The authors thank Anonymous Referee #1 for his/her considered comments.

Reviewer's comments are provided in italics.

Lack of explanations : The authors provide an explanation for the suppressed variability of the western side of the jet stream (though I personally think that one key aspect is missing - see comment below). However, the more complicated part of the story, explaining the jet characteristics over the eastern ocean basin, is not discussed at the same level of detail. I think this is a shame and I encourage the authors to do more analysis to improve that part of the story.

We agree with this reviewer that it is important to disentangle the various possible contributors to deglacial jet changes on the eastern side of the North Atlantic (NAtl). However, this can't be done to the same level as the western jet with the existing set of ensemble runs. The potential contributors that we have considered to the eastern jet changes include:

- Stationary and transient eddy responses to ice sheet topography changes,
- Thermal effects on latitudinal surface temperature gradients in the North Atlantic (and possibly sea ice) due to ice sheet area and height (over North America and Fennoscandia), greenhouse gas concentrations and orbital forcing,
- Indirect effects associated with the pinning of the western side of the jet to a particular latitude, and
- Indirect effects associated with ocean circulation responses to the boundary condition changes.

We will add to the revised text the following conclusions that we can draw based on our existing runs.

1. The presence of elevated and extensive ice sheets provides the dominant controls on the jet characteristics on both sides of the NAtl jet. When the ice sheets are fixed to their LGM configurations in FixedGlac, little change is seen in the position or distribution of the jet on both its western and eastern sides. One notable exception to this is after 4ka BP, when the eastern side of the jet shows a reduced frequency at its preferred latitude and an expansion of its range. This timing is coincident with abrupt warming events in the FixedGlac experiments that are accompanied with abrupt retreats in Northern Hemisphere sea ice extent and warmings of NAtl sea surface temperatures.
2. The eastern side of the NAtl jet is more sensitive to the background climate state than the western side of the jet is. This is evident when examining the differences between the FullyTrans and FixedOrbGHG experiments. By present day, the western side of the jet is mostly unaffected by the orbital and GHG components being fixed to LGM values. In contrast, the eastern side of the jet is centred approximately 5° further south when orbital and greenhouse gas components are fixed to LGM.
3. The position of the western side of the jet has limited control on the position of the eastern side of the jet. This conclusion is arrived at by examining the PDTopo experiment, where the position of the western side of the jet is approximately 8° further north than in the FullyTrans experiments before 20ka BP. In these same experiments at that time, the preferred eastern position of the NAtl jet is shifted approximately 3° further north.
4. The ocean state has an effect on the position of the jet on the eastern side of the NAtl. The differences in the eastern position of the jet between FullyTrans and PDTopo from 4ka BP onward indicate that although their forcings are the same or very similar, the history of the simulation affects the position of the jet on the eastern side of the NAtl. Such a difference is not apparent on the western side of the jet.

What is not clear from the simulations presented here is the role of Eurasian ice topography and extent (ie versus that of the NAIS) and the relative role of background temperature versus temperature gradients. Given the length and content of the current paper, these last questions will be answered in a future study.

Lack of dedicated discussion section:The authors have decided to jump straight from the results section to the conclusions without having a proper discussion section where your findings are put in perspective with the existing literature. This makes it hard to get a sense for how your results differ from earlier studies and how they contribute to our understanding of the atmospheric circulation during the deglaciation. The discussion section is in my mind the most important part of a paper, so its omission feels instinctively wrong and may give this paper less traction than it deserves.
We have changed the Results and Discussion section to Results and changed Conclusions to Discussion and Conclusions. In the Discussion and Conclusions, we have provided much more context for our results in light of existing literature and discussed its implications.

For example, "All of the fully-transient deglacial simulations presented here show that the NAtl eddy-driven jet shifted northward from the Last Glacial Maximum to the preindustrial period, and its latitudinal variability increased. These characteristics match those derived from other studies (Li and Battisti, 2008; Lofverstrom et al., 2014; Merz et al., 2015). However, unlike those studies, neither the PlaSim simulations nor TraCE-21 show much change in jet tilt between these two periods."

"The novelty of this present study compared to those previously mentioned is that it analyses transient changes in the jet over the entire deglaciation in multiple experiments. Additionally, instead of characterizing the jet via the Gaussian statistics of its mean and standard deviation, we present the changes with time of the jet distribution itself."

Page 1, line 5: Remove "we performed" and specify that PlaSim is an intermediate complexity model.
We have made the description more precise. Revised text is "This study characterises deglacial winter wind changes over the North Atlantic (NAtl) in a suite of transient deglacial simulations using PlaSim, an intermediate-resolution earth system model with simplified physical parametrizations, and the TraCE-21ka simulation."

