

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

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General comments:

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 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

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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

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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.

Specific comments:

Abstract: The abstract fails to disclose conclusions of paper.

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.

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

Line 20: New definition for PMIP?

Line 22: “a seasonal cycle. . .”

Line 25: “access surface processes” I don’t know what this means.

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).

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 mid-Holocene time slice, and the “midHolocene” CMIP5/PMIP3 experiment, throughout the paper.

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

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.

Line 42: “consistency of the dataset incorporating different proxies” I don’t know what that means.

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

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

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.

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.

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.

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

Line 73: “colder than the baseline” What baseline? Present-day or preindustrial?

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

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simulations.”?

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

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”).

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?

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.

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.

Line 101: If you're referring to radiocarbon ages, this should be written as 6000 ± 500 14C yr BP)

Line 102: Spell out “three”.

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

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

Line 108: How far?

Line 109: “or by regression”

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

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

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

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

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

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.

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.

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

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

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

Line 153: Spell out eight and five.

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.

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

Line 162: “sunshine percentage (relative to cloud cover)” I don’t know what this means.

Line 164: “main input variables” Are there any others? If not, the variables described are the input variables.

Line 171: Bigelow: not in references.

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, then a quantitative estimate of the bias (as in Table 6 for the present-day reconstructions) should be provided.

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

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

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

Line 192-194: I’m not sure why you’re describing the interpolation of biome data again.

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

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Line 208: I’m not sure why CO₂ concentrations and soil characteristics are being perturbed (i.e. estimated by the inverse approach). We know CO₂, 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?

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.

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

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.

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

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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.

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.

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

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).

Line 310: What are “enhanced vegetation conditions”?

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.

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.

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

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.

Line 322: “among models”

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

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?

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.

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

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).

Line 354: Taylor (and fix reference too).

Lines 363-364: “counteract . . . while they enhance. . .” This needs a little explanation,

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and a tie into the figure.

Line 374-376: Alternatively, the mid-Holocene vegetation could be specified as part of the experimental design. Unfortunately, this would eliminate using vegetation and vegetation-derived reconstructions as benchmarking targets.

Technical comments:

I concur with the Editor and other referees that some work needs to be done 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.

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