Clim. Past Discuss.,
https://doi.org/10.5194/cp-2018-145-AC5, 2019

Climate of the Past Discussions
Open Access

Interactive comment on "Mid-Holocene climate change over China: model-data discrepancy" *by* Yating Lin et al.

Yating Lin et al.

haibin-wu@mail.iggcas.ac.cn

Received and published: 5 February 2019

We greatly appreciate the constructive comments and suggestions on the previous version of the manuscript from Prof. Bartlein. We have attempted to address every point raised. The following is the outline of the changes we have made, with reference to the order of the comments made by the referee.

General comments from the referee:

This paper presents an ambitious attempt at comparing simulations from the CMIP5/PMIP3 "midHolocene" archive with a new synthesis of fossil-pollen data for China. The pollen data are used in two ways: 1) to develop a set of quantitative reconstructions of several climate variables using an inverse-modeling approach (to

Printer-friendly version

Discussion paper

compare with the climate-model output), and 2) to develop a map of “megabiomes” for present and 6 ka (for direct comparison with vegetation simulated by BIOME4 using climate-model output). The paper shows that there is a considerable mismatch between the reconstructed and simulated climates and vegetation. The authors attribute this mismatch to experimental-design issues, in which vegetation and land-cover data in the climate models were fixed at present-day values, thereby limiting the ability of the climate models to correctly represent the potential impact of vegetation (and surface water- and energy-balance) feedback in the paleo simulations.

I have two reservations about the results and conclusions: First, there is insufficient information on the protocols adopted for generating the both present-day and paleo vegetation, as well as the paleo reconstructions. As for vegetation, Fig. 5 shows that there are large mismatches between the observed and simulated modern vegetation. These would naturally arise if the climate-model output were used directly to simulate the vegetation. We know that at their current resolutions, there is still considerable bias in present-day (or PI) climate simulations, and there is no reason to believe that those bias are the same in paleo simulations (or that they somehow go away). My impression of Fig. 5 and S7 is that those biases in simulated climate are indeed large, possibly swamping the real vegetation change, and so we’re not really getting much insight into the nature of the mid-Holocene climate simulations, but instead learning about modern-day bias. As for reconstructed climate, despite the author’s assertion otherwise, there is also considerable bias in the inverse-model reconstruction approach (Table 6). Only for Pjan does the regression between observed and fitted values not differ from one with a slope of 1.0 and an intercept of 0.0. It is not immediately clear how that bias might affect the reconstructed climate, but it reinforces the necessity of looking at the uncertainties in the reconstructions. Again, we may be learning more about the inverse approach than about model-data mismatches. Second, the attribution of the mismatches to the experimental design of the CMIP5/PMIP3 simulations, while plausible, is not really supported by any direct hypothesis tests, or by the consideration and dismissal of alternative hypotheses. The correlation between the temperature responses and cloud-cover feedback (Figure 7) implicates at least one: inadequate simulation of atmospheric circulation as it may influence moisture flux or precipitation-generating mechanisms. I think that if the questions related to the protocol for data-model comparisons are answered, and due consideration given to other possible mechanisms for the mismatch, then the paper will ultimately be publishable.

Response to the major comments: Thanks for the very important comments, in conclusion, two main questions are proposed here:

1. The insufficient information on protocol for model-data comparison of vegetation.

RE: The referee is right when he pointed out that using climate for PD or 6 ka, there is a large mismatch between vegetation reconstructed by BIOME4 from model simulation and vegetation reconstructed from pollen data. Our aim here is to test whether the sensitivity of PMIP3 mid-Holocene simulations, mainly driven by insolation changes, may explain the vegetation changes observed from data. The trend depicted by all the models is cooler conditions during winter and warmer in summer which is consistent with a linear response to orbital forcing for 6 ka. On the contrary, the dataset shows for the seasonal response a warming during both seasons. We agree with Prof. Bartlein that the attribution of those non linear responses to vegetation is not really explained in our original manuscript. This is due to the fact that each model has different ways to account for vegetation, therefore our explanation was plausible but too speculative. To be able to convince the referee with quantitative arguments, we have conducted a supplementary experiment to demonstrate that our mechanism was appropriate. In this revised version, we succeeded to conduct such test in CESM version 1.0.5. The CESM version 1.0.5, developed at the National Center for Atmospheric Research, is a widely used coupled model with dynamic atmosphere (CAM4), land (CLM4), ocean (POP2), and sea-ice (CICE4) components (Gent et al., 2011). Here, we use $\sim 2^\circ$ resolution for the CAM4, configured by $\sim 1.9^\circ$ (latitude) $\times$ 2.5° (longitude) in the horizontal direction and 26 layers in the vertical direction. The POP2 adopts a finer grid, with a nominal 1° horizontal resolution and 60 layers in the vertical direction. The land and sea-ice components have the same horizontal grids as the atmosphere and ocean components, respectively. Two experiments were conducted, including a mid-Holocene (MH) experiment (6 ka) with original vegetation setting (prescribed as PI vegetation for MH) and a MH experiment with reconstructed vegetation (6 ka_VEG). In detail, experiment 6 ka used the MH orbital parameters (Eccentricity=0.018682; Obliquity=24.105°; Angular precession=0.87°) and modern vegetation (Salzmann et al., 2008). Compared to experiment 6 ka, experiment 6 ka_VEG used our reconstructed vegetation in China. Except for the changed vegetation, all other boundary conditions were kept unchanged in these two experiments, including the solar constant (1365 W m−2), modern topography and ice sheet, and pre-industrial greenhouse gases (CO2 = 280 ppmv; CH4 = 760 ppbv; N2O = 270 ppbv). Experiment 6 ka was initiated from the default pre-industrial simulation and run for 500 model years. Experiment 6 ka_VEG was initiated from model year 301 of experiment 6 ka and run for another 200 model years. We analyzed the computed climatological means of the last 50 model years from each experiment here. The new-added Fig.8 in manuscript (enclosed below as Fig. 1) shows the climate anomalies between two simulations (6 ka_VEG minus 6 ka), for both annual and seasonal scale. For temperature, it's clear that the 6 ka_VEG simulation reproduces the warmer annual (∼0.3 K on average) and winter temperature (∼0.6 K on average), especially the winter temperature. For precipitation, the reconstructed vegetation leads to higher annual and seasonal precipitation, which can also reconcile the discrepancy of increase amplitude for precipitation during MH between model-data (data reproduced larger amplitude than model, revealed by our study). This new result strongly suggests vegetation changes may explain a part of the mismatch, which is consistent with our proposal in this study. Nevertheless, there are certainly other possibilities and indeed models that better captured the hydrologic cycle and enhance the precipitation/ evaporation pattern could also explain differences between model and data. Each model has different sensitivity to the boundary change, further work will be carried out in more models to test the influence of vegetation on climate, this is an ongoing work. This response is on pages 16-17, lines 382-400 in the revised version.