Page 2, line 17: Missing space between sentences
There is a space in the latex source file. It is not being rendered in this case, due to the number of characters on the line.

Page 3, line 12: "Low levels of the atmosphere" is ambiguous. Perhaps better to say "lower troposphere".
Done. Revised text is "Of the two dominant features of mid-latitude atmospheric circulation patterns in the NAtl, the subtropical and eddy-driven jets, the eddy-driven jet has the largest presence in the lower troposphere."

Page 3, line 13: Missing word. The extratropical jet stream is due to "momentum-flux convergence" by
synoptic eddies.
Done. Revised text is "The eddy-driven jet (or polar front jet or jet stream) is a narrow band of fast, westerly winds that arises from the momentum-flux convergence of atmospheric synoptic-scale eddies (e.g. extratropical cyclones with lifespans of days) (Lee and Kim, 2003; Barnes and Hartmann, 2011)."

Page 4, line 11: Lofverstrom et al. (2016) showed that stationary waves can influence the jet

characteristics in the eastern North Atlantic at the LGM as well.
Revised text is "However, the influences on the western and eastern sides of the NAtl have been found to differ in atmospheric simulations: the position of the western side of the jet is more affected by ice sheet orography, and the position of the eastern side of the jet is primarily influenced by stationary and/or transient eddies (Kageyama and Valdes, 2000; Lofvestrom et al., 2016)."

Page 4, line 23: Sentence starting with "These changes.." is incorrect. To the best of my knowledge, none of the papers cited here suggest that the subtropical and midlatitude jets entered a merged state over the N Atlantic at the LGM; see, e.g., Fig. 1 in Li and Battisti (2008) where there is a clear separation between the subtropical and eddy driven jets. Page 4, lines 26-28: None of the papers cited here investigated that explicitly.
We see that the citation here was misleading in that it appears to attribute the identification of merged jets to those papers, rather than just the data from which we concluded the change in distribution of heat and moisture transports. We will remove the citation altogether and make it clear that these are the authors' inferences based on those papers.

Nevertheless, we would argue that the separation between the jets at LGM is not as clear in all cases as the reviewer suggests. For example, in Figure 3 of Li and Battisti (2008), there is little difference in the Atlantic zonal wind profiles of the latitude of the peak winds at 200 hPa and at the surface differing at LGM (indication of the positions of subtropical and eddy-driven jets in Eichelberger and Hartmann (2007)). In Figure 2 of Lofverstrom et al (2014), the 800hPa winds across the North Atlantic during LGM are highly zonal, and there is little evidence of any separation between the subtropical and eddy-driven jets. Finally, in Figure 4 of Merz et al (2015), there appears to be a clearer separation between the subtropical and eddy-driven jets at LGM than during Present Day, or a less merged jet state, much as in our study. Delineating whether a jet is merged or not is problematic, since it is based on the separation of the distributions of the subtropical and eddy-driven jets. How much separation can there be and still be considered merged? The only definition for a merged jet that I have encountered is in Harnick et al (2014), who define the Zonal Jet Index as the anomaly with respect to monthy or seasonal climatology of the maximum value of the zonal derivative of the latitude with peak zonal winds. They define a threshold for the jet to be merged as being a negative value in this derivative with time that exceeds one standard deviation.

Revised text is "These changes are consistent with what would be expected if the North Atlantic jet approached a more "merged" state at LGM, although not all of these studies show evidence of this. Irregardless, changes to the path of the jet are expected to result in changes to the distributions of heat and precipitation over Western Europe during this period."

Page 4, line 26: Missing space between sentences.
Done. This was an artifact of LaTex's paragraph formatting.

Page 4, line 32: "The timing.." meaning here is not clear.
We state in the previous sentence that the jet transition detected by Lofverstrom and Lora (2017) occurred at the separation of the Cordilleran and Laurentide ice sheets at 13.89ka BP. A transition at 13.89 ka BP would be unlikely to explain either the abrupt warming into the B-A (occurring nearly a millennium earlier), nor the abrupt cooling at the start of the YD (occurring a millennium later).

Revised text is "A jet shift at 13.89ka BP lies during the middle of the B-A,..."

