We would like to thank again the two anonymous reviewers for their constructive comments. The following text is an updated version of the earlier response to reviewers. We have also submitted a revised version of the manuscript addressing as much as possible all the concerns and questions that have been raised. In the following, we describe how the recommendations have been accounted for.

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 has been more clearly specified in the revised manuscript by adding the following sentence in the last paragraph of the Introduction section: "*Besides gradually increasing the altitude of the North American ice sheet, the originality of the present study also relies on ice-sheet model simulations*". We have also included a comparison to other model results in our discussion section.

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, in winter, the response of LMDZ climatic fields is far from being linear. This is clearly visible for the temperature (Fig. 3) the meridional winds (Fig. 5) and the North Atlantic jet stream displacement (Fig. 9 and new Fig. 11), or the snow accumulation over the Eurasian ice sheet area (Fig. 12). In the revised manuscript, comments have been added (see sections 3.1.1, 3.1.2 and 3.2.3) to better highlight the non-linear behavior of these variables with the increasing altitude of the Laurentide ice sheet.

- *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 very important comment. To answer the reviewer's comment, we carried out two additional LMDZ simulations with new boundary conditions. In both simulations, the FIS has the elevation and extent computed in the 00dhL GRISLI run, and the Laurentide ice sheet is either flat (00dhLFIS) or has its full LGM altitude (100dhLFIS). As suspected by reviewer 1, the FIS is maintained in the 100dhL experiment. However, we show that the surface mass balance of the Eurasian ice sheet simulated in the 100dhLFIS GRISLI run is lower than the 00dhLFIS one with a tendency to retreat westward and northward.

The results have been described in a section 4.2 divided in two subsections "Response of the atmosphere" (section 4.2.1) and "Consequences on the simulated Eurasian ice sheets" (section 4.2.2). In section 4.2.1 we first discuss the differences in the simulated climate between two series of experiments (xxdhL and xxdhLFIS) and then we examine impact of the LIS elevation for runs 00dhLFIS and 100dhLFIS. In section 4.2.2, we present the impact of the new ice-sheet boundary conditions on the simulated surface mass balance and the resulting difference in FIS surface elevation difference. As a consequence, additional figures have been also provided (Figs 14 to 19).

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 came from the saturated colour scale. This has been corrected in the revised manuscript and now the Figure 3 top right panel clearly exhibits the meridional temperature gradient.

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

It refers to total precipitation. This has been clarified whenever it was necessary for a better understanding.

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.

In our first response to the reviewers, we claimed that the reviewer was right. However, it was confusing. Actually, in Löfverström et al. (2014), the strong warming induced by the Fennoscandian ice sheet is located over Asia (see their Fig 8b). In western Siberia, warm temperatures are attributed to the Laurentide ice sheet (see their Fig. 8a)

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 has been addressed in the revised manuscript through additional atmospheric-ice-sheet simulations in which the FIS simulated in the 00dhL run is taken into account. However, as outlined by the reviewer, our aim was to investigate the atmospheric response to changes in ice-sheet 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 have paid a particular attention to present more clearly our objectives (see last paragraph in the Introduction section). Running coupled atmosphere-ocean circulations takes much more time than doing atmosphere-only simulations. It turned out to be impossible for many technical reasons to do such simulations in a reasonable time span. However, we have provided an extensive discussion on how accounting for the ocean may change the atmospheric response (see section new section 5.2).

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.***

We would like to stress on the fact that 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. Following, the reviewer's recommendations we have also added two-sided student-t-tests with a p-value = 0.05 for key diagnostics (see Figures 3, 6 and 7). Including t-tests in these figures only slightly changes the different climatic patterns but did not change the main conclusions.

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.***

This has been done 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?***

The main objective of the paper was to investigate how the evolution of the LIS topography impacts the surface mass balance of the FIS through induced changes in atmospheric circulation. This is why we did not discuss the response of the LIS in our first series of experiments (i.e. xxdhL experiments). However, below we present the simulated LIS geometry after 100 000 GRISLI model years. It appears that a full LGM ice sheet favours a largest ice sheet with a higher elevated dome and a southward expansion of the southern margin. An interesting point is that Alaska is entirely ice covered with a flat LIS, whereas with the full LGM LIS, it is only glaciated in the southern coastal part, which is in a better agreement with the geological reconstructions.

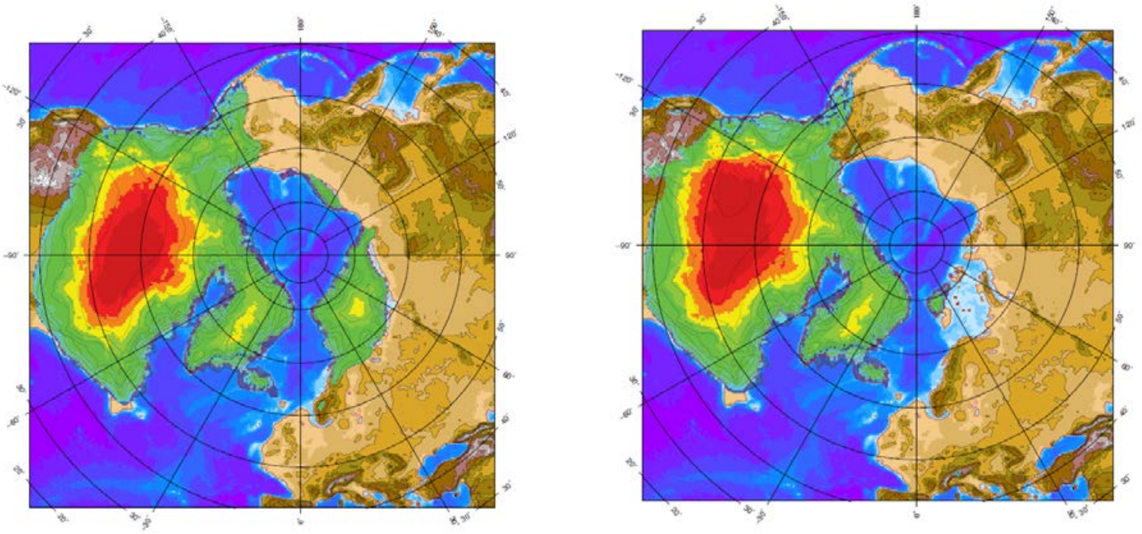

Figure 1: Simulated geometry of the North American ice sheet after 100 000 GRISLI model years in case of a flat LIS (left panel) or a full-LGM LIS (right panel) taken as boundary conditions for the LMDZ climatic simulations.

