Geosci. Model Dev. Discuss., 5, C1436–C1456, 2013
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Geoscientific
Model Development
Discussions

Interactive comment on "Setup of the PMIP3 paleoclimate experiments conducted using an Earth System Model, MIROC-ESM" *by* T. Sueyoshi et al.

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Received and published: 11 February 2013

Reply to the comment by anonymous referee 1

We sincerely thank the referee for providing a thorough review and constructive comments. We also thank for pointing out our language mistakes in the paper. We agree that some changes and clarification would improve the manuscript. We propose to make the revisions outlined below for submission to Geoscientific Model Development.

In the following answers, all reviewer's comments are in blue, and our answers are in black.

In this reply, we answer reviewer's specific points and some technical points which are not language corrections. All of the language correction will be made as reviewer's suggestion in the revised manuscript, and then will ask to the professional English proofreading service again.

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Answer to the Specific comments

- In the paper, the expressions "initial data", "initialization" and "spin-up" should be defined and used in a consistent way. It is not completely clear whether you refer to the whole initialization or spin-up procedure or whether it is the set of data from which the experiment (highlighted in blue in Fig. 3) starts.

We agree that those three expressions are used a little carelessly in the manuscript. We define those as follows:

- we use "initial condition" instead of "initial data", and standarized in the manuscript. In this manuscript, "initial condition" refers to the set of data that is used to start the experiment in general.
- "spin-up" generally refers the integration to adjust the newly given boundary condition.
- "initialization" refers the whole procedure to make the initial condition of the targeted experiments.

We checked the consistency through the manuscript, and will add these explanation in the introduction section.

- I suggest to mention why for 6 ka, seasonal changes are discussed, whereas for 21ka, annual means are presented.

Following the suggestion, we add the following sentences at the end of the chapter 1.

GMDD

5, C1436–C1456, 2013

In our preliminary results, we show the seasonal changes for 6ka, whereas annual means for 21ka are presented. The forcings difference for 6ka is principally the orbital configuration, which causes the change in seasonal pattern of the incoming shortwave radiation. As for 21ka, different equilibrium state is achieved responding to the continental-scale topography and global albedo change, which should be discussed by the annual means.

- ch. 1 p. 2529 l. 9: References to the CMIP table (exp. 3.4,.., Tier 1,..) can be omitted here. It is sufficient to have them in Table 1.
We have revised as suggested.

- ch. 1 p. 2529/2530: "contrasted with attenuation" - I guess what you mean is that at 6 ka, the Asian monsoon is still stronger than today, but due to the ocean feedback, the signal is weaker.
Yes – We have revised the manuscript as follows:

..., and most suggested that there is further enhancement of the African monsoon when the ocean is coupled. As for the Asian monsoon, it is still enhanced compared to 0ka, but the signal is weaker due to the ocean feedback.

- ch. 1 p. 2530 ll. 18-23: This sentence is rather long and would be clearer if split into parts.
We have revised the manuscript as follows:

For example, there are attempts to constrain the climate sensitivity using LGM cooling (Annan et al., 2005; Schmittner et al., 2011). The mechanism

GMDD
5, C1436–C1456, 2013

of the recorded weakening of the Atlantic Meridional Overturning Circulation (AMOC) and the associated cooling in the North Atlantic Ocean (Weber et al., 2007; Murakami et al., 2008) are the subjects since PMIP2. As for the atmospheric circulation, southward shift of the Intertropical Convergence Zone (ITCZ) and weakened monsoon activities (Rojas et al. 2009) have also been studied.

- ch. 1 p. 2531 l. 2/3: "has relatively rich proxy data" => This expression could be improved and combined with lines 13-15 and the References given there (Trouet et al., Mann et al.,...).

