Dear reviewer,

we are thankful for the constructive criticism and the suggestions made about the manuscript. In the following you find detailed answers to the points that were raised.

Reviewer’s comments and author’s answers:

“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.

A: Our approach is slightly slower than a pure model run with the same ensemble size. This is due to the overhead of the particle filter in which some steps have to be performed in serial mode. However, due to the ability to change the parameters of the ensemble members, smaller ensemble sizes and thus smaller total computing time in terms of node hours are possible while still obtaining very accurate parameter estimates. We have now emphasized the advantages of our method more strongly in the manuscript.

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.

A: Valid point. New experiments with larger uncertainties that decrease towards present day have been conducted (new case E of setup 1). The size of observation uncertainties were chosen according to values given in Vacchi et al. (2018) and Khan et al. (2019). Our chosen values are even more conservative than the values given in the appendix of Lambeck et al. (2014). The obtained results were added to the manuscript. The viscosity values still converge to the given ground truth (target values) with a larger ensemble spread according to the assumed observation uncertainties. In order to investigate other influences on the ensemble convergence we use a more idealistic setup such that other effects are not masked by observation uncertainty.

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?

A: This experiment uses synthetic data from an artificial ground truth. It is only an example and does not need to be equal to reality to show the success of the method. We aim at showing that we can recover the target values of a viscosity model on which the synthetic observations are based. Our assumed ground truth differs from values currently assumed valid by the community but that does not affect the results of the study.
Using a larger spread in the initial ensemble is possible and leads to the same assimilation results. However, one must be careful not to end up with a degenerating filter. That can happen if the spread is loo large and only few (or even only one) ensemble member is close enough to the true values to have a significant likelihood. Larger values for observation uncertainty in the beginning of the assimilation (as shown in the new case E of setup 1) or a larger ensemble with denser sampling of the model parameter distribution help to prevent filter degeneracy since in that case models that are quite far from the ground truth can still be have significant likelihood values. If observation uncertainties decrease towards present day, the ensemble spread decreases too and parameter estimations with low uncertainties can be obtained. This was added to the discussion in the manuscript.

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?

A: This is indeed a major source of uncertainty when the approach will be applied to real *sea-level data. A more detailed discussion of this point was added to the manuscript. A comprehensive model set up is beyond the presented study, but the consideration of load history uncertainties is planned for a follow up study.*

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.

A: Statements regarding the difference of the meaning of RSL change in the near and far field were added.

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

A: The goal was to explain our motivation for using the Particle Filter. I agree that this does not become clear and this paragraph was combined with the next one and rewritten.

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

A: A reference to section 3.2 where particle filters are described was added.

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

A: The subscript indicates the particle member in the ensemble. An explanation was added.

-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)?

A: The ensemble consists of 50 independent members with different viscosity values. Each member is propagated through time individually. When observations are available, the fitness of each member is estimated based on the difference between model prediction and observations. Members with poor performances are discarded in favor of model states of better performing members. The latter are cloned and perturbed to reestablish an ensemble of 50 members. Then, the propagation through time is continued. The models are not restarted from t_0. A reference to section 3.2, where the ensemble update is described, was added.

-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.

A: The fact that RSL rates are non-standard was mentioned. The increased uncertainty of RSL rates was already considered in our experiments. It is now also mentioned in the manuscript. As stated in the manuscript, we have used sea-level rates instead of relative sea level due to the fact, that we do not know the sea level at present day during our integration scheme.

-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.

A: Indeed, this is a vital piece of information. There is a factor \sqrt{2} / \delta t between RSL uncertainties and rate uncertainties. The information was added to the manuscript.

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

A: The term “initial offset” refers to initial perturbation of the ensemble members at the beginning of the assimilation. It consists of a random value drawn from a normal distribution with mean \mu_{init} and variance \sigma^2_{init}. With “offset” we describe the mean of the initial perturbation. (see l. 125 in the original manuscript). The function of \mu_{init} and \sigma_{init} are now mentioned in the first paragraph of Sect. 4.1 and a reference to that place was added here.

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

A: This is indeed a typo. The labels were changed to the correct unit “m/kyr”.

-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?

A: During phases of strong melting like melt-water pulses, the sea level rises instantaneously during one assimilation step, resulting in a significant global sea-level change. This change is not caused or affected by any relaxation process. Accordingly, all viscosity structures would reproduce this change, given the ice melt is predefined in our assimilation experiment. In the following assimilation steps, the sea level rate is again governed by the relaxation process of the solid earth, which forces the enlarged ensemble spread to converge again to the best fitting subset. A more detailed explanation was added in the discussion.

-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.

A: Tests with only far-field observations were not successful. This is now mentioned in the text .

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

A: Corrected. Thank you!

-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.

A: The sentence was rephrased to express that ice mass change rate was already low that 10 ka BP and Fennoscandian and Laurentide ice shields are basically vanished by 8 ka BP. After that, ice loss can still be seen in Greenland and Antarctica.

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

A: Computing time strongly depends on the used computer system. Therefore, comparisons can only be made to methods tested on the same system which was not part of this study. However, we can state that due to the overhead of the particle filter, running time is larger than for the dynamic model alone. The amount of overhead depends on the ensemble size since some parts of the assimilation are performed sequentially. In our case with an ensemble size of N=50 the overhead is about 30–40 % depending on the physical allocation of compute nodes in the cluster. This estimation was added in the text.

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

A: We added a scenario in which only observations from ice-free locations were used. This reduces the available observations drastically for early periods. As a result, the convergence is very slow in the beginning of the assimilation period but increases after about 10 kyrs BP. After that we observe good convergence and are able to recover the target viscosity values.

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

A: It is corrected, thank you.

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

A: The reference was added.