Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2019-40-RC2, 2019

Earth System Dynamics Discussions

Interactive comment on "A global empirical GIA model based on GRACE data" *by* Yu Sun and Riccardo E. M. Riva

Anonymous Referee #2

Received and published: 8 October 2019

This submitted research report builds upon work published by Yu Sun et al (2016 a, b; 2019) which developed a fingerprint- empirical orthogonal functional analysis of GRACE level 2 products and demonstrated their utility in dealing with terrestrial water storage and cryospheric change over the time scale of the GRACE mission. These papers were both valuable and intellectually stimulating, and also treated glacial isostatic adjustment (GIA), though without certain nuances that we see in the current submission.

The current submission attempts to isolate a new empirical GIA model by developing an ensemble based upon forward modeling, finding some statistics of those models and then by addition/subtraction from the GRACE-OBP methodology of Sun et al. (2019, GRL 46 https://doi.org/10.1029/2018GL080607) deliver an empirical model

based upon GRACE RL06 alone. This is a worthy goal, but I am unconvinced that the results are valuable enough for a publication at this time.

One of the greatest difficulties that the authors are faced with is that of properly dealing with, and quantifying, the error propagation, and they do not appear to have dealt with this in any way. Furthermore, the ensemble forward model set has little to offer that convinces me that it is a statistical sampling. More damning is the fact that the authors don't seem particularly convinced themselves: A careful reading of the Conclusions (section 6) is in order. The first paragraph states: "In addition, the estimated ocean mass change and the contributions of its individual sources are in line with most of the recent literature". And in the second paragraph they provide equally unenthusiastic statements about GIA: " The uncertainties obtained for the individual contributors . . . provide a realistic quantification of the global role of GIA in GRACE-based estimates of present-day mass redistribution." This statement is tantamount to an admission of failure. My rationale is as follows. The authors set out to find an empirical GIA model. But the model they find has uncertainties as large as the uncertainties in determining GRACE-based mass changes. When I first did a skim reading of this submission, my impression was that this is a small epsilon forward with GRACE-GIA modeling. Upon digesting the paper for review my view changed to thinking that the sign on the epsilon is, in fact, negative.

In light of the fact that the paper has now gotten to the Discussion phase, let me below try to be helpful (yet, none-the-less, quite critical) in some more detailed comments/observations. Perhaps the submission can be resuscitated after a major rewriting and resubmitting with a less 'assertive' title.

Details.

1st sentence Abstract. This statement is generally false. At best, GIA models preform updates between viscosity models and ice sheet history models. It is very rare that they are solved for simultaneously, and especially if the ice flow is computed dynamically.

In the abstract, it would be nice to know what the 1 or 2 sigma uncertainty is in the J2 dot solution using the GRACE-OBP method and that which is GIA-empirically determined.

Line 30, Section 1. The referred papers for earth rotational GIA are more that 30 years old. The modern literature is actually quite rich, and I suggest referencing a paper like those published by Nakada or Mitrovica during the last 5 years.

Line 46, Section 1. The approach is claimed to be like one developed originally by Rietbroeck. But the latter used ocean altimetry, and that is not being used here. The authors need to clarify. Would the current study have been more successful if also employing ocean altimetry, or less so?

Line 72, Section 2.1. The use of the phrasing "allows to" should be changed as it is grammatically incorrect. Use something like, "provides sufficient information for partitioning".

Lines 72-73, Section 2.1 The use of "somehow" is quite odd in this context. (This occurs later). This is a red flag for a reviewer. If it is a 'somehow' then there is something that the reader must take as either suspicious or done with uncertainty, at best. It therefore needs much clarification.

Lines 80 – 85 Section 2.2. As described, this is not an ensemble. No real statistics can be derived from these 4 forward models. One could say: The spread is . . . and we assert that it is representative of the one sigma about some average. But there is utterly no statistical significance to these. And, by the way, each of the models have their own mountains of data that both support and bias them, and that have data gaps and under/oversampling, none of which is being treated in your empirical procedures.

