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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.

Anonymous Referee #2

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General Comments:

The basic intent of this paper is to describe the setup and some general results from the paleoclimate runs of the MIROC-ESM. As such, it represents important background to these runs that often falls outside of the scope of publications addressing scientific results from simulations. Geophysical Model Development is an appropriate journal in which to publish this.

The paper as written is a good step towards these ends and is an important contribution to the paleoclimate model development community. I feel, however, that revisions are required before publication.

These recommendations fall within two broad categories: 1, a more thorough description of some of the setup procedures and the MIROC PI experiment (upon which comparisons are based) are needed to put the results of the paleo-simulations in perspective and 2, more technical changes to language, word choice, etc. to make the paper more clear and easier to read. I believe the scientific quality is high, yet the language used is often confusing and difficult to read. I recommend getting the assistance of an experienced English language editor to help with many of these language related issues.

These recommendations are outlined below.

Specific Comments

Introduction

The term "ESM" means many different things. Some models are considered an "ESM" even if carbon cycling is prognostic in vegetation but prescribed CO2 in atm, etc. Somewhere in this first section please describe better what you mean by it. . .I believe (but am not certain!) you mean only models for which all CO2 cycling (including atmospheric CO2) is prognostic.

The discussion of the LM (p. 2531) leaves out all references to volcanoes!?! This was a very important part of this time period. It may be that decadally paced volcanic activity beginning ∼13th century helped start the Little Ice Age (e.g. Zhong, Y., G.H. Miller, B.L. Otto-Bliesner, M.M. Holland, D.A. Bailey, D.P. Schneider, and A. Geirsdottir, 2011: Centennial-scale climate change from decadally-paced explosive volcanism: A coupled sea ice-ocean mechanism. Climate Dynamics, doi:10.1007/s00382-010-0967-z.).

2 Model and common settings

This section (pp. 2532-2534) reads essentially like a string of acronyms. These might be necessary for anyone who wants to look up a specific component of this complex model. However the section is lacking a overview paragraph describing a basic lin-

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eage of the MIROC-ESM or similarities/differences to preceding version and thus all the acronyms lack a context. Why not just use older version? What changes were implemented (generally – with references for those who want the details) and why?

3.1 Pre-industrial control experiment

The setup lists details (e.g. orbital parameters and GHG levels) but lacks a sentence or two putting them into context. For example, these orbital parameters result in higher/lower solar irradiance compared to 20thC, Holocene, LM, etc.

Spin up procedures are not clear. For example, how was the PI run initialized? Support "linear trends became insignificant" with a plot or two. Likewise, why only use the last 100 yrs of a 630 yr simulation when some climate modes may need longer than that to define (e.g. thermohaline circulation, the Atlantic Multidecadal Oscillation) and particularly when you will be comparing (albeit in detail in other publications) to a 1000+yr LM simulation and presumbably would want similar basis for statistics? Likewise, the 6ka temperatures are compared to the PI starting at the branching time (yr 250 – p. 2537) so more than the last 100 yrs of the PI are used?

A more detailed description of the model performance in the PI experiment is needed to put the results of the paleo-simulations in perspective. For example, investigating the climate sensitivity of the LGM through modeling simulations requires an understanding of the climate sensitivity of present-day model to make adequate comparisons. The authors state that the PI simulation shows a "reasonable SST distribution" that claim that global PI SST is slightly cooler than observations because it's a simulation from mid-1850s. The pattern of SST differences in Figure 4, however, suggest that differences between obs and model are far more complicated that this and must be related to ocean circulation. Furthermore, a comparison with a reanalysis based product such as that described by Hurrell et al., 2008 (Hurrell, J. W., J. J. Hack, D. Shea, J. M. Caron, and J. Rosinski, 2008: A new sea surface temperature and sea ice boundary dataset for the Community Atmosphere Model. J. Climate, 21, 5145–5153) might give a more

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accurate perspective of the basic differences in SST between the MIROC-ESM and observationally-based SSTs.

How is the sea ice initialized and/or set-up? This is important here as the authors state that the Antarctic sea-ice extent is reducing slowing in the model yet it isn't clear exactly what this means. A specific year in the 630-yr integration? Or from intial conditions? Also – is it reducing on an annual or season basis? A time series showing this reduction in Antarctic SIE would be helpful.