2. More information about the considerable bias in the inverse-model reconstruction approach.

RE: In the climate reconstruction based on pollen data, the inverse-modeling approach considers the impact of CO2, and provides sound logic to cope with the no-analogue problem. Moreover, the consequence of changing seasonality of climate forcing or climate response are also taken into account in the inverse-model reconstruction. The quantitative reconstructions derived from pollen data of Eurasia, Africa and Europe (Wu et al., 2007) at the LGM and the mid-Holocene, confirm the ability of the inverse vegetation model (IVM) method to provide spatially coherent patterns of palaeoclimate that are generally in agreement with previous reconstructions from climate proxies. However, the IVM approach is not a panacea. First, it is highly dependent on the quality of the vegetation model BIOME4, because of the particularity of vegetation types in the monsoon region of China, the BIOME4 needs further improvement of vegetation simulation accuracy in this area. This possible bias in simulating vegetation will lead to uncertainty in reconstruction. Second, the output of the model is not directly compared to the pollen data, the conversion of BIOME4 biomes to pollen biomes by the transfer matrix may add the source of uncertainty in reconstruction. These possible bias in climate reconstruction derived from IVM are described in the new version of manuscript (on page17 lines 401-411 in revised version). For the potential uncertainties on data reconstruction, besides the Table 6, we added more information in Table S4 in supplementary information, we gave the climate variables reconstructed from IVM at each site. Moreover, we also showed the bias on data reconstruction by giving the median value (for instance, column named MTCO) and values indicating the 5% (MTCO1)-95% (MTCO2) uncertainty bands.

Specific comments from the referee: Abstract: The abstract fails to disclose conclusions of paper.

RE: We modified the abstract as following: The mid-Holocene period (MH) has long been an ideal target for the validation of Global Circulation Model (GCM) results against reconstructions gathered in global datasets. These studies aimed to test the GCM sensitivity mainly to the seasonal changes induced by the orbital parameters (precession). Despite widespread agreement between model results and data on the MH climate, some important differences still exist. There is no consensus on the continental size of the MH thermal climate response, which makes regional quantitative reconstruction critical to obtain a comprehensive understanding of the MH climate patterns. Here, we compare the annual and seasonal outputs from the most recent Paleoclimate Modelling Intercomparison Projects Phase 3 (PMIP3) models with an updated synthesis of climate reconstruction over China, including, for the first time, a seasonal cycle of temperature and precipitation. Our results indicate that the main discrepancies between model-data for MH climates are the annual and winter mean temperature. A warmer-than-present climate condition are derived from pollen data for both annual mean temperature (∼0.7 K on average) and winter mean temperature (∼1 K on average), while most of the models provide a linear response driven by the seasonal forcing (a decreased annual mean temperature with a warmer summer and colder winter). By conducting simulations in BIOME4 and CESM version 1.0.5, we show that to capture the seasonal pattern reconstructed by data, it is critical to assess surface processes. These results pinpoint the crucial importance of including the non-linear of the surface water and energy balance to vegetation changes.

Line 14: “proxy reconstructions” Aren’t the reconstructions used here “real” reconstructions? I understand the notion of paleoclimatic evidence that can be used as a “proxy” for climate or other phenomena (like land cover). But the reconstructions here are actual reconstructions, not a stand-in or substitute for reconstructions.

RE: We have deleted the word “proxy” in Line 14 in revised version.

Line 18: “continental size” Are you referring to the area of the temperature anomaly or to terrestrial as opposed to marine responses?

RE: We are referring to the area of the temperature anomaly.

Line 20: New definition for PMIP?

RE: Corrected (on page 1 line 20 in revised version).

Line 22: "a seasonal cycle. . ."

RE: Corrected as "a seasonal cycle of temperature and precipitation" (on page 1 line 22 in revised version).

Line 25: "access surface processes" I don't know what this means.

RE: Sorry for the wrong spelling, it should be "assess" (on page 1 line 25 in revised version).

Line 27: "non-linear process associated with vegetation changes in hydrology and radiative forcing" Does this mean "non-linear responses in hydrology and radiative forcing to vegetation changes"? "Radiative forcing" in the context of the midHolocene experiment is usually reserved for describing the insolation forcing, so an alternative expression might be "non-linear response of the surface water and energy balance to vegetation changes" (which is what I think the paper is arguing for).

RE: Thanks for the suggested expression, we have corrected it (on page 2 line 30 in revised version).

Line 34: This definition of the age of the mid-Holocene is inconsistent with what is actually used in the paper (line 101). It might be good to distinguish between the midHolocene time slice, and the "midHolocene" CMIP5/PMIP3 experiment, throughout the paper.

RE: According to IntCal13 (Reimer et al., 2013), the mid-Holocene time slice 6000±500 14C yr BP is about 6800 Cal BP (the average value), which is not totally consistent with the "mid-Holocene" used in CMIP5/PMIP3 experiment (6000 Cal BP). We agree with the reviewer that this is a problem in model-data comparison for paleoclimate, but for a better comparison with BIOME6000 (which defined as 6000±500 14C yr BP), we decided to choose the pollen data at 6000±500 14C yr BP in our study. Thanks to the comment, we will take care of this inconsistency and make better comparison of time slice in the future work.

Line 36: “an increase in insolation in the seasonal cycle” Replace with “an increase in the amplitude of the seasonal cycle of insolation. . .”