Line 89, Section 2.2. "think" -> "thick"

Lines 104-108, Section 2.3. This entire explanation of the procedure to deal with rotationally related surface mass change and GIA has to be redone. I suggest writing out

the terms in equation form: What exactly in the end is solved for, and what is the uncertainty? I am completely lost, and if I read the logic – verbatim – I would be led to the conclusion that the paper is simply wrong. I have faith that the author's understanding of all this is more-or-less sound, so I think it is just a series of poor explanation(s) that is involved. It's really a weak part of the paper, however.

Lines 109-111, Section 2.2. This statement seems contradictory. If not included, why discuss it here and later in the paper? I am quite confused.

Lines 125-129, Section 3. Again, we return to this confusion about pole-tide and its appearance in the fingerprint maps. What does this mean, or does it even have a meaning? Is it perhaps an artifact? I am not criticizing, I am just confused.

Lines 137-139, Section 3. The sentence starting "The uncertainty overall . . . " is disturbing, for this is exactly where an empirical model, if at all valuable, might help advancing science. I return to this criticism below, with respect to the 'coupling' to hydrological signal.

Lines 149-153. Section 3. The proposed explanation for the round region in south Atlantic seems very speculative: a trend in TWS not removed in the pole-tide correction. (?) There should be more explanation to justify this. Why not cumulative errors propagated from GRACE RL06, including the de-aliasing models? Since no error propagation analysis was conducted here, then how do we assess this assertion?

Lines 155-156. Section 3. "At the same time, the uncertainties provide . . .". I suggest the same comment that was mentioned concerning the vagaries of the conclusions. Now those apply here as well. I elaborate on the criticism. I refer to a recent paper by Jensen, L., Eicker, A., Dobslaw, H., Stacke, T., & Humphrey, V. (2019). Long-term wetting and drying trends in land water storage derived from GRACE and CMIP5 models. J.Geophys. Res.: Atmospheres, 124. https://doi.org/10.1029/2018JD029989. In Figure 1 of this paper we see a map of the TWS trends from ITSG-Grace2018s. In that map, note the region of negative ($\sim$-10 to -20 mm/yr equivalent water) to the west and south of Hudson Bay, and that also rings across northern Canada in red, and the outer blue ring at amplitude 5 – 15 mm/yr outside of these. Are those really TWS, or are they mismodeled GIA? We can deduce that such questions should apply just by looking at the Jensen 2019 map. So, what, if anything, has the current paper submitted to ESD contributed to this? Has it (the present paper) just affirmed what we surmise from Figure 1 of Jensen?

Lines 158-160, Section 3. A 300 km buffer is employed. But no Gauss filter is mentioned, so I assume it is not used, and all the signal generated by fingerprint EOFs. What does the buffering do? Some of that buffering will get rid of real signal, not just gravitational artifact, as was the minor point being made by Sterenborg.

Lines 165-167, Section 3. The estimate of GIA signal is compared to that of Tamisiea. But this is difficult to make much of, as the statement is too equivocal: It is better to state it quantitatively: "We estimate a GIA [thing] of x.x $\pm$ y.y, whereas Tamisiea estimated b.b $\pm$ c.c, for that same GIA [thing]."

Lines 175-178, Section 3. A lot is made here of determining a partition of the J2 signal, and convincing (and seemingly rigorous) work was established in Sun et al (2019). But this disambiguation, as reported here, seems notably unconvincing without evaluating error propagation, or at minimum, estimation.

Lines 190-192, Section 4. The statement: "... by using only one dataset we get ... on the final solution", is a good one, and maybe in a Brevia paper to this journal that explicit point can be made, even convincing some that it is important! But it is quite challenging to recast this work into something that would convince us that science is being advanced, even by a small epsilon.

Lines 201-207, Section 5 and final remarks. Again, the big deficit to this paper is that lack of any attention to error propagation, as I suspect that if that were done a similar, but quite useful quantitative conclusion might be discovered. Such quantification could become a valuable thing, especially with respect to planning the next generation of

space gravimetry missions.

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