

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

Anonymous Referee #2

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The paper presents results from a series of aquaplanet experiments examining the atmospheric response to large-scale ocean poleward heat transport. The experiments are performed with a simple GCM (PlaSim) and are interpreted through examining changes in the Lorenz energy cycle and the mean meridional circulation using the Kuo-Eliassen equation. The question being addressed is interesting, the experiments themselves are well designed (building directly on previous work by Rose and Ferreira) and the analysis framework is promising. The discussion, interpretation and analysis is, however, a little disappointing. The diagnostic results are somewhat disjointed and often presented without sufficient explanation. As such, while the science in the paper has great potential and there is a lot of good material for the authors to use, I think some revisions and extensions are required to improve the presentation and discussion. Given the nature of the results, I would also encourage focussing the conclusions

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on the physical/process insights produced from the experiments (e.g., the baroclinic lifecycle energetics are potentially very interesting) rather than making rather large leaps to statements about the full system (see comments about Barriero below).

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Major

Sea ice - The experimental configuration appears to permit sea-ice formation and loss. Presumably this has a significant effect on surface temperatures and associated energy budgets and is alluded to at various points in the paper. There is, however, no figure in the paper that actually explains where the sea ice edge is. I find the comment about the insensitivity of global mean temp to OHT above 2.5PW potentially interesting in this respect (bottom of page 1472) as Figs 2 & 4 would suggest that this is around about the point at which the ocean remains ice-free in summer (at least at latitudes with positive ocean heat flux convergence). Later, the Ferrel cell is observed to start shifting polewards once OHT > 2PW (p1475, line 21). Does this suggest that sea-ice is playing a key role in controlling global mean temperature and/or Ferrel in this model?

p1476, line 1 - Kuo-Eliassen. I like the way this can be used to explore the contributions to the meridional circulation. However, I do not share the authors' confidence that it necessarily works for all the experiments simply because it works for the OHT=0PW case. It would be nice to confirm that the decomposition method works as well for OHT > 0PW before relying on the results.

p1473, line 25 - Statement about annual cycle contribution to C(P_M,K_M) etc. Why does the annual cycle only affect C(Pm,Km), Pm and Km? This is not immediately obvious to me.

p1474, line 14 - Conversion reductions suggestive of baroclinic life cycle. This seems to be potentially quite profound but is rather rushed over here and, as such, is not really convincing. It is more than a potentially interesting coincidence? Could consider

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looking at baroclinic activity diagnostics more directly?

p1474, line 19 - Diabatic heating and friction become less important for Lorenz energy cycle. What is the evidence for this - it isn't clear from the discussion and the source terms are not shown anywhere on the graphs or in the equations.

Conclusions section - I think it is good that the authors connect their work up to a "big picture" view of the implications of OHT. However, the main value of simplified GCM experiments tends to be in understanding processes rather than detailed predictions. I don't particularly object to the final comments (weakening of OHT under climate change giving a potential negative feedback, pg1478 line 20; and value of using these diagnostics for insights pg1479, line 1) but I do think that the comment on Barreiro et al (pg1477 line 12) is ambitious. Given the parameter sensitivity noted in Barreiro, the use of a very simple model in the author's experiments, and all the complexities of ice/atmosphere/ocean feedbacks in the real system, how much evidence is there to support the claim that the "present-day climate is close to a state where the warming effect of OHT is maximised"?

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Minor

p1467, line 26 - What is meant by a zero-dimensional sea-ice model? Would seem to suggest to me that it is a single constant for whole globe but presumably this isn't the case.

p1474 - It is difficult to compare the reductions in the conversions and reservoir terms in text form like this (at one point, one has to compare three sets of three numbers to see decreases in size across the triplets - this is made even more difficult as the triplets are presented in the wrong ordering on the page). I suggest a table would be helpful.

p1476, line 27 - I'd say the results are "consistent with" Stone rather than "confirming" Stone. The model used here is still a very simple GCM and likely very different from

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reality.

p1477, line 10 - Tropical SSTs sensitive to OHT. I thought that Koll & Abbot showed that tropical SST was insensitive to OHT (this is also stated in the literature review 1466, line 28) so is a bit confusing.

p 1478, line 9 - This appears to blame Rose and Ferreira for the coarse resolution used in your experiments here which I think is a bit unfair!

All Figures - make the lines thicker on the colour plots as it is very difficult to see them.

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Typos

p1463 - Title need revision - e.g., preface with words "The impact of . . .".

p1469, line 20 - Equations. Do you mean S_K^* and $[S_K]$ rather than S_E ? Seems to refer to a source of kinetic type rather than eddy type.

p1470, line 15 - Definitions of terms in paragraph near bottom. Use of double square brackets is confusing. Rewrite.

p1474, line 14 - "baroclinic life cycle" (not live).

p1478, line 23 - typo "therefore".

Fig 4 - JJA is northern hemisphere summer (not southern)?

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