We thank the three anonymous reviewers and R. Zech for their critical and thoughtful comments. We have responded to all comments below and provide an updated manuscript.

SHORT COMMENT - R. Zech

- **One aspect, which could be additionally addressed in a revision, would be the continuous obliquity forcing during the Middle and Late Pleistocene (Huybers, 2007). Obtaining robust direct age control for glacial chronologies is challenging, but obliquity may in fact have triggered climate and environmental changes in northeast Siberia (and elsewhere) during the last glacial cycle (Zech et al., 2011).**

  The orbital configurations used in these experiments are idealized and therefore, cannot be assigned to any particular period within the Pleistocene. Given our model results, we would expect to see a large high-latitude climate response to obliquity throughout the Pleistocene, which is consistent with the $\delta^{18}O$ records. While the focus of the paper is the high-latitudes of North America, the modeled climate responses are not limited to North America. We find similar surface feedbacks to obliquity forcing in the high-latitudes of Eurasia as well.

- **I also wonder whether it would be possible to include soil organic carbon in the model (in the future). Recent studies have shown that the amount of soil organic carbon in permafrost regions has been hugely underestimated (Tarnocai et al., 2009). As permafrost regions expanded during glacials, enhanced terrestrial carbon storage might help explaining reduced atmospheric CO2 levels and constitute an important positive feedback. It may even be the case that annual (and integrated summer) insolation at high latitudes triggered changes in permafrost extent and, via greenhouse gases, the rhythm of the Pleistocene ice ages (Zech, 2012). This ‘permafrost hypothesis’ could readily explain the ‘40 ka world’ during the Early Pleistocene and the ‘obliquity skipping’ after the mid-Pleistocene transition, but awaits evaluation by modeling studies.**

  The “permafrost hypothesis” is an interesting idea. Unfortunately, our Earth system model does not currently contain a carbon component, and our experiments do not include orbital variations in greenhouse gases. Carbon modeling is an aspect of the climate system that requires additional exploration but is outside the scope of our current contribution.

REVIEWER #1

A general question

- **What is your reference about the range 2.6-0.8?**
  Here we defined the Early Pleistocene as the period from the start of the Pleistocene epoch (2.588 Ma) to the Brunhes-Matuyama boundary (0.781 Ma). These dates come from the Geological Society of America. This definition has been used previously (e.g. Raymo and Nisancioglu, 2003) and approximately represents the period before the mid-Pleistocene transition.

Suggestions for future work

- **What is important nowadays is to gain understanding of the mechanisms responsible of climate changes and the quantification of factor contribution**
  It is important to keep in mind that 'a simulation study' is a way to gain understanding of mechanisms related to a phenomena and not to conclude about any final conclusion about the real mechanisms. The later needs to be completed by a comparison to real observed data, and a global sensitivity analysis, where all the important parameters (forcing and feedbacks) are varied by large amounts and simultaneously.
  The quantification of direct and combined effect of factors to insolation and feedback changes may be done using a global sensitivity analysis. Sensitivity analysis in a global sense, especially when more than two parameters are varied, need a considerable computational resources and time to be realized. This is due to the big number of simulations required in one hand and the fact of using complex climate models which are expensive in the other hand. The combination of statistical

methods with the classical simulation approach using a computer model, is a way to tackle this burden . Among the existing sensitivity analysis for computer codes Sobol' (1993), Kleijnen, (1997), Oakley and O'Hagan (2004), Saltelli et al. (2008).

More specifically :

In this study, the authors followed a classical methodology in paleoclimatology of sensitivity analysis which is performed using a feedback analysis, based on a single parameter perturbation or a One at a time test, to evaluate the contribution of a specific input parameter to the output change (an input is for instance the obliquity, an output is temperature, a computer climate model is called a simulator). This is assessed by analyzing the difference between a simulation where the influence of this parameter is considered, and of a simulation where the influence of this parameter is omitted. However, explaining the resulted difference when more than one parameter is considered becomes hard to interpret (Stein and Alpert, 1993 ; Alpert and Sholokhman, 2011). Moreover, only small perturbations around a reference state is considered and does not consider the synergism between the different parameters (Alpert and Sholokhman, 2011). This method has been extended by Claussen (2001) by considering feedbacks as well as synergisms.

