Response to reviewer #3

Summary:

This paper investigates the potential importance of SST and sea ice for the climate conditions in the North Atlantic and Greenland in the Eemian interglacial. Simulations are conducted with CAM3 and CAM4, comparing the pre-industrial (PI) and Eemian C1 CPD Interactive comment Printer-friendly version Discussion paper climates using prescribed sea-surface conditions from fully coupled simulations (at different resolutions) with CCSM3. The main conclusion is that sea ice in the North Sea region can have a large impact on the Greenland climate and a reduction of its prevalence generates a substantial warming over the ice sheet. The sea ice in the Labrador Sea is important for the local climate conditions but has a little to no impact on the Greenland climate. The authors conclude that the climate impact is mostly mediated by near surface turbulent fluxes that influence the atmospheric circulation and thereby cause a warming over the ice sheet. The paper is generally well written and is suitable for Climate of the Past, though first after a substantial revision.

We thank the referee for the thorough review and the stimulating comments, which will help to further improve the manuscript. Please find the answers to all specific comments below. We have not responded yet to the fully technical corrections (e.g. wording, details in figures etc.), but will do so when preparing a revised manuscript.

Major comments:

1. Model validation and motivation

(i) In all modeling studies it is mandatory to prove that the model is capable of producing a reasonable climate that conforms to observations or proxy data records (climate reconstructions) when studying past climates. This is a first sanity check that tells the reader that it might be worthwhile spending the time end energy reading the paper. This manuscript only contains difference fields and the reader is never shown the actual climatological states. I suggest adding a figure showing a comparison of the pre-industrial (PI) simulation with either a reanalysis product or a reliable climate reconstruction (show full fields and how they differ from observations). For the Eemian you can compare with proxy data where such are available. Though this type of comparison is mandatory, in this study it is extra important since the model seems to be sensitive to the horizontal resolution.

We fully acknowledge that model validation is an important prerequisite. For the climate models used here (CCSM3 and CCSM4) this exercise has already been tackled in several previous studies. The most prominent examples of CCSM3 model evaluation for present-day conditions are Collins et al., 2006 and Yeager et al., 2006 (for the lowRes version). Similarly, the CCSM4 model is validated in Gent et al., 2011, Neale et al. 2010, 2013 (atmospheric component CAM4) and Evans et al., 2013, the latter looking at the atmospheric-land-only setup of CCSM4 specifically. Furthermore, Vizcaino et al. 2013 compare CCSM4 with observations with a focus on the climate in Greenland.

The set of CCSM3 experiments in this study build on simulations which are used in several studies (e.g. Otto-Bliesner et al. 2013, Lunt et al. 2013, Bakker et al. 2013, Varma et al. 201?). In these studies, the fully coupled simulations are assessed with respect to their ability simulating Eemian climate conditions including comparisons with Eemian proxy records.

The CCSM4 simulations generated for this paper are also similar to previous studies using the same model (PI/Eemian) setup focusing on the climate around Greenland (Merz et al., 2013, 2014a, 2014b)- These studies include comparisons with reanalysis data for several aspects of atmospheric circulation, precipitation and snow accumulation in Greenland.

In summary, we want to avoid too much overlap with existing studies and rather be concise in this topic. We acknowledge the importance of model validation and we will therefore add a summary of the above mentioned studies in section 2.

(ii) I would like to see a better motivation of the study. What is the goal (what do we wish to learn) and why are we interested in this particular problem? The current motivation seems to be that fully coupled models simulate different sea-surface temperature (SST) and sea-ice cover (SIC) in the Eemian. This is perhaps not too surprising given the large model spread in simulations of both present and future climates. It would be better to motivate the study from available proxy data records from ice cores as well as terrestrial and marine records. Given the large model spread, what makes this model better than any other model and can we trust the results presented here (connected to the model validation)? You can also extend the motivation by looking at AMOC in different models and connect that to differences in the sea-surface conditions.

Proxy data is one important source of information on past climates and as a consequence about the sensitivity of the climate system itself. Numerical modeling offers a second, complementary approach, which is what we aim to focus on in this study. Thus, one of our main motivations is that current simulations of the Eemian are not able to simulate a warming of 7-8C over north western Greenland (suggested by ice cores). In two previous studies (Merz et al. 2014a,b) we assessed the role of the ice sheet configuration and associated moisture changes. Another model deficiency is that coupled models tend to generate too much sea ice (already in the present day climate simulations). So, the question which is answered in this study is how much of the Greenland warming may be due to the uncertainty arising from SST-SIC distribution around Greenland.

