We thank the reviewers for providing clear and constructive comment. We feel that our manuscript has improved a lot while addressing the comments. We provide our response to reviewers' comments in the following section. We leave the reviewers' original comments in black text and write our response in blue text. Quotes from the manuscript are in blue *italics*, and new edits made in the revised manuscript are blue **bolded** text.

Reviewer 1 (Wouter Van der Waal) comment:

The paper adresses the problem of long computation times for simulation of sea level changes due to ice melt for (i) long periods or (ii) when high temporal resolution is required. This problem especially occurs when ice dynamic models and solid Earth models are coupled and the complete ice history should be considered at each time step. Large computation time currently limits the application of such simulations. The paper presents a solution which reduces the computation time significantly by using variable time steps with smaller time steps closer to the present (or the epoch of interest). The method can be implemented in different methods for calculating the sea level response. Explanations in the text are clear, figures are well designed and helpful. The method is shown to work in schematic tests and interesting results are obtained for two case studies, for a long time history simulation for the Northern hemisphere ice sheets, and for fast future ice melt in Antarctica.

All in all this is a very nice and complete paper which will benefit sea level modelers and people interested in the application of such models. I have a few general comments below and several specific comments mainly asking for additional clarification in the annoted pdf. Together these constitute a minor revision which can be dealt with by changes to the text. The pdf also contains typos and suggestions for rewording, which the authors can consider and which do not have to be addressed in a rebuttal as far as I'm concerned.

General issues

Some conclusions depend on particular choices made in the paper, which should be made clearer. For example, the increment in the ice history thickness is assumed to take place at the end of the time step (as opposed to at the beginning of the time step) and from that follows several conclusions (for example 'missing viscous signals', line 565). See also the discussion on the choice of time steps in: Barletta, V. R., & Bordoni, A. (2013). Effect of different implementations of the same ice history in GIA modeling. Journal of Geodynamics, 71, 65-73. Also the results hold for a spatial resolution selected (spherical harmonic degree 524)

Whether an error is acceptable depends on the application. The parameter used in the paper for precision is the ice volume and the bedrock topography, but I could imagine for some applications an error in ice volume at 70ky before present is less of a problem than the same magnitude error at 11 ky. If a larger uncertainty is acceptable larger time steps are acceptable, so the suggested time steps in the conclusion are not as general they are presented. Also the word "optimal" implies some optimization which is not exactly what is done, so there could be similar but larger time steps which give similar precision but smaller computation time.

The paper focuses on the sensitivity to the time between loading and present, and briefly mentions the effect of viscosity on this sensitivity. However, the role of viscosity can be large. I would guess that if viscosity is very high, sensitivity to temporal resolution in recent ice thickness changes is low. This should be discussed at least in the conclusions (see also comments in the pdf). It is probably useful to introduce the relation between mantle viscosity and relaxation time.

Here, the reviewer has raised three general issues with our manuscript that are also reflected in their specific comments below. We summarize how we have addressed each of these points here, and we also respond in more detail to related specific comments in the annotated pdf file.

First, the reviewer suggests that we explicitly acknowledge how load changes are applied in the sea level model and the implications for our results, especially those in Section 3.1. We agree that this discussion was missing in the initial manuscript. In the revised manuscript, we now mention that our sea-level model applies ice loading changes at the end of each timestep rather than at the beginning. We also cite the suggested literature (Barletta and Bordoni, 2013) as a reference.

Second, the reviewer raises an issue with our discussion of error in the manuscript and ambiguity in choosing acceptable errors when deriving preferred time window profiles in our experiments. We agree with the reviewer that the acceptable error depend on applications, and this is what motivated us to apply the algorithm in two case studies with differing spatiotemporal scales of loading and rheological structure of the Earth. Our results from Section 3 show that our predicted ice volume is relatively insensitive to the time window profile of the earlier simulation times (see Fig. 3). We have included a sentence in Lines 541-543 in the revised manuscript, "***This indicates that the coupled simulation results are relatively insensitive to the specific choices for the time window profile for the past timesteps compared to the choices for the recent and current timesteps of the simulations.***" In addition, motivated both by this reviewer's comments and by suggestions from Holly Han's PhD thesis featuring this work, in the revised manuscript, we have performed a new suite of simulations applying the coupled ice-sheet – sea-level model to the future Antarctic Ice Sheet with varying temporal resolution. We have presented these results in new figures (Figs. 9 and 10) that show the differences in ice thickness in the Antarctic region and at the grounding lines across the simulations, which serve as another parameter to evaluate the precision in the revised manuscript. We also note that we now use the word "preferred" instead of "optimal" when we refer to the time window profiles that we derived from the numerical experiments.

