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 328-330 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 638) 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 prededefined 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 (Lines 93-98) 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 566), 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 (599), we write "Overall**, 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 218 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 following subsection, we begin by testing the sensitivity of 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 259 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.)"***

Response to Reviewer 1's specific comments

Please refer to the pdf file uploaded in Authors Comments (at https://gmd.copernicus.org/preprints/gmd-2021-126/#discussion) where we have responded directly to the reviewer's specific comments on the pdf version of the initial manuscript. In the file, we have incorporated and addressed all of the reviewer's specific (minor)s comments.

Response to Reviewer 2's specific comments

1. I suggest to replace time window by something like adapted time stepping algorithm. Time window somehow implies that you shift the integration domain.

Our time window algorithm is also capable of shifting the integration domain, but we obtained more accurate results in the approach we took. In addition, we think that the name time window still is valid given that our template shifts forward as shown in Fig. 1b-2. Thus, we prefer to keep the name as it is.

2. l. 8 ff: ~O10^{0-6}, I would write O 10^{0-6}, the tilde makes no sense here.

Changes have been made.

3. l. 12ff: Callling this classic would only be understood by GIA experts. Instead you should be more specific here, that in classical GIA the problem is solved in the Laplace domain considering a normal mode approach.

We have changed the word "classic" to "standard" throughout the text.

4. l. 26: 'improve' -> 'reduce'. More important here is that the cumulative integration time of a coupled model becomes almost linear.

We have modified the sentence to read as follows: *"The time window algorithm **reduces** the total CPU time by ~50 % in each of these examples **and changes the trend of the total CPU time increase from quadratic to linear**."*

5. l. 40: 'slower' -> 'retarded'.

In the revised manuscript, we have removed original sentence (Lines 38-41 of the initial manuscript) because we think that the sentence is unnecessary and to make the text concise. This comment is no longer valid.

6. l. 40ff: You discuss elastic and viscous effects, but what is about the shear relaxation process between, where viscoelasticity takes place.

In the revised manuscript, we have removed original sentence (Lines 38-41 of the initial manuscript) because we think that the sentence is unnecessary and to make the text concise. This comment is no longer valid.

7. l. 43ff: Write simply 'lithosphere and mantle' as the elastic lithosphere cannot have a rheological
structure.

Changes have been made.

8. l. 52: Remove 'in' in front of 'in

We have modified the whole sentence to make it clear, and it now reads as follows (Line 49 of the revised manuscript) : ***"The mechanisms through which spatially variable sea-level change influences ice sheets vary in importance depending on whether the ice sheet is marine based or not."***

9. l. 83: Write 'annual to decadal scale resolutions'

Change has been made.

10. l. 94ff: As stated in the abstract, also here it becomes not clear that the quadratic increase is a consequence of the applied convolution, to solve the linear viscoelastic problem. So, I suggest the authors to clearify this.

We have clarified this in the text (now in Lines 93-98 of the revised manuscript), which reads as follows: *"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.****"*

11. l. 105ff: Skip 'what they called'

Change has been made.

12. l. 109ff: Skip 'classic' as this algorithm is only 10 yr old, and so, not classic. May be, use standard instead.

We have changed the word "classic" to "standard" throughout the text.

13. l. 137ff: Remove the '(18)' of this equation, and discuss that the term in braces implies a summation over the loading steps from 0 to j. Then, the quadratic nature of the integration time of one coupled cylce becomes more clear. ω is not explained.

We have removed '(18)'. We've described omega in the text. We have added a new sentence: ***"The second term of the right-hand side of the equation shows that*** $\Delta\mathcal{SL}_j$ ***depends on the increments of ice and ocean loading and the rotation perturbation over time."***

14. l. 139: change to 'ΔSj-1'.

Change has been made.

15. l. 12ff: 'where the change in topography is defined [...]'

We think that reviewer meant to refer to Lines 146-147 in the original text. We have made the text clear.

