Below please find the Point-by-point response to all reviews. Please note that we substantially rewrote and/or rearrange most parts of the paper in view of the reviews.

Anonymous Referee #1

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

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.

In view of the referees comments 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.

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.

We now embedded the Lorenz energy cycle in the discussion of global thermodynamic properties relating it to the efficiency of the system.

- 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.

We rephrased the respective sentence:

'Overall, the sensitivity of the eddy related conversions appears to decrease following the temporal sequence of a~baroclinic life cycle:...'

- 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.*

We now show the sources in Fig. 16.

- 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.

We embedded the Lorenz energy cycle into the discussion of global thermodynamic properties. The changes in frictional dissipation (and in diabatic heating) are now discussed in the context of the efficiency of the system.

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.

We agree that all components of the flow are strongly linked. The applied (linear) diagnostics by means of the Kuo-Eliassen equation can only give information about the direct effects of a particular sources (as discussed, e.g., by Kim and Lee 2001). We clarified this at the end of Appendix A:

'In addition, as pointed out by Kim and Lee, it should be noted that this diagnostics will only yield direct effect of the particular source. Since all processes are strongly interlinked changes in one source will lead to changes in other terms. For example, according to the equations of motion changes of the meridional eddy momentum transport will have consequences for the frictional dissipation of zonal mean momentum. These indirect effects cannot be identified with our (linear) methodology.'

However, in agreement with referee #2 and #3 we think that this diagnostic can provide interesting information.

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

If eccentricity is not zero (i.e. the Earths orbit not a circle) the distance between Earth and Sun changes during the annual cycle. As a result, one particular season is longer/shorter on one hemisphere than on the other (with todays orbital parameters, winter and fall are a bit shorter on the northern hemisphere), and, thus, on annual average both hemispheres will not receive the same insolation. In our experiments, we include the annual cycle (i.e. we set obliquity to the present day value) but ensure hemispheric symmetry by setting eccentricity to zero.

Anonymous Referee #2

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

Anonymous Referee #3

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*).

This is a theoretical study of the effects of ocean heat transport (OHT) on the atmosphere. A relatively simple slab ocean aquaplanet GCM is used to study the atmospheric circulation response to large changes in OHT. The experimental design is based closely on previous work by Rose and Ferreira (2013 - RF13 hereafter). OHT is imposed as a q-flux in slab ocean, based on a simple analytical formula following RF13. There is nothing new in the experimental design, and the model is of equivalent complexity to that used by RF13. The novelty of this study comes mostly from the diagnostic analyses on the atmospheric circulation: the Lorenz energy cycle and a quasi-geostrophic decomposition of the meridional overturning circulation through the Kuo-Eliassen equation. The manuscript raises interesting questions and the analytical techniques are promising. However I find the results to be largely descriptive and disjointed, and I struggle to identify what has really been learned from these experiments. The argument in favor of using a highly idealized model configuration such as this is usually that it permits a much deeper understanding of the results. In my opinion this manuscript requires some substantial revision to link the results together into a coherent physical picture.

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). We hope that the new version provides sufficient new and interesting results to warrant publication. We uploaded the new paper as supplement.

The paper would be much more satisfying if there was an attempt to relate changes in atmospheric heat transport to the changes in the circulation. The analysis begins by showing a very strong compensation by the atmosphere for enormous changes in OHT. From Figure 2 we can infer that the AHT across 27◦ varies between 0.5 PW and 5 PW in this suite of simulations an enormous range! As noted by the authors, this is not a new result. However I was hoping that the focus on circulation diagnostics in this manuscript would yield some new insight into the mechanisms that achieve this large compensation.

We added a more thorough analysis of the atmospheric compensation by splitting the atmospheric heat transport into its individual components (section 'Results'). The results show that the relative importance of the individual components remain almost the same for all OHTs, despite the large absolute change.

The authors might look at Czaja and Marshall (2006) for some ideas about the scaling of AHT with mass transport and stratification. In particular, I suggest looking at residual-mean overturning diagnostics to get a sense of the importance of eddies versus the mean meridional Hadley cells in effecting this large change in heat transport.

