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Earth System Dynamics
Discussions

Interactive comment on "The climate of a retrograde rotating earth" *by* Uwe Mikolajewicz et al.

Anonymous Referee #2

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*General Comments

The authors use a Earth system general circulation model allowing considerable process complexity (although at a lower spatial resolution than the current state of the art) to simulate the climate of a counterfactual Earth that rotates around its axis in the opposite direction. The study is fascinating in conception, and appears to have been carried out and presented in a clear, careful and scientific manner. I have some comments and queries on specific matters of course, but the paper is sound and could really be published with only minor revision.

*Specific Comments

The biggest single improvement, in my opinion, would come from the authors being

little clearer on the intended aims and conclusions from the study. Given our imperfect understanding of and ability to simulate the world as we see it now, it is obviously a bit of a stretch to apply precisely the same tools to a world for which we have no observational verification or ability to conclusively falsify hypotheses. I don't think that means that we shouldn't carry out an exercise like the one that has been presented here, but having a very clear framework in place, eg "we want to test our general understanding of a certain feature of the real Earth", or "we're simply interested in exploring what such a place would look like for its own sake" - both would be quite valid, as far as I'm concerned - is helpful in interpreting what is shown. The current manuscript feels like it tries to do both at once without clearly framing the context for some of the results, and I'm left unsure, for instance, whether certain aspects of the write-up are really meaningful - one's interpretation depends on the context in which you read them.

For instance, in the RETRO-S experiment, the Coriolis force is reversed but the direction of the diurnal solar cycle is left alone. The configuration is totally artificial and the physics in question is almost certainly not being reproduced "realistically" (even very high resolution NWP models have difficulty getting the physics involved in the daily timings of the afternoon rain in Africa right), so it's questionable that this has direct relevance to learning about our world. But it's an interesting, physically valid part of the artificial construct we're being presented with when seen outside of a real-world context - I think the same sort of comment applies to the climate sensitivity section. The opposite is however true for some of the land-cover and ocean biogeochemistry results, where the RETRO model is tied to real-world distributions of CO2, soil, ice and dust and it's difficult to see what results as being internally consistent in the context of a backwards-rotating ecosystem.

A symptom of this may be that almost the entirety of the Conclusion section is a descriptive summary of what has been seen (and already described) in the simulation, rather than making any attempt to help the reader see what has been learned from the

exercise in toto. Apart from having certain aspects of this fascinating Gedankenexperiment demonstrated for real, I'll admit to personally being unsure what robust conclusion might be taken from this (other than that playing with climate models is fun), so any help the authors could offer would be appreciated.

pg1, paragraph 2: some of the summarised effects and causes in the abstract don't come out very clearly in the main body of the manuscript itself, requiring the reader to piece together material in the different sections and appendices for themselves to make a picture that matches up with the abstract. Some of this could be explicitly outlined in the Conclusions section

pg2, line 29: the literature on monsoons is vast, I find it difficult to believe there is only one study looking at what sets their locations

p3,l27: Does "These simulations did not [...]" refer to just the simulations of Kamphuis et al, or of the results of both Smith and Kamphuis taken together?

p3,l34: "for the most part" is very imprecise

p4,l13: it would be helpful to say where the atmosphere model top is, in terms of how much of the stratosphere is being modelled.

p4,l[20|26]: is the "long" model spinup (l20) different from the 6990 year model integration (l26), and if so, how long was it?

p4,l30: how one interprets the impact of not allowing ice, GHGs and aerosols to change very much depends on how you frame the results of the simulation - see above. The significant changes simulated in biological production and ocean deepwater would be expected to have an impact on the overal GHG concentration in the atmosphere, with a possible first order effect on the climate and possible major ice-sheet feedback - doing this sort of thing with enough detail is clearly out of scope for our present modelling capability, but it would be fun and informative to have some speculation from the authors!

p5,l3: piControl needs explaining

p5,l4: has ESM been defined yet?

p6,l16: why is the Pacific cold tongue so much less evident in RETRO?