Finally, the reviewer suggested we include more introduction and discussion regarding the relationship between mantle viscosity and relaxation time of the solid Earth. To address this, we have added discussion of the relationship between mantle viscosity and relaxation time throughout the revised manuscript. As an example, in the conclusion section (Line 964) of the revised manuscript, we write the following sentence, *"In general, adopting a shorter coupling time comes at the expense of computational cost, and the choice of appropriate coupling time for a given application will depend on both the resolution and timescale of ice sheet variations and the adopted Earth structure model* ***; shorter coupling time will be needed for fast-evolving ice sheets on the solid Earth with low mantle viscosity (like the WAIS), since the relaxation time***

of the solid Earth is faster (slower) for Earth's mantle with lower (higher) viscosity." The sentence we have added in response to the second general comment above also serve as a response to this comment.

Reviewer 2 (Volker Klemann) comment:

The manuscript is very well written and I rate it as an important contribution to GIA and coupled ice sheet-solid earth modelling. Nevertheless I have a number of concerns regarding the setup of the study and the findings phrased between.

In principle there are two results presented. First the authors show that the impact of loading events fades away with age relative to present day. This aspect is well known as fading memory for difusion processes like the solid earth deformability considered as a viscoelastic gravitating continuum. Accordingly, the loading details in the past are of less impact on the present-day deformation state, here discussed as topography change. In consequence, the authors consider this fact for the design of their convolution algorithm which they apply to integrate the viscoelastic field equations.

One drawback of the normal-mode approach, which is usually applied in GIA studies, is that for each additional time step computed with a further load change, the summation over the whole loading history has to be repeated. This results in a quadratic increase of computing time and storage of previous loading steps, making it rather unatractive for dynamic coupling. To overcome this problem, the authors suggest a scheme in which the number of considered loading steps is tapered, applying a skipping scheme where loading steps between are not considered. With the procedure presented here, they can reduce the integration time markedly, almost reaching a linear increase of integration time. But they only implicitly mention, that this problem is restricted to models using the normal mode approach, whereas in codes solving the field equations in the time domain, this problem does not appear. This aspect should be discussed already at the beginning of this study in order to show, where the method is applicable. In models capable of considering non-linear rheologies or lateral variations in the earth structure, the viscoelastic field equations are solved in the time domain and the problem of quadratic increase in integration time does not appear.

The second result, addresses the problem of the coupling interval between ice-sheet and solid-earth models. There, they find suitable values of 200 y for a standard global viscoelastic earth structure, and about 1 y for a structure representing the low viscous region of West Antarctica. They show, that coarser resolutions by a factor of 5 or beyond result in markable deviations in the resulting dynamics of the ice sheets. What they did not discuss are shorter coupling intervals. This would be a nice add on, as they start from the interval already suggested by Gomez et al., and so only confirm what those authors already found.

In a first experiment they consider an idealised rotationally symmetric problem in order to discuss the problem of sampling a predefined glaciation history. The motivation is not clear to me. It shows that the considered integration scheme results in a distinct delay of the forcing with increasing sampling interval. From the discussed deviations, the author could already consider a different integration strategy where load distributions between are averaged. For instance in case of dt = 20 ky for a predefined coupling interval of 0.2 ky, the authors could calcuate the response at time t to the load interval tj from the two load heights

$$H(t_{j-1} = t_j - dt) = H(t_j) = 1/N \sum_{i=i(j)-N}^{i(j)} H_i$$

where *i(j)* is the load index of the *j*-th considered loading step for the integration up to coupling time step *t* and $N = dt/dt_c$. This algorithm should be applied of course only to time steppings larger than the coupling interval dt_c. With such a method I think, the delay could be reduced markedly and the information loss due to skipped load distribtutions can be avoided.

