AUTHOR'S RESPONSE

Our replies to the comments of the two referees are included below, as well as some minor additional adjustments we have made.
We use the following color coding:
Blue: Referee comments (pages and line numbers as in the discussion paper)
Black: Author reply (pages and line numbers as in the revised manuscript)
Red: Changes made to the text

The changes made to the discussion paper have improved the clarity of the paper, and the paper is now better embedded in previous work. Our main analysis and conclusions, however, remain unchanged.

Reply to referee #1 (J. Fastook)

We thank J. Fastook for his review of the discussion paper.
He suggested highlighting the sensitivity analysis that appears late in the paper earlier-on so as to ameliorate the initial feeling that the model is overtuned.
We followed this suggestion by stressing the sensitivity experiments already in the model section:

Sensitivity to the non-CO2 GHG enhancement (factor) of radiative forcing (p. 7 l. 27):
"The sensitivity to this factor will be analysed in Sect. 3.3."

Sensitivity to the formulation of ocean overturning (p. 8 l. 16-17):
"In Sect. 3.3 we discuss its effect on Northern Hemispheric temperature, sea level and Northern Hemispheric overturning strength."

Sensitivity to the positioning of the ice sheets in the ZEBCM (p.11 l. 17-18):
"The influence of the position of the ice sheets in the ZEBCM on the climate will be studied in Sect. 3.3."

Reply to referee #2 (D.M. Roche)

We thank the reviewer D.M. Roche for his constructive and clear comments on the discussion paper. His most important suggestions mainly concerned the embedding of our study in previous work (comments 2, 3 and 20). We agree with these points and have added more discussion and comparison with the suggested previous work. In addition, many clarifications were made. In our opinion, this has improved the quality of the paper substantially. However, it has left our main conclusions unchanged.

Below, we reply in detail to all the specific comments, and indicate the changes made to the discussion paper.

1) p. 2549, line 1-10.: the authors are referring to the temperature records from ice cores. This is misleading. What is commonly used as a temperature record in Greenland are the water isotopes records which

are not quite temperature, especially in Greenland. A quick look at recent publications discussing the matter show considerable differences: for example compare Kindler et al. (2014) with a glacial / interglacial change of 15^{0}C and Simonsen et al. (2011) with an amplitude of 20-22^{0}C. These are only two examples. Please rephrase that paragraph to take into account this particular aspect.

We agree that we should have been more precise on this aspect. A full discussion on the interpretation of water isotopes is outside the scope of this article, but we now mention the uncertainty coming from this, citing the suggested literature.
We changed the text to (p. 3, l. 3-7):
"Ice coring performed on the Greenland Ice Sheet has provided records of several water isotopes (GRIP members, 1993;NGRIP members, 2004; Dahl-Jensen et al., 2013). These indicate local temperature variability, although their interpretation is ambiguous (see e.g. different glacial-interglacial variability in Kindler et al. (2014) and Simonsen et al. (2011))."

2) The manuscript in its entirety deals with the foundation of the Milankovitch theory, namely the link between the changes in incoming solar radiation and the volume of ice-sheet (or sea-level). I am rather surprised that classical references of previous work on the Milankovitch theory are simply ignored: Milankovitch himself of course, Calder (1974), Imbrie and Imbrie (1980) or Paillard (1998) to cite a few (Paillard, 2001 is a good starting point for more on this). Using only recent references as done in the manuscript gives the false impression that the field is an emerging one.

We realize that our statement that 'modelling ice ages is a classic issue in climate dynamics' needs some back-up. To do justice to the rich history of Milankovitch theory, we included a paragraph on some of the earlier work. We keep it brief, however, since we do not aim to provide a review.
We included (p. 3 l. 26 – p.4 l.11):
"Milankovitch recognised that Earth's climate is influenced by changes in solar radiation through three orbital parameters: eccentricity, precession and obliquity (Milankovitch, 1930), all varying with distinct frequencies. In 1941, he was the first to present calculations on the latitude-dependent seasonal solar insolation at the top of the atmosphere based on variations in these parameters (Milankovitch, 1941). Since then, these calculations have been improved (e.g. Berger, 1978; Laskar et al., 2004). Milankovitch proposed that as a result of changing insolation, summer temperature on the Northern Hemisphere would vary, and lead to glacial cycles. Indeed, orbital frequencies were discovered in climate records from marine sediments (e.g. Hays et al., 1976; Shackleton, 2000), and ice cores (Jouzel et al., 1989). The influence of solar insolation changes on the waxing and waning of ice-sheets was discussed in many studies of varying complexity (Calder, 1974; Imbrie and Imbrie, 1980; Paillard, 1998) as well as by studies with ice-dynamical models (Pollard, 1978; Oerlemans, 1980,