Following the suggestion, those paragraphs in p. 2531 was revised as follows:

The LM includes two key climatic epochs: the âĂŽ"Medieval Climate Anomaly" (MCA, ca. 1000-1200 AD) and the "Little Ice Age" (LIA, ca. 1500-1850 AD). Recent progress on proxy-based climate reconstruction has slowly revealed the spatial and temporal extent of those climatic epochs (e.g. Trouet et al., 2009; Mann et al., 2009), which allows better comparison between data and GCM simulations. Such comparison is useful in verifying the ability of a climate model, and assists the understanding of the mechanisms contributing to the climate variability. More specifically, the need to distinguish anthropogenic climate change from natural climate variability at centennial to millennial time scales is one of the key issues of climatology, addressing the social demand for climate research. LM experiments with fully coupled ESM allow discussion of the evolution of the carbon cycle interacting with the climate variation over the last 1200 yr. LM simulations appearing in IPCC 4th Assessment Report (2007) were performed mostly by Earth-system Models of Intermediate Complexity (EMICs) (e.g. Gerber et al., 2003; Goosse et al., 2005). Following some works with coupled models without a carbon cycle (e.g. Gonzáles-Rouco et al., 2003; Ammann et

GMDD
5, C1436–C1456, 2013

al., 2007; Servonnat et al., 2010), a LM experiment using an ESM has been reported recently (Jungclaus et al., 2010), and this study follows the same direction in evaluating the strength of natural variability using up-to-date forcing reconstructions.

- ch. 1 p. 2531 ll. 3-8: This is of rather general nature and could be moved to the beginning of the chapter as a motivation for the whole work.
We have revised as suggested. The sentence is moved to the 2nd sentence of this chapter.

- ch. 2: I would suggest to change the title of this section into "General characteristics of the model".
We have revised as suggested.

- ch. 2 p. 2533 ll. 26-28: "not referring the PFT..." => This needs some clarification. At the end of line 28, you could add "...and neglects..."
Following the suggestion, we have revised as follows:

> In the current configuration, MATSIRO considers the dynamic variations in LAI, while the land index in MATSIRO is fixed, and neglects the PFT predicted by SEIB-DGVM.

We also added the following sentence to p. 2533 l.6., as second sentence of the paragraph.

> It has 11 land cover and soil types (represented as land index; see Fig. 2), which classifies the ground surface conditions and provide the table of parameters for computing physical land processes.

GMDD
5, C1436–C1456, 2013

- ch. 2 p. 2534 ll. 9-13: I think this paragraph (slightly modified) would better fit to the beginning of ch. 3. You could close ch. 2 with "These model components are all activated in our paleoclimate experiments which will be described in the next section.
Thank you for the suggestion. We have revised as suggested.

- ch. 3.1.1 p. 2534 l. 20-22: => "..land index for MATSIRO (Fig. 2a) is based on ....croplands than at present to account for a plausible..." I would also add half a sentence on what the land index actually is. Looking at Fig. 2 and section 2 where 13 PFTs are mentioned could cause some confusion. Can the individual grid cells have fractional contributions from, for instance, sea ice, land,.... Are 0-category grid cells set to "undefined" in MATSIRO? 0 (white) should be included into the colorbar. Where does the "plausible 1850 distribution" with "less croplands" come from?
Following the suggestion, we add the explanation on the land index and PFT fpr DGVM. Those explanation went to the sentence of p.2533 l.26-28 as shown above, and this part (l.20) is revised as follows:

> The PI land index in MATSIRO is shown in Fig. 2a. Plausible distribution in 1850 is parepared based on modern vegetation types and historical land use data.

Also, 0-category denotes the sea surface. We update the colorbar with 0 (white), and the description of "less cropland" mistakingly introduced from other manuscript. We corrected the expression.

- ch. 3.1.2: I would suggest to change the title of this section into "Spin-up for the PI simulation"
We have replaced "PI data" with "PI simulation" as suggested, and for consistency, changed as "Initialization of the PI simulation".

GMDD
5, C1436–C1456, 2013

- ch. 3.1.2 l. 3: Does it mean that the first 380 years were calculated as an AOGCM, with all the other components specified in Fig. 1 switched off?
No, the manuscript was not precise. The expression here should be corrected as follows:

> Starting with the "low-top" version of MIROC-ESM with 20 vertical layers in the atmosphere, a 380-yr integration was performed. Spin-ups of land ecosystem (2000 years) and ocean ecosystem (1245 years) were separately performed beforehand and integrated. 50-yr integration followed with an atmosphere with 80 vertical layers.