We will make an effort to make this clearer in the abstract and the introduction.

2. Modeling approach

(i) Initially you show that the low and high resolution models yield different results in terms of SST and sea ice in the North Atlantic. It is further mentioned that the low resolution model has known problems and does not simulate a reasonable PI climate in the North Atlantic sector (is this also true for the Eemian?). Despite this claim, the majority of the experiments and figures (according to Table 2) are based on results from the low resolution model. This seems like a very odd choice to me. If the model is biased and has known problems, why base almost all figures and analysis on data from this model? Are there even worse problems associated with the high resolution model? If not, can we expect different conclusions if the same analysis is performed on the high resolution data?

The majority of the experiments and results are based on the CCSM4 simulations which all use the same nominal 1° horizontal resolution and showed good ability in simulating the climate in the North Atlantic and in Greenland (Evans et al., 2013, Merz et al., 2013, 2014a, 2014b, Vizcaino et al., 2013). We just use the SSTs and sea ice from EEM_{lowRes} as the basis for our sensitivity experiments shown in the Sections 5.1-5.3. However, we have conducted the same sea ice shift experiments using $EEM_{highRes}$ as basis (see Section 5.4) and come up with very similar results (e.g., compare EEM1/EEM2 numbers in Table 3&4). We will revise the caption and contents of tables 3 and 4 to make this point clearer. We further consider to revise the lowRes/highRes terminology for the CCSM4 simulations (which all use the same resolution) to avoid confusion of the reader.

(ii) I am generally skeptical to the approach taken in sections 4.1 and 4.2 and I am afraid that we are not learning very much from this exercise. CCSM3 and CCSM4 are highly dependent models (e.g. Knutti et al., 2013) that are part of the same model family, meaning that the atmospheric components (CAM3 and CAM4) share the majority of the same code base. The biggest difference between the models is the deep convection scheme, which plays virtually no role in the latitude range of your focus. Consequently, the comparison of the two atmospheric models is largely redundant as you basically compare results from two simulations with almost the same model using identical forcing protocols. I argue that you can omit this whole comparison and just state that you use SST/SIC from CCSM3 in CAM4 and then prove that the simulated climates are reasonable with respect to reliable data. Also, the near surface temperature is not the best field to use to evaluate differences between AMIP simulations. If the model is capable of producing a realistic climate with realistic turbulent fluxes (e.g. near surface gradients), the near surface temperature is by definition largely similar to skin temperature and you basically prescribe the phenomena that you are investigating.

We are fully aware of the fact that CCSM3 and CCSM4 are similar models as they stem from the same model family. The comparison in Fig. 2 of CCSM3 and CCSM4 simulations is to show the agreement between fully-coupled simulations and atmospheric (-land-only) simulations which use the SSTs/sea ice from the fully-coupled simulations. Hence we see the common/similar model physics in the atmospheric components of CCSM3/CCSM4 rather as an advantage for our study.

The comparison of Eemian proxy data with climate simulations (including the low resolution and high resolution CCSM3) has already been done in various studies (Lunt et al., 2013, Otto-Bliesner et al., 2013, Capron et al., 2014. Hence, we don't want to repeat this comparison as we feel that it will lengthen the study without adding too much novel information. We will make an effort to better summarize the results of the aforementioned references (specifically with respect of the CCSM3 simulations) in the revised version of the manuscript.

Lastly, we totally agree that over ocean/sea ice points the surface temperature is largely determined by the surface heat fluxes and thus a respective signal in SSTs/sea ice concentration directly translates in a corresponding surface temperature signal. This is clearly no surprise to the reasons you mentioned. However, we are also interested in temperatures over land (in particular in Greenland) e.g., as displayed in Fig. 2. For the land points the influence of SST/sea ice changes is not as straightforward as for ocean points and hence worth a closer look (what is done in this study). The key message from Fig. 2 is that the warming patterns in panels e) and f) (i.e. EEM-PI_{diff} for CCSM3 fully-coupled vs. CCSM4 atmosphere only) do not only agree over ocean but also to some degree over land points.

