General comments

I thank the authors for having responded to my points, some of my points were misunderstood other I still disagree:

(1) Previous evidences from different observation data (e.g., ΔLOD, geomagnetic field, GNSS, and polar motion) have indicated that the SYO in ΔLOD is an almost stable oscillation with attenuation larger than 180.

I agree that the SYO is clearly observed in ΔLOD, it does not mean a SYO in gravity (associated with mass variations mostly but to a lesser extent to centrifugal effects) would be the same signal, in particular here we are looking at mass contributions. A large part could (I think it is most probable) result from surface mass redistribution. It is possible that a core origin SYO is also present, but the problem I mentioned is how to disentangle it from surface mass redistributions, like hydrology and climatic events, which has large interannual content as demonstrated by the previous studies I mentioned.

(2) Regarding the verifications using global gridded precipitation data, climate indices, GMST, and GMSL, you both posed questions similar to that in validation using hydrological models. Specifically, all of these validations should be conducted using smaller time windows as for SGs. It is imperative to reiterate that it is necessary to use observations for extended durations, preferably in alignment with the ΔLOD time, to ascertain the presence of a stable ~5.9-year fluctuation before establishing a correlation between it with the SYO in LOD.

I agree that it is necessary to have extended durations to validate the presence of a SYO, but in the SG data you do not have the required extent (contrary to gridded precipitation data, climate indices, GMST, and GMSL). But for a fair comparison with your shorter SG data, you should compare precipitation data, climate indices, GMST, and GMSL on the same time windows.

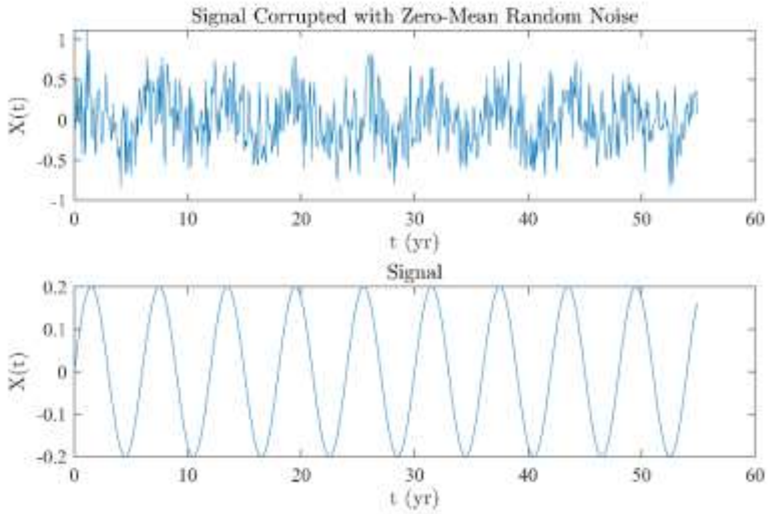
However, it must be founded on a precondition that the SYO is actually present in the hydrological models.

No, there is no need for any precondition about the presence of a SYO, since it is well-known that there are interannual climatic events. As for a precondition that the SYO is actually present in SG data is rather your own hypothesis since when applying the AR-z spectrum we cannot see it and for some SG sites I do not see it either with the FFT. I have downloaded the AR-z code and made synthetic tests using values to mimic the SYO (55 years of data sampled at 0.1 yr, a SYO of amplitude 0.2 buried in a white noise of 0.3) (Fig. (a)).