- ch. 3.1.2 l. 10: This contradicts the discussion of trends in later sections where you state that there is still a trend in PI. So the last paragraph from section 3.1.3. should be moved to 3.1.2. How large is the trend in the last 100 years of PI which are used for the analysis?
Following the suggestion, we have revised the manuscript and added the following sentence.

> The trend is +0.05°C in the last 100 years which are used for analyses.

- ch. 3.1.3: This section should be carefully reformulated. To clearly demonstrate the model performance, a comparison of a present-day simulation with modern observations would be required. One cannot expect a one-to-one correspondence of a PI simulation with modern-day observations.
We agree the reviewer's comment that the PI climate is not one-to-one comparable with the present observation. However, since PI is used as a baseline to compare the paleoclimate experiments with proxy data, we would like to show the characteristics

GMDD
5, C1436–C1456, 2013

of PI climate in this section. Additionally, as for the model performance, detailed comparison of a present-day simulation with modern observations is already presented in Watanabe et al (2011).
Considering thesse situation, we have revised the section 3.1.3. as follows:

> The climate of PI experiment is briefly presented comparing with the modern climate in this section. Although PI simulation is not necessarily comparable with modern-day observations, the aim of this section is to sescribe the characteristics of the PI climate, which is commonly used as a baseline to compare the paleoclimate experiments with proxy data. A detailed comparison of a present-day simulation with modern observations was already presented by Watanabe et al. (2011) using CMIP5 historical experiment. PI mean value differs with the 20th century ensemble mean analyzed in Watanabe et al. (2011) by ca. 0.3°C in global mean surface air temperature, but spatial pattern of the bias is quite similar to each other. From these backgrounds, we discuss the PI-observation in the followings.
>
> The sea surface temperature (SST) is shown in Fig. 4 in comparison with the World Ocean Atlas Temperature data (WOA, 1998; Levitus et al., 1998). PI simulated reasonable SST distribution. In global average, slightly cooler SST in PI is reasonable considering the 20th century warming. The distribution of simulated precipitation in comparison with observational data (Xie and Arkin, 1996) for boreal summer (JJAS) and winter (DJF) are shown in Fig. 5. PI simulates a reasonable representation of the ITCZ and precipitation distribution of the monsoon area. The other basic climate variables (not shown) are generally well represented.
>
> Further detailed comparison with the observational data should be done with the 20th century part of the LM experiment, but it is beyond the scope of this paper, aiming the description of the experimental settings.

GMDD
5, C1436–C1456, 2013

- ch. 3.2.1: Regarding the presentation of Fig. 6, a discussion of the main features would be helpful for the reader.
we added the following sentences at the end of the section:

> Incoming shortwave radiation is increased in boreal summer and decreased in austral summer. As for annual radiation, there is an increase in the high latitude, and a decrease in the tropics. Through these changes, seasonality is enhanced in the northern hemisphere and weakened in the southern hemisphere, especially at mid-latitudes.

- ch. 3.2.2 p. 2536: Do you mean that the spin-ups for PI and 6ka were the same but that the 6ka experiment was branched off from one of the 530 PI years from the "PI Experiment phase" given in Fig. 3a?
Yes – we follow your sentence, and revised the p. 2536 ll.14-15 as follows:

> 6ka started as the same experiment as PI, and brached off from the midst of the 530 PI years. 250th year of the PI experiment run was used for the initial condition of the 6ka. GHG levels and the orbital parameters were set to the 6ka values to start the initialization of 6ka.

- ch.3.2.2 p. 2536 ll. 22-24: What does "physical quantities from the last 25 years...recursively adapted for 2000 years" mean? This is not clear to me.
We are sorry for bad way of explanation. We used the 25 years climate forcing repeatedly for 2000 years. We have revised the manuscript as follows:

> Firstly, 100 yr of integration was performed using the 6 ka GHG concentrations and orbital forcing using MIROC-ESM. Using the last 25 years of this

GMDD
5, C1436–C1456, 2013

output (i.e. last 25 yrs of âĂŽÄú6 ka (1850 land use)âĂŽÄù in Fig. 3b), we ran the off-line model for 2000 simulation years with the cycling 25 years climate forcing. This procedure is following the protocol of C4MIP (Fung et al., 2000; Cox et al., 2002).

