Review of the paper "Annual and semiannual cycles of midlatitude surface temperature and baroclinicity: reanalysis data and AOGCMs simulations", by V. Lembo, I. Bordi and A. Speranza.

This paper analyses some statistical properties of the global atmosphere in comparison with results obtained from general circulation/climate models. In particular, the annual and semiannual cycles of near-surface temperature and of an index of baroclinic activity are examined in detail.

The technical execution is basically correct and applied analysis methods are reasonably funded and appropriate. At least part of the results is original and interesting. What is basically missing, in my opinion, is a deeper physical discussion of the working hypotheses and of the results. This deficiency makes this paper to appear more as a preliminary contribution than a conclusive one (the authors are aware of that, as they write in several circumstances "... require further investigation" or so). I think that this paper would gain in quality if a discussion is provided at some appropriate points and in the conclusions, as expressed in the following general comments. Therefore, I consider that this paper can be made worth of publication if a revision will be made in response to my indications given below.

General comments.

1. Two parameters are chosen for the analysis, the near-surface temperature (T2m) and a tropospheric-averaged baroclinicity index (σ_{BI}), expressing a theoretical grow rate of baroclinic instability. I think that this choice of the parameters should be better explained. In fact, the physical link between such quantities is very complex and can be very different depending on the time scale of the processes involved. The fact that they are strictly correlated (actually, anti-correlated) in the annual cycle is quite obvious, with reference, for example, to the results exposed in the paper by Donohoe and Battisti (2003, cited in this paper): considering the large impact on the atmospheric seasonal heating due to direct absorption of the solar radiation (not neglecting, of course, the role of land and ocean fluxes), and considering the very large annual cycle of both solar radiation and of the meridional gradient of it (which even changes it sign between solistices), it is not surprising that both quantities are strongly modulated by the astronomical cycle. However, this does not mean that the two quantities have a simple relation between each other, stemming from the atmospheric circulation dynamics! It seems that the choice of T2m has been made having in mind the application of climatic models to the long-term global changes (see for example line 18 of page 2), but this is something different form considering the seasonal-annual variability. On the other hand, the baroclinic index may be a subtler indicator of long term variability, while it is closely related to many aspects of the general circulation dynamics, including of course mid-latitude baroclinic "turbulence" and its limiting factor.

2. Concerning the evaluation of the AOGCMs, the intercomparison based on seasonal-annual harmonics may not be very appropriate to assess model performance with respect to long-time variability, associated for example with the dynamics of slow processes like those related to ocean deep circulation, evolution of the cryosphere and the biosphere etc. However, I agree that a "good" climatic model should behave well also in simulating/predicting intra-annual time scales.

3. Regarding the appreciation of annual vs semiannual cycles, one should consider, in principles, that while the astronomical forcing is basically annual ta mid latitudes, it is

basically semiannual at the equator. The analysis of the paper is restricted to the mid latitudes, but how does the intertropical semiannual variability affect the extratropical variability? The authors correctly mention possible relations with the SAO, but a more specific discussion (at least more detailed references to the literature) is needed in this respect, regarding SAO and perhaps more general semiannual aspects of the atmospheric global circulation.

4. As anticipated above, the discussion of the results and the conclusion miss a sufficient consideration of the physical implications in terms of known properties of the general circulation of the atmosphere. I am asking for at least a somehow better indication of the perspectives derived from the results of this paper in terms of (a) possible relationships between the ERAI-based results and some known aspects of the global atmospheric dynamics (for example the SAO), and (b) somehow less vague indications of the possible implications concerning AOGCMs' performance, so as to provide modellers with more specific hints (only the horizontal resolution is mentioned at line 33 of page 10, which is perhaps not the most important aspect: I assume that many more subtle physical and dynamical problems have still to be solved to improve the model accuracy).

Minor comments.

1. Title (and also in the text: for example, at line 9 page 1, lines 21 and 24 page 2): it is not clear (before reading the definition in Sect. 2.2) if the word "surface" is referred also to "baroclinicity". So I suggest to use "tropospheric baroclinicity" or something similar in the tile and in the text, before the exact definition is given in Sect. 2.
2. Page 2, line 2: contribute.
3. Pls. specify that "seasonal heating" is intended as the heating variability after subtracting the annual mean.
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.
5. Page 2, line 17: perhaps "... the first efforts *in understanding* the climate impact...".
6. Page 2, line 18: proxy of/for what? of globally averaged temperature? Pls. specify.
7. Page 2, line 19: "*its* key role".
8. Page 2, line 24: AOGCM in place of GCM (as elsewhere in the text).
9. Page 3, lines 2-3: consider this change "there are regions of enhanced eddy activity ("storm tracks"), where... decay.".
10. Page 3, line 5: the word "suppression" here seems too strong – maybe "limitation". Unfortunately, the annual cycle of baroclinicity in the NH Pacific is not reported in any figure - if I am not wrong - in this paper.
11. Page 3, line 15: self-sustain – why "self"?
12. Page 4, line 11: "isobaric levels" in place of "vertical levels" (also at line 4 of page 5).
13. Page 4, lines 18-19: the sentence "For the pressure analysis 8 pressure levels..." should be dropped because it is a repetition.
14. Page 4, lines 27-28 ("INM-CM3..."): also this sentence is redundant.
15. Page 5, lines 21 and following: for the sake of clarity, pls. introduce here the full definition, specifying averages (zonal, vertical, latitudinal) of both quantities – part of the specifications is given below (lines 29 and following), but it should be anticipated before any further description and comment for readability. U is the zonal wind component, not the horizontal wind component.
16. Page 5, lines 29: "is averaged over" in place of "takes into account" (too generic).

17. Page 7, line 22 (and elsewhere): the standard acronym for geopotential height is GPH, not GPT.
18. Page 7, line 22: "... variance in the *same* midlatitude belt". Moreover, the word "variance" is not used here in its technical sense and should be better defined here or substituted with "variability" (also at line 25 and maybe elsewhere).
19. Line 24: is the 15-day moving average "tapered"? If not, some high frequency noise is retained (but this problem may affect the high frequency part of the spectra, not the 6-month period).
20. Page 8, lines 1-3: perhaps insert here (or also in the conclusions) a comment referring to the results of the cited paper of Donohoe and Battisti.
21. Page 8, line 4: pls. specify better which aspects of the MSLP cycles can be related to SAO.
22. Page 8, lines 16, 17 (and elsewhere, as for examples lines 31 and 32): I think the article "the" should be put in front of NH and SH (unless, of course, they are used as adjectives) – please try to be consistent throughout the text.
23. Page 8, line 18: perhaps "For a comparison", in place of "As an example".
24. Page 9, lines 1-2: the relationship between surface temperature and baroclinicity cycles cannot be deduced by the simple analysis of this paper, considering that (in particular for the annual cycle) both quantities are subject to solar "forcing" (see also my general comments 1 and 3).
25. page 9, lines 9-10: ".... for the analysis ERA (and in Figures 8-9 for model output, described below), respectively".
26. Page 9, line 15: the word "correlation" should be intended here in the statistical sense, with no physical/dynamical implication (see comment 24 above).
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.

Figures:

Figure 1 perhaps contains too many lines, with no labels - so it is not easy to distinguished the curves on the basis of colours only. I suggest to drop the GPH at 500 or 300 hPa, since they are very similar.

Figure 6: captions need to be corrected, because, unlike Fig. 5, this figure depicts spectra only for T2m, not for baroclinicity.