The climate of a retrograde rotating Earth
Response to Reviews

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Reply to Reviewer 2

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 counter factual 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.

We thank the reviewer for the positive feedback and for the helpful and constructive comments. In the following we addressed all the points raised by the reviewer.

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.

We agree with the reviewer, that the aims of the study were not as clear formulated as they could be. This goes back to the comprehensive nature of the study, where we tried to analyze all components of the climate system in order to get a full picture of the changes. Our first priority of the study is a more comprehensive understanding of the general features of the Earth, specifically in respect to spatial asymmetries and circulation regimes, as is highlighted in the introduction of the manuscript. To emphasize this, we changed the conclusions and moved some of the sensitivity experiments, which were conducted in order to get a deeper understanding into the different factors controlling the climate under a reversed rotation, to the Appendix. We also connected the individual parts better, in order to point out interactions between the components of the Earth system.

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

We have attempted to address this by sharpening the presentation of the abstract and conclusions, but also, and more substantially, by moving some of the text and figures (on storm tracks and moisture transport) from the supplementary material into the main text.

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

Yes the monsoon literature is vast, but very little of it focuses on the general causes for the zonal distribution and extent of the monsoons and their relationship to the deserts. This contrast, between the large literature on the topic of monsoons and the relative sparseness on questions related to its zonal extent was actually what we had hoped to highlight as the simulations have a bearing on this. To make this clearer we have slightly rephrased this discussion.

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?

Here we mean the two experiments of Kamphuis et al. We reformulated the sentence to: *"... This study did not, however, show evidence of a complete*

reversal in the role of sub-polar North Atlantic and Pacific..."

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

Rephrased to: *"[...] and wherein most components in the Earth's oceans and biosphere (terrestrial land surface) were allowed to freely adapt to the changing conditions [...]".*

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.

We added that the atmosphere reaches up to 10hPa in the text.

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?

Yes, there was an additional spin up of almost 8000 years with some changes in the marine biogeochemistry model component during the run. This is now mentioned in the model and experiments section.

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 overall 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!

Planetary-scale features of the marine biogeochemistry such as zonal and global means (e.g. net primary production, air-sea carbon fluxes) are largely unaffected by the direction of Earth's rotation. Hence, marine carbon inventory increases only by 100 PgC in RETRO compared to CNTRL. Given the ocean buffer capacity this would correspond to a decrease of atmospheric CO_2 concentrations of the order of 8 ppmV. However, in a system with a fully coupled carbon cycle any increase of the land carbon inventory with a corresponding impact on the atmospheric CO2 would be, to first order, compensated by the ocean. The projected change on land (increase by 86 PgC) is rather small compared to the marine carbon inventory ($\sim$ 38000 PgC). Thus, we expect only a rather small change of 1-2 ppm in a fully coupled simulation and consequentially very minor effects on climate. We deleted the sentences on the potential impact on the atm. CO_2 in section 4 and included a brief discussion in the conclusion section.

For the ice sheets we have looked into summer snow depth and temperature fields, from which we expect that prescribing present-day ice sheets does not seem to have a major impact on both simulations. We added a short paragraph at the end of section 4:

Another prescribed present day feature are the ice sheets. In the RETRO and CNTRL simulations, no permanent snow cover was simulated outside of the prescribed glaciated areas (not shown). Additionally, the simulated summer temperatures over the ice sheets (not shown) do not indicate a strong inconsistency between the prescribed ice sheets and the simulated climate both for RETRO and CNTRL. Therefore, we did not find a major inconsistency from the prescribed present-day ice sheet configuration and the simulated climate

p5,l3: piControl needs explaining

We added 'pre-industrial conditions' for clarification.

p5,l4: has ESM been defined yet?

Thanks for pointing this out. We now introduced ESM (Earth System Model) in the beginning of this section.

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

If you would expect a Pacific cold tongue in RETRO, for obvious reasons it would be in the West Pacific. The Indopacific acts here largely as one ocean, so the Indian Ocean would be the right place to search for a cold tongue. Indeed the Indian ocean has overtaken to a large extend the function of the tropical East Atlantic with strong upwelling.

Due to the presence of landmasses around the Northern Indian ocean, a strong monsoon signal occurs, which does not allow the cold tongue signal to develop.

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

Each model is different and shows different responses, as can be seen in a variety of model inter comparisons. E.g. Cheng et al. (2013) showed that a weakening of the MOC varies considerably among different models under similar concentration pathways (RCP4.5, RCP8.5). They analyzed 10 different CMIP5 models and find that for RCP4.5 a weakening ranges between 5% and 40% between models and for RCP8.5 between 15% and 60%. Reasons for these differences can be attributed to differences in the gyre locations, deep water formation areas, precipitation and evaporation, etc. Such model dependent differences make it difficult to evaluate why we see exactly these differences.

This is in line with findings from other model inter-comparison studies investigating the response of the AMOC to perturbations, e.g. Gregory et al. 2005, or Swingedouw et al. 1013 or 2015). In all these studies it has not been possible to derive a clear explanation for the differences in the AMOC sensitivity of the different models, although the complete model output in some of these studies was available.

Here the situation is similar like in a warming climate, all models show changes into the same direction, but the magnitude of the response is different. A speculation on the reasons for the differences between different models would lack substance.

