Review to the Article ’The possible 5.9 years oscillation identification from superconducting gravimeter observations’ by Wei Luan and Hao Ding

June 19, 2023

1 General comments

The paper raises the interesting question if a 5.9 year oscillation (SOY) 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 $5\,\mathrm{nm/s^2}$ to $9\,\mathrm{nm/s^2}$.

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.

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.

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

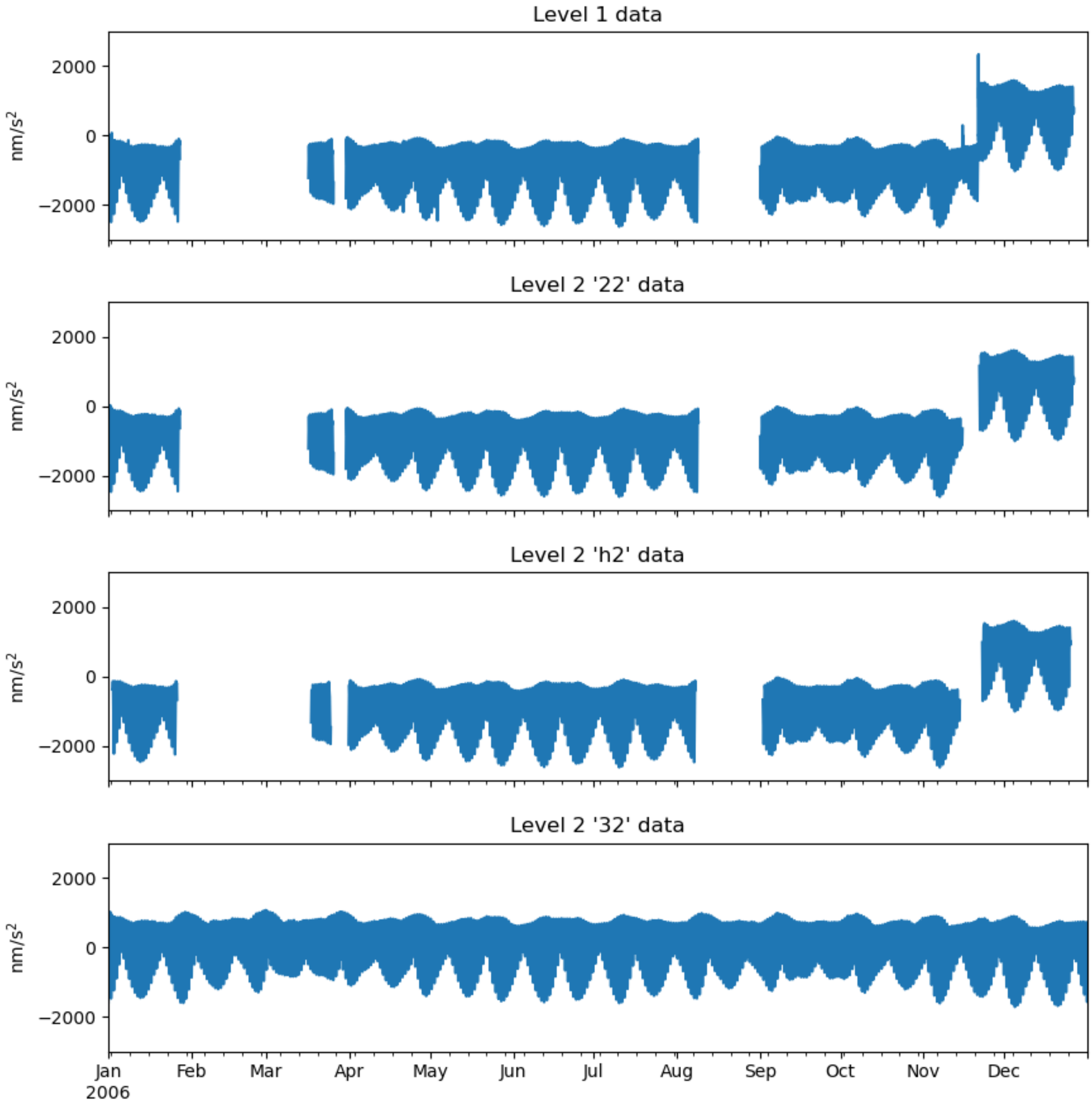

Figure 1: **Comparison of Level 1 and Level 2 IGETS data.** The figure shows data of the SG026 at Strasbourg for the year 2006. In the top panel calibrated level 1 data can be found. The other panels show the three different kinds of level 2 data.

if it is completely valid. If I understand it correctly, your fitting process can be described as

$$g(t) - PM(t) - LOD(t) = A \cdot \begin{pmatrix} s_1 \\ s_2 \\ . \\ . \\ . \\ s_N \\ a \end{pmatrix} + \epsilon(t)\,, \tag{1}$$

where $g(t)$ contains your preprocessed data (after removing spikes, synthetic tides, atmospheric and non-tidal ocean loading) and $PM(t)$ and $LOD(t)$ are the models for the polar motion signal and the length of day signal. $s_1 ... s_N$ are the steps size you estimate, a is a possible linear trend in the data and ϵ contains all kinds of uncertainties and remaining signals in the data. The design matrix A is

$$A = \begin{pmatrix} H(t-t_1) \\ H(t-t_2) \\ . \\ . \\ . \\ H(t-t_N) \\ t \end{pmatrix}^T \qquad \text{with} \qquad H(t) = \begin{cases} 0 & t < 0 \\ 1 & t \geq 0 \end{cases}$$

