

Reply to the Review on

Annual and semiannual cycles of midlatitude near-surface temperature and tropospheric baroclinicity: reanalysis data and AOGCMs simulations

by

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We thank the Referee #2 for his/her comments. Changes in the text are in **red**.

Reviewer #2

I have appreciated the effort made by the authors in preparing the present revised version: the paper quality has improved. However, I do not think they have fully replied to my major comments; still, I am aware that some questions I posed were difficult to be answered within the limitations of this work. Also, perhaps I was not clear enough. The authors have also considered the majority of my minor comments. However, there are two of them which I think need additional consideration before I consider the paper worth of publication - I report here my original comment and the authors' reply:

4. Page 2, line 8: "... and the atmosphere is heated from below": this seems to contradict the previous sentence that most of the (seasonal) atmospheric heating is due to direct atmospheric absorption. Pls. clarify. Reply: It is not a contradiction since the dominant oscillation is the annual one that is strictly related to the insolation. As known, the atmosphere is almost transparent to the short wave radiation that is absorbed by the Earth's surface.

My comment: Donohoe and Battisti (2013) indicate that the "DIRECT shortwave absorption" by the atmosphere is an important contribution for the annual cycle budget, so the heating from below is not the only relevant term. It seems to me that this means that the atmosphere CANNOT be considered almost transparent to shortwave radiation. I was myself a bit surprised by reading that, but I must confirm my previous comment: there is a contradiction in the sentence in the present paper and the authors should resolve it, after carefully reconsidering the results of D.&B (unless they have reasons to question them...).

Reply:

In the Introduction we revised the text as:

"The seasonal cycle of the heating of the atmosphere is one of the most prominent features of the Earth's climate (e.g., Kiehl and Trenberth, 1997; Trenberth and Stepaniak, 2004). A recent study by Donohoe and Battisti (2013) suggested that while in the annual average heating is dominated by upward energy fluxes from the surface, such as longwave, latent and sensible heat fluxes, most of the seasonal heating (i.e., the heating variability after subtracting the annual mean) is due to the direct shortwave absorption within the atmosphere, that is quite constant throughout the troposphere."

27. Page 9, lines 33-34: again, a physical interpretation is missing here. The sentence "... the role of the semiannual variability in shaping eddy activity" is meaningless: "variability" is a physical/statistical property, not a physical factor. Reply: We changed "shaping" with "modulating" that is more appropriate.

My comment: "modulating" is better than "shaping" - however, it was the subject, not the verb, that I questioned. The "variability", without specifying of which physical quantity, cannot be considered to be a physical variable/factor... this is the point I tried to make. It is not only a matter of

language. If not better specified, it risks to be a tautology: the modulation of eddy activity is itself a variability!

Reply:

At section 3.2, page 10, we revised the text as:

"At the semiannual frequency, a phase shift of about 50° is observed in the SH and about 80° in the NH Pacific, with surface temperature delaying by about 1 month or more compared to the opposition of phase: results seem in agreement with the SAO phenomenon and may be indicative of the role of the semiannual harmonic in modulating synoptic time-scale baroclinic eddy activity (an example is the midwinter suppression characterizing the North Pacific storm tracks)."