RE: Corrected. On page 2 line 40 in revised version.

Line 38: “climate response to changes in the seasonal distribution” It’s not the response to the seasonal variations of insolation that you’re looking at here, but instead the response to changes in the distribution.

RE: Corrected. On page 2 line 43 in revised version.

Line 42: “consistency of the dataset incorporating different proxies” I don’t know what that means. ãĂĂ RE: We have changed it into “much work has been done to reconstruct the paleoclimate change based on different proxies” (on page 2 line 46 in revised version).

Line 45: Again, the data are real, not proxy.

RE: Corrected (on page 3 line 43 in revised version).

Line 47: “the source of discrepancies. . .”

RE: Corrected it as “the source of discrepancies between model and data . . .” (on page 3 line 52 in revised version).

Lines 50-51: But see Marsicek et al. (2018, Nature) the “Holocene conundrum” apparently arose from comparing apples and oranges. A different example might be more convincing.

RE: Yes, the reconstruction used in Marcott et al. (2013) is mainly from the marine records (∼80%) and the cooling trend is largely associated with North Atlantic. And

Printer-friendly version

Discussion paper

in Marsicek et al. (2018), they show a better consistency of temperature between model-data for Europe and North America continents during Holocene based on 642 sub-fossil pollen data. The different trends of pollen- and marine-based reconstruction indicate the spatial variability of annual temperature change during MH over the globe, which has already been investigated by Bartlein et al. (2010). Here, we use Liu et al. (2014) to pinpoint the decreased annual temperature in MH simulated by model, compared to PI.

Line 62: The sheer expanse of the country. . . Why should the synthesis of paleoclimatic data or simulations necessarily be restricted to political subdivisions? Extending the area of the comparison deeper into the interior of Eurasia would generate a bit more “leverage” in comparing the data and models, but I understand the logic of restricting the analysis to China.

RE: For this study, we only focus on China, but we agree that extending the area into Eurasia or globe is more comprehensive, which is carrying out by other colleges in our group now.

Line 64 (and elsewhere). The article “the” is required before “MH” in this context (i.e. when “MH” is being used as a noun). Elsewhere, as in line 55, where “MH” is used as a modifier of another word (“precipitation” in this context), the article is not used.

RE: Corrected.

Line 66: “warmer and wetter than present. . .”

RE: Corrected it as “warmer and wetter than present conditions” (on page 3 line 71 in revised version)..

Line 73: “colder than the baseline” What baseline? Present-day or preindustrial? RE: The baseline period of 36 models are different. 10 of 36 models refer to present-day, while others refer to preindustrial.

Line 75: “This study” Which study? Reword as “That study. . .” or more explicitly “Jiang

Printer-friendly version

Discussion paper

et al. (2013) were the first to point out the model-data discrepancy over China during the MH, but the lack of seasonal reconstructions in their study limits comparisons with simulations."?

RE: Corrected (on page 4 line 81 in revised version).

Line 83: Bartlein et al. didn't synthesize land-cover changes.

RE: Corrected.

Lines 86-91: The terminology here needs to be sorted out. The "process-based biogeographic model" alluded to here is BIOME4, and it is employed in making inferences about past climates using an "inverse modeling through iterative forward modeling" (IMIFM) approach (Guiot et al. 2000; Wu et al., 2007, 2009). (See Izumi and Bartlein, 2016, GRL for further discussion.) So BIOME4 is the vegetation model, while the overall approach (which employs that model) is "IMIFM" (or after that is all explained, simply "the inverse approach").

RE: Corrected (on page 4 lines 92-94 in revised version).

Line 91: "In the case of models. . ." Which models? Is it the case that you're evaluating the PMIP3 simulations made with state-of-the-art climate models using reconstructions of temperature and precipitation?

RE: Yes, we mean the PMIP3 models.

Line 94: "thanks to the seasonal reconstruction" But in all previous applications of the inverse modeling approach using BIOME4 or related models, some sort of reconstruction or estimation of the seasonal variations in climate must have been involved, because BIOME4 requires monthly temperature, precipitation and cloudiness (or sunshine) data as input.

RE: Yes, the monthly climate variables are required in BIOME4 or related models, here we only emphasis that our study is to reconstruct the seasonal cycle of MH climate change over China with a synthesis of pollen datasets.

Lines 95-96: “the forcing factor we used for MH is essential the seasonal change.” I think that what’s going on here is that the midHolocene CMIP5/PMIP3 experiment is essentially one that looks at the response of the models to changes in the seasonality of insolation, and that you are attempting to derive reconstructions of both summer and winter temperature and precipitation to compare with the simulations.

RE: Corrected, according to your suggestion (on page 5 line 100 in revised version).

Line 101: If you’re referring to radiocarbon ages, this should be written as 6000 $\pm$ 500 14C yr BP)

RE: Corrected (on page 5 line 107 in revised version).

Line 102: Spell out “three”.

RE: Corrected (on page 5 line 108 in revised version).

Line 105: I don’t understand the notion of “distinct” pollen records. Distinct in the sense of “unique” or distinct in the sense of “clearly readable”?

RE: We have corrected it into “clearly readable” (on page 5 line 111 in revised version).

Line 107: Criterion 2 seems to be combining two things: sampling resolution and data present within the age range. Please reword.

RE: Corrected (on page 5 lines 112-114 in revised version).

Line 108: How far?

RE: We abandon the pollen records in our dataset if the published paper mentions the influence of human activity on the pollen. (replaced the “far away” by “abandon the pollen records if the published paper mentions the influence of human activity” on page 5 line 114 in revised version.

Line 109: “or by regression”

RE: Corrected (on page 5 line 116 in revised version).

Line 113: Fix the Webb (1985) citation. (Webb, T. III, etc.)

RE: Corrected (on page 5 line 119 in revised version).

Lines 110-113: Reorganize sentence to describe the ranking scheme first, and the results second.

RE: Corrected (on page 5 lines 116-120 in revised version).

Line 114: Add a citation for the concept of “biomization” (which will be a mystery to modelers).

RE: Corrected, we have added Prentice et al., 1996 (on page 5 line 120 in revised version).

Line 116: CQPD. Not in references. Also “sedimentary” what?