where $t_1 ... t_N$ are the times of the steps.

In this approach ϵ contains all the periodic gravity signals $g_p(t)$ that you analyse afterwards, the stochastic part of the gravity signal $g_s(t)$, the remaining small steps, $s(t)$ the uncertainty of the polar motion model ϵ_{model}, and the noise of the SG ϵ_{SG}:

$$\epsilon(t) = g_p(t) + g_s(t) + s(t) + \epsilon_{\text{model}} + \epsilon_{\text{SG}}\,.$$

In adjustment theory, minimising the square of ϵ in Equation 1, is based on the assumption that $\epsilon(t)$ is stochastic with zero mean (Teunissen 2003). This might be true for $g_s(t)$, ϵ_{model}, and ϵ_{SG}, but the remaining small steps do probably not have zero mean and the periodic signals in the gravity residuals are not stochastic.

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 $10\,\mathrm{nm/s^2}$ (Figure 2(a)), six steps of $4\,\mathrm{nm/s^2}$, which are upward and downward (Figure 2(b)) and a SOY signal with a period of 5.9 years and an amplitude of $6\,\mathrm{nm/s^2}$ (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 $8.1\,\mathrm{nm/s^2}$, which means a deviation of more than $2\,\mathrm{nm/s^2}$ 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 $1.8\,\mathrm{nm/s^2}$. 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 $4\,\mathrm{nm/s^2}$. 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.

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 $\frac{1}{T} \approx 0.5\,\mathrm{cpy}$ 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.

