Anonymous Referee #2
Received and published: 5 December 2014

We thank the referee for the constructive criticism and the helpful suggestions. In the following we answer (in normal text) the remarks by the referee (in *italic*).

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 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).

In view of the comments of referee #1 we merged the old version with work on the global thermodynamic properties (and add a new co-author). In doing so we substantially rewrote and/or rearrange most parts of the paper, thereby accounting for the referees comments and suggestions (see below). In particular we added new results concerning the atmospheric compensation of meridional heat transport and the residual mean circulation. We hope that the new version provides sufficient new and interesting results to warrant publication. We uploaded the new paper as supplement.

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 suggests 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?

We have included zonally averaged sea ice cover in Figs 2 & 3. It can be noticed that the sea ice edge gradually moves poleward for increasing OHT. Indeed, for OHT_max > 2PW there is no latitude completely covered with sea ice during the summer months. However, we did not found sufficient evidence that sea ice play an active role in controlling global mean temperature and/or the Ferrel cell.

We added to the summary:
'Sea-ice gradually decreases with increasing OHT. Though on annual average sea-ice is present for all simulations, for OHT_max>2PW areas of open water are present for all

latitudes during summer. This may suggest that sea-ice is playing an important role in controlling the global mean temperature and/or the position of the Ferrel cell. However, we did not find sufficient evidence to support this hypothesis.'

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.

To give an estimate of the error we included the respective values for the actual data in Fig. 9, and upload the reconstruction for all experiments as supplement.

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.

In the original version of the Lorenz energy diagnostics, the temporal variability of the zonal averaged quantities ([u], [v], [T]) is neglected (since the energetics is typically computed for individual seasons). Here, we apply the diagnostics for the whole year and thus would expect contributions (correlations) due to the annual cycle. However, a comparison to results were we have used annual mean values for ([u], [v], [T]) shows that only P_M and K_M, and the conversion C(P_M,K_M) are affected.

We tried to clarify by writing (Appendix C):

'We also note that by using above equations the computed annual averaged values include contributions from the annual cycle. It turns out, however, that only the reservoirs P_M and K_M, and the conversion C(P_M,K_M) are affected.'

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 looking at baroclinic activity diagnostics more directly?

We think that the correspondence between the changes and the baroclinic life cycle is worse to note. However, to verify whether these changes are due to changes in the baroclinic life cycles or just a coincidence would demand further analysis which is beyond the scope of the present paper.

We added:

'However, to verify whether these changes are due to changes in the baroclinic life cycles or just a~coincidence, further analysis is necessary, which is beyond the scope of the present paper.'

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.

We added the sources to Fig 16.

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"?

We agree that one have to be careful comparing idealized simulation with comprehensive ones, but we think that it may be worth to note some similarities. We changed the respective part to

'A tropical cooling for imposed oceanic heat transports somewhat
larger than present-day values has also been found by Barreiro et al. (2011) in a more complex coupled atmosphere-slab ocean model with present-day land-sea distribution. They argue that this suggests present-day climate being close to a state where the warming effect of OHT is maximized. Barreiro et al. related the tropical cooling to a strong cloud-SST feedback and showed that the results are sensitive to the particular parameterizations. Though our simulations are highly idealized and do not represent all the complexities of the real climate system, it is interesting to note that we find almost no further increase of the global near surface temperature for OHT_max>2.5PW and maxima in Θ+ and Θ- for about the same value of OHT.'

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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.

We changed to 'one layer thermodynamic sea-ice component'

p1474 - It is difficult to compare the reductions in the conversions and reserviour 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.

In the present version we restrict the analyses to annual values which makes the respective part more clear.

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 reality.

Agreed

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

We changed the respective part to
'...we observed a slight warming and a reduction of the gradient with increasing OHT. The latter is consistent with results from Koll and Abbot (2013). However, in their aqua-planet the tropical temperature show very little sensitivity with small increases for all imposed (positive) OHTs (up to 3PW).'

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!

It was by no means our intention to blame Rose and Ferreira. We now omit the reference to Rose and Ferreira when stating the potential effect of the low vertical resolution.

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

Done

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

We appreciate suggestions to improve the title. Unfortunately, we do not understand what is meant by 'preface with words'.

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

Corrected

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

Those brackets have been inserted by ESD's editorial work. We changed it.

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

Corrected

p1478, line 23 - typo "therefore".

Corrected

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

Corrected