(iii) A large part of the analysis is based on differences between difference fields (EEMPIdiff). These results are almost impossible to wrap ones head around and I wonder what we can learn from such a comparison, especially since the low resolution model has known biases. Also, it would help the interpretation of the results if you used the same color scale in all figures showing the same/similar quantities.

Since the analysis is based entirely on the nominal 1° CCSM4, where some simulations use boundary conditions of an earlier coarser resolution CCSM3 simulation, our results are minimally affected from model or resolution biases. All new simulations that we carried out for this study are classical sensitivity simulations where a single aspect of the model setup is changed at the time to isolate different influences and processes. So, the difference EEM-PI denotes the sensitivity of a single model to the two respective sets of boundary conditions, lowRes or highRes. This is the familiar concept of a climate sensitivity, albeit in this case not related to CO_2. The quantity EEM-PI_{diff} is the difference between these two sensitivities. We will revise the text of the manuscript to clarify this term.

For a given variable the color scale is consistent except for Fig. 7 where the SAT range (which is from -5 to 5 C in the other Figures) is from -3 to 12 C. We feel that adapting the range of Fig. 7 to -5 to 5 C would be unfavorable to display the effect of the warming induced by the shift experiments. We consider adding a respective note in the caption of Fig. 7 to make it clear to the reader that the color scale of Fig. 7 differs from the one in other figures.

(iv) My main concern has to do with the sea-ice retreat experiments. First of all, the amount by which you shift the sea ice seems to be arbitrarily chosen and should be motivated.

We will motivate the design of our sensitivity experiments better in the revised manuscript. The character (direction and magnitude) of the shift was chosen to resemble the EEM-PI_{diff} sea ice anomaly in the respective region (compare Figs. 4 and 5).

We also tried different magnitudes of the shift (not shown in the manuscript) which give results in agreement with what is shown in the current manuscript. We consider mentioning these additional experiments in the revised manuscript.

Second, I am not convinced that these perturbation experiments are designed in a way that they will teach us anything useful about the last interglacial climate. In steady state (no drift due to external forcing) the circulation in atmosphere and ocean is by definition what determines the sea-surface conditions; the SST/SIC is essentially determined by the internal heat flux (Qflux) in the ocean mixed layer and the balance between radiative and turbulent surface fluxes in the atmosphere (SST ~ SWnet –LWnet – LHflux – SHflux – Qflux). When you prescribe the sea-surface conditions and introduce local changes in the SST/SIC, you also introduce a local climate forcing that could never happen in the real world as it is not supported by the rest of the climate system (the open water that is introduced is not consistent with the general circulation).

We agree that any change to the coupled system will result in an imbalance and thus potentially invalidate the new solution. However, in the sea ice shift experiments, this only applies to the ocean circulation. The atmospheric circulation will adjust to the prescribed SST/SIC. Several analyses in the manuscript ensure that (a) our atmosphere-only simulations are not in contradiction with the physically consistent coupled simulations that the boundary conditions were taken from, and (b) that the unavoidable inconsistencies in the ocean implied by the shift in sea ice do not invalidate the basic finding and the benefit of these idealized simulations. We will revise the text to make these points clearer.

If we assume that the sea-ice cover in the Labrador Sea collapsed (for whatever reason), the climate system would do everything it can to rebuild the sea ice over the next few seasons (as is evident from the almost 100W/m2 imbalance in sensible and latent heat fluxes that are reported in the analysis). If we instead assume that we could collapse the Labrador Sea ice and keep the region ice free, the rest of the ocean circulation (and atmospheric circulation for that matter) would have to be different to sustain the reduced sea ice; i.e. there would be changes in the SST field elsewhere and the turbulent fluxes would almost certainly be lower as sea-ice otherwise would form. I know that the chosen modeling approach is not new and that other people have done similar experiments before you (e.g. Deser et al., 2010), but I am concerned that this modeling approach does more damage than good in this particular study. I don’t have a patented solution to the problem but I argue that it would be better to run a slab ocean model and alter the internal heat flux convergence in the mixed layer (in a conservative way so it doesn’t introduce a global climate forcing) so that the sea ice retreats from the desired regions. This is arguably a better solution as the surface temperature and sea-ice margin are determined by the surface energy balance, which means that it is theoretically possible to construct a climate where there is no sea ice in the desired regions but you have sea-surface conditions that are in balance with the circulation and external forcing. Whether or not this climate state is realistic is of course another question.