1982). However, simulating ice ages solely by solar variability remains difficult, because of lacking spectral power in the eccentricity (100 kyr) band (Imbrie et al., 1992)."

3) An even more surprising aspect is the complete lack of reference and comparison to earlier very similar work by the group of Louvain-la-Neuve with the LLN-2D model. They also used a simplified ice-sheet climate model and addressed the question of the ice ages. This comment is not limited to the introduction, but should be addressed both in the introduction and in the discussion part. Good starting points are Gallée et al. (1992) and Berger et al. (1999).

   In the revised manuscript, we have included discussion on the similarities and differences with the work of the Louvain-la-Neuve group. In particular, we revised the section on the influence of CO2 and insolation, as suggested in comment #20. Analysis on the influence of ice sheet-climate interaction is difficult, because the LLN 2D model does not include a Southern Hemisphere. Nonetheless, we have tried to include a qualitative comparison.

   We included in the introduction (p.6 l. 1-7):
   "Our set-up bears resemblance to the LLN 2D climate model, which also consists of a zonally averaged climate model coupled to a one-dimensional ice sheet model. It was used to study ice ages (Gallée et al., 1992; Berger et al., 1998; Loutre and Berger, 2000; Pépin et al., 2001) and the past 3 Myr (Berger et al., 1999). However, our model includes the Southern Hemisphere and Antarctica, and we force CO_2 using a longer ice-core record. We will compare our results with these studies in Sect. 3.5 and 3.6."

   In Section 3.5, discussing the relative influence of CO_2 and insolation, we added (p.20 l. 13-25):
   "A contrasting conclusion was drawn based on work with the LLN 2D climate model (Loutre and Berger, 2000). Using this model, glacial-interglacial variability in good coherence with – but slightly smaller than – observations, was simulated with a constant CO_2 level of 210 (Gallée et al., 1992; Berger et al., 1998; Pépin et al., 2001), comparable to our simulation with 230 ppm CO_2. However, keeping insolation constant at PD level and using the Vostok ice-core record as forcing for CO_2, they hardly found any variability in temperature and sea level (Loutre and Berger, 2000; Pépin et al., 2001). This result is very different from our fixed PD insolation case, where considerable variability is found (Fig. 11; blue line). In addition, keeping CO_2 high at 290 ppm did not prevent glaciation of the NH in the LLN 2D model (Berger et al., 1998) as is the case in our 280-ppm experiment (Fig. 12; orange line). These differences may be attributed to the LLN 2D model having a small sensitivity to GHG changes (Gallée et al., 1992), due in part to ignoring non-CO_2 GHGs."

   And finally, in Section 3.6 we added (p.22 l. 17-28):

"In our model, the influence of ice on the climate is about equally large, as was obtained by Gallée et al. (1992) with the LLN 2D model. This is judged by considering the LGM-PI temperature difference, averaged over the whole Northern Hemisphere (0–90^0 N). By including ice-sheet climate interactions, this difference is enhanced by a factor 4.3 in their model, and by an almost equal factor 4.5 in ours. However, in the LLN 2D model the influence is less localised to the regions where the ice sheets grow. This may be caused by a lower albedo of ice in our model (0.80 as opposed to their 0.85), but this seems unlikely because land albedo is also lower in our model, offsetting the land-ice albedo difference. More likely, differences in atmospheric and oceanic transport play an important role. A thorough analysis, however, is hampered because the LLN 2D model does not include a Southern Hemisphere. Furthermore, Gallée et al. (1992) compared runs with only varying insolation, while we compare runs with CO_2 changes also included."

4) p. 2552, line 19-21: could you specify whether the land cover type is interactive or not?