The aim of using this method is to understand the amplification of the output initial signal due to feedbacks and synergisms. This approach provides an easy way to objectively separate the pure contribution of one input parameter from its synergism with the others. It is applied to better understand the contributions of a single feedbacks and synergisms to an output simulation result.

Many applications of these methods may be found in Paleoclimatology. For instance, Berger (2001) to analyze the impacts of vegetation changes on climate over the last glacial- interglacial cycle and Crucifix and Loutre, (2002) during the he Last Glacial Maximum.

We agree that a complete sensitivity analysis is a valuable next step towards better understanding the various feedbacks and their interactions. We discuss the limitations of our current analysis in a new "Caveats" section (section 5) at the end of the manuscript. We are currently researching the interactions between obliquity and precession forcing to address these questions.

Minor comments

- **May I suggest to the authors to write more specifically, in the introduction, that the analysis needed for their study/aim is a sensitivity analysis. Moreover, to specify that the analysis needed/applied here is a sensitivity analysis known as the feedback analysis using a climate model (for instance make changes in the paragraph from 3771.27 to 3772.2).**

  In paragraph 4 of section 1, we have added the sentence: "We examine the high-latitude climate response to insolation forcing through a sensitivity analysis in which we separate the system responses to obliquity and precession." Hopefully this will provide a better introduction to the analysis we then discuss.

- **3770.17: change to : These climate variations known as Milankovitch cycles are quasi- cyclic.**

  3770.17-18 : change to : They are attributed to the direct and combined effects of changes in the astronomical forcing parameters (obliquity, precession and eccentricity)

  We have added most of the suggested change but left "…. Earth's degree of axial tilt (obliquity), direction of axial tilt (precession), and circularity of orbit (eccentricity)." We think a brief description of the different orbital parameters is helpful for the uninformed reader.

- **3770.19 : add a reference.**

  We have included a citation to Hays et al., 1976.

- **3771.10: add a reference.**

  We have included a citation to Lisiecki and Raymo, 2005.

- **3770.21-3770.22 : The influence of the three Earth's orbital and rotational parameters.**

Changed.

- **3778.31-3778.20 : Move this paragraph to the end of the section. Combine it with the last sentence (3779.26) "Future work will examine the combined interactions between obliquity and precession.". Change the later to '"Future work will examine the effect of interactions between obliquity and precession ' or '"Future work will examine the combined effect of obliquity and precession'. Explain that an adequate sensitivity analysis, which is able to take into account the combination effect, is needed to verify this "idea" in one hand, and to estimate/quantify the contribution of direct and combined effects of the factors in the other hand (see for the methodology Stein and Alpert, 1993 ; Alpert and Sholokhman, 2011)**

  We considered moving the paragraph, but determined that the paragraph fits the paper better in its current location. We did move the last sentence of the manuscript to the end of this paragraph and have added the statement: "…to help quantify the synergistic interactions." We also discuss the importance of a complete feedback analysis in the "Caveats" section (section 5).

- **3779.24: change 'a new solution' to 'a new explanation of the mechanisms related to...'.**

  Changed.

- **3779.25: note: 'emphasize the importance of using complex models ' : It is important to consider more climate components and feedbacks in climate models to best represent the physical processes. But, be aware that complex climate models are expensive to achieve alone a global sensitivity analysis.**

  We agree that complex models are computationally expensive. We have included a new "Caveats" section (section 5) that we believe highlights the limitations due to computational expense.

