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 are reported in *italic* and in the text 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 ware 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:
We agree with the reviewer that the two sentences in the first period of the paper might be misleading as such. Our aim was here to emphasize that there is a clear separation between the factors influencing the atmospheric heating at the seasonal and annual time scales. This is not meant to contradict Donohoe and Battisti (2013) by stating that the atmosphere is transparent to shortwave radiation, either in the seasonal, or in the annual timescales. Indeed Donohoe and Battisti (2013) clearly evidenced the dominant contribution of direct SW absorption for the seasonal cycle of the atmospheric heating.
In addition to that we might specify that in the annual mean roughly one fourth of incoming SW radiation is directly absorbed into the atmosphere (e.g. Wild et al., 2013, arguing that they amount to 340 and 79 W/m2, respectively). This estimate also accounts for a small part of SW absorption reflected by the surface, but this is marginal with respect to the SW radiation directly absorbed by the atmosphere. The heating from below is mainly effected by upward LW radiation, latent and sensible turbulent heat fluxes (accounting for 398, 84 and 20 W/m^2 respectively, according to Wild et al., 2013). To a zero-order approximation the net energy balance for a climate system in thermal equilibrium requires an annual mean LW emission to outer space of 240 W/m^2 (which equals the

amount of net SW radiation entering the system). Thus, the balance between LW radiation emitted by the surface and that exiting from the Top of the Atmosphere amounts to about 150 W/m^2, which in addition to the heat fluxes at the surface ensures that most of the atmospheric heating comes from the surface
We would appreciate if you might consider the revised version of the first part of the Introduction, where the annual-seasonal timescale separation is more clearly expressed:

"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 (e.g. Wild et al., 2013), most of the seasonal heating (i.e., the heating variability after subtracting the annual mean) is attributable to the direct shortwave absorption within the atmosphere, with an amplitude that is quite constant throughout the troposphere."

Wild, M., Folini, D., Schär, C., Loed, N., Dutton, E.G., König-Langlo, G.: The global energy balance from a surface perspective, Climate Dyn., 40, 3107–3134, 2013.

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:
We thank again the reviewer, because he/she evidenced that it was not clear enough in the text that we were resuming our results, referring to the modulation of the baroclinic eddy activity by means of the semiannual harmonic. This statement mainly involves the statistics of baroclinic eddies (whose 3–7 days timescale, often referred to as "synoptic timescale"); their activity is of course modulated by the incoming radiation annual cycle, affecting the annual cycle in the meridional temperature gradient, to whom the baroclinic index here used is proportional. Our results show that the baroclinic index is also characterized by a semiannual harmonic in both hemispheres, which modulates the synoptic scale baroclinic eddy activity. These results are in line with what was previously found in the SH mid-latitudes, and, at a regional level, in the NH Pacific mid-latitudes.
In order to improve the readability of this period, we agree that using the term "variability" is not appropriate, since it is not specified that we particularly refer to the six-month harmonic in the baroclinic index as a modulator of the synoptic scale baroclinic eddy activity. We thus changed the text in section 3.2, page 10 as following:

"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 NH synoptic time-scale baroclinic eddy activity (an example is the midwinter suppression characterizing the North Pacific storm tracks)."