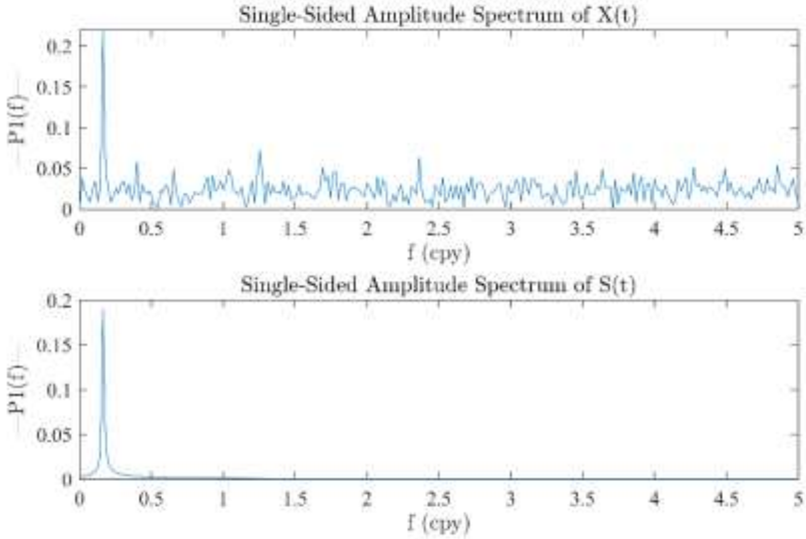
```
Fs = 10;              % Sampling frequency
T = 1/Fs;               % Sampling period
L = 55*Fs;               % Length of signal
t = (0:L-1)*T;          % Time vector

% construct the signal data
B = 0.2;
S = B*sin(2*pi*1/6*t);
% add the noise data
X = S + 0.3*randn(size(t));
```

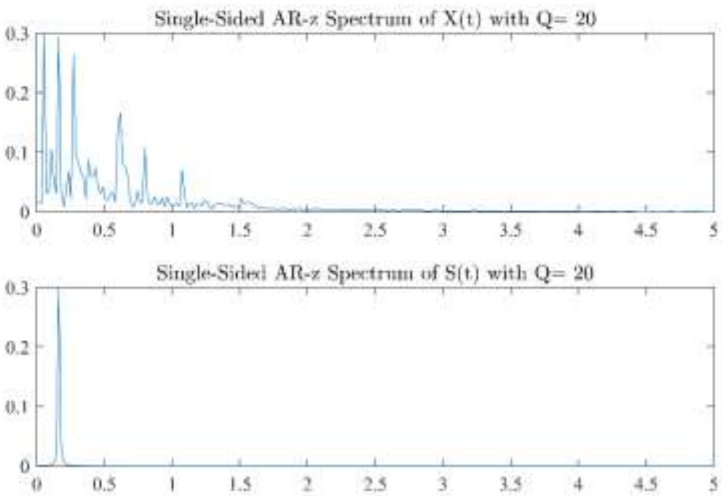
(a)


Signal Corrupted with Zero-Mean Random Noise
X(t)
t (yr)
Signal
X(t)
t (yr)

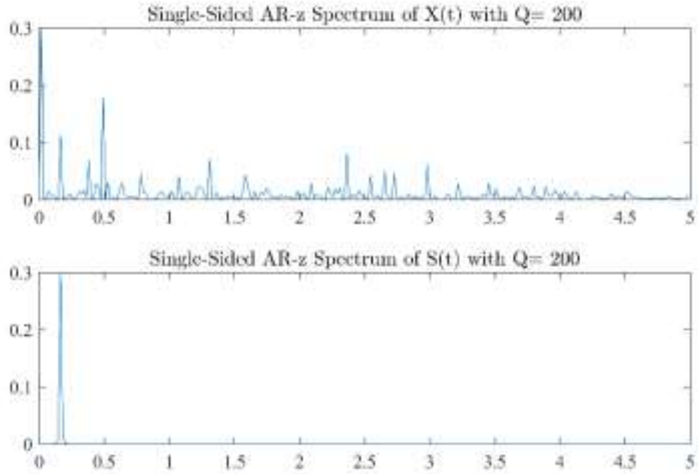
(b)


Single-Sided Amplitude Spectrum of X(t)
P1(f)
f (cpy)
Single-Sided Amplitude Spectrum of S(t)
P1(f)
f (cpy)

(c)


Single-Sided AR-z Spectrum of X(t) with Q= 20
Single-Sided AR-z Spectrum of S(t) with Q= 20

(d)



When there is noise (even just white noise not colored one) besides the SYO, the FFT clearly shows the spectral peak Fig.(b), but with the AR-z spectrum, (Fig.(c) and (d)) it is difficult to obtain the correct peak, even with different Q-values, since many spurious peaks appear. When the random noise has changed, the AR-z spectrum is also quite different and the peaks are not same. With the stable version, I assume you will need to adapt weights computed as noise amplitude in different frequency bands, but here also you need a strong *a priori* since you aim at enhancing a peak around 6 yr.

