**Comments by Referee #1:**
(Responses by the authors are highlighted in blue)

I read with interest the manuscript "Chaotic oceanic excitation of low-frequency polar motion variability" submitted by Börger et al. for possible publication in Earth System Dynamics. The paper utilizes the OCCIPUT large ensemble with 50 realizations of time-variable eddy-permitting ocean mass and flow fields to calculate effective ocean angular momentum functions characterizing the excitation of changes in the solid Earth's orientation with respect to inertial space. The paper is very well written und certainly fits into the scope of the journal. I recommend this work for publication as soon as a number of comments have been reasonably well addressed.

(1) The analysis presented in this paper is based on the SPACE2018 series of Earth Orientation Parameters as processed at the JPL. Authors should explain in more detail why SPACE2018 is used here instead of the associated COMB2018 series, or a more recent reprocessing of the same data (i.e., COMB2019). Authors should also consider to use the newly published EOP series from the ITRF2020 computation that are operationally updated as EOP 20 C04 by scientists from the Paris Observatory. In any case, it needs to be discussed in the article how a particular choice of the EOP series might affect the interpretations of the results presented here.
Use of SPACE2018 on our side has carried through from previous studies, but we will switch to newer solutions in future work. Repeats of our excitation budget analysis with the mentioned series (COMB2019/2022, EOP 20 C04) showed essentially no sensitivity to the choice of the rotation data: PVE values in Table 2 only changed by 0.2% at most. We will add this result as a footnote to Table 2 in the revised manuscript.

(2) Time-variable gravity field representations from GRACE that are additionally augmented by SLR and DORIS observations to extend the time-series have been used only rarely in Earth Orientation Parameter research. In view of the cautious comments provided by the authors in line 240, I propose to explicitly show the hydrological angular momentum functions derived from GRACE+SLR+DORIS for the whole time-period 1995-2015, and compare it with GRACE-based excitation functions -- ideally derived from publicly available Level-3 products, like the Cost-G combination solution available via gravis.gfz-potsdam.de to make results traceable -- and an independent model-based hydrologic excitation function published elsewhere. Please note that a detailed discussion of the contributions from Greenland and Antarctica is not necessary at this point.
Good idea, and we have in fact done such comparisons during our study. We would like to show **Figure S1** in a newly created supplementary, where we compare our low-pass filtered hydrological $\chi_{1,2}$ estimates with time series from the COST-G Level-3 product and the Global Land Data Assimilation System (GLDAS). Given that excitations from hydrological models are generally deemed uncertain (see the literature on the subject), we will refrain from mentioning the GLDAS results in the main text. However, we will point to some of the differences between the GRACE+SLR+DORIS solution and the COST-G time series, as these differences fit into the discussion near line 240 about possible remaining limitations in the utilized hydrological angular momentum estimates.

Note that in addition to Figure S1, our results have already been made traceable by providing all our (unfiltered) angular momentum time series on zenodo; see the “Data availability” statement.