REVIEWER #2

- **The analysis helps to quantify the relative ratios of precession and obliquity components in ice-volume variations. The paper would benefit from a comparison of these model-based ratios with those obtained from benthic d18O stacks. I have the impression that in comparison to the paleo-data the model still generates too much precessional variability over the obliquity component (Figure 3a,c). It needs to be explicitly stated that the proposed climate feedback mechanism provide a means to enhance the obliquity signal over the precessional signal, but that this enhancement is still far away from the complete muting of precessional variability during e.g. the Early Pleistocene.**

  We agree. We had previously stated in the manuscript that without some amount of hemispheric offset, we would be unable to replicate the Early Pleistocene δ^{18}O signal. Our results suggest that even when forcing the model with a realistic orbit, the precession spectral power will be greater than that found in the Early Pleistocene δ^{18}O stacked records (Lisiecki and Raymo, 2005). Because these experiments only look at the individual contributions of obliquity and precession to ice volume, it is difficult to assess exactly how much of a precession signal to expect from a more realistic configuration. The following sentence has been added near the end of the first paragraph in section 4 to make this point more explicit: "However, without invoking a hemispheric offset we expect our current model configuration will produce a smaller, yet significant, precession ice volume signal, which is not found in the Early Pleistocene ice volume proxy records."

There are several minor more editorial points that need to be addressed

- **Abstract "power by" should be "powered by"**

  We think the original wording is correct.

- **"...these differences cause obliquity to have a greater effect than precession on integrated summer energy amplitude above an ice-melt threshold". As far as I remember this was already shown in a paper by Andre Berger. Please cite appropriately. Unfortunately, I do not have his reference handy.**

  We looked but could not find the appropriate reference. We would be happy to include it if presented to us.

- **Why is the sea level lowered everywhere by 275m? This seems quite arbitrary. Please provide more explanation.**
  We chose a value of 275 m because it was the smallest value that allowed growth over the Hudson Bay when considering isostatic subsidence of the crust. We added the following sentences to the end of the first paragraph in section 2: “We found ~275 m to be the smallest amount of sea level lowering required to prevented flooding of the Hudson Bay when considering isostatic subsidence. The sea level lowering of 275 m has little influence elsewhere in the model because this is a terrestrial ice model with no explicit marine physics.”

- **For the "asynchronous technique" please cite G. E. Birchfield, J. Weertman, A. T. Lunde, Quaternary Research 15, 126, (1981).**
  Added.

- **"resulting in a larger sea-ice feedback" should be "resulting in a larger sea-ice response"**
  Fixed.

- **"(see Supplement)" - there is no supplement in this paper, as far as I could see. I guess, you are referring to Figure 2a, correct?**
  There is a supplemental figure that shows the differences in monthly average temperature response to insolation forcing from obliquity and precession. There is a link to this figure at the end of the main manuscript, before the references.

- **What is missing in this paper is a discussion of how the annual mean covariance between anomalous seasonal cycle of solar radiation multiplied with the average and anomalous seasonal cycle of albedo looks like [<Q’(t) (1- abar(t)-a’(t))>]. After all this is the effective annual mean shortwave forcing. In this case, clouds will also play an important role. Please discuss the role of cloud-albedo feedbacks versus sea-ice**

albedo feedbacks in amplifying the obliquity cycle.

We have looked at cloud albedo changes above the high-latitude oceans in response to orbital forcing and found only minimal and rather noisy responses. While this result is interesting, it does not seem to have a large impact on the climate response we discuss in our manuscript and, therefore, we have decided to omit the details from our analysis. We have added the following sentence: “Interestingly, the sea-ice difference does not lead to a large cloud albedo response. In April, when the difference in sea-ice coverage is largest, the variation in high-latitude cloud albedo over the ocean is only 0.013 for obliquity and precession.” to the end of the second paragraph in section 3.1.

- **High latitude summer insolation forcing remains the largest single factor for determining ice-sheet volume response" - this is absolutely not obvious in the figures. What do you mean by "high-latitude summer insolation forcing"? Is this referring to the integrated summer insolation, or peak summer? Please clarify. Is this statement referring to Figure 3b?**

  What we mean is that of all the insolation forcing signals, high-latitude summer insolation has the best correlation with ice volume change. Near the beginning of the last paragraph in section 4, we have added: “…shows the strongest correlation with ice volume rate of change (r = -0.85 for obliquity and -0.89 for precession)” to make our statement more clear. Figure 3b does illustrate this relationship.