   It is indeed important for the reader to know that the vegetated land cover in our model is not dynamic.
   We included in the text (p.7 l. 2-4):
   "While the glaciated land cover may vary, the ratio of grassland to forest is specified by present-day conditions and remains unchanged.
   That means vegetation dynamics are not included."

5) p. 2554, line 7: "To obtain a better transient temperature response" Better than? Relative to?

   We altered the text to clarify this (p.8 l. 18-22):
   "Further adjustments with respect to the original set-up of Bintanja (1997) have been made to the diffusion coefficients for the Northern Hemisphere (D_0 (north), set to 0.2x25 $10^{11}m^2\ yr^{-1}$), Southern Hemisphere (D_0 (south), set to $0.5x10^{11}m^2\ yr^{-1}$) and vertical ocean mixing (D_z, set to $5x10^3m^2\ yr^{-1}$). This ensures a better transient temperature response of the ocean, than was obtained by using the original values (see Sect. 3.2)."

6) p. 2554, line 26-27: Why do you choose one ice-sheet being parabolic and the other linear? Shouldn't this choice be variable in time?

   We are considering the initial bed profile here, which cannot be variable in time. Indeed, bedrock adjustment will change the shape of the bed, while the shape of the surface is affected by the mass balance and ice dynamics.
   In the setup used by De Boer et al. (2010), all ice sheets had linear initial bed profiles. In our coupled set-up, the EuIS en NaIS responded better to temperature (a closer match with our tuning target Rohling et al. (2009))

if the initial bed profile was made parabolic. For Greenland and Antarctica there was little difference, so we decided to keep these profiles linear. To clarify, we altered the text (p.9 l. 5-9):
"The continents are initially cone-shaped, thus being uniquely described by the initial height of the center (Hcnt) and the slope (s) of the initial bed. These quantities determine the maximum size of the ice sheet and its sensitivity to temperature (Table 2). For the GrIS, WAIS and EAIS we used linear initial slopes as in De Boer et al. (2010), while for the EuIS and NaIS they are parabolic, to realize better temperature sensitivity (see Sect. 3.2)."

7) Coupling scheme (p.2556, line 19 and further): why do you choose to communicate between the models only every 1,000 years? It cannot be for computation reasons as is usually done, since both models are very fast…

   Our coupled model does not include enough detailed physics to accurately model sub-millennial climate dynamics. Particularly the parameterisation of ocean overturning is relatively crude. Therefore, our desired output timestep is 1,000 years. Tests with shorter communication timesteps down to 100 years did not show significantly different results. While both models are indeed very fast, the increased amount of data transferred slows down the model severely. For this reason, we decided to set the coupling timestep to 1,000 years.
   We included in the text (p.11 l. 4-8):
   "The coupling time step serves to obtain the desired 1000-year output resolution, as the physics in our model are not detailed enough to simulate sub-millennial climate dynamics. Although it is possible to use a shorter coupling time step, it slows down the model and does not significantly change the results."

8) p. 2557-2558: the choice of starting point is rather strange, why are you doing this for the PD and PI climate? We know that the present-day ice-sheet is not at longterm equilibrium with the pre-industrial climate and even less with the present-day … Running 50,000 years with present-day climate (350 ppm) seems extremely strange. I do not think that the justification of looking good with ERA-40 is enough in that case to justify this approach. Using the PI is a relatively standard procedure for coupled climate model, but PD?

   We want to include a test to see if our model correctly simulates PD climate (as indicated by ERA-40). However, we share the referee's concern that present-day ice sheets are not in long term equilibrium with PD climate. Therefore, we repeated the experiment described, but keeping the ice sheets constant in the ZEBCM at their final PI value during the last 50,000 years. We changed Figure 2 and the description of the experiment accordingly (p.12 l. 1-7).
   "First, an equilibrium PI/PD model-test was performed by running the coupled model for 100 kyr, starting with no ice, PI insolation and PI CO2

of 280 ppm as input. After 50 kyr, the input was replaced by PD values (350 ppm CO2). The ice sheet forcing in the ZEBCM was kept constant at the final PI level, because PD land ice is not in long-term equilibrium with PD climate. ”
The outcomes of the new experiment, however, were not very different from the old one. This is not surprising, because in the old experiment PD ice volume was almost the same as PI ice volume; only Greenland shrunk a little bit (~1 m s.l.e.), but this this hardly had an effect on the large-scale) climate.