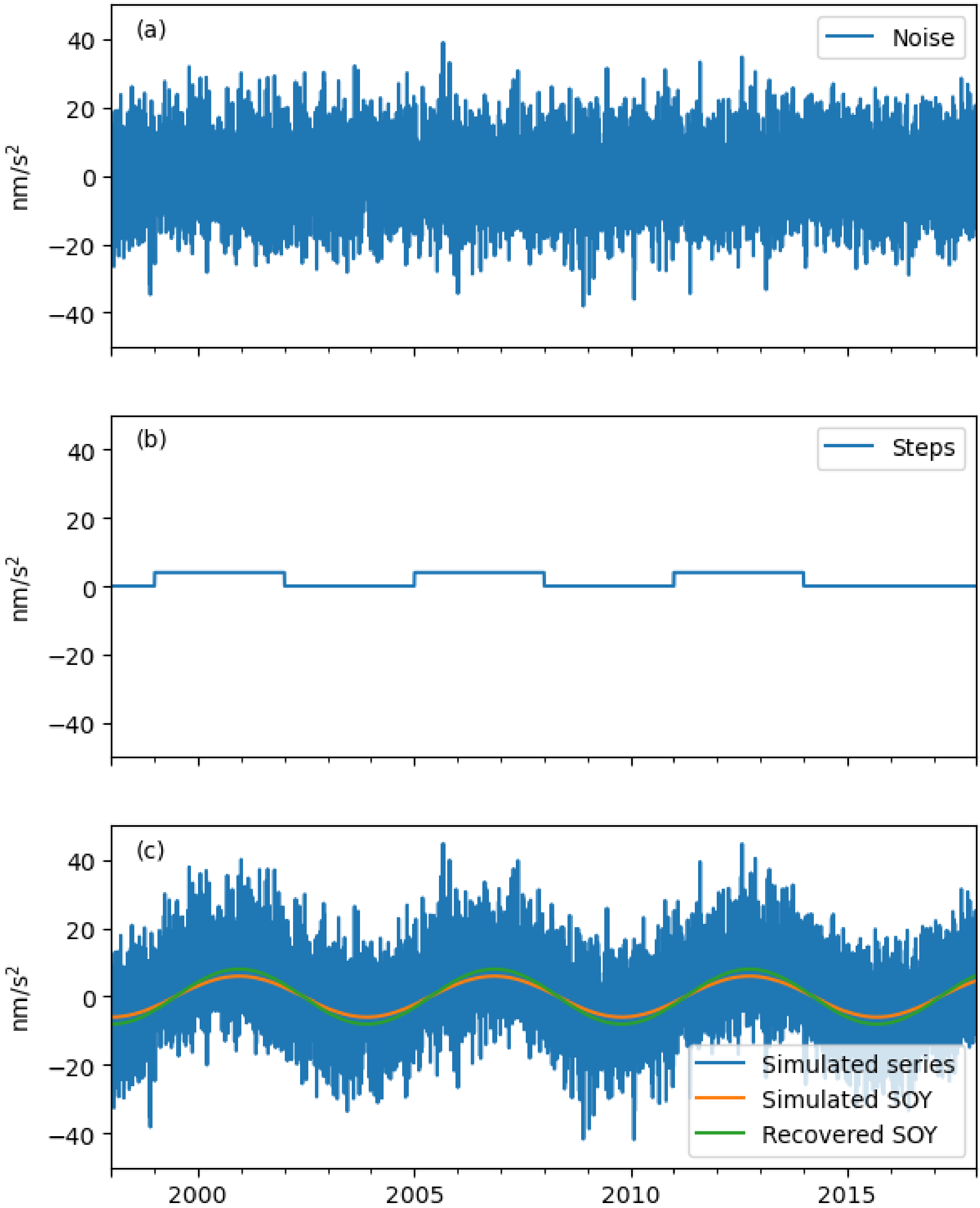

Figure 2: **Synthetic test for the effect of small steps on the SYO signal retrieval.** (a) shows the simulated noise series, (b) shows the input steps and (c) shows the simulated SOY, the simulated time series and the recovered SOY.

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

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.

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.

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?

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.

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

Line 181: You write that you are fitting 8 harmonics: $\sim$ 18.6/13.5, $\sim$ 8.5, $\sim$ 4.2, $\sim$ 3.65, $\sim$ 3.2, $\sim$ 2.6, $\sim$ 2 and $\sim$ 1 year. What does '$\sim$' 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?

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

References

Boy, J.-P. 2022. *Description of the Level 2 and Level 3 IGETS data produced by EOST (version 2).* https://isdc.gfz-potsdam.de/fileadmin/isdc/docs/EOSTproducts_v2.pdf.

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