REVIEWER #3

General comments

- **The main conclusion of the manuscript is based on the amplification of obliquity forcing by surface feedbacks (ocean heat, sea ice and vegetation). The impact of these feedback processes should be addressed in detail, as well as their relative importance. One way to accomplish this is to include sensitivity experiments investigating each of the feedback processes separately.**

  We agree that a complete sensitivity analysis would provide valuable insight into relative feedback strengths. Unfortunately, the computational expense of these runs prevents us from carrying out a complete sensitivity analysis. Each climate-ice experiment discussed in our manuscript took over 50 days to complete. We conducted a more limited vegetation feedback strength analysis by switching the vegetation outputs of obliquity and precession from the climate-only experiments and then rerun the snapshots to equilibrium. The results confirm our expectation that boreal forest warms the atmosphere more than tundra. We also conducted some similar sensitivity tests to investigate the ocean feedbacks. However, these sensitivity tests are unphysical because the prescribed SSTs are not in energy balance with the orbital forcing. Therefore, we were unable to quantitatively compare the ocean feedback strengths. We acknowledge this limitation more explicitly in paragraphs 2-3 of the new "Caveats" section (section 5).

- **The atmospheric model used includes a slab ocean model. According to the authors the absorption of heat by the ocean is one of the key process giving an amplified response to obliquity forcing. How the exclusion of the deep ocean as well as ocean dynamics impact this result should be addressed in detail.**
  Inclusion of a dynamic ocean model in our experiments would be ideal but computational limits prevent its implementation. The first paragraph of the "Caveats" section (section 5) acknowledges and addresses key aspects not included in our model as well as compares our results to those including a dynamic ocean model. We find similarities between our results and those conducted with a dynamic ocean. While we refrain from speculating too much on the biases produced by our slab ocean, we acknowledge that the ocean response requires additional research.

- **As stated in the manuscript, the simulated ice volume changes are very small compared to early-Pleistocene proxy records. How would a larger simulated initial (or minimum) ice volume impact the results and the relative role of obliquity and precession?**
  We added paragraph 3 to section 4 addressing the potential role of a larger ice sheet and how this might alter the surface feedback enhancement to obliquity forcing. We recognize that a North American ice sheet as large as that of the last glacial maximum

might significantly alter the surface feedbacks discussed in our paper.

Specific comments

- **page 3772, line 4: missing ref for GENESIS model and lack of details regarding slab ocean model**.

  Pollard and Thompson (1997) is meant as a reference for the GENESIS model in general, which includes the AGCM and land surface model. We prefer to keep model details to a minimum in the manuscript, since these have been described elsewhere, and instead reference these sources. However, we do include some additional details about the slab ocean and sea-ice models in paragraph 1 of the "Caveats" section (section 5).

- **page 3774, line 10: it is stated that "Because the obliquity cycle generates variations in annual- mean insolation, the high-latitude oceans absorb a greater range of insolation annually from obliquity than precession". However, this is not a sufficient explanation for why the ocean response to obliquity at high latitudes is greater than for precession. This needs to be elaborated. A nonlinear response to seasonal insolation (dominated by precession) could give a large annual mean response (see e.g. Huybers & Wunsch, GRL, 2003).**

  We do see an annual sea-ice response to insolation forcing from precession despite no annual insolation signal. However, because obliquity still produces a larger sea-ice response than precession, we believe the non-linear response to seasonal forcing is of secondary importance. The following text, located at paragraph 3 in section 3, addresses this issue: "Although smaller than obliquity, precession does have an effect on annual-mean high-latitude ocean-absorbed insolation and sea-ice coverage, despite no annual-mean insolation forcing, due to changes in the timing of seasonal insolation and interactions with sea-ice coverage. The summer insolation amplitude of obliquity also has some effect on the amount of ocean-absorbed insolation, but it is smaller than precession, and smaller still than the annual-mean effect of obliquity, so is of secondary importance in the transient obliquity experiments."