p18,l29: could the authors speculate from the published model specifications and results why each model sees the MOC changes it does?

p21-24. Section 6 describes one of the most interesting aspect of the results and the importance of using an ESM, rather than a purely physical climate model, to do this kind of simulation. It's a real shame that the connections aren't there to either include the impact of changes in landcover and dust on the biogeochemistry, nor the follow on impact on ocean CO2 sequestration of the biogeochemistry and physical ocean changes together. The authors end by claiming that they expect that their model limitations don't affect the main features of the ocean carbon cycle, but I'm much less sure, and I'd welcome either more justification of this statement, or some intersting speculation on what could happen instead.

p27. Section 7 feels unfocussed, and (given the main "framing" comment above) I'm really not sure what one is supposed to take from it

p28,l7: I don't think "numerous" sensitivity experiments have been shown, just 2: RETRO-S and a 4xCO2 run?

p28,l12: I also don't think one can credibly call ice-sheets, soils and GHGs "minor" consituents of the Earth system!

p30, Appendices: In my opinion some of the most interesting analyses have been relegated to relatively brief descriptions in the appendices. Personally, I would rather see all of this material in the main text in preference to the current section 7, although some of it may be a bit specialised and the Editor may want to take a view based on the intended audience for the journal. The other issue would be whether the features are really modelled robustly enough to be part of the main attraction - the functioning of an ENSO and its teleconnections on a RETRO planet could probably be a paper all on

its own rather than 18 lines in an appendix, if the relevant physics in the model could be shown to be robust enough.

p34,l8: How many years of data have been used in the power spectrum? The confidence intervals don't look very convincing - if the 1-10yr signal in CNTRL is supposed to look significant then so is 10+ in RETRO...?

*Technical Corrections

The authors seem to have a habit of trying to make sentences out of phrase fragments that have no main clause eg pg3, line19 "One which includes different net freshwater forcing regimes for the ocean, while keeping the present continental geometry.", p6,l34 "The area with the largest amount of annual precipitation being near Ascension Island in the Southern Tropical Atlantic (...).". There are several occurrences of this construction throughout.

Several features are described as (objectively) "surprising" or "(un)expected", which are subjective assessments

pg2, line27: "that what would be caused" - what should be which

p3,l5: "how the ocean transport heats"

p3,l[11|15]: "stabeliz[es|ing]"

p3,l25: "FAMOUS" needs an explanation/citation if you're going to specify the model

fig 1. why does the time axis go from 1000-8000 for runs that are described as 6990 years?

p4,l7: "MPI-ESM" and "CMIP6" need explanation/citation

p4l13: "vertic"

p5,l12: "mid-altitudes have surface easterlies

p6,l2: "feint" should be "faint"

p6,l16: "the American's warm"

p6,l34: "like presentday Palau"

fig3: "(10 Mm versus 8Mm)" I don't understand this part of the caption

fig4: whilst very pretty, this figure lacks adequate labels as to what the flux components are, what time averaging has been applied, and takes up a lot of space illustrating something that "hardly differs from that of the CNTRL" and (pg9,l10) whose quantities might be expected to vary most interestingly on a hemispheric basis, rather than the global average actually shown

p11,l3: new paragraph at "Changing the planetary rotation [...]"

p12,l11: "vareity"

fig11,13: "Indic" Indian Ocean?

p21,l27: "[...] occur only very localized."

p24,l10: Despite the fixed atmospheric CO2, the changes in the land biosphere in section 4 are extrapolated to an estimate of their potential impact on the CO2 concentration in the atmosphere - could a similar estimate not be made from the changes in the ocean carbon inventory?

p29,l16: "Euro-Africa, with American" doesn't need the comma

p29,l20: "Other studies" needs references

p33,l1: "Kongo"

p33-35: figs C2,C4 not actually referred to in text.

p34,l4: "In RETRO" is repeated in this sentence

p37,l13: "n/a-n/a" in citation

p38,l[25|32]: two web links in citation

p39,l11: two web links in citation

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