We agree that the sea ice shift experiments partly break the physical consistency of the coupled system. This is a general and irremediable aspect of atmosphere-only simulations. However, it also represents an opportunity to investigate the impact of changes when applied within physically reasonable limits. In this context, we argue that the northwestward shifted sea ice edge does not surpass these limits, because it is not generally inconsistent with possible states of the ocean circulation as shown by the fully-coupled CCSM3 simulations. The LabS-shift and NordS-shift experiments are designed to resemble this coupled simulation regionally in order to disentangle the effects of one ocean basin versus the other. A slab ocean model is not suited here because it only includes meridional heat transport in the ocean. In the case of the Labrador Sea, the zonal ocean heat transport is very important.

To illustrate that distinct surface heat fluxes are not only an artifact of atmospheric-only simulations but also possible in CCSM4 fully-coupled simulations we show here sensible and latent heat flux anomalies for a LGM compared to a PI simulation (Fig. R1) (diagnostics provided by the NCAR: http://www.cesm.ucar.edu/experiments/cesm1.0/diagnostics/) Note that such diagnostics are not available for an Eemian simulation. Similar to our atmospheric-only CCSM4 simulations, the heat flux anomalies from the fully-coupled model shown below are the result of distinct changes in SSTs and sea ice (see also Fig. R2) originally caused by changes in external forcing (here LGM vs. PI, in our manuscript EEM vs. PI). More precisely in the LGM the sea ice strongly increased in the Labrador Sea and the Norwegian Sea (Fig. R2) leading to distinct negative heat fluxes in these regions. At the same time adjacent areas in the North Atlantic show distinct positive heat flux anomalies building the dipole structures alike the ones found in our atmospheric simulations.

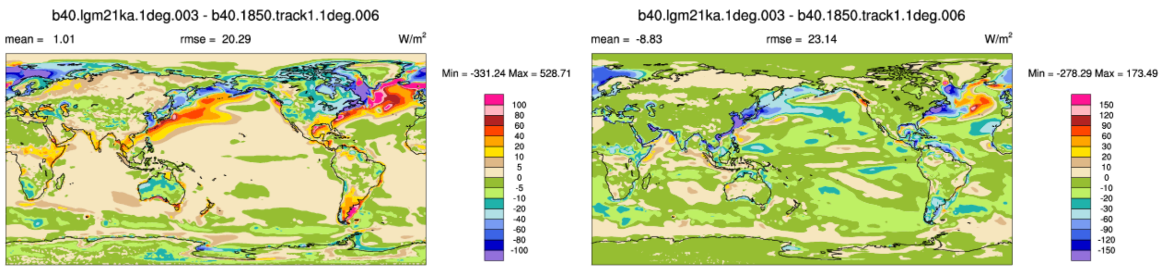

Fig. R1: Winter mean (DJF) LGM minus PI change in (left) sensible heat fluxes (W/m^2) and (right) latent heat fluxes (W/m^2) based on CCSM4 fully-coupled simulations.

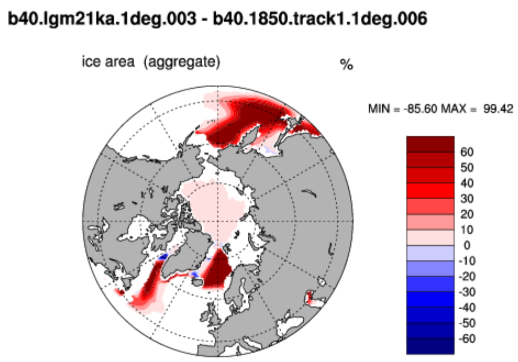

Fig. R2: Winter mean (DJF) LGM minus PI change in (left) sea ice concentration (%) based on the same CCSM4 fully-coupled simulations as used for Fig. R1.

3. Interpretation of results

(i) Following the previous comment, it is not at all surprising that you get very strong turbulent fluxes in the sea-ice sensitivity experiments. The prescribed SST/SIC implies that the climatological atmospheric circulation is more or less determined by the prevailing sea-surface conditions. When making local changes to the SIC and prescribe SSTs that are not consistent with the circulation, you introduce regions where the climate “wants” to have sea ice, as cold air is advected over open water, but the prescribed sea-surface conditions prevents it from forming. This gives rise to artificial vertical gradients and turbulent fluxes that would never happen in nature as the SST/SIC would respond and go back to an ice covered state. This in turn induces and anomalous atmospheric circulation that has no real world analogue, at least not in a climatological state which is what is investigated here.

