In the following we answer (in normal text) Dr. Lalibertés remarks (in *italic*)

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

This is my second review of this manuscript and after thoroughly re-reading the revised manuscript, it is my opinion that it is ready for publication except for minor technical corrections. I found several typos or phrasing decisions that could be improved, described in an annotated pdf file. Most of these corrections are either minor or, when they are concerning phrasing, should be taken merely as suggestions.

Again we thank Dr. Laliberté for his thorough and positive evaluation of our manuscript. In the revised version we took into account his corrections and insertions. In addition, we did a further check for typos, grammar and clarity, e.g. breaking the 'Result' section into subsections for better readability.

I have only one more important comment, concerning a citation of my own work. Around line 185, the authors cite Laliberte & Pauluis (2010) to explain why the moist isentropic circulation cannot be easily cast onto the latitude-pressure plane but this publication does not really mention the problem. In fact, it is Laliberte et al. (2012) that addresses this problem and I think it would be a preferable citation.

We now cite Laliberte et al. (2012) instead of Laliberte & Pauluis (2010).

Finally, I have a comment about the interpretation of Figure 10. In this figure, I would be tempted to say that the apparent "negative contribution" from surface fluxes is simply a consequence of the choice of moist entropy variable. If I'm not mistaken, the way the material entropy production is computed corresponds to a moist entropy where moisture is converted to heat only when it precipitates (i.e. the liquid-water entropy, see Emanuel 1994). This is why the precipitation leads to large entropy production in Figure 10. For some people (and maybe many) in the atmospheric community this could be a little bit confusing. Usually, precipitation is associated with pseudo-adiabatic motions and therefore intuitively with no material entropy production ("adiabatic"). It is my intuition that if you were instead computing latent heat fluxes corresponding to a moist entropy that is mostly conserved in pseudo-adiabatic motions (i.e. theta_e) then Figure 10 would show only small contributions from precipitations and most contributions would come from surface fluxes and rain-snow conversions. This would then tie in nicely with the results of Laliberte et al. (2015) and several of the papers by Pauluis et al, allowing the authors (if they wished to!) to explain some of the changes they are observing in terms of moistening inefficiencies. At this point in the publication process, I do not recommend that the authors change their analysis. This comment could however allow them to reformulate paragraphs 365 and 645 slightly to achieve more impact.

To account for the potential influence of the particular definition of 'moist entropy' on the interpretation of the respective results we added to the discussion of Figure 10:

'In addition, we note that the way we compute the entropy production associated to the hydrological cycle relies on focusing on water phase changes and related latent heat release/absorption, see Eqs. B5–B6. Using a moist entropy that is mostly conserved in pseudo-adiabatic motions would lead to a different partitioning of the material entropy production between precipitation, surface latent heat flux and rain–snow conversion.'

However, to explain our results in terms of moistening inefficiencies would, in our opinion, require substantial additional analysis, which we schedule for future work.