Page 5, line 5 and section 2.1: Perhaps nit-pick but this not technically correct. PUMA (the dynamical *core of PlaSim) is indeed a dry primitive equation model. However, the extra layer of physics on top of the dynamical core makes PlaSim more than a primitive equation model. More correct to say that it is a simplified general circulation model or, better yet, an Earth-system model of intermediate complexity (EMIC).*

Most importantly, we would like to point out that PlaSim solves the moist primitive equations, not the dry. We agree with the referee that PlaSim is more than just a primitive equation model but so is any current generation Earth System Model. Our main goal in pointing out that the foundation of the atmospheric model is based on the moist primitive equations is to illustrate that the dynamics have not been simplified beyond what is common in many Earth System Models today. Rather, the simplifications in the atmospheric model arise mainly in the parametrizations included: no treatment of volcanic or anthropogenic aerosols, only a single greenhouse gas species explicitly accounted for, etc. We intentionally avoided the term EMIC, because it has become a vague term encompassing a wide range of models with different combinations of sophisticated and simplified components.

Revised text, "PUMA is an atmospheric general circulation model whose dynamical core is based on the wet primitive equations. The primary simplifications in this component of PlaSim are found in the physical parametrizations incorporated in the model: for example, carbon dioxide is the only greenhouse gas whose radiative effects are considered and the radiative transfer scheme is much simpler (and thereby much faster) than that used in current state of the art GCMs."

Page 5, line 28: It was recently shown by Lofverstrom and Liakka (2018) that T42 resolution is sufficient to reasonably capture planetary waves in simulations of the LGM climate.
This reference was added. Revised text is "Herein we use 10 vertical levels at a spectral resolution of T42 (approximately 2.8°x2.8°), which has been previously shown to be sufficiently high to resolve phenomena of interest to the eddy-driven jet (Barnes and Hartmann, 2011; Lofverstrom and Liakka, 2018) while enabling fast enough model run times to make multiple deglacial experiments feasible."

Page 5, line 29: The description here is not correct. The Gaussian grid is the 128 x 64 cell grid in real space that the data is outputted on ("Gaussian" refers to how the grid is generated). The primitive equation are partially solved in spectral space (wave space) and are thus transformed between grid space (on the Gaussian grid, in this case 128 x 64 grid points in lon x lat) and the spectral representation in wave space, which supports at most 42 harmonics in the zonal and meridional direction, respectively. (Hence the name T42, where the T is short for "truncation" or more specifically "triangular truncation").

We agree with the referee. Revised text is "The dynamical atmospheric solutions are generated in spectral space, while the remaining calculations (e.g. phase changes, heat exchange with the land, sea ice or slab ocean, and any changes in those sub-components) occur in real space on a Gaussian grid with 64 latitude points and 128 longitude points. The only exception to this is LSG, which is run at 2.5°x5° horizontal resolution."

Page 6, line 1: Not sure if I understand how the LSG models works. Is it a dynamic model that only runs in the mixed layer? If yes, how can a realistic ocean circulation be established if there is no deep ocean? Do you parameterize fluxes between the deep ocean and mixed layer? If yes, how are these fluxes calculated? What is the depth of the mixed layer? Prescribed or dynamic? Studies have shown that the mixed layer depth was substantially greater at the LGM (e.g. Sherriff-Tadano et al., 2018), which can have profound implications for the ocean heat contents and energy exchange between the *ocean and atmosphere.*

LSG is a three-dimensional general circulation model for the entire ocean. It solves the primitive equations for the ocean under assumptions of large spatial and time scales, which filters out relatively fast components like Kelvin waves. Given these assumptions and that LSG solves its equations implicitly, the time steps used are much longer than would be commonly used for other dynamical ocean models (in this study, approximately 4 simulation days). However, in order to allow the model to respond more quickly than this to abrupt or short-lived changes at the ocean's top surface, a mixed-layer ocean model is used as an intermediary between LSG and the rest of the model. The mixed-layer model is fixed to a 50m depth, which corresponds to the depth of the top layer of LSG. LSG itself does not have a fixed mixed-layer depth, and fluxes between the deep ocean and the mixed-layer are calculated as part of an LSG integration. The mixed-layer ocean model is made to relax gradually toward the LSG solution over LSG's timestep via an applied bottom-boundary heat flux, but it is also free to respond to changing thermal forcings at its top boundary from the atmosphere and sea ice.