As detailed in our reply to the above comments, we understand this concern and we agree that physical inconsistencies due to the *uncoupling* of the formerly consistent ocean-atmosphere system requires great care and a critical discussion of what can be concluded from such idealized numerical experiments. We regret that the current manuscript falls short in this regard and we will revise the text to avoid future misunderstandings. However, we also feel that the comment above is too general in its criticism. Regions where very cold continental air meets open ocean surfaces do exist in the real world. They are not always a model artifact. Specifically, in the Labrador Sea, it is important to note that the coupled version of CCSM4 does simulate open waters here (e.g., Jahn et al., 2012) that produce a strong air-sea heat flux. CCSM4 uses the same atmosphere model as our atmosphere-only simulations, CAM4, which illustrates that a situation similar to our idealized LabS-shift experiment is not physically impossible in this model.

(ii) In my mind, one of the most interesting results in the whole paper is the changes in the lower tropospheric wind field (Fig. 9) that results from manipulating the local SST/SIC in the North Sea. However, no explanation is provided as to why the wind field changes the way it does. I want to see a dynamic argument made for the somewhat counterintuitive response where the lower tropospheric winds impinge on Greenland from seemingly the wrong direction; SE instead of NE where the forcing is located.

We agree with the referee that this is a interesting result worth a closer look. We will report on the results of an extended analysis of this issue when submitting the revised manuscript.

Line-by-line comments:

Page 1, line 1: I would be careful suggesting that the Eemian is a possible analog to the climate in the near future. The Eemian was warm primarily as a result of increased insolation whereas future climates are warm because of higher greenhouse gas concentrations. The former only plays a direct role during parts of the year (in high latitudes) whereas the latter influence the longwave radiation in all seasons.

We are fully aware of the diverse causes and impacts between the Eemian and the current/future warming. Still, the Eemian period remains a valuable test bed period for studying the dynamics of the high-latitude climate system for atmospheric/oceanic conditions warmer than present. We will revise the statement to make it clearer.

Page 1, line 19: This time interval contains both warm and cold phases.

We changed the definition of the last interglacial (Eemian) to 129-116 ka which corresponds to IPCC AR5. The last interglacial is clearly known as warm period although the time interval may include also parts of the transition phases with the preceding/following glacial period. Defining the exact interval of any glacial/interglacial period is a challenge on its own.

Page 4, lines 19-25: This is more of a curious comment than anything else but when you regrid the T31 SST/SIC to the T85 grid, you implicitly introduce an outline of the T31 grid but at the higher resolution. Do you have a feeling for if this will influence the results?

We are a bit uncertain what the referee exactly means here. We extrapolate the SSTs from both types of CCM3 simulations, i.e. 3deg grid (T31x3 simulation) and 1deg (T85x1deg), across all land points and regrid it to the 0.9x1.25 resolution, so there is no "outline" of the original 3deg/1deg land mask.

Page 5, line 24: How does the absence of inter-annual variability in the SST/SIC degrade the representation of the storm track? Add a sentence explaining that.

The study Raible and Blender (2004) shows that the Pacific storm track is shifted north in the absence of inter-annual SST/SIC variability (mainly due to the missing ENSO variability. In the Atlantic there are also changes, in particular more storms move zonally and less to the Northeast. Please note that using a mixed ocean model instead lead to similar behavior of the storm track as for simulations with no inter-annual SST/SIC variability.

Page 6, line 7: The -1.8_C temperature is only used for the SSTs underneath sea ice. The actual temperature of the sea ice is determined by the local surface energy balance, which is generally much lower. It is therefore a bit misleading to use the SST as a measure of the surface temperature and I suggest showing the actual surface temperature instead.

Your comment is completely valid for areas with partial sea ice coverage in terms of that the atmosphere is feeling the surface temperature of the ice according to the local surface energy balance (calculated by the thermodynamic module of the sea ice model CICE). Nevertheless, we prefer to show SSTs in Fig. 1,3,5,6 as it can be shown for both partially ice-free and fully ice-free regions rather than showing the ice temperature for the small areas with partial sea ice coverage what would complicate the illustrations. The SSTs further provide information about how much energy from the surface ocean is available for the atmosphere.

Page 7, end of section 4.1: Determine whether the difference in temperature signal is due to the PI, Eemian or both climate states when going to the lower resolution.