9) p. 2559, line 13-20: you really need to add a zoomed in version of your figure centred on the last 100 kyr. If not it is quite impossible to relate your text to the figure.

We included the suggested figure (Figure 4b) and proper references to it.

10) p. 2559, line 14-16: “The coupled model captures the 40 kyr-fluctuations in sea level at 90 and 60 kyr ago better than the model of De Boer et al. (2010).” I disagree on the basis of the figure presented. If the Red Sea record if your reference on figure 4 (blue dots) then the best approximation of those dots is the green line (DB10), not the red line …

What we mean here is the strength of the fluctuations at 90 and 60 kyr ago. Sea level increases ~40 m and ~20 m in the data around these times respectively. Indeed De Boer (2010) captures the general trend during the last glacial cycle better. However, the fluctuations around 90 and kyr ago have a much lower amplitude. In our model the strength is simulated better. We have tried to make the text more clear (p.13 l. 19-21):
“Although the general trend is simulated more accurately by De Boer et al. (2010), the strength of the ~40 kyr-fluctuations in the sea level data at 90 kyr ago and 60 kyr ago, is captured better by our coupled model.”

11) p.2559, line 18-20 “In most glacials, the model seems to underestimate the sea level drop.” I am sorry, but I do not see what you are pointing to on the figure. At the LGM, you have the right amplitude, not the right timing ; at the previous glacial maximum, you have too much sea-level drop … not enough at the one before that. So the sentence seems to be inaccurate. In any case, if I remember correctly, the Antarctic ice-sheet is estimated about 12-15 meters of sea-level drop at the LGM: how can this account for large differences in sea-level?

Antarctic ice volume is too stable in our simulation, because of the simplified geometry and lack of ice physics and dynamics. This leads to underestimated Antartic ice volume during glacials and overestimated Antarctic ice volume during interglacials. We now realize that this message doid not come across in the discussion paper. Therefore, we restructured some of the text and made the reference to the glacial periods more specific. This will hopefully also clarify the issue raised in comment #15.

We changed the text to (p.13 l.13 – p.14 l.3):
"The model seems to slightly underestimate the sea level drop at 250, 350 and 440 kyr ago. This may at least in part be attributed to an underestimated sensitivity of the Antarctic ice sheets to temperature changes. Recent estimates of the LGM Antarctic ice volume show an increase of ~8-10 ms.l.e with respect to PI (e.g. Maris et al. (2014)). In our model, a ~4 ms.l.e. size increase of the West Antarctic ice sheet during glacials, is almost entirely offset by a decrease of the East Antarctic ice sheet due to decreased precipitation. This stability is a consequence of the simplified geometry of the one-dimensional ice sheet model and the lack of ice shelf dynamics, as modelling studies using more sophisticated ice sheet/shelf models show larger Antarctic ice-sheet glacial-interglacial differences (De Boer et al., 2013; Pollard and DeConto, 2009)."
And (p.15 l. 4-6):
"Although in our model, we do include Antarctic volume changes, the combined Antarctic ice sheet shows too little variability to produce markedly higher-than-PI interglacial temperatures (see also Sect. 3.1)."

12) p. 2559, around line 25: how I the scaling chosen for d18O / temperature?

We included a reference to the paper discussing the scaling (p.14 l. 1-3):
"A comparison is made between the modeled Greenland temperature anomaly and a 2 kyr-smoothed data reconstruction (Johnsen et al., 1995) based on $\delta^{18}O$ from the GRIP core (GRIP members, 1993) (Fig. 5a)."

13) p. 2560, line 4-6: "In reality, local temperature differences can be much larger than …, especially over areas prone to large albedo and height changes such as Greenland". At least this is incorrect for the last glacial – to present anomaly, since Greenland did not change much since the LGM. This may be true if you melt partially Greenland like during the Eemian though. A better example is the Laurentide ice-sheet in this respect.

