Response to Referee #2

1 General comments

The paper raises the interesting question if a 5.9 year oscillation (SYO) observed in other geodetic time series, can also be detected in SG data. I think this is a relevant question and it might help to interpret the origin of such an oscillation.

The authors come to the conclusion, that they can detect SOY signals in the data and estimate their amplitudes and phases.

However, the preprocessing of the data, especially the correction of instrumental disturbances, is very critical, when analysing very long time series of SG data. Especially, for the step correction I have doubts if the methods used in this paper are completely valid. Some of the methods also need more explanation. Further, the accuracy of these corrections might be overestimated by the authors, which highly affects the detection and the interpretation of the 5.9 year oscillation with the small amplitude of 5 nm/s^2 to 9 nm/s^2.

Further, as highly discussed between the authors and Reviewer 1, the origin of such an oscillation might still be debatable. As I see my expertise rather in the processing of SG data, I don't want to add many comments to this discussion.

Response: We appreciate your valuable comments and suggestions that may lead to significant improvements of the manuscript. As previously discussed with Reviewer 1, in this study, there are two questions that really deserve in-depth investigation: (1) Dose the SYO originate from an internal or external source? (2) How to conduct the reasonable SG data pre-processing, especially for repairs of data steps and gaps, which will seriously affect the retrieval of annual-to-decadal fluctuations? Regarding Question (1), we had detailed discussions with Reviewer 1, and we were greatly inspired. Regarding Question (2), we had preliminary discussions with Reviewer 1, and we also emphasized that we had taken great care in the pre-processing work to ensure the correctness of the SYO signal extraction. Here, we will provide a detailed response to your comments related to Question (2).

2 Specific comments

2.1 Data source

I would like to comment on the confusion about the “h2” database of IGETS.

For most of the stations in the IGETS database three different kinds of Level 2 data exist. They are specified by the code in the ending of their filename: 22.gpp, 32.gpp and h2.gpp. The meaning of the code “22” and “h2” is documented by Voigt et al. (2016). The procedure to produce the “32”-data is explained by Boy et al. (2023), however the code “32” is only mentioned by Boy (2022).

The “22”-files should contain data with a sampling interval of 1 min, where gaps and disturbances are filled with synthetic data and offsets are adjusted, but it seems (see below) that this is at least not true for big gaps and big offsets. The “h2” files contain hourly sampled data, but Voigt et al. (2016) do not specify which kind of preprocessing was performed. The comparison below indicates that it is the same as for the “22” files.

The preprocessing for “22” and “h2” data is done at UFP (University of French Polynesia).

The “32” files are produced by EOST (Ecole et Observatoire des Sciences de la Terre). They contain data with a sampling interval of 1 min, where gaps are filled and offsets are adjusted.

To clarify, I provide a comparison of the calibrated Level 1 data and all kinds of Level 2 data for the year of 2006 and the gravimeter SG026 at Strasbourg. It is shown in Figure 1. It can be seen that the data spikes in November were removed in all versions of level 2 data. All gaps were filled and steps were adjusted in the “32”-data, while this is not the case for “22” and “h2” data. From visual inspection the “h2” seems to be a downsampled version of “22” data.

As you use your own method to adjust steps, I think it is reasonable that you use a data set, where steps were not adjusted before, which might be the case for “h2” data although it is stated differently by Voigt et al. (2016). In line 86 of your manuscript you mention that you also removed spikes. Does this mean that there were remaining spikes in the “h2” data that you removed on your own? Please clarify on this. If you anyway correct spikes and steps on your own, I think the safest option would be to use the level 1 data. Then you would know that no preprocessing was done and an uncertainty about the preprocessing done by IGETS cannot influence your results.

Please clarify in your manuscript which datasets from IGETS you use (level and code) and which preprocessing steps where done by IGETS.