RE: The CQPD reference was added, and it should be “sediment” (on page 6 line 123 in revised version).

Lines 117-119: Add region names to Fig. 1?

RE: Corrected, we added the “Tibetan Plateau” and “Loess Plateau”.

Lines 120-135: There seem to be two tasks described in the paragraph: 1) interpolation of modern climate data (from some unspecified source, and by some by some unspecified approach) to the locations of the pollen data, and 2) interpolation of biome scores onto a regular grid using ANN. I suggest breaking this paragraph up, while providing more information on the first task.

RE: The modern climate data are based on the datasets (1951-2001) from 657 meteorological observation stations over China, we also added the data source in the manuscript.

Line 129: If the ANN is calibrated using present-day biomes, then I don’t see how it can be used to interpolate anomalies. Or was it the case that present-day and paleo biomes were independently interpolated onto the grid, after which anomalies were calculated.

RE: The high spatial coverage of present-day pollen records and the application of ANN in interpolating the biome scores makes it possible to reconstruct the past spatial variability with a few pollen sites. In our study, at each pollen site, we firstly used the biomization to get the biome scores for both present-day (PD) and mid-Holocene (MH). Then we calculated the biome score anomalies between two periods (MH-PD). Based on the artificial neural network (ANN), we got the interpolated spatial pattern of biome scores for both PD and anomalies (MH-PD). The spatial pattern of MH biome scores were obtained by overlay the PD pattern with anomalies pattern (MH-PD). Finally, the biome with the highest index is attributed to each grid point, and thus, the spatial pattern of MH vegetation was obtained. The detailed scheme is provided in the enclosed Fig. 2 as below.

Lines 142-143: What are “climate anomalies in the present day”?

RE: Corrected it into “climate anomalies in the past periods” (on page 7 line 152-153 in revised version).

Line 145: Delete “in which the PI experiment was defined.”

RE: Corrected.

Line 146: Here it would be good to refer explicitly to the “midHolocene” experiment.

RE: Corrected.

Line 153: Spell out eight and five.

RE: Corrected (on page 7 line 163 in revised version).

Line 156: “in order to calculate” These variables could also be calculated on the models’ native grids. The motivation for interpolation onto a common grid is simply to get the data onto a common grid.

RE: Corrected (on pager 7 lines 167-168 in revised version).

Line 160: Either delete the hyphens here, or put them into other instances of "biogeography" or biogeochemistry".

RE: Corrected (on page 8 line 170 in revised version).

Line 162: "sunshine percentage (relative to cloud cover)" I don't know what this means.

RE: The sunshine percentage is related to cloud cover (not "relative"). We have corrected it as "an inverse measure od cloud area fraction (on page 8 line 172 in revised version).

Line 171: Bigelow: not in references.

RE: Corrected (on pages 19-20 lines 453-459 in revised version).

Line 173: Were the climate variables downscaled in any way (as in the apply-the anomalies approach, Harrison et al., 1998, J. Climate, Harrison et al., 2014, Climate Dynamics). If not, then the climate fields will not contain the spatial variability of modern climate that in topographically complex areas can have a major impact on vegetation. Fig. S7 attests to the existence of bias in the PI simulations. If the simulated climate values are used directly, than a quantitative estimate of the bias (as in Table 6 for the present-day reconstructions) should be provided.

RE: We directly used the climate fields from models without downscaling. But according to the Taylor diagrams (Figure 1, enclosed below as Fig. 3) from Jiang et al. (2016, Int. J. Climatol), the GCMs from PMIP is reliable to simulate the mean state and year-to-year variability of surface air temperature and precipitation over China for present day even without downscaling. Of course, we agree with the reviewer that there are bias and in the future work, we will try to downscale the climate variables before applying them into regional study.

Line 174: "more than 30 years" How much more? Why not use the same number of years for each model?

RE: Corrected (on page 8 line 184 in revised version).

Line 176: Replace “model-data discrepancies” with “differences between simulated (by the climate-model output) and reconstructed (from pollen). . .”

RE: Corrected (on page 8 lines 186-187 in revised version).

Line 183: Replace “estimate” with “describe”.

RE: CorrectedïijĹon page 9 line 194 in revised versionïijĽ.

Line 192-194: I’m not sure why you’re describing the interpolation of biome data again. ãĂĂ RE: We have deleted this sentence.

Line 197: “Inverse Vegetation Model” See earlier comments.

RE: Corrected (on page 9 line 205 in revised version).

Line 208: I’m not sure why CO_2 concentrations and soil characteristics are being perturbed (i.e. estimated by the inverse approach). We know CO_2, and earlier you argued that soils were assumed not to differ. Also, Table 3 implies that anomalies (or better put, long-term mean differences between present and past) were iteratively generated, which implies that, as is standard procedure, they were applied to present-day climate values and passed to the biome model. If so, what were those present-day values?

RE: Sorry for the inaccurate expression here, the CO_2 concentration and soil are being perturbed by model. For the soil properties, because of a lack of paleosol data, soil characteristics were assumed to have been the same during the MH. While the atmospheric CO_2 concentration for the MH was taken from ice core records (EPICA community members 2004), and set at 270 ppmv; the modern CO_2 concentration was set at 340 ppmv, because most of the modern pollen samples were collected during 1970s and 1980s.

Lines 216-218: I don't know if I'm reading Table 6 correctly, but if I am, the slopes and intercepts are anything but close to 1.0 and 0.0. Only for the case of Pjan is the slope within two standard errors of 1.0, and only for MAP and Pjan is the intercept within two standard errors of 0.0. It would be useful to see scatter diagrams of the observed and estimated values for each variable.

RE: According to your suggestion, we added more information in Table S4, in which you can find the median value and values indicating the 5% -95% uncertainty bands for each variable reconstructed by IVM at every pollen site. From Table 6, there are bias between high and low values of some variables, which indicates the uncertainty in IVM reconstruction. However, we compared our reconstruction with previous studies in Fig. 4. Compared to PI, most reconstructions reproduced a warmer and wetter annual condition during MH, generally same as our study. In other word, this discrepancy between model-data for climate change over China during MH is common and robust in reconstructions derived from different proxies. Our study reinforces the picture given by the discrepancies between PMIP simulation and pollen data derived from a synthesis of the literature.