Revised text is “Rather than specifying a fixed deep-ocean heat flux to the mixed layer ocean, PlaSim estimates these fluxes by executing LSG every 32 atmospheric time steps (equivalent to 4.5 days). LSG is a three-dimensional, global, ocean general circulation model that solves the primitive equations implicitly under assumptions of large spatial and temporal scales (Maier-Reimer et al.,1993). This formulation permits stable solutions on longer time steps than other components of PlaSim with the trade-off that it filters out gravity waves and barotropic Rossby waves (Maier-Reimer et al., 1993). Since the time steps of LSG are so long, a slab-ocean model is used as an intermediary between LSG and the rest of the model in order to allow the ocean to respond to abrupt or short-lived phenomena. The slab-ocean model is fixed to a 50m depth, which corresponds to the depth of the top layer of LSG. Thus, at the start of an LSG integration, fields in the top layer of LSG are initialized to those from the slab-ocean model, and heat fluxes and wind stress fields are read from the sea ice and atmosphere components, respectively. The LSG integration is performed, and a spatial map of differences between the mixed-layer temperature at the end and the start of the LSG time step are calculated. These temperature differences are used to define a map of deep-ocean heat fluxes, which are subdivided by the number slab-ocean time steps before the next LSG integration and applied as bottom boundary conditions to the slab-ocean model. Thus, under constant atmospheric conditions, the slab ocean model relaxes toward the LSG solution. Under changing atmospheric conditions, the surface component of the ocean will tend toward a mixture of the LSG solution and a thermal response to the surface forcing.”

Page 6, line 17: Please clarify, you update the boundary conditions every simulation year, but with 10x acceleration, meaning that you effectively only run every 10 years from LGM to PI. Is that correct?

As far as the forcings go, the referee's description is correct. However, since the model is run continuously forward in time under these accelerated conditions, the solution will not be the same as if we extracted one of every 10 years from an unaccelerated simulation. The differences are described in Appendix A2 and are most noticeable in phenomena with a decadal or longer response timescale. For example, if the ocean's mixed layer takes approximately 30 simulation years to fully adjust to a change in atmospheric boundary conditions, the forcings will have progressed 300 years in this time. Thus, the timescales of these responses will appear lengthened.

Revised text is “This acceleration was not found to alter the main conclusions of this study when tested with a single unaccelerated run, but it is expected to lengthen the apparent timescales of processes. For example, if the ocean's mixed layer takes approximately 30 simulation years to fully adjust to a change in atmospheric boundary conditions, it will appear to take 300 years from the perspective of forcing changes.”

Page 7, line 12: "...temporal resolution of 100 years". This seems to conflict with the synchronous update of boundary conditions described above.

As stated on page 7, lines 1-3, not all of the boundary conditions were available at annual or decadal timescales. Where they weren't available, we interpolated their values linearly in time between available bracketing time points. Thus, all of the boundary conditions were updated at the start of every simulation year.

Revised text is "These data are interpolated spatially to the model grid, with land-sea mask defined so the topography of ocean grid cells lies below the contemporaneous sea level. The data are also linearly interpolated in time in order to provide updates every simulation year (i.e. every 10 forcing years)."

Page 7, line 5: Please clarify how this process works. You can't fit an even number of 0.5◦ grid cells in a T42 cell (which is around 2.8◦), so there must be some partial overlapping cells. Also, what does "effective higher-resolution grid cell length" mean?

We calculated the variance of higher-resolution grid cells within the T42 grid cells in the following manner. Writing the variance as the SUM(x^2)-(SUM(x))^2 and using conservative remapping as an area-weighted sum over overlapping regions in grid cells between the two grids (for a description of conservative remapping, see Jones, 1999), we conservatively remapped both the elevation and the square of the elevation from the higher-resolution grid to T42. We then squared the remapped elevation and took the difference as in the equation above to get the variance. The number of high-resolution grid cells contributing to this variance was approximated by the ratio of the total number of grid cells in the global high-resolution grid divided by the number of T42 grid cells. The effective higher-resolution grid cell length was then defined by dividing the area of the T42 grid cell evenly over the number of high-resolution grid cells contributing to the variance and taking the square root (assuming each of these grid cells are squares).

Revised text is "For each T42 grid cell, the roughness is equal to the variance of all 0.5°x0.5° ice sheet grid cells contained within it divided by an effective high-resolution grid cell length. This calculation is performed by first conservatively remapping the elevation and the square of the elevation from the higher-resolution grid to the lower-resolution grid. The variance is then the difference between the square of the remapped elevation and the remap of the squared elevation. The effective higher-resolution grid cell length is the square root of the area of the T42 grid cell divided by the number of higher-resolution grid cells per T42 grid cell (taken here to be the ratio of the total number of grid cells globally in each grid)."

Page 8, line 18: Ivanovic et al. (2016) is double cited.
The second citation has been removed. Revised text is "Data for CO_2 , N_2O and CH_4 concentration changes over the deglaciation are consistent with the prescriptions of the PMIP4 Deglacial experiment (see Ivanovic et al. (2016) and data sources Luthi et al. (2008) Meinshausen et al. (2017), and Loulergue et al. (2008))."