Response: We fully endorse your description of the data set published by IGETS. Indeed, there are some differences between the actual published SG data and declaration by Voigt et al. (2016), especially for the Level 2 products. Voigt et al. (2016) and Boy et al. (2019) claimed that the Level 2 products are the data corrected for gaps, spikes, and steps. Actually, it is not true for big gaps and big offsets (you mentioned above too). That is where we get confused. We used the “h2” data, focusing on the corrections of big gaps and big steps, and with no correction of spikes. We are very sorry for our mistake, that caused your misunderstanding. The corrections of the spikes from the raw data to “h2” has been almost completely conducted.

Indeed, as you said, the most reasonable pre-processing way is to start with the “00” data, and the integrity of the data can be guaranteed. We initially did consider using the “00” data; however, the implementation proved to be exceedingly challenging. The minutely “00” data are riddled with numerous spikes, gaps, and steps, which are from the instrument problems and environmental effects. The method of IGETS to remove part of them by filling with synthetic data should be a relatively simple and reasonable way at present. Therefore, we ended up using the “h2” data, with the elimination of most of gaps, spikes, and steps in the IGETS processes with synthetic tides. However, some big steps were retained. That is what we focused on analyzing. We were very careful in repairing the steps, and we also did the error analysis to ensure the reliability.

In the revised manuscript, we will add more description related to the used SG dataset, including data information and preprocessing steps where done by IGETS, according to your suggestion.

2.2 Step correction

I have some questions and comments on your step detection and step size determination process.

First, I consider it a new and interesting idea to estimate the step sizes by fitting the data to the polar motion times series. I have not seen this approach before. However, I have some doubts if it is completely valid.

…

Therefore, I would propose to estimate the size of the steps, and all amplitudes and phases of the periodic signals at the same time. Otherwise you have to justify, why your method is still valid, or, if it is not completely valid, how big the errors are introduced by using it anyway.

Further, please also explain the method in more detail in your paper. As you write in the end of the paper, the best would be to also include absolute gravity measurements in the step size and drift estimation.

For the smaller steps you use a second method to find and remove them, which is explained in the supplementary material. From there it is very clear what you did to get to the green curve in Figure S2(c). However, please explain how you determine the step times and sizes from the green curve. This is not clear.

Especially, for the bigger steps in SG data a problem in estimating their size is that they often occur together with data gaps and that sometimes a running-in behaviour of the data occurs after the step. How is the accuracy of your step removing procedures influenced by this problem?

Beside the question if and how hydrology needs to be removed, the step correction is the biggest uncertainty in your data processing. Therefore, I appreciate that you analyse the influence of errors in the step corrections on your results. However, how do you come to the conclusion, that continuous downward steps cause the maximal amplitude deviation? I did a synthetic test similar to the one you present in the supplementary material, but for a mixture of upward and downward steps: I simulate a synthetic time series containing white noise with a standard deviation of 10 nm/s^2 (Figure 2(a)), six steps of 4 nm/s^2, which are upward and downward (Figure 2(b)) and a SOY signal with a period of 5.9 years and an amplitude of 6 nm/s^2 (Figure 2(c)). To simplify the synthetic test, I only estimated the amplitude of the SOY in the synthetic dataset with a linear least squares estimation and assumed the phase to be known. The simulated time series, the input SYO signal and the estimated SYO signal are shown in Figure 2(c). The estimated amplitude of SYO in the synthetic data is 8.1 nm/s^2, which means a deviation of more than 2 nm/s^2 from the input value. It is possible that the deviation is even bigger for other configurations. This test should just show that the deviation can be bigger than 1.8 nm/s^2. Furthermore, your synthetic test is only meaningful, if you can be sure that errors in your step determination and step correction process are not bigger than 4 nm/s^2. How did you come to this estimate?

In conclusion, I think the accuracy of the step correction might be overestimated. This leads to an underestimation of the errors of the SOY signal's amplitudes and phases, which finally affects the interpretation of the SOY signal.