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 present new simulations in which the FIS has the elevation computed by GRISLI in the 00dhL run and the LIS is either flat or has the full-LMG altitude. This second series of simulations has been carried out to test the self-consistency of our previous results owing to the fact that both North American and Eurasian ice sheets actually co-evolved throughout the last glacial cycle. These results are presented in section 4.2. However, our experimental setup does not allow the feedbacks of FIS on LIS to be investigated. This would require another set of experiments with varying heights of the Fennoscandian ice sheet to be conducted. 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 reside in 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 a 6 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 with a centre of mass rather located over the Keewatin dome, similarly to ICE-5G. A possible explanation at the origin of the differences between ULL14 and the present study may come from the fact that, beyond a certain threshold on the ice-sheet altitude, the simulated climate no longer changes. Moreover, in our earlier response to the reviewer, we also suspected the absence of the Fennoscandian ice sheet to be a likely candidate in explaining the differences between ULL14's findings and ours. Thanks to our second series of experiments (00dhLFIS) and (100dhLFIS) we have been able to test the impact of the presence of the Fennoscandian ice sheet on the large scale atmospheric circulation. It turns out that at the global scale the changes are very weak and cannot explain the differences between both studies. However, the elevation of the FIS considered here is lower than that the ULL14 one. 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.

As outlined in the discussion section, the response of the atmospheric circulation to changes in ice-sheet elevation is highly model-dependent. In the absence of any more in-depth analysis of inter-model differences, we can only hypothesize the origin of the differences between ULL14 and our own results.

We have added a discussion of different model results (Ullman et al. 2014, Löfverström et al. 2014, Zhang et al. 2014, Roberts et al. 2014) to highlight the diversity of these results and the need for further studies to understand this diversity.

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 have provided an additional figure showing the SST and the sea-ice edge. This figure is discussed in section 2.3. However, since we do not deal with the interannual variability in the paper, the 2D figure of the variance (see below) has not been implemented in the revised version of the paper.

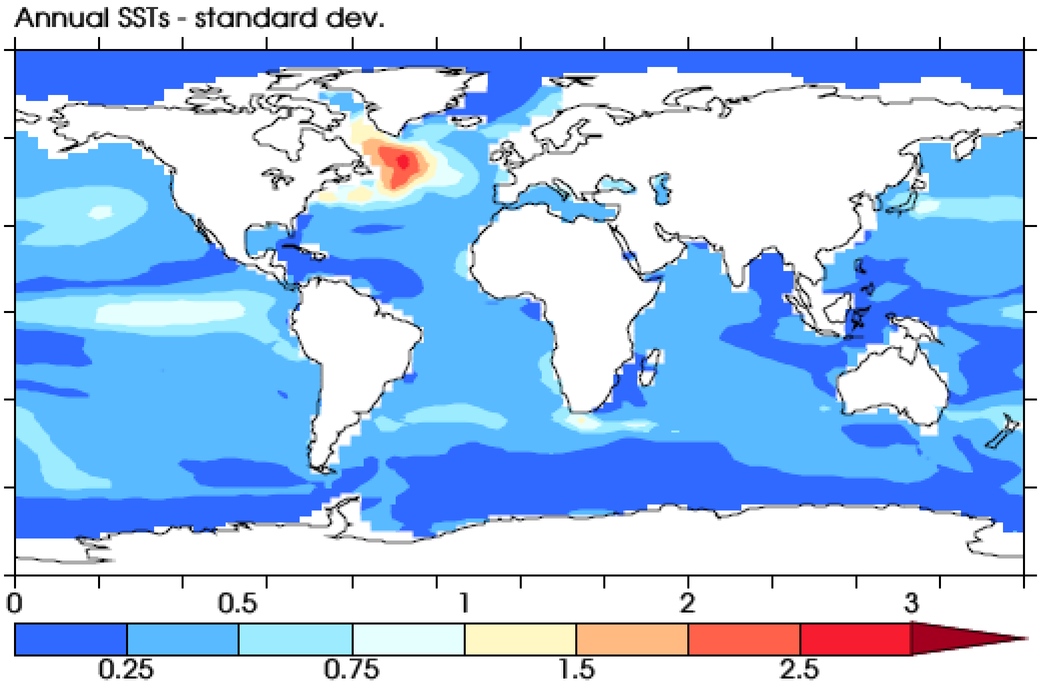

Figure 2: Standard deviation of the mean annual sea surface temperatures computed in LGM PMIP3 runs (Kageyama et al. 2013) for 50 consecutive years.

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

Yes, it does. This is now 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 is rather based on the new Figure 11 showing meridional cross sections for both the East Laurentide sector and the Atlantic sector of the 500hPa geopotential height anomaly (i.e zonal mean removed from the 500hPa height). This figure clearly shows the southward expansion of the Labrador and the Atlantic throughs when the LIS altitude increases along with the southward shift of the 500hPa zonal winds.

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