General comments

This paper shows that a 6-year oscillation is visible in the time-varying gravity recorded by 6 Superconducting Gravimeters (SGs). The origin of this 6-year oscillation also seen in other geophysical and geodetic time-series is still debated today. A core origin has been suggested but surficial climatic events could also be responsible for that periodic oscillation. This paper completes the catalog of observables containing a 6-year oscillation. It confirms that it is a global effect. They then try to prove that it is of internal origin, but as discussed here after, it is not so clear, and hydrology still prevails. The methods they employed are not new either, and one of their method (AR-z spectrum) could even raise some criticism. There are also a few scientific flaws and worries that need to be considered and corrected, in particular with respect to the published literature.

Specific comments (individual scientific questions/issues)

- This paper is a follower of a series of studies by Pr. Hao Ding and co-workers who support a core origin for the 6-year oscillation observed in geodetic and gravimetric data, despite some evidence for a more probable surficial origin by Rosat et al. (2021). The criticism can be reproduced here, since hydrological loading does also contain a 6-yr oscillation contrary to what the authors claim (Fig. S5 is not at the same scale as Fig. 3 so it is misleading). If you do plot the FFT spectra of ERAin or ERA5 (or ERA5_land) hydrological loading products, as I did, you will see a non-negligible contribution around 6-year. You can also plot the time-series of SG gravity residuals with respect to hydrological loading, band-pass filtered them around 6-yr, and you will see a good correlation between both time-series, for most worldwide SG stations of sufficient data length, but with a time-shift for some stations. You have to consider cautiously the sign of the local contribution of hydrological loading for underground stations like Moxa, Membach and Strasbourg. Indeed, another group of researchers has shown that the continental hydrology as well as other climatic time-series exhibit a 6-year oscillation e.g. Pfeffer et al. (2022, https://doi.org/10.5194/egusphere-2022-1032), Cazenave et al. (2023, https://doi.org/10.5194/egusphere-2023-312) and Pfeffer et al. (2023, http://ssrn.com/abstract=4388237). This hydrological signal contributes to the observed 6-yr gravity change and would mask any potential signal originating from the core. Consequently, as long as you do not correctly deconvolve gravity data from this hydrological signal, you cannot interpret the 6-yr oscillation as a signal of core-origin.
- Besides, the authors still employ the AR-z spectrum. This method has been used in many papers now, but the code is still not made publicly available. If it is so much better than the FFT, why you do not share it? This AR-z spectrum always displays additional peaks with respect to the FFT. How do you explain the additional peaks that are visible on Fig. 3 but not visible in FFT?
- P. 11, section 4.2: the argument of the ratio δ/h is not sufficient to propose an internal origin for the 6-yr oscillation. This ratio for surface loading is also very different from the tidal one (see for instance de Linage et al. 2007, doi: 10.1111/j.1365-246X.2007.03613.x and de Linage et al. 2009, doi: 10.1111/j.1365-246X.2007.03613.x, who have estimated this ratio for various loading and have shown some variability).

Local hydrology would also affect this ratio, particularly at underground stations like Moxa, Membach, Strasbourg (e.g. Rosat et al. 2020, https://doi.org/10.1007/1345_2020_117). This argument is hence not sufficient to justify your interpretation of the 6-yr oscillation as the signature of an internal process.

- Lines 270-272: the statement here is wrong. In Gillet et al. (2020) they used pressure Love numbers exactly as in Greff-Lefftz et al. (2004). You can check the values for the Love numbers h in their respective Table 1 and see that they are the same. The mistakes are in Fang et al. (1996) who have considered the pressure flow as a surface load but they have ignored the deformation of the equipotential surfaces in the core. They only considered the deformation of the mantle, while in Greff-Lefftz et al. (2004) and in Gillet et al. (2020), they both considered the deformation of the mantle and of the equipotential surfaces in the core. The Love numbers and surface deformation estimates by Gillet et al. (2020) are hence correct.

<u>Technical corrections</u>

- Lines 51, 59, 64 etc… satellite laser ranging should be abbreviated as SLR not SRL
- Line 73: the GGP project does not exist anymore, it has been replaced by the IGETS.
- Lines 86-87: you say that you used level-2 products that mean that major disturbances have already been corrected from the data. Else, please precise what you are referring to as “h2” corrections since official IGETS products are called Level 1, Level 2 and Level 3 products.
- Line 154: in the processing of data, a tidal analysis was performed with ETERNA software to remove tides. So why is there still the 18.6-yr tide? You did not include it in the groups of waves to be analyzed? Why?
- Line 160: some spurious or unexplained peaks are visible in the AR-z spectrum (between annual and QBO, and at 2.6-yr). Why you do not discuss them? Are they artefacts of the AR-z spectrum? How confident are you on the AR-z spectral peaks? You should provide some confidence levels with this method, since many spurious peaks seem to appear…