Page 8, line 22: Century should probably be millennium here (you discuss 21 ka - 20 ka and 1 ka to 1950), right?
Since the PlaSim simulations were generated with the forcings accelerated in time by a factor of ten, a millennium of forcing changes elapsed during the first or last centuries of the simulations. Thus, we were analysing 100 years of output and comparing them against a millennium of data from unaccelerated simulations.

Revised text is “We compare the climate conditions during the first and last century of the fully-transient PlaSim simulations (corresponding to forcing years 21-20ka BP and 1ka BP to 1950AD, respectively, due to acceleration) to the results of LGM and past1000 experiments in the Climate Modelling Intercomparison Project (CMIP) 5.”

Page 9, line 10: Sentence can be simplified; e.g.: "Also, the path of the NPac jet" -> "Also, the NPac jet"
Done. Revised text is “Also, the NPac jet is displaced further north and is more tilted in the PlaSim past1000 simulations.”

Page 9, line 14: I agree with this assessment and a similar conclusion was reached by Lofverstrom et al. (2016); see their discussion about sensitivity simulations with extensive sea ice in the eastern N. Atlantic (their Fig. 6).
Done. Revised text is “We speculate that this eastern shift is connected to the much more southern extent of sea ice on the eastern side of the NAtl, as was found in CAM3 simulations forced by present-day ice sheets with LGM sea surface temperatures and sea ice extent (Lofverstrom et al., 2016).”

Page 16, line 6: Write out explicitly that you are referring to Fig. 11 here.
Done. Revised text is “In contrast, in Figure 11 the eastern side of the jet over the eastern NAtl is less focussed than the west, with the jet occupying its preferred latitude 50 to 70% of the time.”

Page 16, line 5: Typo? ...range or its tilt -> ...range of its tilt (?)
The sentence has been reworded to avoid confusion. Revised text is “Since these jet characteristics and the timing of their changes differ on the eastern and western sides of the NAtl jet, we attribute them separately in the next section.”

Page 16, line 15: What standard metrics? Page 16, line 17: Replace "instead" with "as well", and remove "For those interested".
“Standard” here is intended to mean “commonly-used.” The definitions employed here for jet latitudinal position and tilt arise from Woollings et al (2010) and a combination of Woollings and Blackburn (2012) and Lofverstrom and Lora (2017), respectively. However, variants on these definitions have been used in many of the papers discussed in the Introduction.

Revised text is “Due to this sensitivity and the important differences in jet characteristics in the two regions, we argue that analyses over the western and eastern jet regions are more instructive than the more commonly-used jet latitude and tilt metrics, and would encourage other authors to present these metrics as well.”

Page 17, line 5: Meaning here is not clear. Do you mean flat ice sheets (i.e., only accounting for the albedo effect)? Also, the ice sheet height is not the only thing influencing the circulation. As you say elsewhere, the spatial extent is also important.

The PDTopo experiment is defined with all forcings varying in time except the ice sheet thickness. The thickness of the ice sheets are fixed to present-day values, so the land elevation remains the same as during present day at all times. Nevertheless, the ice sheet area varies from an LGM extent to present-day (i.e an infinitessimally thin ice sheet). This allows the role of the ice sheet orography to be separated from the influence of its albedo and the rest of the forcings.

We do not claim that ice sheet height is the only thing influencing the circulation. Rather, the text says, "The component of the ice sheets that appears most important to this effect is their elevation." We support this claim by noting in the results of the PDTopo experiment (with time dependent ice area) at LGM, the primary location of the jet is not shifted equatorward and is only slightly more focussed than during present-day. Since we know that the ice sheet is the primary control for the western side of the jet being equatorward-shifted and highly focussed via the results of the FixedGlac experiment (where all other forcings vary in time, but the western side of the jet remains predominantly in the same state throughout the deglaciation), this suggests that either the ice sheet orography or the combined effect of the orography and area are creating these effects. Since our DarkGlac experiment differed very little from the full-forcing runs due to extensive snow cover, we can not differentiate between these two possibilities. However, we can say that an elevated ice sheet is required for this effect.