Response: Many thanks for the detailed mathematical formula explanation. In accordance with the equation (1) you have presented, it is indeed the most rigorous approach to consider all potential steps throughout the entirety of the data analysis period. This entails estimating the sizes of the steps, as well as the amplitudes and phases of the periodic signals concurrently. However, the current operational approach is impractical, because of the existence of numerous gaps within the time series, thereby dividing the data series into distinct segments. Hence, we first identified the suitable segments based on the presence of gaps, and then piecewise fit the PM+ΔLOD series to the SG residual data (which has been subjected to filtering and smoothing) one by one. In this process we used an iterative search method. Given the relatively short time span of approximately 20 years, it is reasonable to approximate the long-term fluctuation as a linear trend and incorporate it into the fitting procedure. Naturally, this methodology falls short of attaining the level of rigor you have described, bur this approach just serves as a preliminary correction, primarily targeting significant gaps that are readily discernible to the unaided eye. Considering the need for small step correction in the next step, it is possible that the current correction, although imperfect, can be adjusted in subsequent correction. Besides, it is worth mentioning that we also employed the TSAnalyzer software developed by Wu et al. (2017), which is used for preprocessing the GPS data. This software is commonly utilized for correcting significant steps in the initial stages. However, the outcomes obtained from this approach were not as satisfactory as our current method. The small steps were identified according to the average fluctuations before and after a suspected step with respect to the overall fluctuation during the analysis segment. The process will be elucidated further in the subsequent revision.

For the synthetic test, we found the reason for the difference between our synthesis result and yours. It is mainly attributed to the different time points of the first step we two set. We chose the same step occurrence time as you, and got the same result as you. Hence, we redesign a random synthesis test, which makes the input white noise (with a noise level near or lower than those of the SG residuals) random, the initial input SYO phase random, the input step occurrence time random in every half SYO cycle, and the input step rise or fall random in every half SYO cycle. Under these conditions, we conduct 1000000 random trials for the simulated data within 18, 21 and 24 years (corresponding to 6, 7, and 8 steps of 4 nm/s^2), and calculate the deviation values of the amplitude and phase between the input and retrieved SYO (see an example in Figure R1). The statistical result shows that: 1) The percentages of the deviations less than 0.3 μGal are more than 99.5% for all three data lengths; 2) The percentages of the deviations less than 0.2 μGal are about 95%, 96%, and 97% respectively for all three data lengths; 3) The phase deviations are less than 0.15π. The results also shows that the longer the data length is, the less interference the steps have on the retrieval of the SYO. Therefore, from a statistical point of view, we believe the residual offsets may affect the observed ~5.9 years signal for the amplitude less than 0.2 μGal.

We sincerely appreciate you repeating the synthesis test and alerting us to the test's flaw, and we will modify the synthesis test and its relevant explanation in the revised supplementary material and manuscript. Some clarification you proposed will be also added in the revised manuscript.