Line 224: The "collected data" is your data set, right? How was the comparison statistic calculated?

RE: Yes, the collected data is the dataset used in this study. The comparison statistic calculated by the match number of pollen sites. We categorized our pollen records into megabiomes, and 145 of 159 (more than 90%) pollen data match well with the BIOME 6000 during MH, while the match number is 149 for PI.

Lines 226-239: How are the changes or differences in the reconstructions calculated? As differences between the mid-Holocene reconstructions and present-day observations, or present-day inverse-approach estimates? There is considerable bias in the estimates for the present day (Table 6). How would that contribute to the mismatch between simulations and observations? Section 3.1 (throughout): No information on the uncertainties of the reconstructions is given. These are customarily obtained from the variability of the “feasible” climate vectors generated in the optimization step in the inverse approach (e.g. Izumi and Bartlein, 2016, Fig. 3). For that matter, there is no information on the spatial variability of the simulations. Uncertainties for both could be displayed by plotting boxplots in Fig. 3, as opposed to bar graphs.

RE: The changes or differences in the paleoclimate reconstructions are calculated as the differences of biome scores between mid-Holocene and present-day times by inverse approach. And the considerable bias listed in Table 6 between observation and estimation for present-day will add the uncertainty in IVM climate reconstruction. We agree with the reviewer that boxplot is a good way to display the uncertainty, but it can’t show the bias at each site. So we give the columns for every variable indicating 5-95% uncertainty bands. More detailed information about the uncertainties from reconstruction can be found in Table S4.

Line 249: “a decreasing trend” Conventionally, trends are described in the sense of a change from one time to another, or the change over a fixed period of time, so here, if the mid-Holocene MTWA values are lower than present, the trend would be positive or increasing over time (i.e. from the mid-Holocene to present). Check the discussion of precipitation trends too. It would be best to simply drop the notion of “trends” and concentrate on the change between midHolocene and PI.

RE: Corrected.

Line 265: “more detailed information about the geographic distribution of simulated temperature. . .” Section 3.1 (overall): It would be interesting to see a comparison for Pjan, the single variable with an intercept of 0.0 and a slope of 1.0.

RE: Corrected (on page 12 line 276-277). For Pjan, although its intercept and slope are most close to 0 and 1 when compared to other parameters, the amount of Pjan is small in China. As East Asian monsoon area, the annual precipitation is effected much more by summer rainfall (Pjul) rather than winter rainfall (Pjan).

Line 273: “which would introduce a bias. . .” That’s certainly plausible, but right now it’s simply a conjecture.

RE: Yes, it’s true that we haven’t quantified the impact of different MH vegetation setting on the role of vegetation-atmosphere interaction in the MH climates among all PMIP3 models. But by giving Fig.5, Fig. 7 and Fig.8, we think that the failure to capture MH vegetation change has influence on model-data discrepancy for climate change.

Line 309: “However, none of the models succeed in capturing these features,. . .” I agree. However, the differences between the simulated and reconstructed biomes for the midHolocene simulations strike me as apparently similar in magnitude to those for the PI, and casual comparison of Figs. 5 and S7 suggests to me that some of the patterns of disagreement in the midHolocene case are inherited from the PI. This makes me wonder again about the protocol followed for generating the midHolocene simulations (see comment on line 173).

RE: According to the Taylor diagrams (Figure 1) from Jiang et al. (2016, Int. J. Climatol), the GCMs from PMIP is reliable to simulate the mean state and year-to-year variability of surface air temperature and precipitation over China for present day even without downscaling. However, the BIOME4 is a globe model, because of the particularity of vegetation types in the monsoon region of China, the BIOME4 needs further improvement of vegetation simulation accuracy in this area. This possible bias in simulating vegetation will lead to uncertainty in reconstruction. We agree with the reviewer that the downscaling is very important in applying global model into regional study, which will be taken into account in our future studies. In this study, to be able to convince the referee with quantitative arguments, we decided to conduct a supplementary experiment to demonstrate that our mechanism was appropriate. This new modeling strongly suggests vegetation changes may explain a large part of the mismatch (as shown in Fig. R1), which is very consistent with our proposal in this study. Nevertheless, there are certainly other possibilities and indeed models that better captured the hydrologic cycle and enhance the precipitation/ evaporation pattern could also explain differences between model and data.

Line 310: What are “enhanced vegetation conditions”?

RE: The “enhanced vegetation conditions” refers to the transition from grassland to forest in the northeast during MH. We have modified it as “transition from grassland into forest”.

Line 311: “. . .a cumulating inconsistency in the model-data comparisons . . . because of the vegetation-climate feedbacks.” Except for the two AOV models, vegetation-climate feedback is only present in the real, as opposed to simulated, climate, i.e. in the reconstructions.

RE: Yes, all models except for the two AOV models present the real vegetation-climate feedback in PI, but they failed to present the real feedback in MH. The vegetation during MH is prescribed as PI in these 11 models, which means no change of such vegetation-climate feedback for PI and MH among them. This will lead to a cumulating inconsistency.

Line 315-316: “wetter and warmer in MTWA, colder in MTCO” This makes no sense. You might say “higher temperatures in the warmest month of the year,” but did you indeed look at precipitation in the warmest month? I think what you want to say is “higher (than present) July precipitation and MTWA, lower than present MTCO” or something like that.

RE: Corrected (on page 14 lines 325-326 in revised version).

Line 318: Trend again. Data show higher-than-present MTCO during the mid-Holocene while models simulate lower-than-present MTCO.

RE: Corrected.

Line 319: My reading of Fig. 3 shows that CNRM-CM5 and HadGEM2-ES are consistent with all of the other models in simulating lower-than-present MTCO. ãĂĂ RE: We have corrected the expression of this sentence. On page 14 line 329-330.

Line 322: “among models”

RE: Corrected (on page 14 line 338 in revised version).

Line 323-324: Replace “shed light” with “raises” (the question). (“Shedding light” implies that the variability referred to would answer the question.)