Revised text is "Ice sheets provide the primary control over the deglacial jet changes described in the previous section. Simulations with fixed LGM ice sheets (FixedGlac in Figures 10 and 11) reproduce neither the deglacial changes to preferredjet latitude nor the bulk of changes to its variability on both sides of the jet. This effect is most prominent for the western side of the jet, which shows almost no change over the deglaciation when the ice sheets are fixed to their LGM state. In the east, the preferred position of the NAtl jet does not change under fixed LGM ice sheets, but the frequency of time the jet spends at this latitude decreases, and its range of variability increases. Only orbital and greenhouse gas forcings are changing at this time, so this may indicate a sensitivity to those forcings (perhaps mediated by the changing sea ice extent and sea surface temperatures).

Other sensitivity experiments can help decompose which attributes of the ice sheets are enacting this control on the NAtl jet. The PDTopo experiment isolates the thermal forcing associated with ice sheets' relatively high albedo from the orographic forcing due to the elevation of the ice sheet by fixing ice sheet thickness to present-day values while allowing ice sheet area to vary. Thus, LGM ice sheets are infinitessimally thin but extensive. In neither the east nor the west is the NAtl jet as focussed, or as equatorward-shifted in PDTopo (Figures 10 and 11) as it is at LGM in the FullyTrans runs or throughout the FixedGlac runs. Consequently, we conclude that the ice sheet albedo alone is not the primary controlling factor on the NAtl jets, and that the elevation of ice sheets is important. However, it is not clear from the present experiments whether orographic changes to the ice sheets alone are sufficient to explain the jet changes, or whether the ice sheets need to be reflective. Since the ice sheets became quickly covered with highly-reflective snow in the DarkGlac simulations, that experiment did not resolve this question. "

Page 18, line 1: How did you arrived at this specific number (725 m)?

We determined the elevation threshold above which the jet appeared latitudinally restricted by empirical testing. The southernmost latitude of the ice sheet in eastern North America was identified by applying a mask for regions at or above the elevation value being tested and identifying its southernmost latitude value in this region. Then, as in Figure 12, the North Atlantic jet latitude for each month was plotted against the corresponding ice sheet minimum latitude. If the jet latitude ever exceeded the ice sheet latitude in any of the FullyTrans simulations, then that elevation value was rejected as the threshold.

Revised text is " This number was arrived at empirically and represents the lowest threshold tested that did not have instances of the western side of the NAtl jet exceeding its location."

Page 18, line 9: Typo? "jet does not always move the the latitude of the jet."

Fixed. Revised text is "It should be noted, the jet does not always move to the latitude of the ice sheet margin."

Page 19, lines 3-10: This explanation is a bit too simplistic. I agree that the presence of the ice sheet constrains the jet latitude in the west, presumable in part because of obstruction of the flow by the topography. However, the thermal gradient at the southern ice margin can influence the flow in a similar fashion (this is not mentioned here as far as I can see) - both the change in albedo at the ice sheet margin, and the adiabatic cooling of the flow by the implied elevation difference. The modern (PI) jet is also less variable in the western ocean basin because of the strong thermal gradient at the sea-ice edge. This is clearly a different mechanism than the presence of a big ice sheet, but the effect is similar.
We reject the hypothesis that the albedo change along the southern margin of the ice sheet plays an important role in constraining the jet that we detect over North America, because there is no such constraining effect in the PDTopo experiment (See Figure 12). The PDTopo experiment includes a time-evolving, but infinitessimally-thin ice sheet, so the albedo change along the ice sheet's southern margin varies the same way in time in the PDTopo experiments as it does in the FullyTrans runs. Thus, the ice sheet must be elevated in order for this barrier effect to occur. Our analyses can not distinguish between whether the elevated barrier operates via a dynamical effect alone or whether there is a role for the thermal effects of the ice to play.
Revised text is "Other sensitivity experiments can help decompose which attributes of the ice sheets are enacting this control on the NAtl jet. The PDTopo experiment isolates the thermal forcing associated with ice sheets' relatively high albedo from the orographic forcing due to the elevation of the ice sheet by fixing ice sheet thickness to present-day values while allowing ice sheet area to vary. Thus, LGM ice sheets are infinitessimally thin but extensive. In neither the east nor the west is the NAtl jet as focussed, or as equatorward-shifted in PDTopo (Figures 10 and 11) as it is at LGM in the FullyTrans runs or throughout the FixedGlac runs. Consequently, we conclude that the ice sheet albedo alone is not the primary controlling factor on the NAtl jets, and that the elevation of ice sheets is important. However, it is not clear from the present experiments whether orographic changes to the ice sheets alone are sufficient to explain the jet changes, or whether the ice sheets need to be reflective. Since the ice sheets became quickly covered with highly-reflective snow in the DarkGlac simulations, that experiment did not resolve this question."