We referred to Greenland here, because that is the data we show. Although not a lot, the Greenland ice sheet shows glacial/interglacial variability, amplifying temperature perturbations locally. However, to make it more general we now refer to ice sheets. We also included a reference to data uncertainty, as the referee mentioned in comment #1. The new text reads (p.14 l. 12-19):
"Some uncertainty resides in the interpretation of the data, as Greenland temperature records inferred from different water isotope data show different glacial/interglacial variability (Kindler et al., 2014; Simonsen et al., 2011). However, most likely this underestimation of temperature variability is predominantly a consequence of the zonal averaging in our model, which distributes temperature anomalies over the entire meridional band. In reality, local temperature differences can be much larger than the zonal average, especially over ice sheet areas, which are prone to large albedo and height changes."

14) p. 2560, line 9-16: your model seems to have an overturning influence on

temperature which affects mostly the south, not the north. This is exactly the opposite to what is shown in more complex models (GCMs …). How do you reconcile this aspect? One could argue that this is an consequence of oversimplification of your modelling tool.

It is now clear to us that the overturning circulation, and particularly the effect of the shifting midpoint, needs some clarification. Bintanja and Oerlemans (1996) noted that in the ZEBCM, SH temperatures were more sensitive to ocean overturning strength than NH temperatures. This may be different from GCMs, but also in more complex models there is little agreement on the role of changing ocean overturning (e.g. Weber et al. 2007). Bintanja and Oerlemans (1996) manually introduced decreased overturning strength in the SH to obtain lower Antarctic temperatures, more in line with observations. We wanted a more physical approach, so we used the strength dependence on equator-to-pole water-density difference. This mechanism alone does not lead to low enough Antarctic temperatures as it does not transport heat over the equator. Therefore we combined it with a shifting midpoint of ocean overturning. The shifting midpoint serves to increase inter-hemispheric heat transfer toward the NH, and thereby mimicks the effect of relative increase of Atlantic cross-equatorial flow. Our overturning strength (in both hemispheres) is thus dependent on the equator-to-pole water density difference and on the pole-to-pole density difference through the shifting midpoint. In Section 2.3 we study the robustness of this approach, by testing scenarios with only changing ocean overturning strengths depending on equator-to-pole water-density difference (no midpoint shift), and fixed PD overturning. These tests are indeed somewhat artificial (as mentioned in comment #18), but they provide insight in the mechanisms used in this model. We have tried to explain this better in the text. Agreement with Ritz et al. (2011) on ocean overturning strength, and with a deuterium-based record on Antarctic temperature, justify our approach.

In the model section, we included an explanation why we use the shifting midpoint (p.8 l. 14-17):
"This midpoint-shift mechanism is included to increase inter-hemispheric heat transfer towards the Northern Hemisphere, leading to lower glacial temperatures in Antarctica, that are more in line with the observations (see Sect. 3.2). In Sect. 3.3 we discuss its effect on Northern Hemispheric temperature, sea level and Northern Hemispheric overturning strength."

In the discussion of Antarctic temperature, we add a description of the effect of the changing midpoint (p.14 l. 23-26):
"These shifts have a profound effect on Antarctic climate. By increasing inter-hemispheric heat transfer, they lead to lower Antarctic glacial temperatures that are more in line with observations."

In the discussion of sensitivity to ocean overturning, we explain the two ways in which ocean overturning strength is altered (p.16 l. 14-16):

"In our model, overturning strength is variable in two ways (see Sect. 2.2): it is dependent on the equator-to-pole water-density difference, and on the pole-to-pole water-density difference through the shifting midpoint."

In addition, we add a reference to Bintanja and Oerlemans (1996) and discuss the effect of the varying ocean overturning strength and midpoint-shift. Also, we justify our approach with agreement with Ritz et al. (2011) on ocean overturning strength and agreement with Antarctic temperature data (p.16 l. 11-14):
"The strength of our SH ocean overturning is ~13 and ~27 Sv in glacials and interglacials respectively. Both ranges are similar to the results of Ritz et al. (2011), who used a more detailed ocean model."

And (p.17 l. 28 – p.17 l.5):
"The difference between NH and SH (polar) temperatures in sensitivity to oceanic heat transfer in the ZEBCM was earlier noted by Bintanja and Oerlemans (1996). They used manually-set reduced SH overturning strength to obtain a lower Antarctic LGM temperature. In a more physical way, our approach of changing ocean overturning strength and a shifting midpoint, mimicking the effect of relative increase of the Atlantic cross-equatorial flow, also leads to good agreement with the data (see Sect 3.2).