- ch. 3.2.2 p. 2537 l. 2: "200 simulation years" - This confuses me when looking at Fig. 3b. Text and Figure are not consistent here.
Text was wrong here. It should be "180 simulation years"

- ch. 3.2.3 p. 2537 ll. 5-7: This would become more clear with a slight modification of Fig. 3.
We tried to update Fig.3 to help understanding (attached).

- ch. 3.2.3 p. 2537 last paragraph: If you show Figures, you also need to discuss a few aspects. Therefore I would suggest to briefly say something about the land-sea contrast in the temperature response or about the increase in precipitation over the Indian Ocean and the tropical Atlantic.
We realized that the structure of this section was quite confusing. 2nd and 3rd paragraph discussed the temperature response and the precipitation change already, but again mentioned those in the last paragraph. We believe that the last paragraph is unnecessary, so we removed the paragraph and moved the contents to the previous paragraphs.
(Please let me know if my revision is going to wrong direction.)

- ch. 3.2.3 p. 2537 l. 11: From Fig. 6a, wouldn't one expect a warming of the tropical oceans for JJAS? Is it a delayed response, as raised in earlier papers (I think Amy Clement has worked on this, but also others)?
As indicated, tropical oceans for JJAS doesn't show the warming despite the increased

GMDD
5, C1436–C1456, 2013

incoming radiation. The lag of the response is considered to be the main reason. (Braconnot et al., 2000) indicated that two month lag for warming and 2-3 month lag for cooling. This JJAS response agrees with the PMIP2 multi-model ensemble (Braconnot et al., 2007).

> Following this discussion, we revised the manuscript as follows (p.2537 ll.10-12): The tropical oceans are slightly cooled despite the increased incoming radiation, which is mainly due to the 2-3 months lags in ocean response (Braconnot et al., 2000). The result is consistent with the PMIP2 multi-model ensemble (Braconnot et al., 2007).

- ch. 3.3.1 p. 2538 l. 4: You should mention a little bit more about the salinity setting / initialization according to PMIP and why you did not change it.

Following the suggestiion, we revised the last sentence of the paragraph as follows:

> PMIP3 protocol recommend to set the mean ocean salinity +1 PSU everywhere at the beginning of the simulation. In our experiment, however, the salinity setting was unchanged from the PI condition. Salinity-modified experiment remains to be done in near future. Change in sea water density leads the change in mixing layer, which may result the air-sea interaction.

- ch. 3.3.1. p. 2538 l. 16: Do the different reconstructions mainly vary in elevation or extent?

Difference is mainly in elevation, or, more precise, shape of the ice sheet. The locations of the peaks (domes) or ridges are the main differences. The extent is almost. We added the mention for that.

- ch. 3.3.1 p. 2538 l. 20: "as other boundary data" - Please be more specific.

We changed the sentence as follows:

GMDD

5, C1436–C1456, 2013

The horizontal resolution of the data provided in PMIP3 (1° x 1°) was re-gridded to T42 resolution and to the spectral wave number space in order to adjust to the atmosphere and land component.

- ch. 3.3.1 p. 2538 l. 21: Is there a justification/reference for the 90 m in contrast to the "classical" 120 m?
Sea level change was set to 90m in order to maintain the consistency with the PMIP3 topography reconstruction (i.e. ice sheet topography). We will add the mention for that.

- ch. 3.3.1 p. 2538 l. 28: "as the surrounding grids". I guess you mean "grid cells" here. How did you do this? Did you chose the vegetation from neighbours in the zonal or meridional direction? Was some weighting applied in the selection process?
We intended to mean the "neighbour grid cells" in the zonal direction. If the grid cell has more than two neighbour cells, the majoroty was taken. Those sentences are added to the manuscript.