The temperature signal assigned to $EEM\text{-}PI_{diff}$ is by definition a combination of both climate states. The positive $EEM\text{-}PI_{diff}$ temperature signal tells us that the difference between the absolute temperatures in EEM_{lowRes} and $EEM_{highRes}$ is larger than the difference between the absolute temperatures in PI_{lowRes} and $PI_{highRes}$.

Page 8, line 1: What is the relationship between the SST and the sub-polar gyre?

The circulation of the subpolar gyre influences SSTs in several ways. Firstly, a stronger gyre results in a stronger Irminger Current that transports heat and salt south of Iceland in a westward direction. While this causes a weak direct warming, the salt transport is more important. The enhanced influx of saline waters into the relatively fresh Labrador Sea strengthens deep convection in this region. Since subsurface waters are warmer than the strongly cooled surface waters in this region, this second effect also results in a warming. Lastly, in broader terms, the gyre heat transport dominates over the overturning heat transport in the subpolar latitudes of the North Atlantic. Thus, a stronger subpolar gyre transports more heat northward across the entire width of the ocean basin.

We will revise the manuscript to clarify this statement without distracting from the main focus by adding too much information.

Page 8, line 11: Show the PI SST, it is important for the story!

We are considering including a Figure with the PI SSTs although we think that it is not of too crucial importance as we are mostly discussing EEM-PI changes rather than absolute EEM or PI values. The absolute PI SSTs currently are only mentioned in one single sentence.

Page 8, line 18: particularly strong on SAT above oceanic grid cells... Don't you use identical SST/SIC in CAM3 and CAM4? If so, you expect to see very similar SAT as it represents the temperature just above the ocean surface.

Yes, the SSTs in the CCSM3 simulations (fully-coupled) and the CCSM4 simulations (atmosphere-land-only) are identical and hence we expect very high similarity. Any difference is related to differences in the CAM3 and CAM4 physics/numerics and due to the fact the CCSM3 highRes/lowRes simulations slightly vary with respect to external forcing. This has been pointed out by Reviewer 2 and will be clarified in the revised manuscript.

In contrast, the CCSM4 highRes/lowRes simulations use identical setups (e.g., external forcing) apart from the SST and sea ice fields prescribed as lower boundary conditions and hence are more reasonable test beds to retrieve the role of SSTs and sea ice for the EEM-PI SAT warming pattern.

Page 8, line 19: How much is the winter insolation decreased in winter?

The decrease in insolation depends on the latitude. Please refer to Fig. 1 in Lunt et al., 2013 for specific values. We will add a respective statement to the manuscript.

Page 9: What can we possibly learn from ($\Delta 1$ - $\Delta 2$) when at least one of the Δ#s have known biases?

Even though both models (1 and 2) might have biases in terms of absolute values, we can investigate the EEM-PI changes in both models and study the influence between different fields (SSTs, sea ice, temperature, precipitation etc.) as these changes base on the physical principles employed in the climate model. Assuming that the physics in the model are correct, we can learn about the importance of single processes and their interactions with other components in the complex climate system.

Page 9, line 11: Which terms does Qnet contain? Radiative fluxes? Turbulent fluxes? Internal heat sources in the ocean? A combination of all or a subset of the above?

Qnet refers to the atmospheric energy balance and is defined as follows:
Qnet =LH+SH+LWnet.

We omit SWnet in the definition due to reason stated in the manuscript (page 9, lines 17-20):

[Note that we omit the shortwave component in the calculation of Qnet (shown in Fig. 5b) because increased downward shortwave radiation resulting from modifications in surface albedo (e.g., by changing an ocean grid cell from ice-covered to ice-free) does not warm the atmosphere directly but warms the ocean, an effect that is suppressed in our experimental setup where SSTs are prescribed.]

We will further add the definition to the manuscript in the revised version.

Page 9, lines 21-29: You have prescribed SST, which means that you easily get artificial turbulent surface fluxes as the ocean temperature acts as an infinite source and sink of energy (sign depends on atmospheric conditions).

In all atmospheric simulations with prescribed SSTs, the SSTs are static and hence the atmosphere finds its own equilibrium given the regional heat input by the ocean surface. In agreement with your comment and as described in the manuscript (page 9, lines 21-29), the surface heat flux response to an initial SST change are therefore stronger than in a fully-coupled simulation run in its equilibrium. However, keep in mind that the purpose of the atmospheric CCSM4 simulation is to mimic the sea ice, SST changes (and consequently also the resulting surface energy flux changes) found in the fully-coupled CCSM3 simulations (Fig. 5). As the SST and sea ice anomalies stem from fully-coupled CCSM3 simulations run in their equilibriums, these anomalies are based on physical mechanisms.