3.4 LM The term "anthropogenic land use" is used often throughout the paper and in particular here in the LM section in a manner I find very confusing. Much better would be to state "land use for 1850" or "20thC land use" or whatever it was ("anthropogenic" means human-caused. It has no exact time frame – although most commony used for time period 1850-2000+ in modeling circles. . . .). It's use here is misleading – using "anthropogenic land use" – vegetation is not prognostic? Or is? Carbon cycle is but vegetation is not? Land use changes over period humans were thought to significantly modify it (maybe 1750 onwards, or maybe not . . . .maybe 1850. . . .). Then at the very end on p. 2543 it is stated that "anthropogenic land use was assumed to be unchanged". Over the entire Last Millennium? 850-2005? Really?

Technical Corrections:

p. 2528, lines 1-3.

Awkward, cumbersome wording – simplify.

p. 2528 lines 6-7

Add reference to volcanic aerosols - more important than land use change and provide a critical forcing and method by which to test a climate models' response to radiation changes.

p. 2528 line 10

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define “MIROC” the first time it’s used

p. 2530 line 9

verb tense 9 – I recommend keeping the same tense (“are” rather than “were”)

p. 2529 line 17

“vegetation” or “vegetation activity”? “activity” unusual word choice with “vegetation”

p. 2529 lines 23-24

delete “enhancement” and replace with something like “sufficiently high Holocene precipitation to . . .”

p. 2532 lines 5-6

This definition of MIROC should come at first use and deleted here

p. 2532 lines 21-24

It’s not at all clear what the differences or similarities are between the “full version” MIROC-ESM and the “CHASER” are except one uses more computational resources. . .

p. 2535 lines 5-9

Had to read this sentence a couple of times. . .2nd half is incomplete. Reorder such that the list of “cycle components” comes in parenthesis after “components”.

p. 2536 line 9-10

I believe you mean that no volcanic activity was used in forcing 6 ka simulation, not “volcanic changes” which imply constant, but not necessarily zero, volcanic activity?

p. 2536 lines 17-19

This is confusing. PI I thought was forced at constant (non-transient) conditions, in-

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cluding land use types, yet these sentences imply that land use changed during this simulation. “initialized” and “steady state” are at odds – “initialized” means a one-time initialization, whereas “steady state” implies keeping at a steady state with time. . . I’m guessing initialized with land-vegetation types assumed relavant to 6 ka and derived from ?

p. 2536, lines 21-24 to p. 2537 lines 1-2

Very confusing and Figure 3 doesn’t help at all. . .I don’t understand at all how vegetation was initialized or carbon-cycle spun up for 6 ka.

p. 2537 line 17

substitute “while” for “whilst”

p. 2540 lines 5-11

“Initialization” implies a one-time starting point, however this has “initialization process” which sounds more like a spin-up?. . ...Clean up language/description for clarification.

p. 2541 line 6

eliminate “peak value of AMOC”

p. 2541 line 7

add “as suggested by the proxies”

p. 2543 lines 5-19

“Spin-up”, “initial state” seem to be interchanged (and should not be).

p. 2543, lines 12-14 “CO2 concentration was set to be free” then “CO2 concentration was reset” These statements contradict one another.

p. 2544, line1

Time period from which anomalies in Figure 12 are calculated are different in the text

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(this line) and in Figure 12 caption. Text states 1961-1990 and Figure caption states 1970-200.

p. 2544, line 11

re-word “low-pass characteristics”

p. 2544 line 18

substitute “large” or “very large” for “huge”

p. 2544 lines 21-25

Reword. Solar irradiance is not “visible” at all – but the effects are! Volcanic aerosols do not “control” surface temperature but they do effect them, or influence them, etc. Likewise “flat” applies to a line segment on a graph, perhaps, but not solar forcing. Perhaps “relatively constant”.

Figures.

Figure 12. panels (a) and (b) are mislabeled in caption (or in reverse order). Here panel (a) is volcanic forcing and (b) is solar (and is stated the other way around in caption).

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