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

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"A global empirical GIA model based on GRACE data" by Yu Sun and Riccardo E. M. Riva presents a joint least-square inversion on a global scale of GIA and contemporary surface water mass redistribution solely constrained by GRACE data. In addition to providing a spatial description of both components, the authors also provide estimations of their respective contribution to global mean sea level change, changes to the Earth oblateness coefficient and geocenter motion. The approach is built on solid theory and combines all of the important sources of signal to analyze the signal. In addition to enriching the spectrum of GIA models available to correct GRACE data, this work has the potential to bring new and valuable information for the GIA modeling community by highlighting the ability of GRACE to constrain GIA signal, and to point out

how the resulting solutions would place themselves among or differ from the existing GIA models. Unfortunately, the present version of the manuscript shows very limited comparisons with other GIA models and the uncertainty quantification scheme is not sufficiently explained for the reader to assess (one way or the other) the robustness of the inversion and the degree of independence between all of the fingerprints used. I think the impact of the paper would be much greater if these points were elaborated on and the statistical analysis was shown more explicitly to be both rigorous and supporting the conclusions robustly. I believe the authors should already have all of the right statistical tools at their disposal to answer these questions. Provided the authors address these points in a subsequent revision I would certainly support the publication of this manuscript.

Major points: -l39: I know that other authors have used these terms in previous papers in a somewhat interchangeable manner, but I think it is important to distinguish data-driven from empirical. GIA models derived from partial differential equations (e.g. using love numbers) are not empirical (they are based on a physical theory) but they can be data-driven if their parameters are inverted from a dataset. Among such models are for example Peltier et al. (2015), Lambeck et al. (2014) or Caron et al (2018). Because the authors use such theory to generate their fingerprints, I would argue that their approach is not empirical (and I believe the title should reflect that), and in fact amounts to rescaling the loading history via the least square coefficients as was done in the aforementioned papers, and others before them. In my opinion, that is something the authors could put forward as an advantage of their approach, as it means it is consistent with how we otherwise model and understand the physics behind surface loading and deformation of the Earth interior.

In particular, it means we could compare the GIA scaling coefficients (here the inverted coefficients of the fingerprints) with the values found in the literature and that are based on inverting RSL, and other datasets. That exercise cannot easily be done with true empirical models as they are not built on comparable basis functions. An important question this paper could (begin to) illuminate by showing these coefficients is therefore: are GIA models preferred by GRACE statistically different from the ones constrained with traditional datasets?

-l81: What is the impact of the number of evaluated cases (here 4) on this statistical analysis? Would the authors expect a lot of differences from a more comprehensive exploration of the parameter space (particularly the viscosity profile)? How much does this limit the applicability of these results to correct GRACE?

-l88: Ice histories such as that of ANU and ICE-6G_C have been crafted such that when combining all of their regional components, they are able to explain paleo RSL data (especially through the eustatic sea level curve). By recombining regional components of different models, is the solution still consistent with what we know about past RSL - and therefore part of what validates these ice histories in the first place?

-l113: The authors unfortunately do not really elaborate on their uncertainty quantification approach, and only state that they combine all 4 solutions into an average. How did the author calculate their standard deviation map? Did they: a) take the least-square optimized signal of each of the 4 cases, and then calculated the standard deviation between them (which the first sentence at l119 seems to point to), b) calculated the variance/covariance matrix of the coefficients for each case from the least-square system, which using the notations of Yun et al. (2019) should be a term with a form along the lines of (F'T'PTF)ˆ-1, and then averaged that covariance matrix between the 4 cases, c) a method similar to b), with a weight associated with each of the 4 cases in the averaging process to take into account that some of them allow smaller residuals than others, d) use yet another method?

Out of these possibilities, a) is not an appropriate estimator, it would underestimate the uncertainty as it neglects the level of constraint of each least-square inversion. One could imagine a situation where all 4 best fit produce a similar signal for a given grid point or Stokes coefficient, but with a high variance/low confidence for that value. b)

assumes that all 4 cases should have the same weight, which would be acceptable if they yield a similar sum of the residuals, c) being be more indicated otherwise. As this explanation is missing, it is difficult for me to understand and critically examine the results section of the manuscript, and going back to Yun et al. (2019) which details the method, I could not find the information related to uncertainty quantification either. I would add that if the authors mean to provide their model to the GRACE community for correcting GIA, it is very important that the treatment of uncertainty quantification be transparent. -l179-182: An additional benefit of showing the covariance matrix of the least-square coefficients is that one can verify the degree of independence (or some measure of it, at least) between the different fingerprints by transforming it into a correlation matrix. This way sufficient orthogonality does not have to remain an assumption.

Minor points: -l8: if the authors are referring to RSL indicators, they only point to a local level, not global

-l35: This reference should be Caron et al. 2018, not 2017

-l78: Why do the authors assume the Earth to be compressible for the fingerprints of the previous section but incompressible for GIA deformation? Is this not inconsistent?

-l81: It was not clear for me at first read whether the authors were combining ICE-6G in one region with another model in the other region, despite the previous sentence. I suggest rewording along the lines of: "we use either GLAC1D (Tarasov et al. 2012) and ANU (Lambeck et al. 2010) in North America and Northern Europe, respectively, or ICE-6G_C (Peltier et al. 2015) in both regions."

-l81: “of” should read “or”

-l88: The authors reference Ivins & James (2005) for the IJ05 model, which had an updated version (IJ05_R2) released in 2013 (Ivins, E. R., T. S. James, J. Wahr, O. Schrama, J. Ernst, F. W. Landerer, and K. M. Simon (2013), Antarctic contribution to sea level rise observed by GRACE with improved GIA correction, Journal of Geophysical Research: Solid Earth, 118(6), 3126–3141). If the authors used the updated version, this is simply a matter of updating the reference, but if not I would be curious to know why they chose the old version and if they expect a significant change from this choice. The volume of the Antarctic ice sheet at the LGM is different by about a factor 2 for example.

-l116: “rounder”: do you mean smoother or with a more circular shape?

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