- **page 3774, line 15: it is stated, but not shown, that the simulated sea ice coverage changes are an indirect response to changes in absorbed insolation by the ocean. The alternative is that the sea ice cover is impacted directly by insolation, thereby giving a change in absorbed insolation by the ocean. This should be addressed.**

  Once the outputs reach equilibrium, the high-latitude oceans do absorb a larger range of insolation in the obliquity runs than the precession runs. Our results suggest absorption of insolation by the ocean is largely controlling the sea-ice response. For example, in April there is a larger range of sea-ice fractional cover over an obliquity cycle than over a precession cycle. However, the direct insolation variations during March and April are larger for precession than obliquity. If insolation were impacting the sea-ice cover directly, then we would expect a larger precessional response. Furthermore, when mean-annual insolation forcing from obliquity is largest, the winter insolation is lowest. However, at these times, there is less sea-ice all year, suggesting the annual signal overpowers the direct signal due to ocean heat storage. Nevertheless, direct insolation must accounts for some of the melting. Near the middle of paragraph 2 in section 3.1, we now state the following: "Direct insolation also accounts for some of the sea-ice melting. However, the strong correlation between annual absorbed insolation and seasonal sea-ice coverage suggests the direct insolation signal is of less importance."

- **page 3775, line 16: it is stated that the response of the vegetation is due to changes in annual-mean insolation. It is not made clear why this is, and why seasonal insolation is less important for vegetation. This should be addressed.**

  High-latitude boreal forest extent is limited mainly by annual temperature and length of the growing season, which are more influenced by obliquity than precession. We have added "Due to annual-mean insolation changes, obliquity produces a larger range of annual temperature and sunlight reaching the surface and accordingly, a larger amount of tundra/boreal forest exchange. While the precession cycle causes large seasonal insolation fluctuations, it cancels on an annual basis, which reduces the annual mean changes in incident insolation and temperature. As a result, NPP and GDD variations favor obliquity…" near the middle of paragraph 6 in section 3.1, to better distinguish the influences of obliquity and precession and why obliquity produces a larger vegetation response.

- **page 3776, line 27: it is stated that differences in the meridional fluxes of heat and moisture between orbits is less important than local changes. This result is key and should be elaborated by including a figure to support this statement.**

  We agree. We have added to the description of this section (last paragraph of section 3.1) and included a supplemental figure (S2) of high-latitude heat and moisture transport with descriptive captions to illustrate our point.

- **page 3777, line 10: please clarify how ocean heat flux, sea ice and vegetation influences the simulated ice volume.**

  The greater high-latitude temperature sensitivity to insolation forcing from obliquity enhances ice sheet growth during periods of low obliquity and retreat during periods of high obliquity. Near the middle of the first paragraph in section 3.2, we have added the sentence: “The enhanced temperature range promotes ice growth and retreat.” to make this more explicit.

- **Figure 3b: The simulated symmetry of the decay and growth rate of ice for precession is surprising. This should be addressed in the text.**

  Because we keep eccentricity fixed with the largest value of the Pleistocene, the forcing is fairly symmetrical and strong. Furthermore, we allow no fluctuations in greenhouse gas concentrations. Therefore, we would be surprised to see a large asymmetry in these experiments. Paragraph 3 in section 3.2 states the reason for near-symmetry.

- **Figure 1: This figure is very hard to understand and needs to be improved. E.g. for clarity the x-axis in a/d should be labeled, the color of all curves should be mirrored in the y-axis, and the labels of OBL and PRE should be overlain on the respective figures.**

  We do not label the x-axes in 1a, 1c, 1d, and 1f because it references two different time scales that have been standardized for ease of comparison. We have tried using two x-axes for each plot and double labels on a single x-axis for each plot but found both methods created too much clutter. Likewise, we attempted to add OBL and PRE labels to the figures but again found that only added to the confusion. We have included the reviewer's suggestion to have the y-axes colors correspond with the line colors.

Technical comments

- **page 3775, line 6: here a reference to Fig. 1b is given before introducing the contents of this figure. Should specify that it is ocean-atmosphere heat flux, not to confuse the reader. "ocean heat flux" can easily be confused with horizontal fluxes of heat (see also line 10).**
  Fixed.

- **page 3775, line 15: "to assessment" should be "to assess"**
  Fixed.

- **page 3775, line 17: should refer to Fig. 1c.**
  Fixed.

- **page 3778, line 19: reference should be (Raymo et al., 2006; Lee and Poulsen, 2009)**
  Fixed.

- **page 3779, line 19: correct to "..much smaller..."**
  Fixed.