General comments

I thank the authors for having responded to nearly all my points and brought some new information related to my comments. However, the new analysis do not bring convincing arguments for a core-origin of this 6-yr oscillation, on the contrary, they demonstrate further the role of surface mass variations, as I explain here after:

(1) Verification using hydrological loading models at IGETS sites

Thank you for making the analysis of various hydrological loading models. When looking at the CWT spectra of Fig. 3 in the Reply_comment.pdf, we do see clearly a 6-year oscillation at most SG sites. The scales are not the same than in the paper, in particular the time window is much longer here, giving a less stable impression for this oscillation. We also see from the FFT spectra that the interannual content between different hydrological loading products is quite different. The hydrological models being imprecise, there is no reason to have peaks exactly aligned with the SYO, particularly also, because you performed the analysis on different and longer time windows than for SGs. So we cannot exclude from this analysis the role of hydrology since the models are not precise enough (large dispersion in their interannual content). That confirms also the analysis by Pfeffer et al. (2022, https://doi.org/10.5194/egusphere-2022-1032) that I mentioned in my previous comments.

(2) Verification using global gridded precipitation data

Here also, you have performed the wavelet analysis on a much longer time window than for SGs, hence a frequency shifted with respect to the SYO, and less clear. We clearly see on Fig. 6 and Fig. 7 the presence of an oscillation around 6-years. If you consider the same shorter time-window than for SGs, the SYO will be clearer. It is particularly stronger and clearer after the 1990s, when you indeed analyzed the SG time-series…

Consequently, I disagree when you write that you did not find any significant and consistent 5.9-yr oscillation in hydrological loading model and global gridded precipitation data, since in fact we do see it. We would see it better if you use the same time-windows as for SGs.

Line 118-119: I disagree also with the claim that the "hydrology-excited LOD time series does not contain the ~5.9-year signal" since we do see it in Fig. 4(c) of Rosat & Gillet (2023, https://doi.org/10.1016/j.pepi.2023.107053) with a yet small amplitude around 0.012 ms.

(3) Verification using climate indices, GMST, and GMSL

Same remark can be made on Fig. 8, where, contrary to what you wrote, we do see a signal around 6 year, which will also be clearer when using smaller time-windows as for SGs.

In Fig. 9 also the time-window is much too long to identify the 6-yr content since we add more spectral content and cannot be compared to the shorter (and less resolved) SG time-series.

In Fig. 10, the trend should be removed before computing the FFT and the CWT else you introduce large long-period content. The time-window used is here also much too long to be compared with SG time-series.

(4) Verification using the oblateness ΔJ_2

Only a certain distribution of terrestrial water storage variations would result in ΔJ2 since ΔJ2 is related to zonal degree-2 pattern (C20 in terms of Stokes coefficient). The hydrological content at interannual time-scales is mostly related to degree-2 order-2 geographical pattern (C22 and S22 in terms of Stokes coefficient; see for instance Meyssignac et al. 2013). Interannual variations in degree-2 Earth's gravity coefficients C2,0, C2,2, and S2,2 reveal large-scale mass transfers of climatic origin, Geophys. Res. Lett., 40, 1-6.), so of course it would not contribute much to ΔJ2. This argument is then inappropriate.

<u>About the AR-z spectrum</u>

I thank the authors for sharing the information about the availability of the AR-z spectrum method. Since "the power of the peaks in the AR-z spectrum directly correlates with their stability rather than their actual amplitude", I would be curious to see how it performs on the hydrological loading time-series (same time-windows than SGs) that you analyzed by FFT but for some better hydrological models like ERA5_land. The ERA-interim model that you used in the Supplementary material is of poor quality (as seen by comparison with GRACE for instance).

<u>Concerning the ratio δ/h for continental hydrology</u>

You have not responded to my remark concerning the influence of **local contribution** of hydrological mass changes that play an important role on the value of this ratio.

Lines 274-275-277: Again, I disagree in the fact that hydrological loading has a negligible influence (see previous comments and wavelet spectra that exhibit 6-yr content, particularly during the time-periods on which SG data were analyzed).

<u>Concerning SG data</u>

I have downloaded Level-2 SG time-series from IGETS website and performed the CWT and FFT analyses of all time-records longer than 18 years. The step corrections as well as the instrumental drift correction are very sensitive processes that would modify consequently the interannual spectral content. For a few stations, the SYO is clearly conspicuous but for others it is not. It really depends on the station considered and the pre-processing of data. Even among the stations you have used, I have quite different spectra. I do see it clearly for Strasbourg and Metsahovi (with a nice anti-correlation with hydrological loading), but not for Canberra, Cantley, Medicina, Membach, Moxa (not at 6-yr but different periods, like 4-yr, 5-yr or 7-yr). If the SYO was so stable, a different pre-processing of same datasets (with a weak proportion of steps and gaps) should still make it visible when the time-series is long enough.