16. l. 152ff: Again, remove 'classic'. Later you also phrase it 'standard' which is somehow better.

We have changed the word "classic" to "standard" throughout the text.

17. l. 162ff: How do you motivate the choice of just four time intervals?

First, we have slightly changed wording, so the text now reads *"This algorithm allows users to assign non-uniform time steps …. **as many as** four time intervals.*"

We add the following text in the revised manuscript (Lines 166-170): ***"(We note that our model can be easily implemented and adopt more number of time intervals, we found that four is sufficient in both lengthy paleo simulations and short simulations with rapidly retreating ice sheets: for paleo simulations, one can set coarser temporal resolutions for the early simulations times, and for shorter simulations, the computation can be still feasible with fine temporal resolutions, and both cases do not require too many different time intervals.)"***

18. l. 172ff: Here it is better described than in the caption of Fig. 1

We agree and have slightly edited the caption to read as follows:

*"Figure 1. Schematic diagram of **standard** and time window algorithm in forward sea-level modelling. (a) The **standard** forward model algorithm in which the surface (ice sheet and ocean) loading history **is** captured in uniformly discretized temporal resolution. (b) The time window algorithm that captures the details of the loading history in non-uniformly discretized temporal resolution. The user assigns k number of internal time windows, each of which has a total length of L_ITW_k and internal time steps of size $\mathtt{dt}_k$. The ice load files shown as blue vertical bars are multiplied by a template element with a value '1' **(considered by the sea-level model),** and grey bars are multiplied by '0' **(ignored by the sea-level model)."***

19. l. 180ff: Can you give a formula how many ice load files have to be considered by this scheme? This might be also interesting for the cumulative integration time. My first guess was that a quadratic nature remains, but in the later figures it becomes clear that the integration time is dominated by the shortest time stepping of the last interval which is of constant length and, so, becomes almost linear.

We provided an expression for the number of ice files that the sea-level model needs to read in in Fig. 1 in our original manuscript (please see N_{total} in Fig 1b-1).

20. l. 182: Rephrase 'amount of surface loading history', as it is not clear what you mean.

We have rephrased it to "number of ice history steps"

21. l. 191ff: I doubt that you adopt your scheme to a 3D earth model. Most 3D codes are solving the equations in the time domain. Accordingly, I rate the statement 'We adopt 1-D Earth models [...]' as rather misleading, as your method is designed for 1-D Earth models.

We have deleted the sentence *"We adopt 1-D Earth model in all simulations;"* and have rephrased the sentence to read as follows (Lines 198-201): *"In the next section, we perform a suite of sensitivity tests performing standalone sea-level simulations and coupled ice-sheet – sea-level simulations to test the sensitivity of model results to the temporal resolution of the **1D** sea-level model**, which incorporates radially varying Earth Structure. T**he elastic and density* *profile of the Earth structure are given by the seismic model PREM (Dziewonski & Anderson, 1981).”*

22. l. 193 ...: The unit should be abbreviated as 'Pa s'.

Change has been made throughout the text.

23. l. 196ff: Regarding the Antarctic experiment in Sec. 3.3, which lithosphere thickness and upper smantle viscosity do you consider?

We provided this information in the original manuscript. Please see Lines 194-196: *“For section 3.3.2 in which we perform simulations over Antarctica, we adopt the best-fitting radially varying Earth model from Barletta et 195 al. (2018), characterized by a lithospheric thickness of 60 km and upper mantle viscosities of ~10^{18}-10^{19} Pas.”* No change has been made.

24. l. 207ff: Can you specify the cross section, I guess it is a lying parabola, and the relation between thickness and radius? Only during the text it becomes clear that the radius is varying with thickness, and not everybody has the book you refer to at hand.

We have included a new figure panel (Fig. 1g) showing the cross section of the ice sheet – the panel shows the growth of ice thickness with radius.

