.r.t. the southward expansion of the Labrador trough and westerlies positions.

We acknowledge that left panels in Figure 5 are actually quite difficult to interpret. In the revised manuscript, the discussion of the results will be rather based on Figure 11 that provides the same kind of information. We also plan a more specific figure showing a zonal section of 500 hPa winds as a function of the latitude.

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