RE: Corrected (on page 14 line 335 in revised version).

Line 326: Replace “amplitude” with “amplitude and pattern”. (You emphasize pattern as much as area.) Also, it’s not the failure of the models to simulate vegetation change that’s important, it’s the fact that (apart from HadGEM2-ES and HadGEM2-CC) they can’t, because the vegetation is not interactive. However, can’t albedo still vary, through variations in soil color and snow cover?

RE: The “amplitude” has been replaced by “amplitude and pattern”. To the second comment, yes, the albedo could vary through the variation in soil and snow cover, so we checked the monthly surface albedo change (MH-PI) of all models with prescribed vegetation. The Table enclosed below as Fig. 5 indicates that the surface albedo change caused by snow cover between two periods is very small (no more than 0.005), which could be neglected in this study. About the soil color, to our knowledge, it is prescribed as PI during MH in PMIP3, so this impact on albedo change is also negligible for our study.

Line 337: “Reconstruction showed. . .” I thought you were talking about the two AOV models. This sentence implies that you estimated the overall albedo change from the vegetation reconstruction, and compared with the two models with interactive vegetation. Is that right? If not, please explain a bit more.

RE: Yes, it’s right.

Line 348-349: “should act” or “most likely would act” (We don’t really know if it would.)

Printer-friendly version

Discussion paper

RE: Corrected.

Line 351-353: It may well be the case that cloud radiative feedback (or rather, inadequate simulation of that) could play a role in the data-model mismatch, but if so, that points to a completely different kind of model inadequacy, involving atmospheric circulation, moisture flux, and cloud-producing or cloud-suppressing mechanisms. Those mechanisms have been implicated in explaining the mismatch between simulations and reconstructions in the Eurasian midcontinent (Bartlein et al., 2017, GRL).

RE: It's a very important comment. We agree with your proposal that cloud radiative feedback may play a role in model-data mismatch, which indicates another kind of inadequacy. For this study, we simply focuses on the surface land change, we are not able to quantify the possible influence of mechanisms related to cloud on this model-data discrepancy for now, but we can do more in the future.

Line 354: Taylor (and fix reference too).

RE: Corrected (on page 16 line 366 in revised version).

Technical comments: I concur with the Editor and other referees that some work needs to be Corrected on the references and data-availability aspects of the paper. References: Format varies from reference to reference. Tables 4, 5 & 6: Replace commas with periods (decimal points). Fig. 7: Define dotted horizontal and vertical lines. Maps (throughout): Why does the "nine-dash line" inset vary in size and shape? I realize that the inset has to be there for geopolitical reasons, but why does it change from map to map? Fig. S7: What is the white horizontal line? SI, p. 1: Dallmeyer et al. (2017), not in references. SI, p. 3: Material at the bottom of the table is hard to read. Please reformat into a table-like arrangement. SI, p. 5: Add citations to original data sources.

RE: All these technical comments mentioned above have been done in the new manuscript.

The revised version of manuscript and supplementary information are enclosed below as supplement.zip.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-145/cp-2018-145-AC5-supplement.zip

---

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2018-145, 2018.

Printer-friendly version

Discussion paper

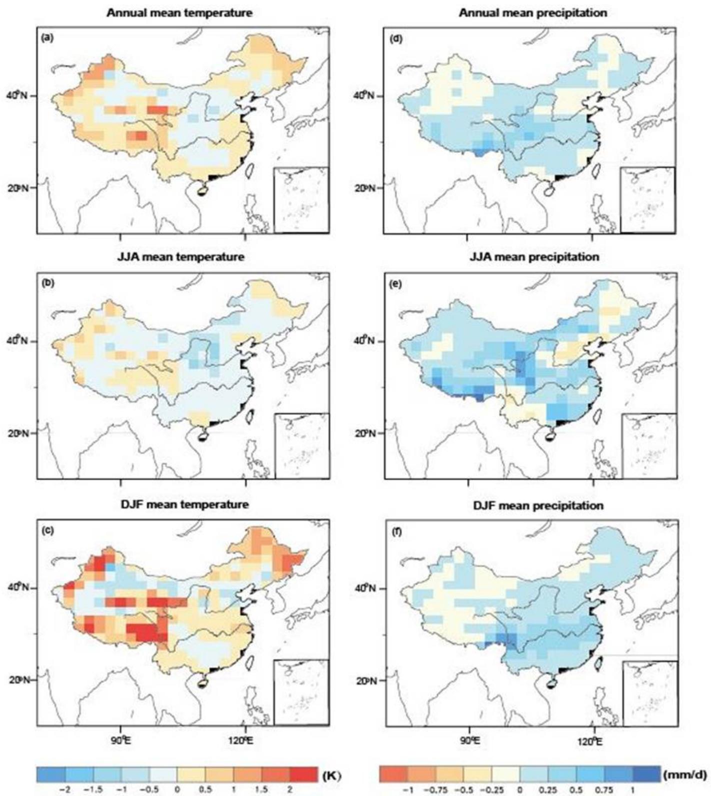

Figure 1. Climate anomalies between the two experiments (6 ka and 6 ka_VEG) conducted in CESM version 1.0.5. The anomalies (6 ka_VEG-6 ka) of temperature and precipitation at both annual and seasonal scale are presented, and all these climate variables are calculated as the last 50-year means from two simulations.

Fig. 1.