25. l. 226ff: The vaning experiment again starts from a hydrostatic equilibrium state, and the load is considered to be negative?

That is correct that the experiment starts from a hydrostatic equilibrium as any initial condition of a sea-level model simulation would assume. We have included a new frame in Fig. 2 (frame g) to show ice loading profile in each scenario of the experiment.

26. l. 232ff: From the model setup, I would rate 20 ky as rather unrealistic, as it expresses only a delayed heaviside forcing. More interesting in view of the later discussions would be to discuss changes between 0.2 and 1 ky as such variations you disuss with respect to the coupling interval. If the changes are much smaller here, you can directly show that the deviations in the coupled runs are due to the ice sheet interaction.

We agree that the 20-ky long loading scenario is unrealistic, but here we are performing *idealized* tests. We think it is useful to include these results because our point in this section is to show the sensitivity of sea-level model results to the timing and frequency of loading. We use the unrealistic 20 ky simulation to illustrate what the sea-level calculation solution looks like when the size of ice stepping is very coarse with respect to the simulation length. The main goal of this section (Section 3.1) is to show that predicted sea-level changes are smaller in magnitude with coarser temporal resolution, and the dramatic differences from the 20ky scenario serves the purpose well even though the time stepping size is unrealistic.

27. Sec. 3.1: The experiments nicely show the fading memory effect with regard to the delayed response. But for the further discussions I would rate them as less helpful; also see my general statement above. I really suggest, to shorten this section markedly.
The outcome of this section I would comment as: Interesting view, that a delay in loading is of less impact on the displacement when ongoing in time. Spada and Stocchi I think also discussed

this. To my understanding this is a direct consequence of the linear viscoelastic relaxation process you consider.

For the delay in loading we see in our results, we have included the following sentence in the revised manuscript (Lines 259-264): ***"(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, the viscous signal could be captured if the ice loading change happens at the start of each time step rather than the end of the time step, e. g., Barletta and Bordoni, 2013.)"***

We also have shortened the section.

In Figs. 2b-2 and 2b-2 I would plot instead the topography change, as the topography is otherwise dominating and the delayed response becomes much clearer.

We think that showing topography is useful because it shows how topography evolves relative to its initial position (above or below sea level) at different locations (i.e., central, peripheral and equatorial points) especially when the initial position is different (e.g., above sea level in the central and peripheral points and below sea level in the equatorial point in the case of ice buildup in Figs. 2a-c). The delayed response is also shown well in the last panel of each frame by comparing difference in topography between each simulation and the benchmark simulation.

28. l. 250ff: I would address this due to the longer wavelength of the water loading, at which the relaxation process is slower.

This sentence does not exist in our manuscript anymore as we have shortened the section as the reviewer recommended in comment on Section 3.1. Thus, the comment is no longer valid.

29. l. 261ff: Again I would adress this as a consequence of your considered integration scheme.

This sentence does not exist in our manuscript anymore as we have shortened the section as the reviewer recommended in comment on Section 3.1. Thus, the comment is no longer valid.

30. l. 272ff: Why not specifying this at the beginning describing the model setup. I suspected this during reading of this discussion, but never could be sure. At the beginning you could write how rload and Vload are related to hload which you change linearly.

We have moved the sentence, ***"We note that the ice thickness at the centre of loading (as shown in the top frames of Fig. 2a&d) changes linearly, but the actual volume change is nonlinear because of the changes in the ice-sheet extent; the volume change across one time step is greater when the ice sheet is more extensive."*** to the very first paragraph of the section where we introduce the model set up (Lines 222-225).

31. l. 284ff: This is an important aspect which you do not further consider in Sec. 3.3.

