Responses to Reviewer 1's comments

In italics bold

Pfeffer et al discuss the evidence for 6 year oscillations in the atmospheric angular momentum, winds and length of day. The topic is of interest to readers of ESD and is an area where significant questions remain so the manuscript is very appropriate for this journal. The 6 year variability shown in this study is intriguing, not least because it appears in atmospheric winds and is *negatively* correlated with length of day data (assuming I have understood the paper correctly). This is very different to interannual variability in LOD and AAM in general suggests that there must be a third entity to complete angular momentum conservation and this is suggested to be the fluid outer core of the Earth. The paper does various filtering and compares to ENSO variability - an obvious possible culprit given its known connections to length of day. However, I have two main criticisms. Some of the statistical analyses and figures are not very convincing and very similar versions of some of the results have been published elsewhere (e.g. Cazenave et al 2025, Pfeffer et al 2023). I therefore recommend major revision after the arguments are strengthened and it is made clear what is really novel here.

We thank Reviewer 1 for his/her comments

MAJOR POINTS:

A) A number of papers have been published in recent years that show diagnostics of the 6 year variability. For example Cazenave et al 2025 and Pfeffer et al 2023. These previous papers already show some of the results presented here. For example, that the 6 year variations in LOD appear to be anticorrelated with the atmopsheric angular momentum (Fig.6) and the results in Fig.8 on solid Earth dynamics are similar to previously published results. I think there is a need to establish what is novel and to focus on that.

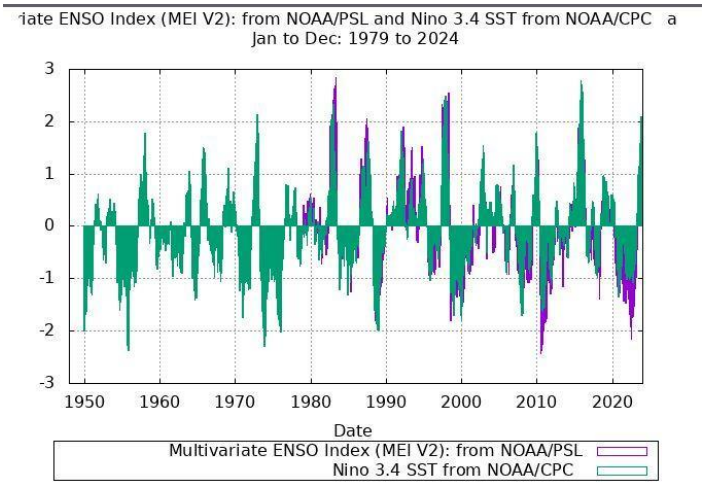
We have removed figures 6 and 8 of the original manuscript (shown in previous publications by Pfeffer et al., 2023 and Cazenave et al., 2025) and just recall in the introduction and discussion sections previous findings. The revised version now essentially focuses on the new results about the 6-yr cycle in the atmospheric zonal wind circulation.

The new contribution of the present study relies on the following three aspects:

(1) Systematic assessment of robustness of the 6-yr cycle across different atmospheric datasets
(2) A detailed characterization of the zonal wind 6-yr cycle
(3) An explicit comparison with other atmospheric modes of interannual variability

B) The authors use an ENSO index that depends on a range of indicators and this could be somewhat circular when later comparing against the atmospheric winds etc. To check that this is not the case they should recalculate Fig.4 and Fig.5 using the more commonly used Nino3.4 index - does it give the same results?

The MEI and Nino 3.4 indices have very similar variability as shown in the figure below. But we also used Nino 3.4 index in addition to MEI to check whether the results differ or not.



C) There is significant amplitude change with time in Fig.2 and along with the preselection of 6 year variations using the filter and the fact earlier periods show less sign of 6 year cycles (L232), this all makes me worried that the apparent 6 year cycles you get here are really a produce of one short time interval when single variability events such as the El Nino of 1997/98 happen to alias onto 6 year variations. Can you show that doing a separate analysis of the first and second half of the record in Fig.1 and 2 and omitting the large El Nino of 1997-1998 gives similar spectra and that Fig.5 looks the same in both halves of the record? I think this is important if the claims are to be convincing.

A novel aspect of this revised version is the removal of the ENSO signal in the zonal wind data before any subsequent analyses, via a regression between an ENSO index and the zonal wind time series (after either global averaging over the Earth's surface or after longitudinal averaging). The spectral analyses have been recomputed with the ENSO-corrected data sets as well as all other figures (Hovmöller diagrams, etc.)