Cited references:
Bintanja, Richard, and Johannes Oerlemans. "The effect of reduced ocean overturning on the climate of the last glacial maximum." *Climate dynamics* 12.8 (1996): 523-533.

Ritz, Stefan P., Thomas F. Stocker, and Fortunat Joos. "A coupled dynamical ocean-energy balance atmosphere model for paleoclimate studies." *Journal of Climate* 24.2 (2011): 349-375.

Weber, S. L., et al. "The modern and glacial overturning circulation in the Atlantic ocean in PMIP coupled model simulations." *Climate of the Past* 3.1 (2007): 51-64.

15) p. 2560, line 20-25: This is difficult to understand. Do I get correctly that your model simulate a reduction of WAIS during glacials of 4 meters s.l.e. And that the EAIS is increasing by a comparable, opposite 4 meters? If that is correct, then that means that the complete Antarctic contribution is … zero? If yes, please state it clearly. State as well that this is likely to be the opposite expected, since colder conditions should dry out the EAIS and therefore yield to a slight reduction in volume there, while the WAIS is expanding to the shelf break … Also link to my previous remark #11

See reply to comment #11. In fact, what happens in our model, is exactly what you expect, and not the other way around. Hopefully, we have made this more clear.

16)p. 2561, line 10-12. I do not understand why local effects may amplify the deep ocean response. What is the controlling factor of the deep water temperature changes is the surface temperature at their source, not local effects where the deep water is considered?

By local effects, we meant the circulation of deep water, not surface processes. The text was not clear on this, so we changed it (p.15 l. 15-17): "Moreover, the zonally-averaged ocean is only a crude representation of the real ocean system, which has three main basins, and where the deep-ocean temperature is also determined by the zonally-varying circulation of deep water."

17)p. 2561, line 18-27. I have to admit I did not follow your scaling argument nor its underlying physical meaning. "... a 0.2 scaling factor relates local deep-ocean temperature anomalies to local atmospheric temperature anomalies." This cannot be a general rule, since there is in general no relationship between the local atmosphere and the isolated deep ocean. This may only work for deep water formation regions, as mentioned in #16. Could you please explain this better?

The last two sentences are confusing and not in line with the argument we want to make. Therefore, we deleted it from the text. We hope it is clear that that the scaling is based on the model output, and refers to the way deep-ocean temperatures were treated in earlier work. We do not wish to make any physical statement.

18)p. 2562, line 9-25: I am a bit puzzled by the physical interpretation of fixing the location of the overturning and its strength independently. In reality of course it cannot be the case (not even in more complex models) since the deep water strength is dependent on the surface conditions where the formation is possible. How robust can your result for the differing north / south response be here?

See reply to comment #14.

19)p. 2564, line 7: "... only ice volume" I guess you mean "albedo" here?

In fact, ice volume is transferred to the ZEBCM. The ZEBCM then infers albedo, as described in the model-section. It gets a little confusing here, so we changed the text to (p.18 l. 9-13):
"The ISM is still forced with ZEBCM temperatures, but no information (blue line), only surface-height change (orange line), or only ice volume (black line) is transferred back to the ZEBCM. The missing information needed to calculate ice sheet size (albedo), or surface height, or both (see Sect. 2.3), is prescribed as PD values."

20)p. 2565, bottom: this is one location where discussion of the comparison to the LLN-2D results is drastically missing.

See reply to comment #3.

21) p. 2567-2568: very interesting discussion, well done.

Thank you very much.

22) p. 2569, line 7: Peltier & Fairbanks is a sea level reconstruction, not ice-sheet if I am not mistaken. Peltier (2004) is an ice-sheet reconstruction.

Thank you for this careful observation. Indeed this was the wrong reference, we revised it.

Further adjustments

In addition to the changes indicated in the replies above, we have corrected two typo's and a wrong figure:

- Our zeta-value (measure for ocean overturning strength) is 6, not 8 (p. 8 l. 5)
- Table 2 contained an erroneous value for the slope of the North American Ice Sheet
- Figure 8 showed the wrong information (not matching the caption)