Page 21, line 25: Meaning here is not clear - this seems to be the definition of a shift in the jet latitude.
We agree that the wording used does not effectively bring out our point. What we intended to argue here is that the frequency maps calculated from the Trace-21ka data do not show any change in the preferred jet tilt between 14 and 13ka BP. These results are contrary to what is suggested by the results of Lofverstrom and Lora (2017), but there are two important differences in the methodology here from what they used. Firstly, although both studies examine the TraCE-21ka data, the abrupt increase in jet tilt presented in Lofverstrom and Lora (2017) was detected at 250 hPa, whereas we analysed jet changes over 700-925hPa. Secondly, the jet tilt was defined in Lofverstrom and Lora (2017) as the difference in jet positions between 10-20°W and 70-80°W, whereas we defined the jet tilt as the difference between 0-30°W and 60-90°W. When we alter our analysis conditions to match those of Lofverstrom and Lora (2017), we see that the preferred angle of jet tilt does not change around 13.9 ka BP. Rather, the frequency of time spent at this tilt and less tilted values decreases, while the range and frequency of tilts increases for more positive values. This combination of phenomena matches the abrupt increase in mean jet tilt presented by Lofverstrom and Lora (2017).

Revised text is "Note that this value is less than that calculated in Lofverstrom and Lora (2017) from TraCE-21ka data (between 3° and 4°), but they calculated jet tilt from upper-tropospheric winds and different longitude ranges in the western and eastern regions of the NAtl jet (270 - 300°E and 330 to 360°E in this study versus 280 - 290°E and 340 to 350°E).” “This results stands in contrast to Lofverstrom and Lora (2017), who diagnosed a rapid increase in jet tilt at 13.89ka BP. The source of this discrepancy is discussed further in Section 4.” “This contrasts with previous work on TraCE-21ka that identifies an abrupt increase in tilt in this dataset that occurred at 13.89ka BP (Lofverstrom and Lora, 2017). These two TraCE-21karesults were obtained from different wind data extracted from the same dataset: lower-tropospheric winds are analysed in this study, while Lofverstrom and Lora (2017) examine upper tropospheric winds at 250hPa. There are additional differences in the range of longitudes used to specify the western and eastern regions of the NAtl jet: 280 to 290°E and 340 to 350°E, respectively in Lofverstrom and Lora (2017) versus 270 to 300°E and 330 to 360°E in this study. We are able to reproduce the results obtained by Lofverstrom and Lora (2017) (except for the timing, which we date to 13.87ka BP) when we calculate the jet tilt in the same manner as they did (figures are presented in Supplemental Figure S5, alongside corresponding figures using the methodology employed in this study). “

Page 22, line 16: What Figure is discussed here?
Figure 12, bottom left plot. Revised text is “As in the PlaSim simulations, this change is driven by a shift on the western side of the jet, plotted in the bottom-left panel of Figure 12.”

Page 22, line 25:I would encourage you to think a little bit more about this and try to give a mechanistic explanation for this phenomena. Doesn't have to be a full explanation, but at least something that adds a little bit more to the story.
We will expand on our discussion as discussed at the start of this document.

Revised text is “In contrast, the downstream side of the jet over the eastern North Atlantic does not show the same sensitivity to the marginal position of the North American ice complex, but it is affected by the presence of elevated ice sheets. Whether this control is exerted via changes to the stationary waves (e.g. Kageyama and Valdes (2000); Lofverstrom et al. (2014)), transient eddies (e.g. Merz et al. (2015)), surface thermal gradients from sea ice and sea surface temperatures (e.g. Li and Battisti (2008)), or some other mechanism is not entirely apparent from this study. There is some evidence that sea ice and sea surface temperatures may play a role, as the range of jet latitudes increases after abrupt sea ice retreats and sea surface temperature warmings in the FixedGlac experiment, and the eastern jet distribution is centred around different latitudes at the end of the deglaciation in FullyTrans and PDTopo even though their forcings are the same at this time. “

Figure 1: What ice sheet remained in North America through the Holocene and is 1.5 km thick?
Figure 1 plots the peak elevation of ice sheet-covered areas in North America and Fennoscandia. There is no 1.5km thick ice sheet over North America during the Holocene, but there are regions that have an elevation in excess of 1.5km that are covered by ice: glaciers in the Rockies, for example. Due to this confusion, we have changed Figure 1 to only include ice sheet area and elevation east of the Rockies.