In summary, I would suggest 'minor revision', mainly with regard to the setup of the study in the introduction the authors should make clear from the beginning that their method is only applicable to 'standard' 1D GIA modelling.

Regarding additional modelling I would rate my points to be considered as suggestions, which from my point of view would improve the proposed algorithm markedly.

The reviewer makes several general comments that the manuscript needs: 1) further clarification of the applicability of our time window algorithm, 2) additional discussion and exploration of the sensitivity to the coupling time interval in our model and 3) further clarity on and motivation for the idealized experiments (Section 3.1) that test the sensitivity of a standalone 1D sea-level calculations to the model temporal resolution. We address each of these comments in turn below.

1. In response to the first comment, we have made changes and additions in the beginning (in Abstract and Introduction) as well as in the last section (Discussion and Conclusions) of the manuscript:

In Abstract, we have modified a sentence to read, *"In this study, we introduce a new "time window" algorithm for **1D pseudo-spectral** sea-level **models based on the normal mode method** that enables users to define the temporal resolution at which the ice loading history is captured during different time intervals before the current simulation time."*

We also add a sentence in Introduction (Line 141) of the revised manuscript), we edited the sentence: *"The **standard** forward sea-level modelling algorithm adopted in coupled models employs a uniform temporal resolution throughout a simulation, which leads to a linear increase in the amount of surface loading history with the length of a simulation **and a quadratic increase in computation time. We note that the quadratic increase is associated with calculations performed in the spectral domain requiring the full integration of loading and sea-level changes from the initial to the current time step of simulations."***

We also make this clear in the conclusion section in the revised manuscript. In the conclusion section (Line 871), we write, "*We have developed a new time window algorithm that assigns nonuniform temporal resolution to the input ice cover changes and restricts the linear increase in the number of ice history* ***steps (or equivalently, the quadratic increase in computation time)***

in 1D pseudo-spectral sea-level modelling**."* Also, in Line (995), we write "*In this study, we have presented a new time window algorithm* ***that can be applied to *global* ***1D forward sea-level models based on normal mode theory (Peltier, 1974)****."*

2. Regarding the second issue, we refer the reviewer to Fig. 3 (black dashed line in Fig. 3a) and associated text in which we discuss a coupling time interval of 100 years, which is a shorter that the 200 years that Gomez et al. (2013) suggested. We note that differences between coupled simulations with 100 and 200 year coupling time intervals are minimal, in agreement with these earlier findings. Since this was already included in our original manuscript, we have not made any changes to address this issue.

3. Lastly, we have made our idealized experiment section more clear with several edits (please see Section 3.1 in the revised manuscript for all the edits made). In particular, we have edited the beginning of Section 3.1 to state our motivation for the idealized tests. In Line 305 of the revised manuscript, we now write, *"Before exploring* ***the sensitivity of coupled ice-sheet – sea -level modelling to the model temporal resolution in the next sections, we begin by testing the sensitivity the standalone standard sea-level modelling to the temporal resolution*** *(Fig. 1a)."*

On the delayed response (expressed as "missing viscous signal" in our original manuscript) that we saw in the idealized experiment, we note that this phenomenon is more related to the timing of the ice loading changes that our algorithm uses (also discussed in response to Reviewer 1) as well as the linear viscoelasticity of the Earth model, rather than the integration scheme that we adopt. As we have mentioned in the above paragraph and in the revised manuscript, the motivation of this section is simply to test (rather than to improve) the standard sea-level model outputs to the temporal resolution. Then, improvement in modelling results is made where we apply the new time window algorithm in Sections 3.2-3.3). Thus, we have not made changes to the existing standard model algorithm. However, we do agree that this needed to be explained more clearly, so we have added sentences in Line 418 of the revised manuscript: "***(We note that this result is the direct consequence of the linear viscoelastic relaxation process of the solid Earth. The result also depends on the timing of ice loading changes. That is, a viscous signal would be evident if the ice loading change was applied at the start of each time step rather than the end of the time step. This issue has been discussed in GIA model inter-comparison and benchmarking efforts, e. g., Barletta and Bordoni, 2013.)"***