- Gregory, J. M., Dixon, K. W., Stouffer, R. J., Weaver, A. J., Driesschaert, E., Eby, M., Fichefet, T., Hasumi, H., Hu, A., Jungclaus, J. H., Kamenkovich, I. V., Levermann, A., Montoya, M., Murakami, S., Nawrath, S., Oka, A., Sokolov, A. P. and Thorpe, R. B. (2005) A model intercomparison of changes in the Atlantic thermohaline circulation in response to increasing atmospheric CO2 concentration. Geophysical Research Letters, 32 (12). doi: https://doi.org/10.1029/2005GL023209

- Swingedouw, D., C.B. Rodehacke, S.M. Olsen, M. Menary, Y. Gao, U. Mikolajewicz, and J. Mignot (2015) On the reduced sensitivity of the At-

lantic overturning to Greenland ice sheet melting in projections: a multi-model assessment. Climate Dynamics 44, 3261-3279. doi:10.1007/s00382-014-2270-x.

- Swingedouw, D., C.B. Rodehacke, S.M. Olsen, M. Menary, Y. Gao, U. Mikolajewicz, and J. Mignot (2015) On the reduced sensitivity of the Atlantic overturning to Greenland ice sheet melting in projections: a multi-model assessment. Climate Dynamics 44, 3261-3279. doi:10.1007/s00382-014-2270-x.

- Cheng, W., Chiang, J.C.H., Zhang, D. (2013). Atlantic Meridional Overturning Circulation (AMOC) in CMIP5 Models: RCP and Historical Simulations, J.Clim, 26, 7187-7197, doi.org/10.1175/JCLI-D-12-00496.1

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 land cover 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 interesting speculation on what could happen instead.

In agreement with the reviewer we certainly would have favored a consistent run with full carbon cycle coupling and a dust input field matching the projected pattern changes in land cover and atmospheric wind/precipitation fields. However, this is far beyond outlay of this study. As outlined in the comment above we speculate that the rather small changes in the carbon inventories of the land and marine realm would lead to only small changes in the atmospheric CO_2 concentration and, thus, would have no or a very minor effect on climate in a fully coupled set up. With respect to the impact of a fixed dust field on cyanobacteria growth in the Northern Indian Ocean we added a back of the envelop calculation to compare the iron supply from upwelling versus atmospheric input (Appendix D). We find that a large fraction (40-80%) of the iron demand of cyanobacteria could be delivered by ocean upward transport of the almost constant background iron concentration. We changed the main text correspondingly.

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

According to the reviewers suggestions we decided to move all perturbation studies to the appendix. Both chapters in Section 7 are sensitivity experiments, conducted to better understand the model simulations. The RETRO-S experiment allows us to estimate how much of the climate signal can be contributed to the rotation and how much to the reversal of the sun path. The section on climate sensitivity is important to estimate the response of a model to changing climate conditions. Given that the mean climates in RETRO and CNTRL

are very different we consider it interesting whether the climate sensitivity, and therefore feedbacks and interactions in the model system, change.

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

We agree with the reviewer and removed the word "numerous" here, as only two sets of sensitivity experiments were conducted.

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

We replaced "minor" with "other".

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.

We tried to strike a balance in how much weight we gave each section so as to better emphasize the integrated aspects. But given some ambiguity in how best to do this, the reviewer's input was helpful and suggested that we could add more material describing the atmospheric circulation with out overloading the reader. Therefore, we moved key results from former appendices A (storm tracks) and B (moisture transport) to the main section of the manuscript. As the reviewer also correctly pointed out, a complete analysis of teleconnections and ENSO would be too comprehensive and out of the scope of the current manuscript. However, we think that including a short note, as is done in the Appendix, is still interesting to the reader who is interesting in this topic. Therefore, we would like to leave the former Appendix C as is.

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...?

For this analysis 1000 years were used. For CNTRL we see clearly more energy in the band 1-10 years, especially compared to periods longer than 20 years (about 1/2 order of magnitude). For RETRO we do not see more energy in a specific frequency band for the period range from 2 to 100 years.

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.

We have made changes to the specific sentences pointed out by the reviewer and several other places throughout the manuscript.

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

We removed most occurrences of the word surprising. 'Expected', however, is left in the paper, as it indicates deviations from what we would expect from the classical theories.

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

Thanks, we changed this.

p3,l5: "how the ocean transport heats"

Changed.

p3,l[11—15]: "stabeliz[es—ing]"

Changed.

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

Added "with the climate model FAMOUS (Smith, 2012)" to clarify.

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

The original time axis referred to model years. For technical reasons we have to start in year 1010, so the real model years of the simulations are 1010 to 7999. We agree with the reviewer that this is confusing for the reader and have changed the time axis to start at 1.

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

MPI-ESM and CMIP are now introduced and relevant citations added.

p4l13: "vertic"

Changed.

p5,l12: "mid-altitudes have surface easterlies

Changed.

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

Changed.

p6,l16: "the American's warm"

Changed.

p6,l34: "like presentday Palau"

Changed.

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

The figure caption has been revised. The phrase in question hinted towards the different scale of northern and southern hemispheric plots. Probably more confusing than helpful, therefore we decided to remove it.

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

Yes, we agree it is a lot of real-estate to show very little change. But we wanted to draw attention to how similar the energy budget is despite large regional differences. This question has resonance when one considers that most of what we are sure about in terms of global warming is the global mean changes

to the energy budget, and hints at how different circulations can be for the same global mean.

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

Changed.

p12,l11: "vareity"

fig11,13: "Indic" Indian Ocean?

Thanks. Changed.

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

Changed into ' only few regions exist, where..'

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?

We moved the discussion on potential implication of changes in the land and ocean carbon inventories to the conclusion section.

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

Thanks, changed.

p29,l20: "Other studies" needs references

We included a reference to the Warren 1983 paper.

p33,l1: "Kongo" Changed into 'equatorial Africa', now in main text.

p33-35: figs C2,C4 not actually referred to in text. the former fig. C2 has been removed, the former fig. C4 was actually referenced, but with the wrong fig. number. This has been corrected.

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

Corrected.

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

Corrected.

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

Corrected.

p39,l11: two web links in citation

Changed.