D) Fig.3, L406. This is not very convincing of a stable cycle as the latitude varies greatly and could easily be generated by aliasing of variations onto the 6 year filter timescale. Similarly, Fig.4 suggests the 6y cycles occur on the edge of, or near to ENSO variations, which could be an indication of variability in the ENSO cycle that happens to project onto 6y periods. Can you first regress out any ENSO variability from the data using the Nino3.4 index or similar and then reproduce this figure as a second panel?

As suggested by Reviewer 1, and as indicated above, the ENSO signal has been removed in the zonal wind data before any subsequent analyses via a regression of the zonal wind time series (after either global averaging over the Earth's surface or after longitudinal averaging) with the ENSO indices. The spectral analyses have been recomputed with the ENSO-corrected data sets as well as all other figures (Hovmöller diagrams, etc.). This avoids any aliasing of the 6-yr cycle with ENSO.

E) L52, L480: I think the authors need to better acknowledge here that it is quite plausible that the 6y cycles could originate in the surface climate and drive the LOD and fluid core variability. After all it is very unlikely that the atmosphere would produce detectable responses in 0.1ms/24h changes in rotation rate whereas the climate system contains much chaotic internal variability on multiyear timescales.

We agree that interactions between the climate system and Earth rotation deserve careful consideration. However, there is clear evidence for the core role in the system. First of all, there is evidence for a core-related origin of the 6-yr LOD signal. In effect, several studies have shown that the 6-yr cycle in LOD cannot be explained by angular momentum exchange with the atmosphere, oceans, or continental hydrology, and is therefore most likely linked to CMB interactions. More recent investigations confirm that the ∼6-yr cycle in LOD is primarily driven by transfer of angular momentum between the core and the mantle, as inferred from geomagnetic data and core flow models.

Secondly, several studies have shown that the atmosphere is a major contributor to subseasonal, seasonal and ENSO-related LOD variations, with the corresponding atmospheric angular momentum being in phase with LOD (indicating transfer of angular momentum from the atmosphere to the mantle rotation). This is not the case at the 6-yr periodicity where the atmospheric angular momentum is in phase opposition with LOD, suggesting that mantle and atmosphere oscillate in the same sense at this particular frequency.

The direction of causality between the signals observed in the atmosphere, Earth rotation (LOD), and deep-Earth processes remains unresolved. This issue has been discussed in Cazenave et al. (2025), who discuss several possible interpretations of quasi-6-year variability across the Earth system, including core-driven processes or external forcing scenarios causing independent core oscillations (the latter driving LOD variations) and climate variations at the 6-year periodicity.

Our intention was not to imply a preferred causal pathway. Rather, the purpose of the present study is to document the atmospheric expression of the quasi-6-year signal, its structure, and its robustness across independent datasets, and to place these results in a broader Earth-system context. New investigations are definitely crucially needed to understand the mechanisms causing a 6-yr oscillation of the whole Earth system.

MINOR POINTS:

Fig.1: Is it pure coincidence that the 2 peaks contain the frequencies that differ by a factor of two? Is it possible these are related, or even harmonics?

In effect, we cannot avoid such a possibility. New investigations should be carried out on this important issue.

Fig.1: Are the winds area weighted before averaging? It is important to do that but I did not see it in the description. Also, are the winds deseasonalised to remove the annual cycle first or not?

Yes, a latitude weighting has been applied.

L138: better to say 'could be related to the solar cycle'. A reference to Abarca Del Rio et al., J. Geodyn., 2003 is also appropriate here.

The reference has been added.

Please can you add a plot of the full, unfiltered data to Fig.2? If the 6y oscillation is 25% of the variance it should be visible to the eye.

The zonal wind data are subject to various interannual signals, with ENSO and biennal oscillations being strong enough to mask lower amplitude signals (including the 6-yr oscillation).

L347: please be clear about what you mean by 'is corrected for AAM' here

The sentence has been clarified

L401: Vertical coherence is seen in many other atmospheric variations

We added such a sentence

Fig.3 shows signs of poleward propagation and the hemispheric symmetry mentioned on L426 is very similar to Scaife et al, Nat. Geosci., 2022 so I think some discussion is needed here.

A few sentences have been added here

L403,417: This is an interesting point. Can you emphasize this is very different to what is seen for the total interannual variability of the AAM and LOD?

We think that the proposed discussion is already consistent enough.

Fig.8: are the core flow models shown in Fig.8 empirical in nature, if so, are some of these statements circular?

We removed Figure 8 as it was previously presented in Cazenave et al. (2025).