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Earth System Dynamics
Discussions

Interactive comment on "The impact of oceanic heat transport on the atmospheric circulation" *by* M.-A. Knietzsch et al.

Anonymous Referee #1

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"The impact of the oceanic heat transport on the atmospheric circulation" by M.-A. Knietzsch, V. Lucarini, and F. Lunkeit

This paper explores the impact of changes in the Ocean Heat Transport on the atmospheric state, using idealized Aquaplanet simulations with a slab ocean. This study recovers results from previous publications on this topic. The authors carry on by applying two diagnostics to their experiments: a Lorenz energy cycle budget and a decomposition of the meridional overturning following the Kuo-Eliassen equation.

My main concern with this paper is that it is essentially descriptive. The diagnostics are applied, results are presented and described, but there is little more. Interpretation of the diagnostics and what they tell us about the atmospheric dynamics are almost non-existent. When there is an interpretation, it is unclear and incomplete (and

perhaps misleading); potentially interesting points are suggested but left out for future publications.

By the end of the paper, the reader is left wondering what has been learnt. Maybe the diagnostics presented here could lead to an interesting and enlightening interpretation of the atmospheric changes in response to OHT changes, but there is simply too little in this paper to judge.

This paper requires significantly more work: refinement of the diagnostics, a deeper analysis, and perhaps a revision of its scope (or maybe it needs to be merged with another publication).

Therefore, I do not recommend publication of this paper. Further comments can be found below.

1) Interpretation of the Lorenz cycle diagnostics: the discussion is limited to the bottom of page 12 and the top of page 13. It is short (for such a complicated topic) and unclear.

- line 15: "However, the sensitivity appears to decrease following the sequence of a baroclinic live cycle": I do not understand what is meant by this.

- Then, the authors refer to "zonally asymmetric diabatic heating/friction". Are those the external sources/sinks of the equations in section 3.1 (bottom of page 7) Se*, Sp* ? If they are key to the interpretation, it would be useful to plot them.

- line 17-19, page 12: "The convergences of the conversions with increasing OHT indicate that zonally asymmetric diabatic heating and friction become less important for the Lorenz energy cycle". Why are these terms changing with OHT? It seems important to explain how zonal asymmetries in a zonally symmetric set-up are key to explain the response to changes in meridional OHT (i.e. a zonally symmetric perturbation). I suspect that changes in the zonally asymmetric diabatic heating and friction terms simply reflect changes in the eddy activity with increasing OHT: changes in air-sea fluxes because of the non-linearities of the bulk formulae (similary for the frictional

terms: the surface stress is proportional to U^2, and the energy sink proportional to U^3).

If this is indeed the case, the zonally asymmetric diabatic heating and friction just tell us that about the weaker eddy activity. We knew this already. But this would also highlight the ambiguity of the diagnostics: eddy effects are in many terms.

Simply stating that frictional terms in the energy budget change is not useful if we don't know why.

2- The meridional overturning analysis. Again this is very much descriptive, with little discussion/interpretation. Nonetheless, the authors suggest that "the behaviour of the Ferrell cell is mostly controlled by friction" (based on Fig. 9). But why does the friction component change? Again, just describing the behavior is not useful.

It is likely that the surface wind and the surface wind stress weaken in response to increased OHT. We know that the surface stress is in balance with the vertical integral of the convergence of the eddy momentum flux: \tau_x = - \int d(\rho u'v')/dy dz (1)

So, are the friction changes just telling us that eddy momentum fluxes weaken in response to an increased OHT? Again we would already know this. One could hope that the applied diagnostics would give us a different (and interesting) perspective, but it is not convincing to me. The partition between the "eddy momentum" and "frictional" components of the atmospheric overturning is potential misleading as eddy effects are in both components. (In fact, eddies appear "twice" in the frictional component: directly because the surface stress is a non-linear term, and indirectly through the coupling between surface stress and upper tropospheric eddy momentum fluxes, as in Eq. (1)).

Similar questions could be raised about the changes in the heating and friction components of the Hadley cell sources.

Minor point:

-page 6, line 4-5: It is implied that the insolation is hemispherically symmetric because

the eccentricity is zero. I thought that this was always the case regardless of the eccentricity, no?

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