We believe that by "this aspect", the reviewer refers to *"...higher resolution information about ice cover changes is required for the ice history immediately prior to the current time step in a simulation, and lower resolution will suffice for earlier ice cover changes. The specific temporal resolution required will depend on both the rates of change of the ice cover and the Earth's viscosity structure, which we explore in two contrasting examples in Section 3.3."*. We do consider this in the following section (Sections 3.3) in designing the profiles of time window in each experiment. In addition, we have also performed a new suite of coupled simulation for the future AIS scenario in Figs. 9 and 10 incorporating this aspect.

As in our response to Reviewer 1's general comment, our Fig. 3 shows that the coupled model results (at least in terms of ice sheet volume) are not strongly sensitive to the details of the time window profile for the earlier time windows. Moreover, given a range of applications and Earth structure choices (e.g., number of layers in the Earth, specific parameters within those layers, ice loading scales), an explicit and generic relationship with the specific time window profiles would not have been feasible to come up with. This is way we instead explain a rule of thumb with some examples and show that the results are not that sensitive to the exact choice of time window parameters.

32. l. 287ff: I would skip this paragraph, as it is clear from what you stated above.
In total, I suggest to shorten this subsection, and concentrate on a more realistic time stepping change of 5 ky and smaller, as the given ones are obviously much too coarse.

We have removed this paragraph and shorted the section. Regarding performing an additional benchmark with a smaller time stepping, we feel that our goal (i.e., testing the sensitivity of sea-level model results to the temporal resolution) and main conclusion (i.e., coarser temporal resolution leads to underestimation in calculated sea-level change) will not change by adding new idealized simulations. Thus, we keep our original results and instead add clarification in our motivation of this idealized experiment section (see our response to the second major point above).

33. l. 315ff: Not sure, if this is also the case for any dynamic process. Comparing dt 0.1 0.2 and 1 ky, 0.2 ky results in slightly larger volume, whereas 1 ky results in smaller volume.

We have added "generally", and now the text reads: *"generally yield a higher volume of....."*

34. l. 323ff: 'unstably' I would rephrase as specific dynamics of the coupling is not resolved.
We have added changed "unstably" to "more strongly"

35. l. 323ff: Is this not a repitition of Gomez et al. 2013?
Gomez et al. 2013 have applied the coupled model to Antarctica, and we are drawing results for the Northern Hemisphere.

36. l. 330ff: This is not clear to me. Does this mean, that in the ice model an iterative time integration scheme is implemented?
The ice model performs iteration method in solving ice velocities with new ice thickness. We have added the citation to the PSU model in the text.

37. l. 335ff: So, 45 hr @ 0.2 ky and 98 hr @ 0.1 ky. From here it is not clear if these integration times only represent the sea-level calculation or also that of the ice sheet. Furthermore in Fig.

3a, you discuss the sensitivity on the coupling step.

It is difficult to attribute the increase in computation time separately to ice sheet calculation and sea level calculation only based on the simulations we performed. Also, we think that it is not necessary to separate the time delay effects as our focus is to show increased computational efficiency in coupled simulations.

38. l. 342ff: Well, this plot is quite obvious, more interesting would be also here to plot the cumulative integration time, as you do in Fig. 5.

We think that showing the number of ice history steps is equally interesting and the cumulative integration time is equally obvious. No change has been made.

39. l. 352ff: Change 'causes less [...]' -> 'is inadeqate to use as a coupling interval'.

We do not agree with the suggested change. No change has been made.

40. l. 355f: Remove 'In the same way [...] infeasable' as main aspect remains that such a scheme is only applicable in diffusion like processes like linear viscoelasticity in case of a normal mode approach.

We have addressed the comments regarding the resctricted applicability of the time window algorithm to 1D sea-level calculations using the normal mode theory in our response to the reviewer's major comment. Please refer to our response on the top of the page.

41. l. 416: Set 'and' between in roman.

We have corrected the mistake.

42. l. 419ff: But still, this has to be done a priori. The question is, can you assess what time stepping is necessary depending on the viscosity structure and the ice load variability?