We further feel that the physical inconsistency in the atmospheric-only simulations is a small price to pay for the flexibility to investigate a specific detail of the coupled system. Note also that our CCSM4 atmosphere-only simulations are run for only 30 years so that an energy imbalance from a small region does not integrate over very long distances as the ocean's response times (to adjust all ocean circulations) is much slower.

Please also refer to our responses to your major comments.

Page 10: Why do you use the low resolution model when it has known biases?

We use the atmosphere-land-only CCSM4 model which has a nominal 1° resolution for all sea ice experiments and prescribe SSTs/sea ice from both the low resolution and the high resolution model as input (see Chapter 5.4 for an overview of all simulations). However, as we are mostly interested in changes between two simulations, the absolute nature of the SSTs/sea ice input fields is not of importance. Note that the sea ice shift simulations have been repeated starting from the unperturbed conditions of the high resolution coupled CCSM3 simulation (described in Section 5.4). The results and conclusions from these additional simulations are virtually identical with the shift-experiments starting from the CCSM3 low resolution SSTs/sea ice.

Page 10: Fixed SST is almost certainly the source of the strong turbulent fluxes that are highly artificial as they would never happen in nature in the way described in the manuscript, at least not over a long period of time.

The idealized SST and sea ice fields are artificial but they do resemble the regional conditions in the EEM-PI CCSM3 highres coupled simulation and therefore are not fundamentally at odds with a physically consistent system. Note that distinct surface heat fluxes are also found in observations and fully-coupled simulations (Bates et al., 2012)

Please also note our reply to similar comments above.

Page 11, lines 1-3: Why does the warming spread over Greenland? Comment on changes in atmospheric circulation.

The role of changes in the atmospheric circulation is discussed in Chapter 5.1. It is shown that in the NordS-shift experiment the Greenland anti-cyclone is weakened allowing warm air from the Nordic Seas to be advected towards Greenland's interior. In contrast, for PI conditions as well as in the LabS-shift experiment the Greenland anticyclone is stronger and fosters the cold isolated climate in Greenland.

Page 11, line 8-10: Eq. 1 is written in advective form, not flux form. The terms you refer to are therefore showing temperature advection and not heat flux convergence.

We agree with the reviewer and will change to 'horizontal and vertical temperature advection.'

Page 11, lines 16-19: Are you talking about month to month variability or the climatology? The terms have to be identically equal to zero in the latter if the model is in balance.

See next answer

Page 11, line 20: The temperature tendency has to be identically zero for the model to be in balance. You are looking at a climatology after all, or...?

Yes we are looking at climatology. As the model is never to 100% in balance, the temperature tendency is only almost zero.

Page 12, line 6: How much is actually resolved at T31?

Note that all CCSM4 simulations (for which the heat budget calculation is applied) have 0.9x1.25 (not T31) resolution which corresponds to a grid space in Greenland of ca. 50km. Only the two lowRes CCSM3 simulations have T31 resolution.

Page 12, line 13: How does that hang together with the enormous increase in LH flux? I would expect to see a great moistening of the atmosphere when the LH flux increases that much, which in turn increases the cloudiness.

The moisture released by the positive latent heat flux anomaly is constantly transported away by enhanced moisture advection (see Fig. 10). Hence, the increase in atmospheric humidity above the moisture source region is limited as is the increase in cloudiness.

Page 12, line 33-Page 13, line 9: This paragraph is very confusing because you first talk about what you expect to see and then you show that the expected circulation is in fact not true.

We agree that this passage in confusing and it will be revised.

Page 12: What happens to mid- and upper tropospheric winds in these experiments?

The winds in the mid- and upper troposphere show no significant changes.

Page 13, line 20: I don't see a southeastward transport in the figure.

Will be changed to "eastward".

Page 13, lines 20-23: Is this also true in these experiments? Have you done the proper analysis or is it just a conjecture?

We haven't performed a cyclone analysis, which is beyond the scope here, but it is well-known from the literature.