Revised caption is “a) Peak elevation in ice sheet-covered areas (bedrock elevation plus ice sheet thickness) and b) ice sheet area for the Laurentide ice sheet and Eurasia (FIS). ”

Figure 4: Panels showing LGM and past1000 are mixed up (LGM is shown in middle panels). This is correct. The labels are fixed in the revised version.

Figure 4: The top of the LGM ice sheets (and indeed some modern topography) is higher than the 700 hPa isobar. I would advice against extrapolating the wind field in these regions and

instead treat it as missing data, as extrapolation can cause some weird effects when doing statistical analysis (e.g. when determining the latitude of the strongest winds in the western N Atlantic).
The reviewer raises a good point. Since our analyses were performed on vertical averages of winds over 700-925hPa, where there is an increase in wind speed with height (Figure 5), it is not clear what pressure level should be used to create a mask of topography. To get around this problem, we recalculated the jet statistics for FullyTrans1 only looking at 850 hPa and omitting grid cells where the land surface lies above this pressure level. The results of these tests are provided in Supplemental Figure S9. The timing and characteristics of jet transitions in the western and eastern regions and for the jet tilt are unchanged from those presented in the main paper. However, when the jet is averaged over longitudes 270ºE to 360ºE, there are differences compared to the FullyTrans1 results in Supplemental Figure 3. These differences can be attributed to changes to the longitudinal range over which the winds are being averaged when grid cells with an elevated ice surface are masked from the analysis. Due to the differences in characteristics of the jet on its western and eastern sides, changing the weighting of these two regions with time by excluding a changing number of grid cells on the west leads to a mixture of jet characteristics in the mean jet. This issue highlights the problems with using this metric. However, since the conclusions of our study are not affected by the inclusion or exclusion of winds interpolated onto levels below the ice sheet surface, we leave our analyses as is.

Revised text is "Note that the ice sheet elevation in some grid cells exceeds the bottom pressure level of the analysis range, so we interpolated the winds to these levels to not bias the resulting average. The land surface does not pass above the 700hPa pressure level in any of the grid cells included in our analysis, so we are not introducing winds to a region where there was none. The impact of this choice was tested on the 850hPa level and found to not change the jet results (see Supplemental Figure S9) except when the jet is calculated over the entire longitudinal range. In this case, excluding grid cells from the analysis on the western side of the region effectively weights the mean jet toward characteristics of the eastern region. This issue illustrates the sensitivity of this diagnostic to the longitudinal range that is employed. Due to this sensitivity and the important differences in jet characteristics in the two regions, we argue that analyses over the western and eastern jet regions are more instructive than the more commonly-used jet latitude and tilt metrics, and would encourage other authors to present these metrics as well."

Figure 4 caption "Where the land surface impinges on the vertical range, winds are interpolated to not bias the vertical average. Further discussion can be found in Section 4."

Figure 6 - 8:Use the same range on the spines on the right hand side for easier comparison (e.g., in Fig. 6: 0-80 % in top panels and 0-35 % in lower panels).
Done.

Figure 9: 10 successive years is a bit ambiguous because it can be done in at least two different ways: (1) a sliding mean where the input and output arrays have the same length; (2) form decadal averages where the input array is 10x longer than the output array. These methods will yield slightly different results. I doubt that the difference will be of su#cient magnitude to challenge your conclusions, but this type of information is important for reproducibility.
Neither of these methods were used to generate the plots of Figure 9 as no averages were performed. Instead, monthly jet latitudes and tilt were collected for every month in DJF for 10 successive years. Then, the fraction of time that the jet spent in each latitude/tilt bin was calculated by summing over the number of months with a jet in the given bin and dividing by the total number of months in the sample (= 30 months/per run * 4 runs = 120 months).

Revised text is “Frequency maps of NAtl, lower-level, jet latitudes and tilt aggregated over 10 successive winter seasons and all ensemble members of the FullyTrans experiment. Frequencies represent the percentage of months (out of a total of 120 months) that the jet was identified at a particular latitude, where each latitude bin has a width of 2.8° at T42.”

Figure 9 - 11 and 13: Write out lat and lon bounds and pressure level(s) used in statistics.
Done.

Figure 11: Caption appears to be wrong as you show latitude here, not difference in latitude across theN Atlantic.
The reviewer is correct. Revised text is “Frequency maps of ensemble-average, NAtl, lower-level, eastern jet latitude in 10 successive winter seasons for FullyTrans, FixedOrbGHG, FixedGlac, and PDTopo experiments. Colours indicate the percentage of months with the difference in jet latitudes between 330°E to 360°E and 270°E to 300°E within each bin of width 2.8°.”

Figure 12: Use same latitude range on vertical axis for easier comparison.
Done.