Printer-friendly version

Discussion paper

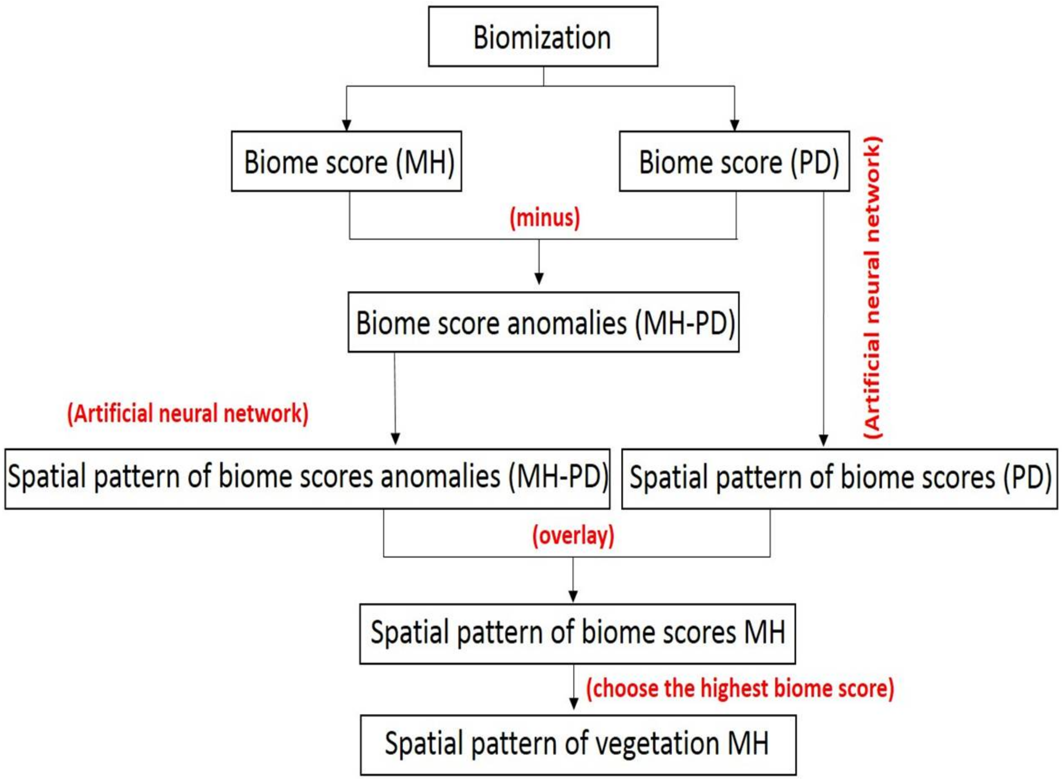

Figure 2. The schematic diagram of artificial neural network.

Fig. 2.

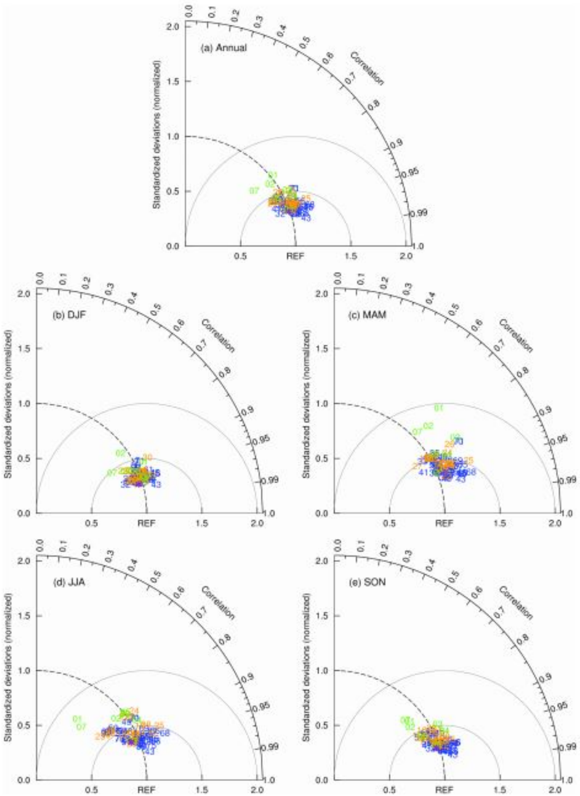

Figure 1. Taylor diagrams displaying normalized pattern statistics of climatological (a) annual, (b) DJF, (c) MAM, (d) JJA, and (e) SON temperatures over China between 77 GCMs and observation for the period 1961–2000. The radial co-ordinate gives the standard deviation normalized by the observed value, and the angular co-ordinate gives the correlation with observation. The normalized CRMSE between a GCM and observation (marked as REF) is their distance apart. Numbers indicate GCMs listed in Table 1. Colour coding is green for TAR, orange for AR4, and blue for AR5 GCMs. Red and purple asterisks indicate the ensemble mean and the median of the 77 GCMs, respectively.

Fig. 3.

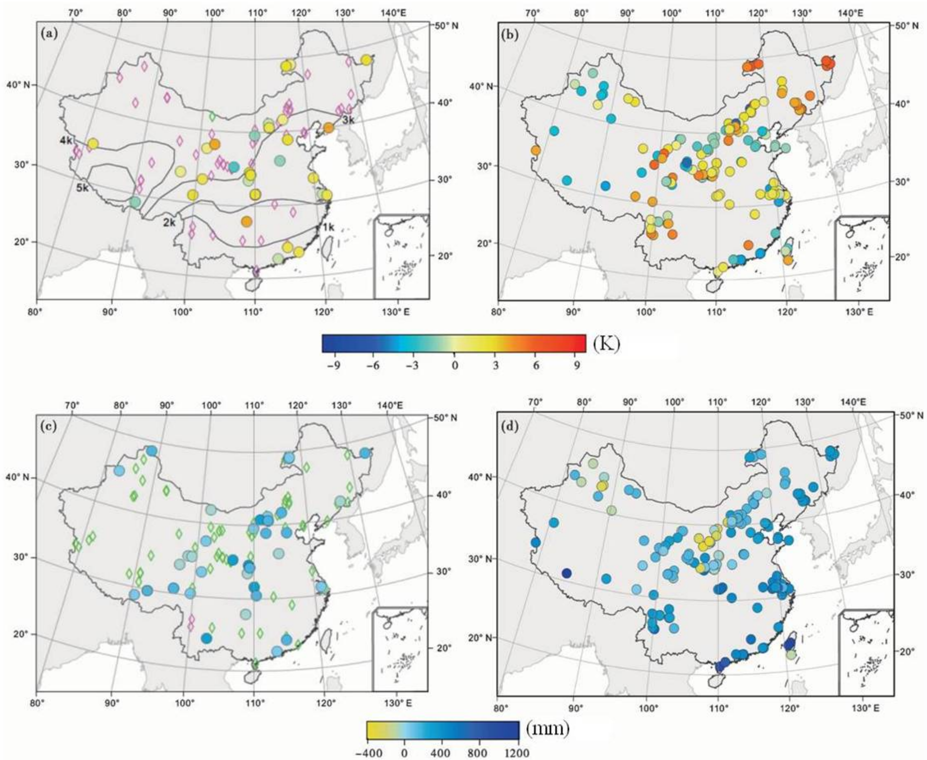

Figure 4. Comparison between our climate reconstruction and previous reconstruction. (a) Previous temperature results. Diamond is the qualitative reconstruction, red is the temperature increase and green is the temperature decrease; Circle is quantitative reconstruction; (b) Mean annual temperature reconstruction in this study; (c) Previous precipitation results, diamond is the qualitative reconstruction, red is the precipitation increase and green is the precipitation decrease; Circle is quantitative reconstruction; (d) Mean annual precipitation reconstruction in this study.