Page 14, lines 15-19: This is the heart of my concern. Everything in the climate system acts to build sea ice where it has been removed but the prescribed SST/SIC don't allow the sea ice to regrow. Since the summer temperature is higher, there will not be any regrowth in the summer season and you don't see equally outrageous turbulent fluxes.

Please see our responses to your main comments on our thoughts why we feel that the sensitivity experiments are still valid.

Page 14, lines 27-34: This is not very surprising either. There is a prevailing southwesterly flow over the northeastern Atlantic, meaning that warm and moist air is advected over the region where you remove the sea ice. There is thus a smaller "urge" for the climate system to regrow sea ice there and you don't see equally large turbulent fluxes.

Figures 4 and 5 show that in both regions (LabS and NordS) removing sea ice leads to distinct winter heat flux anomalies as in both regions cold air is exposed to a relatively warm sea surface. We agree with the reviewer that the winter temperatures in the Labrador Sea are even colder than over large parts of the North Atlantic due to the local advection of cold air from the American continent in contrast to warmer air masses advecting eastward across the Atlantic. Nevertheless, the different magnitudes (in LabS vs. NordS) in winter heat flux anomalies shown in Fig. 11 mostly relate to the chosen boxes across we calculate the averages plotted in Fig. 11. As stated in the manuscript (page 14, lines 29pp) negative heat flux anomalies stemming from the dipole effect are included in the NordS box but not in the LabS box.

Page 18, line 9-10: Have you adjusted the Greenland elevation in these simulations?

Greenland is set to present-day conditions in all experiments presented here. Please refer to Merz et al. 2014a,b for results of simulations with a modified Eemian Greenland ice sheet.

Page 18, line 15: A 3.1_C temperature difference could in principle be due to a lowering of the ice sheet. Since the sea level was quite a bit higher in the Eemian, this is not a bad first guess that could be explored in a greater detail in the manuscript.

Please refer to the extensive analysis of Merz et al. 2014a referenced here.

Page 18, lines 29-34: This section is a bit speculative. Maybe you can extend the discussion to include the importance of precipitation seasonality and the temperature inversion relationship recently discussed by Pausata and Löfverström (2015).

We will consider including the paper in the discussion. Please note that the statements in our manuscript refer to the study by Sime et al. (2013).

Page 19, lines 20-23: You haven't really shown or discussed any proper atmospheric dynamics in this paper. The main focus is on the turbulent fluxes that no doubt will influence the atmospheric circulation. This has not been shown properly though so this statement is merely a conjecture.

We don't feel that is a valid comment as large parts of Section 5.1. are dedicated to changes in atmospheric dynamics.

Figure 2: Validate the model by showing full fields as well as a climate reconstruction.

As stated in our response to your main comment #1 we feel that a lengthy analysis of the full fields and a comparison with climate reconstructions is beyond the scope of this study and has already been done in earlier studies. We will make an effort to better discuss the results of these studies in our manuscript.

Figure 3: The large sensitivity of SIC to the model resolution is curious. Is there any proxy data you can compare this with?

To our knowledge, there is no sea ice proxy available for that period which could be used to judge about either Eemian sea ice mask produced by the two model versions.

Figure 3: What is the purpose of this figure when Fig. 4 shows almost exactly the same thing, though extended to show the response over land as well?

Figure 3 shows sea surface temperature (SST) and sea ice concentration (SIC) whereas Fig. 4 is showing surface air temperature (SAT), so they are not showing the same fields. It is worth showing both the SSTs/SICs (i.e. here used as a forcing as they are prescribed) and the SATs (i.e. the temperature response in the low-level atmosphere). The comparison shows how strongly the atmospheric temperature response is related to changes in SSTs and sea ice (not only over ocean points but also over land)

Figure 10: I am curious as to why there are such large differences in e.g. the Norwegian Sea and southwestern Greenland?

We are not sure what differences the referee refers to but if he/she thinks of the differences between Fig.10c,d it is likely that the our calculation of the moisture fluxes (through finite differences) is not able to fully close the moisture budget diagnosed by P-E.

Additional references used in response (and not yet included in manuscript)

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Vizcaino, M., Lipscomb, W. H., Sacks, W. J., van Angelen, J. H., Wouters, B., and van den Broeke, M. R.: Greenland Surface Mass Balance as Simulated by the Community Earth System Model. Part I: Model Evaluation and 1850-2005 Results, Journal of Climate, 26, 7793–7812, 2013