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Earth System Dynamics Discussions

Interactive comment on "Annual and semiannual cycles of midlatitude surface temperature and baroclinicity: reanalysis data and AOGCMs simulations" *by* Valerio Lembo et al.

Anonymous Referee #1

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The authors investigate the annual and semiannual cycles of atmospheric near surface temperature and baroclinicity (maximum Eady growth rate) in midlatitudes. They analyze the statistical relationship between the two quantities, and assess the ability of CMIP3 and CEMIP5 models to reproduce properties derived from ERA Interim reanalysis. The results show high coherence between the two variables for both the annual and the semiannual cycle, but with different relative phases. The CMIP models show good agreement with reanalysis for coherence at annual and semiannual frequency. For relative phase at semiannual frequency larger differences between models and reanalysis and among the models are observed. Improvements for CMIP5 models compared to CMIP3 are found.

General To test the ability of climate models to simulate the present day climate, and, thus, to give some confidence in their projection of potential future climates the simulated annual and semiannual cycles are appropriate testbeds. In addition, near surface temperature and baroclinicity (or maximum Eady growth rate, as an indicator for eddy activity) are important quantities defining the climate state. Thus, the study conducted here addresses relevant scientific questions and fits the scope of Earth System Dynamics. The methodology applied is sound and overall presentation is well structured and clear. Though the paper does not present novel concepts, ideas, tools or data, I think that it presents potentially valuable new results. However, I have three specific points the authors need to address before I can recommend the paper to be accepted.

Specific 1) I appreciate the spectral analysis of near surface temperature and baroclinicity individually, but the significance of doing the spectral coherence/phase analysis between both as presented in this paper is not clear to me. The authors only give vague motivation for doing the coherence analysis by saying that '... temperature is taken as a proxy in climate change studies' (P2L18) and '... investigating the possible relationship...may help to better understand the role played by the latter...' (P3L16+17). For me, the combined analysis of near surface temperature and baroclinicity appears a bit arbitrary and the meaning/interpretation of the results seems little conclusive:

(i) The results seem to indicate that the coherence is mainly restricted to the annual cycle (and higher harmonics) which may simply because both variables have an annual cycle without any physical relation between them. At the moment, I cannot see a significant indication for a relation other than a pure statistical one (see also 2). If understanding of the relation between near surface temperature and baroclinicity is an aim of this study, there need to be some more statements/discussions on this in the conclusion. At the moment the paper gives some discussions about baroclinicity and temperature gradients (e.g. SAO), but the link to temperature itself is missing, or appears to be an indirect one which is related to changes in near surface temperature gradients. In other words: for analyzing a possible relation between near surface temperature and baroclinicity the (equator-to-pole) temperature gradient may be a more obvious quantity. Why do the authors not consider this directly?

(ii) A second aim of the study is the evaluation of CMIP models. Given the somehow unclear (physical) interpretation of the observed coherences and relative phases between baroclinicity and temperature (see above), looking at the absolute phases of both variables may be of significant benefit. This may also help to identify the source of the discrepancies with respect to the semiannual cycle: do the phases of the baroclinicity or the near surface temperature (or both) differ? In this respect: L12 of the abstract suggests that also the absolute phases are considered.

2) P12L11-13: The authors state that '... the presence of a statistically significant semiannual peak in surface temperature spectral estimates, may suggest that the internal forcing exerted by baroclinic eddies play a role in modulating the annual cycle'. I do not understand this statement: Why does a significant semiannual peak observed in the surface temperature indicate a forcing different from the solar one, and, moreover, a forcing related to baroclinic eddies? Please clarify.

3) In the paper the authors use surface temperature and near surface temperature as synonymously, but actually atmospheric near surface temperature is used. Thus, to be more precise/clear, the authors may use the term near surface temperature throughout the paper. I do not think that the results are very sensitive to the particular choice of surface or near surface temperature. However, there are major differences between both variables, e.g. while the surface temperature is prognostic in the models, the near surface temperature is diagnosed from the lower atmospheric levels by using a somehow artificial role related to the computation of the surface fluxes.

4) The summary and conclusions section suggests that zonally (and vertically) averaged values are used in this study (P11L7). But, from the results section we learn that for the northern hemisphere the analysis focus on the Pacific. This should be made clearer (more consistent) in the summary and conclusions (Otherwise, e.g., the origins

of results (iii) and (v) are unclear for readers focusing on the summary and conclusions only).

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