We added the analyses on the residual mean streamfunction and relate our results to the findings by Czaja and Marshall (2006). It turns out that the change in streamfunction strengths explains most of the decrease in atmospheric heat transport for increasing OHT and the poleward shift of its maximum. Changes in stability contribute to the changes in strength but not to the latitudinal shift (see section 'Results').

This would also provide a natural framework to link to the Lorenz energy cycle analysis. As it is now, Section 4.1 points out some interesting global results, but seems to defer any serious discussion of the meaning of these results to a companion paper.

We now embedded the Lorenz energy cycle in the discussion of global thermodynamic properties relating it to the efficiency of the system.

I think that the use of the Kuo-Eliassen equation to decompose the mean meridional circulation is the most important new contribution in this manuscript. It seems like a promising technique. However, since (to my knowledge) it has never been applied in this way (a suite of simulations that spans a large range of dynamical regimes), the authors need to document the errors more carefully. We are told in p. 1475 line 27 that the reconstructions are in "good agreement". Please be more quantitative here. Does the nature of the error change substantially between 0 and 4 PW OHT?

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.

As noted above, I suggest also looking at the residual mean overturning to get a complementary perspective on the role of eddies on the heat transport. The residual mean can also be decomposed into different forcing terms using quasi-geostrophic relations, see Peixoto and Oort (book referenced by the authors).

As suggested by the referee we included the diagnostics concerning the residual mean circulation (sections 'Diagnostics (Appendix A)' and 'Results').

Minor points:
Page 1465, Line 7: I don't understand what the authors mean by a negative feedback here. In what sense does the flattening of temperature gradients stabilize the climate system? In fact heat transport is a key component of the de-stabilization of the climate system in the case of runaway glaciation, see e.g. Roe and Baker (2010)

Baroclinic instability is strongly linked to temperature gradients (in particular to the large scale meridional temperature contrast). On the other hand, these temperature gradients are reduced by heat transport from eddies which are generated by baroclinic instability. Thus, the eddy heat transport acts as a negative feedback on the baroclinic instability mechanism and stabilize the system.

Indeed, heat transport can also be a key component of positive feedback mechanisms acting on climatological time scales (as studied by Roe and Baker). Perhaps, the term 'climate system' was a bit misleading by suggesting processes on climatological time scales.

Since we have substantially rewritten the introduction, the respective part does not occur anymore.

P. 1466, Line 25: "altitude" should be "latitude"

Corrected

p.1468, Line 4: What obliquity is used?

We use present day obliquity (=23.4) which is now stated in the 'model' section.

p. 1470, formula on line 13: I assume tg mean tangent. Can you use the more conventional notation tan(phi) here?

Changed

p. 1472, line 22: "budged" should be "budget"

Corrected

P. 1473, line 22, and Figures 4 and 5: the labeling of the northern and southern seasons is all mixed up. e.g. caption on Figure 4 reads "Southern Hemisphere summer (June-August)" I assume from the plotted temperatures that the plot is actually for June-August, or Southern winter. Please fix this caption, and the text on lines 21-22. Given this confusion, I am not confident that Figure 5 is properly labelled. Please verify.

Indeed there was a mix-up in the respective caption. We corrected the caption of Fig.4 (now Fig. 3). Since we now restrict the discussion of the Lorenz energy cycle to the annual values, the respective figure (now Fig. 16) and the discussion in the text has changed.

p. 1474 line 14: "live cycle" should be "life cycle"

Corrected

p. 1476, top: need some information about how big the errors are for non-zero OHT

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.

References: A. Czaja and J. Marshall (2006), "The partitioning of poleward heat transport between the atmosphere and ocean", J. Atmos. Sci. 63, 1498.

G. H. Roe and M. B. Baker (2010), "Notes on a Catastrophe: A Feedback Analysis of Snowball Earth", J. Climate 23, 4694.