- ch. 3.3.2 p. 2539 l. 3: "of LGM experiments" - Is it actually from several LGM experiments or a set of restart files for the different model components from ONE previous LGM simulation using MIROC?
Using of plural form was a mistake of the author. It was from one previous slimulation. We corrected the sentence as follows:

The initial condition of the ocean physical field is inherited from the result of a LGM experiment with PMIP2 experiment protocol by an earlier version of MIROC ....

- ch. 3.3.2 p. 2539 l. 26: It is not completely clear what you mean by "both options

GMDD
5, C1436–C1456, 2013

were applied".
This sentence was wordy. We revised the l.24-26 as follows:

> As recommended in the PMIP3 protocol, we changed the surface elevation gradually (in two stages for MIROC-ESM), and adjusted the initial pressure field to the LGM surface elevation in starting the integration under the new topography.

- ch. 3.3.2 p. 2540 l. 13: What do you mean by "was given recursively to the off-line model"?
We are sorry for bad writing. we meant that the off-line model was forced with LGM climatology. The sentence is revised as follows:

> Starting from the PI condition, the off-line model (Chikamoto et al., 2012) was integrated for 3500 model years with the LGM climate forcing.

- ch. 3.3.2 p. 2540 l. 21: For the sake of completeness, I'd mention the final 100 model years used for analysis.
As suggested, we add the sentence to the end of the section, mentioning that the final 100 model years used for analysis.

- ch. 3.3.3: It would be nice to discuss briefly the land-sea contrast. Units (°C) are missing in this section.
We corrected the missing units. Following the suggestion, we add the discussion on land-sea contrast as follows (p.2540 l.26):

> Comparing the temperature response over land to the ocean, the continents cooled more than the ocean as earlier works (e.g. Braconnot et al.,

GMDD
5, C1436–C1456, 2013

2007, Braconnot et al. 2012, Laîné et al. 2009). This relationship quantitatively agrees with other climate model outputs and proxy reconstructions (Schmidt et al., 2012).

- ch. 3.3.3: The large-scale precip. changes in the South Pacific Convergence Zone (Fig. 11) could be discussed.
Following the suggestion, sentence is added as follows (p.2541 l.3):

The exception is the increased precipitation along the South Pacific convergence zone (SPCZ), which is consistent with the earlier works (e.g., Toracinta et al., 2004; Yin and Battisti, 2001). Toracinta et al. (2004) suggested that the difference in surface temperature gradients plays a major role to enhance the SPCZ during LGM.

- ch. 3.3.3: How has the MOC been defined? "various forcing conditions" (l. 10) is very vague. How does the AMOC temporal evolution look like throughout the experiment?
AMOC was defined by the maximum value of the stream function of the Atlantic meridional circulation at 30 degrees north. This explanation is added to the manuscript.
Also the following sentence about the temporal variation of the AMOC is added (p.2541 l.6):

LGM AMOC shows the stronger variation in amplitude ($\pm$ 3-5 Sv), with longer in period (5-8 years) while PI AMOC shows weaker variation ($\pm$ 1-2 Sv, 3-4 years).

As for l.10, we have revised as follows:

GMDD
5, C1436–C1456, 2013

Further investigations are required to understand its behavior.

- ch. 3.4.1 p. 2541 l. 24: It would be good to provide a bit more details here (temporal resolution, implementation in the model).
We have added the following explanation.

> Orbital forcing is based on a provided table of parameters, the value is updated every year on January 1st. The orbital parameter affets the seasonality of incoming short wave radiation, while total intensity is given by solar forcing data.

- ch. 3.4.1 p. 2542 l. 5: "scaled linearly" - Is this part of the authors' work?
No. Intention was to explain the method of reconstruction of WLS and DB. We think it's better simplify this sentence. We have revised as follows:

> WLS is based on a spectral reconstruction and on a flux transport model using the observed sunspot record, while DB is based on an Antarctica stack of 10Be records. To splice WLS to DB, the amplitude of DB is scaled to the modern-to-Maunder Minimum TSI in the WLS reconstructions.