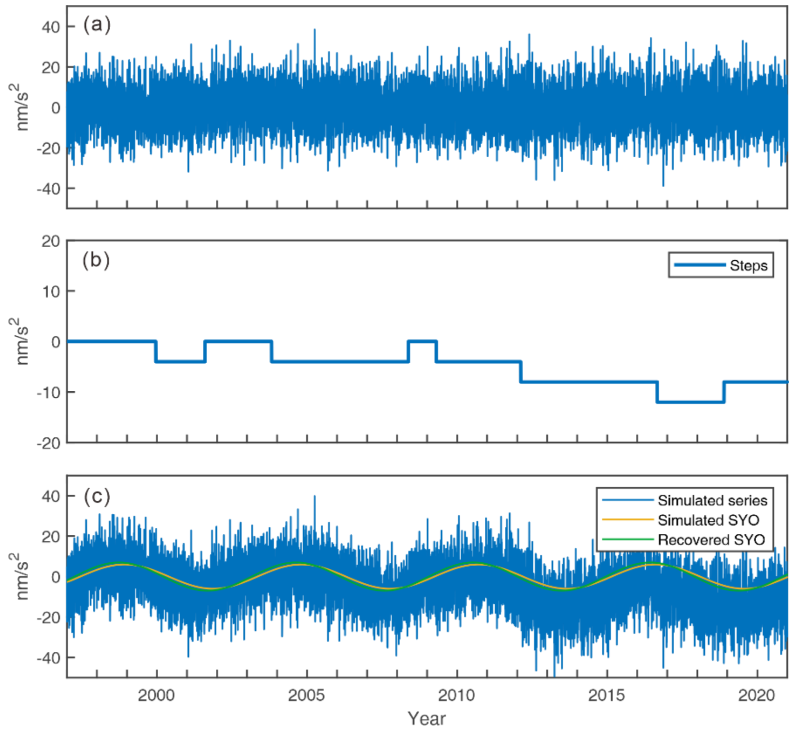

Figure R1. Synthetic test for the effect of small steps on the SYO signal retrieval. (a) shows the simulated noise series, (b) shows the input steps, and (c) shows the simulated SOY, the simulated time series and the recovered SOY.

2.3 Spectra

You emphasise the better frequency resolution of the AR-z spectra compared to Fourier spectra. I agree that the frequency resolution of the Fourier spectra of $1/T \approx 0{:}5$ cpy is not enough to resolve signals in the 3-5 year band, the 8.5-18.6 year band or the SYO. Is it possible to quantify the frequency resolution of the AR-z spectra. If yes, please do so. This would help to know the accuracy of your determined frequencies.

Did you use any taper to compute the Fourier spectra? If yes, please specify.

Response: When applying the AR-z spectra method, we typically densify our spectral spacing by a factor of 3 (or 5) over the Fourier elementary spacing. It requires the calculation of discrete Fourier transforms in the AR solution, which in turn can be efficiently done by FFT with zero-padding (see details in Ding & Chao, 2015, GJI). We used the Hanning taper to compute the Fourier spectrum. The relevant information will be added in the revised manuscript.

2.4 Retrieval of the SYO signal

How do you finally obtain the periods of the SOY ranging from 5.84 to 5.92 years? (Line 188-189). Do you read them from the Morlet wavelet spectra or from the AR-z spectra? In Figure 5 you give uncertainties for these periods. How did you obtain them? Please specify.

For the same reasons as disused in Section 2.2, I think the estimates for amplitudes and phases of the SOY signal would be more stable, if you estimate them together with the amplitudes and phases of all the other periodic signals.

Response: The SYO period value was obtained by the Lorentz fitting estimation of the Fourier spectrum peak of the SYO after removing other long-period signals by least-squares fitting. The uncertainty is the estimation error, which is related to the background noise level.

We agree with the method that you suggested, and we have even experimented with it. However, in order to determine the estimated uncertainty, we ultimately employed the approach of initial elimination followed by fitting. The validity of this approach was further validated through simulation experiments. Please see Materials and Methods 3 in our Supplement.

2.5 18.6 year period

In the conclusion you claim you have identified an 18.6 year period in the SG data. However, in the Section 'Spectral analysis' you state correctly that the data length is too short to identify this oscillation.

Response: We appreciate you pointing out our significant mistake. The accurate determination of the signals within the entire 8.5-18.6 band is indeed lacking. The statement made in this context is incorrect, and it will be modified in the revised manuscript.

2.6 Origin of the SYO

It is not clear to me, why you think the SYO should originate from the Earth core. Even if you can exclude external and loading sources, why do you think the SOY more likely originates from the core than for example from the mantle?

