Nonlinear Processes in Geophysics Discussions

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Comment on npg-2021-22

Anonymous Referee #1

Referee comment on "An approach for constraining mantle viscosities through assimilation of palaeo sea level data into a glacial isostatic adjustment model" by Reyko Schachtschneider et al., Nonlin. Processes Geophys. Discuss., https://doi.org/10.5194/npg-2021-22-RC1, 2021

"An approach for constraining mantle viscosities through assimilation of paleo sea level data into a glacial isostatic adjustment model" by Schachtschneider et al., present a data assimilation approach to recover mantle viscosity parameters for a 3-layer glacial isostatic adjustment (GIA) model using synthetic relative sea-level change data. Based on a range of experiments, they conclude their particle filter based method was able to find the ground truth mantle viscosity parameters given different initial ensemble probabilities, observational uncertainties and spatial-temporal data coverage. This is a novel application of the data assimilation approach for GIA modelling studies, which has a good potential to help the community to better constrain the mantle viscosity parameters of a commonly-used 3-layer GIA model. This is a well-written manuscript which explains a complex subject in a very clear way. I have two major points and other minor comments. Provided the authors address these points in a subsequent revision I would certainly support the publication of this manuscript.

Major points

Firstly, given this is mainly a methodological paper for finding the mantle viscosity parameters, it is unclear what the major advantages of this data assimilation approach are compared to other commonly-used statistical approaches for finding the best-fit GIA model parameters. For example, a common approach is to use chi-square misfits to find the confidence interval of mantle viscosity (see Lambeck et al., 2014), which allows the best-fit GIA parameters to locate anywhere within the initial distribution. The authors have mentioned that their approach can converge to viscosity values that are not covered by the initial ensemble, which can also be approximately achieved given a denser sampling of possible viscosity parameter values within a forward modelling scenario. In this case, the computation time for this new approach is a very important information to report. The authors should at least demonstrate one advantage of this new approach either regarding the inversion power or the computational efficiency.

Secondly, the experiment set-up is over-idealized. In order to apply this method for real GIA problems in the future, I suggest the authors consider relaxing several assumptions, or at least discuss how relaxing these assumptions will impact the final results:

1. The authors assume a temporally-uniform relative sea level (RSL) data uncertainty of 0.5/0.25/0.1 m throughout the Last Deglaciation, which is however very difficult to achieve in reality. The RSL data uncertainty tends to be larger for earlier time intervals (e.g., a large proportion of pre-Holocene RSL data are coral-based records, which usually have >2 m uncertainty range) and gradually decreases for more recent time intervals as more stratigraphy-based data became available. Therefore, the assumption of 0.5 m upper limit for RSL data is not solid, and it is important to check how a temporally variable (sea level index point) SLIP uncertainty distribution and a larger SLIP uncertainty would impact the assimilation results.

2. The authors assume a σinit of 2x1020/2x1019 Pa s for lower/upper mantle viscosity, which is a very small range given that the authors suggest the initial ensemble should cover the ground truth parameter value (c.f. the Lambeck et al., 2014 search space is 1019 -1021 and 5x1020-1024 Pa s for upper and lower mantle respectively). My query is what if the authors use a larger σinit value, would it affect the final assimilation results?

3. The authors assume no ice loading history uncertainty in the synthetic experiments, but the ice loading history is the biggest uncertainty for GIA modelling problems. It would be useful to discuss the potential problems caused by uncertain ice loading history, in other words, are there some possible solutions for the situation if we are uncertain about where

to propagate the particles?

Minor points:

-l25: The authors should pay attention to the differing role of mantle viscosity in controlling RSL change in the near-field and far-field regions. For example, in line 25 here, on-going GIA-governed far-field sea level change is primarily due to the ‘fingerprint’ effect (i.e., the spatially variable RSL change associated with changes to the shape of the gravitational field), which is largely not related to the mantle viscosity parameters. The mantle viscosity parameters are more important for near-field regions where viscoelastic land deformation dominate the local RSL change. Similarly, for line 186, the Earth deformation only dominates the near-field region, not RSL change for all regions. I would suggest the authors including some statements about the RSL change differences between the near- and far-field regions and highlight that the mantle viscosity parameter are more important for the near-field RSL change.

-l65: What is the point of this paragraph? do you use this approach or not?

-l69: Including a reference for the particle filter here would be useful.

-l93: What does the subscript i mean for equation 2?

-l130: How does the ensemble propagate through time? Do the authors run 50 forward GIA models with different mantle viscosity parameters or just one reference model to guide the propagation length and direction? I think you will update the mantle viscosity parameters during the deglaciation experiment. In order to calculate the RMS for that ensemble member, do you go back and calculate RSL value for this updated model from the start of the experiment (25.5/9.5 ka BP)?

-l146: It would be useful to mention that RSL rate is a non-standard type of observation; it is more common to compare model output with absolute RSL observations. When dealing with real-world data, the uncertainty on RSL rate will be greater than the uncertainty on absolute RSL due to the cumulative impacts of spatial-temporal uncertainty on the individual observations.

-l167: You quote the uncertainty on RSL observations, but you compare model output with RSL rates. It would be useful to know the RSL rate uncertainty (m/ka) produced by 0.1 and 0.5 m RSL uncertainty.

-l169: Please define what you mean by ‘initial offset’ here.

-l198: Since the synthetic observations are RSL rate, why are the RMS values expressed in m instead of m/ka?

-Figure5: Can the authors slightly expand on why there are RMS spikes after meltwater pulses? Specifically, meltwater pulses are produced by large ice mass loss from North American and Fennoscandia Ice Sheets which will produce a large signal of land deformation in those regions (which could be larger than the observational uncertainty), this will help the assimilation process to find the optimal solution. Therefore, given a better converged mantle viscosity ensemble after meltwater pulses, why are there such large spikes immediately after the improvement in the mantle viscosity solution?

-l225: Both the selected regions for Setup 2 use near-field region SLIPs where RSL change are sensitive to the mantle viscosity value. If the authors just used the far-field region SLIPs, does the assimilation approach work as well? If not, the authors should note their method should only be applied to near-field GIA problems.

-l259: “As a consequence less models…”, should use “fewer models” here.

-l296: “There are no large ice mass changes after 10 ka BP.” However, based on Figure 7 of this paper, the continental ice volume was still changing after between 10 and 5 ka BP.

-l318: It would be useful if more details about computation time can be given here.

-l324: How would the temporal variability of the data availability affect the results?

-l325: "… ~8 to 12 ka BP and is considerable smaller", should be considerably smaller here.

-l337: I suggest the authors cite Khan et al., (2019) as the reference for the HOLSEA community here.

References

Lambeck, K., Rouby, H., Purcell, A., Sun, Y. and Sambridge, M., 2014. Sea level and global ice volumes from the Last Glacial Maximum to the Holocene. *Proceedings of the National Academy of Sciences*, *111*(43), pp.15296-15303.

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