- ch. 3.4.1 p. 2542 l. 11/12: Is there a specific reason for this?
We updated the manuscript as follows:

> The parameterization for solar-derived ozone variations suggested by the PMIP3 protocol was not used either, to maintain the consistency with the already-conducted 20th century experiments.

GMDD
5, C1436–C1456, 2013

- ch. 3.4.1 Regarding the volcanic forcing, please be more specific on which variable is provided in the datasets by Crowley and Gao (AOD?). The description of how the data are used in the model is not fully clear to me.
We have revised the paragraph as follows, with clear mention to that the reconstructed data is provided as AOD table.

> Two reconstructions of the volcanic forcing: Gao et al. (2008) and Crowley et al. (2008, 2012) are provided by the PMIP3 protocol, as time series of aerosol optical depth (AOD) variation. Both datasets are based on polar ice cores, but differ in their selection of ice cores, and the Gao data show stronger forcing in general. AOD estimates are based on a correlation between sulphate in the Antarctic ice cores and satellite AOD data, which was calibrated with the 1991 eruptions of Mt. Pinatubo and Cerro Hudson (Sato et al., 1993). Aerosol size estimation is based on Pinto et al. (1989). In our experiment, Crowley data were used (Fig. 12a). The volcanic forcing is calculated using the AOD at 0.55 Œ°m and the effective radius calculated in MIROC-ESM for scaling. The data are provided at four latitude bands of equal area. After year 1850, Sato et al. (1993, updated) is used for AOD data.

- ch. 3.4.1 p. 2542 l. 25: The authors decided to perform the experiment as a "prognostic CO2 experiment". You could put this into perspective by briefly mentioning why you decided this and whether this is an exceptional approach compared to other CMIP5 groups.
Since we focused on the carbon cycle behavior in LM, both prognostic CO2 (i.e. prediced CO2) and diagnostic CO2 (i.e. CO2 given) experiment were planned. We could submit prognostic CO2 only, however, due to the limitation of the resources. Since the majority in CMIP5/PMIP3 LM runs are "CO2 given" experiments, it resulted

GMDD
5, C1436–C1456, 2013

to be an exceptional approach among CMIP5/PMIP3 experiments. We added the following sentence in p.2542 l.27 (after the parenthesis) to explain this.

> ...) aiming to discuss the response of carbon cycle to the solar- and volcanic- forings during LM. Since the majority in CMIP5/PMIP3 LM runs are "CO2 given", it resulted to be an exceptional approach among CMIP5/PMIP3 experiments.

- ch. 3.4.2 p. 2543 l. 16: Is this resetting procedure a common approach in the "millennium community"?

The basic idea of this resetting is following the the protocol of C4MIP (Fung et al., 2000; Cox et al., 2002). Since not many "CO2 prognostic" LM runs are done already, we cannot say that it is not the common approach, yet we believe that the CO2 should be treated in a similar manner with C4MIP. We have added the mention to the C4MIP.

- ch. 3.4.3 p. 2544 l. 3-4: How do you separate anthropogenic and natural parts? What would be the amplitude of the trend?

It was not precise expression in the manuscript. Here we are simply comparing the amplitude of the warming until 19c and 20c. We removed "anthropogenic" from this sentence, and also put the amplitude of the trend in the brachet:

> The warming drift is visible during the period until the 19th century (ca 0.09°C/100yr), while the warming during the 20th century is more pronounced (ca 0.5°C/100yr).

- ch. 4 final sentence: remove "we hope" and find a better link to the previous sentence

Following suggestion, we have revised as follows:

GMDD

5, C1436–C1456, 2013

Further analysis of each setting, as well as multi-model ensemble analysis will follow this paper to contribute to the CMIP5 modelling activity.