Response: We must first demonstrate that the SYO is a long-period fluctuation on a global scale. So far, the discussions about the origins of thus Earth's interannual-to-decadal fluctuations mainly focuses on the surface process or deep interior dynamics, and the evidence from the solid mantle is very little. A large number of surface load observation data can provide the interpretation of some interannual signals; Other long-period signals that cannot be explained by the surface loads (see Ding, 2019, EPSL, for examples) are mostly attributed to the dynamic processes of the Earth's core, which are often controversial because the structure of the Earth's core is not well understood.

In this study, our main objective was to provide evidence from surface gravity monitoring, which is different from previous evidence from satellite observations (e.g., SLR and GRACE), for the core origin interpretation of the SYO. Here we are trying to find a more reasonable interpretation for our SYO gravity observations. Our core idea is that the SYO signal in ΔLOD is driven by core processes. The 6-year related gravity changes, which may include core motions and some unknown 6-year changes due to strong coupling interactions between the mantle and core. Namely, the 6-year related surface gravity changes may be the result of a superposition of multiple internal motions in the Earth's coupling layers.

3 Technical corrections

Line 32-46: A non-expert to core dynamics would have big difficulties to understand the basic ideas of this paragraph. In my opinion this would be fine for a paragraph in the discussion part. For an introduction, however, I think the basic ideas should also be understood by non expert readers. So it would be helpful if you could rewrite this paragraph in a less technical way.

Response: Previous discussions about the origin of SYO have mainly focused on the dynamic processes of the Earth's core, which are currently controversial. In this paragraph, we mainly wanted to briefly introduce some of the current conjectures or explanations for the SYO excitation mechanism. In the process of revision, we will carefully revise this paragraph with reference to your suggestions for the convenience of readers.

Line 49: (PM, Ding et al. 2019, 2021; Chen et al. 2019)→ (PM) (Ding et al. 2019, 2021; Chen et al. 2019). The same applies to line 51.

Response: Thank you very much for the technical correction. We have modified as follows: "In recent years, the fluctuation characteristics and excitations of the SYO have also been investigated using some continuous and long-span geophysical/geodetic observations, including the polar motion (PM) (Ding et al., 2019, 2021; Chen et al., 2019), GPS (Global Positioning System) displacements (Ding and Chao, 2018a; Watkins et al., 2018; Rosat et al., 2021), geomagnetic fields (Ding and Chao, 2018a), and gravity-field satellite laser ranging (SLR) (Chao and Yu, 2020; Rosat et al., 2021)."

Line 181: You write that you are fitting 8 harmonics: ~18.6/13.5, ~8.5, ~4.2, ~3.65, ~3.2, ~2.6, ~2 and ~1 year. What does '~' mean in this context? As I understand, you estimate the amplitudes and the frequencies by the fitting procedure, while the frequencies are fixed? If this is the case, which are the exact frequencies you are using? What do you mean by 18.6/13.5? Do you use both frequencies? Is it one frequency for each station?

Response: Here '~' refers to the meaning of 'about'. No special meaning! All these frequencies are from the estimated values using the AR-z method in this study and previous empirical values (see our Section 3 in the manuscript). We were just keeping them one decimal place or two decimal places. This is true of all same symbols throughout the text. All these fixed frequencies associated with their cosine functions were used to fit each SG residual time series, to obtain their corresponding amplitudes and phases. Since the time is too short, we could not distinguish the peak with the period over 10 years, which may come from the overlap of adjacent spectral peaks. According previous empirical values, we chose two more reasonable values of 18.6 and 13.5 years to fit the peak regarded as a quasi-periodic oscillation. In practical fitting applications, we will prefer the value of 18.6 or 13.5 years or both, to achieve the best fitting of the SG time series from the perspective of the Fourier spectrum.

Line 259: When talking about pressure changes, are these air pressure changes or pressure changes inside the Earth? Please clarify.

Response: The pressure here refers to the non-hydrostatic pressure on the core-mantle boundary from the liquid outer core (Please see Dumberry, 2010, GJI). We will clarify it in the revised manuscript.

References

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