Fig. 4.

Table. The monthly albedo change caused by soil color and snow cover among models.

	Model	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
PI	bcc-csml-1	0.149	0.156	0.156	0.154	0.142	0.127	0.136	0.143	0.147	0.153	0.152	0.145
MH	bcc-csml-1	0.149	0.156	0.157	0.155	0.142	0.126	0.134	0.140	0.142	0.148	0.147	0.143
MH-PI	anomaly	-0.001	0.000	0.001	0.001	0.000	-0.001	-0.001	-0.003	-0.004	-0.004	-0.006	-0.002
PI	CCSM4	0.170	0.172	0.173	0.174	0.164	0.145	0.143	0.153	0.159	0.168	0.168	0.164
MH	CCSM4	0.170	0.174	0.175	0.176	0.166	0.146	0.142	0.150	0.156	0.167	0.166	0.160
MH-PI	anomaly	0.001	0.001	0.003	0.002	0.002	0.001	-0.001	-0.003	-0.002	-0.001	-0.003	-0.003
PI	CNRM-CM5	0.151	0.164	0.164	0.161	0.146	0.128	0.130	0.140	0.143	0.152	0.152	0.141
MH	CNRM-CM5	0.149	0.161	0.161	0.159	0.144	0.124	0.130	0.137	0.138	0.145	0.147	0.140
MH-PI	anomaly	-0.002	-0.003	-0.003	-0.002	-0.002	-0.003	-0.001	-0.003	-0.005	-0.007	-0.004	-0.001
PI	CSIRO-Mk3-6-0	0.169	0.181	0.178	0.171	0.156	0.140	0.147	0.161	0.167	0.171	0.169	0.162
MH	CSIRO-Mk3-6-0	0.170	0.180	0.179	0.172	0.157	0.139	0.145	0.161	0.164	0.168	0.164	0.162
MH-PI	anomaly	0.001	0.000	0.001	0.001	0.000	-0.001	-0.001	0.000	-0.003	-0.004	-0.005	-0.001
PI	FGOALS-g2	0.170	0.172	0.170	0.168	0.154	0.137	0.141	0.155	0.156	0.164	0.168	0.161
MH	FGOALS-g2	0.172	0.175	0.173	0.173	0.159	0.141	0.142	0.156	0.158	0.167	0.168	0.163
MH-PI	anomaly	0.002	0.003	0.004	0.005	0.005	0.004	0.001	0.002	0.002	0.003	0.000	0.002
PI	FGOALS-s2	0.165	0.173	0.171	0.170	0.161	0.148	0.153	0.164	0.165	0.168	0.166	0.161
MH	FGOALS-s2	0.164	0.174	0.173	0.172	0.162	0.142	0.150	0.159	0.160	0.165	0.160	0.156
MH-PI	anomaly	0.000	0.001	0.002	0.002	0.001	-0.006	-0.003	-0.005	-0.005	-0.004	-0.005	-0.005
PI	GISS-E2-R	0.144	0.153	0.154	0.150	0.131	0.114	0.119	0.126	0.132	0.138	0.138	0.134
MH	GISS-E2-R	0.144	0.153	0.153	0.149	0.131	0.114	0.111	0.122	0.124	0.129	0.132	0.130
MH-PI	anomaly	0.000	-0.001	-0.001	0.000	0.000	0.000	-0.007	-0.004	-0.008	-0.009	-0.006	-0.003
PI	IPSL-CM5A-LR	0.149	0.158	0.161	0.159	0.148	0.133	0.140	0.149	0.152	0.155	0.148	0.142
MH	IPSL-CM5A-LR	0.149	0.158	0.162	0.160	0.149	0.133	0.140	0.147	0.150	0.152	0.144	0.139
MH-PI	anomaly	0.000	0.000	0.001	0.001	0.001	0.000	0.000	-0.001	-0.002	-0.003	-0.004	-0.002
PI	MIROC-ESM	0.162	0.171	0.173	0.167	0.146	0.123	0.129	0.136	0.143	0.153	0.155	0.151
MH	MIROC-ESM	0.164	0.174	0.176	0.172	0.150	0.124	0.131	0.137	0.144	0.154	0.153	0.152
MH-PI	anomaly	0.002	0.002	0.003	0.005	0.004	0.001	0.002	0.000	0.000	0.001	-0.002	0.001
PI	MPI-ESM-P	0.170	0.164	0.160	0.155	0.153	0.155	0.151	0.145	0.145	0.151	0.161	0.173
MH	MPI-ESM-P	0.169	0.163	0.160	0.157	0.154	0.154	0.148	0.141	0.141	0.147	0.160	0.171
MH-PI	anomaly	-0.001	-0.001	0.000	0.001	0.001	-0.001	-0.003	-0.004	-0.004	-0.004	-0.001	-0.002
PI	MRI-CGCM3	0.183	0.194	0.194	0.190	0.173	0.156	0.158	0.172	0.176	0.187	0.189	0.180
MH	MRI-CGCM3	0.183	0.195	0.196	0.191	0.174	0.154	0.156	0.168	0.172	0.183	0.183	0.175
MH-PI	anomaly	0.000	0.001	0.001	0.002	0.001	-0.001	-0.002	-0.003	-0.005	-0.004	-0.005	-0.005

Fig. 5.