- solar forcing for LM (Fig. 12b): Why are there two parts where high frequency components seem to be missing (around 1400-1500 and around 1620-1650)?
It is due to the reconstruction methodology. They assumed the amplitude of high frequency components (i.e. ca.11-year period variation) is dependent to the solar activity. This resulted the very small amplitude in high frequency components during 15c and 17c. We added this explanation in section 3.4.1,

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Answer to the Technical corrections and language suggestions

As mentioned before, all of the language correction will be made as reviewer's suggestion in the revised manuscript. Authors are very thankful to the reviewer's work for correction. Here we answer to some of the points which needs additional explanation.

p. 2532 l. 3: "based on a global climate model" => "based on the MIROC AOGCM" (introduce the MIROC acronym earlier in the paper)
We revised the p.2528 l.23 as follows:

> Here we describe three paleoclimate experiments using MIROC-ESM, an Earth System Model (ESM) based on a global climate model MIROC (Model for Inter-disciplinary Research on Climate). The experiments are conducted as a contribution for the Intergovernmental Panel on Climate Change 5th Assessment Report (IPCC AR5).

p. 2542 l. 9: "integrated TSI over the spectra" => please rephrase
We revised the sentence as follows:

GMDD
5, C1436–C1456, 2013

so that the integrated TSI over the spectra agrees with the reconstructed total TSI.

p. 2546 l. 12: Do you mean "typical timescales" instead of "time constants"?
Yes – "time constants" was wrong expression. We revised the manuscript following the suggestion.

References:

We corrected the errors pointed out by the referee.

Table and Figures:

Fig. 6: What exactly is shown here? Net SW, incoming SW.... Try to use the same contour intervals for the colouring and the contour lines. The caption is missing a) and b).
It shows the incoming shortwave radiation. We'll update the figure as suggested, and figure caption

Fig. 10: Is it 2m air temperature over the oceans or SST (Fig. 7 has "surface temperature" as well, but the Figure title says "2m air temperature")? I also would suggest a finer resolution for values between -2 and 0°C. This would help for the reference to MARGO on p. 2540, l. 26, as well.
We are sorry for inconsistency. It means 2m air temperature over the oceans. In Fig. 7, the plot is 2m air temperature. We correct the figure captions.
Fig. 7: The 2m air temperature change between 6ka and PI, for (a) JJAS and (b) DJF.
Fig. 10: The annual mean 2m air temperature change between LGM and PI.

GMDD
5, C1436–C1456, 2013

All Other indicated typos and errors were corrected following the referee's suggestions.

Interactive comment on Geosci. Model Dev. Discuss., 5, 2527, 2012.

GMDD

5, C1436–C1456, 2013

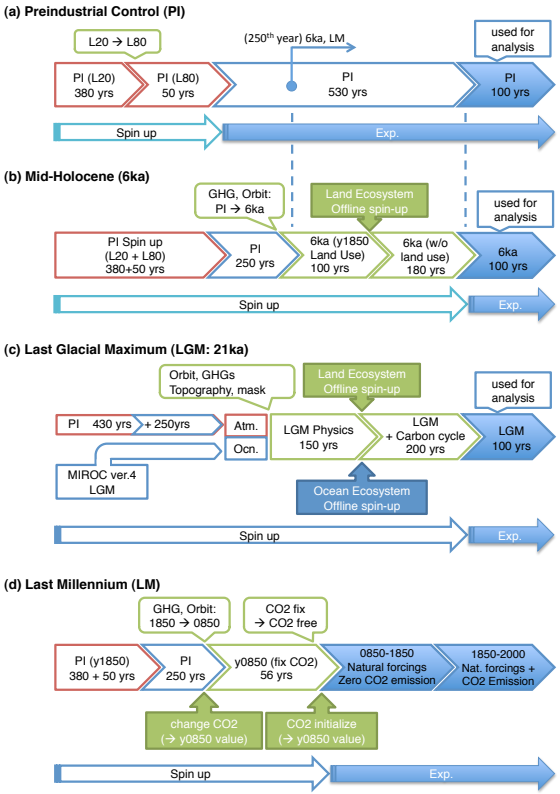

Fig. 1. Update of Fig.3. The procedure of initial data preparation: (a) Pre-industrial control: PI, (b) Mid Holocene: 6 ka, (c) Last Glacial Maximum (LGM): 21 ka, (d) Last Millennium: LM.

GMDD
5, C1436–C1456, 2013

Interactive Comment