Anonymous Referee #3
Received and published: 12 December 2014
Review of "The impact of oceanic heat transport on the atmospheric circulation"
by M.-A. Knietzsch, V. Lucarini, and F. Lunkeit (Earth System Dynamics discussion manuscript esd-2014-61)

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.