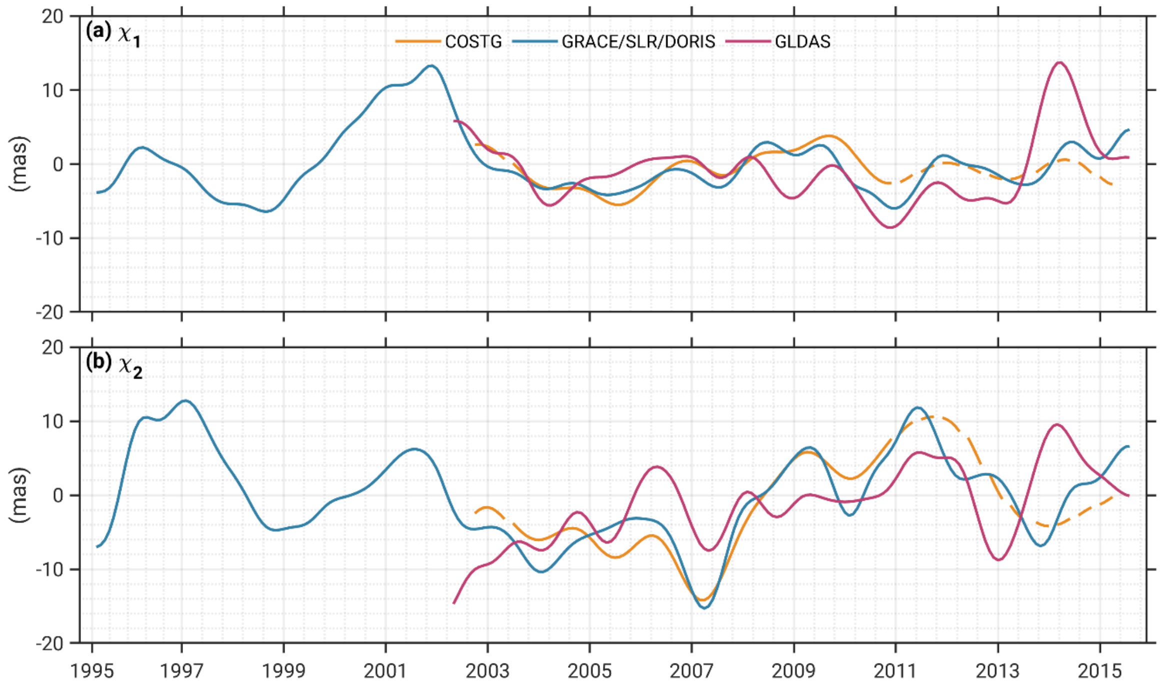


**Figure S1**: Hydrological contribution to interannual polar motion excitation (mas) deduced from the GRACE/SLR/DORIS gravity field solution described in the main text (1995/01–2015/12, blue lines), the COST-G GravIS RL01 continental water storage anomalies (2002/04–2015/08 with gaps, yellow), and the GLDAS model (2002/01–2015/12, red). Each time series has been filtered to periods longer than 14 months, cut back by 4 months at the respective end points. Trends and mean of the COST-G and GLDAS time series have been adjusted such that they agree with the GRACE/SLR/DORIS trend and mean over a common time period starting early 2002. Note that GRACE/SLR/DORIS was detrended over 1995–2015, as in the main text.

(3) It is quite surprising to see that the largest interannual surface mass variations outside Greenland and Antarctica are found on the Malakka peninsula in South-East Asia. This is not really intuitive from a hydrometeorological perspective and calls for further investigations. In particular, it should be thoroughly checked if poorly treated tectonic signals associated with the 2004 Sumatra-Andaman earthquake (and later events in neighboring areas) are responsible for this feature. Please report in detail about any modifications made to the GRACE+SLR+DORIS processing, which is not yet really well covered in the scientific literature.

Thank you for this keen observation. Additional checks by us have shown that the large-magnitude surface mass variation near Phuket is indeed the manifestation of an uncorrected post-seismic signal. However, the anomaly is of very limited spatial extent and sits in low latitudes, such that the hydrological polar motion excitation remains virtually unaffected: When setting the terrestrial water storage fields over the area in question to 0, the PVE values in Table 2 changed by 0.2% in $\chi_1$ and by 0.4% in $\chi_2$ (brute force sensitivity test). For the revisions, we would like to point out the Phuket anomaly in the caption of Fig. B1 and add one sentence to Appendix B, indicating that co- and post-seismic deformation signals have not been removed from the GRACE+SLR+DORIS solution.

(4) Authors speculate in both abstract and conclusions about possible implications of this work for EOP prediction, but fail to elaborate it further in the article. I suggest to remove this comment from the abstract in order to avoid raising unrealistic expectations with the reader. In any case, rigorously assessing the potential consequences for EOP predictions should be left for future study.

Thanks for the suggestion, but we disagree. It is quite natural and common in papers to highlight implications, even if they are not treated at length but nevertheless clear from the context – as is the case here. One could go even further and argue that such cross-links are part of the meat of interdisciplinary journals and papers. In any case, given the way how the EOP prediction aspect is mentioned in the last sentence of the abstract, there should be no ambiguity that it is meant as a future line of research.