(3) *Observing Fig. 4(c) in Rosat & Gillet (2023), an extremely weak 5.9-year signal with an amplitude of ~0.012 ms is present in the wavelet transform coefficient spectrum of the HAM data. Nevertheless, it can be observed that the amplitude is nearly congruent with the background noise level of ΔLOD. As depicted in Fig. 1, the background noise level of ΔLOD after removing the AAM and OAM effects is ~0.01 ms. It also can be seen in Fig. 1d of Ding (2019, EPSL) and Fig. 1b of Ding et al. (2021, JGR). That is to say, despite the presence of a ~5.9-year signal in the hydrology-excited LOD, the associated peak is likely to be buried in the background noise (the SNR is very low). Statistically, the signal is unreliable.*

I agree that the observed SYO in HAM in that paper is very small, but their statistical levels on Fig. 4(c) still detect it. Your synthetic test is something I did also, and when using a colored red noise, it is even worse. This noise effect could also be what you observe in SG data. Only statistical significance tests could help to decide whether you see noise or not, particularly in your case, when SG time series are not long enough. As for your FFT spectra on Fig. 2 in your reply 2, they are not to-scale, since they also should exhibit the fake peaks as seen in the CWT. A proper scaling would show it. That is why it is also important that you put statistical levels in your AR-z spectra and CWT. By the way, as concerning SG data, using a colored noise model would be more appropriate than a white noise at such long periods, since SG noise levels (represented by power spectral densities) increase with periods.

it may be more appropriate to rephrase the statement as follows: The HAM effect made very small contributions to the intradecadal period band in the ΔLOD.

I agree with that sentence but it is written in their paper:" HAM excitation function has a small but non-negligible contribution to the interannual LOD fluctuations."

(4) *It can be affirmed that the terrestrial water storage (TWS) variations can result in the C20 variations, which are directly reflected in ΔLOD. Besides, it is worth noting that Chao and Eanes (1995) demonstrated global gravitational changes due to atmospheric mass redistribution, which can also manifest in the C20 variations. Therefore, if an SYO in the TWS variations is observed, it must appear in ΔJ_2, and further will be reflected in ΔLOD. However, in our last reply, it has been determined that there is no observable SYO signal present within ΔJ_2. Given the stable SYO signal present in ΔLOD, the only explanation is that the SYO signal in ΔLOD does not have any correlation with the TWS variations.*

"The hydrological content at interannual time-scales is mostly related to degree-2 order-2 geographical pattern". Sorry, I should have been clearer in my sentence (use "most important" instead of "mostly"). Meyssignac et al. (2013) have shown indeed that variations in land water storage are more important on C20, but they also showed that there are large water mass redistributions over the oceans, particularly due to climatic events, in longitude (Y22 pattern), and it is not negligible on S22. So restricting hydrology signal to land water storage in a Y20 pattern results in an approximation that miss a part of water mass signal. Since SG gravity measurements are very sensitive to mass changes, they will record it too. Using J2, which is related to C20 and Y20 pattern, as an argument for the absence of the SYO in hydrological data is hence invalid.

Besides, you claimed that "Only a certain distribution of terrestrial water storage variations would result in ΔJ_2." It has no basis at all. According to Chao et al. (2020), "J_2 is the (normalized) zonal quadrupole of the Earth's density." "The zonal [degree-2, order-0] component of any mass redistribution will contribute to the time variation ΔJ_2.

This is exactly what I said! "*Only a certain distribution of terrestrial water storage variations would result in ΔJ_2*" that is the Y20 spherical harmonic pattern. The Y22 mass redistribution does not affect C20/J2, but hydrology has a non-negligible Y22 component (Meyssignac et al. 2013).

(5) *The revised manuscript will present the results of implementing AR-z into some better hydrological models like ERA5_land.*

Ok, thank you.

(6) *As demonstrated in our last reply, both current global hydrological models and global precipitation data do not indicate the presence of a stable and consecutive ~5.9-year oscillation*

Well, not with SG data either since the time windows used are too short with respect to the ones you used for hydrological models and precipitation data. And your synthetic tests with white noise in Fig. 2 of your reply 2 show it could be an artefact of noise too… Statistical confidence levels would be necessary.