In developing this study, we initially set out to do what the reviewer suggests here, but we found that establishing a generalized relationship between necessary time stepping for a given Earth model and ice load variability would require infeasible degree of sensitivity tests (as we mention above in response to comment # 31). Furthermore, we find that due to the low sensitivity of coupled results to the specific time window algorithm profile adopted (e. g. compare the lines in Fig. 3), deriving a specific relationship is not necessary, and example profiles for different applications is sufficient.

Hence, this is why we choose to derive time window profiles using the approach we described in this line for each application of our numerical experiments. We choose ice volume, ice thickness and topography as parameters to evaluate precision of our simulations that incorporate the time window algorithm. To demonstrate the performance of the time window profile that we derive and to show that our approach works, we have included new figures Fig. 9 and 10 in the revised manuscript.

In our new last paragraph in Section 3.3 describe these new results (Figs. 9 and 10) in Lines 550-562 of the revised manuscript: ***"To test the performance of the time window derived in Fig. 7 in coupled ice-sheet – sea-level simulations, we perform a suite of coupled Antarctic Ice Sheet-sea level simulations incorporating different coupling time intervals with the same climate forcing (RCP 8.5) scenario used in DeConto et al. (2021). Figure 9 shows smaller Antarctic***

ice volume and thickness for simulations with longer (uniform) coupling intervals such as dt = 25 and 50 yr. This is because a shorter coupling interval results in stronger ice-sheet stabilization. Geographically, West Antarctic region that goes through the most intense retreat shows the largest differences in ice thickness go up to hundreds of meters (second and third column frames of Fig. 9). On the other hand, results from the coupled simulations that incorporate the time window algorithm shows substantially smaller differences in ice volume and thickness compared to the benchmark simulation (fourth-column frames of Fig. 9). This is also shown in Fig. 10, which shows the cross-section of ice thickness and topography along the red line shown in Fig. 6d. The differences in ice thickness and topography are only a few meters when we incorporate the time window profile derived earlier in this section."

These new results support our original approach that we do not need to derive a specific and explicit relationship between ice loading, Earth Structure and the time window profile.

43. l. 425: Change to 'because the time stepping of the time window profile coincides with the 0.2 [...] standard or benchmark simulation'

We have re-written this sentence to make it clear to read as follows: *"Varying the internal time step between 5-40 ky for this period (Fig. 5a-d), the RMSE in predicted topography is zero for the first 120 ky (Fig. 5b)* ***because all four time window profiles assign temporal resolution of 0.2 ky for the first 120 ky of the simulation, which is the same time resolution as*** *in the benchmark simulation (as shown in the black bar indicating 120-0 ka in Fig. 5a)."*

44. l. 427ff: Change to 'starts to increase for all'

Change has been made.

45. below l. 435: Considering the deviation at 5ka, in the adjustment of the time stepping also ice thickness changes should be considered. It seems the larger deviations there, come from the ITW2 reaching the ice retreat around 130 ka.

In the revise manuscript, we have included the following sentence in Line 448: ***"We also note that the peak in RMSEs at ~5 ka is related to the internal time window covering 240-120 ka with dt = 40 ky not capturing the intense deglaciation phase at 140-120 ka (see Fig. 4a)."*** We note that this two-glacial cycle experiment is done based on the standalone sea-level model rather than the coupled ice-sheet-sea-level model, so ice thickness input is prescribed. However, we do show ice thickness from coupled simulations in our new experiments included in Fig. 9 and Fig. 10 now.

46. l. 441: Again 'simulation' is missleading here, I think you mean the integration time of the sea level part of the coupled simulation.

In response to the comment by the other reviewer and to this comment, we have removed "entire 240-ky long simulation" from the original text and modified the sentence to read as follows in the revised manuscript: *"**In t**he standard simulation,* ***CPU time increases quadratically with a linear increase*** *in total number of ice history steps that goes up to 1200, and the CPU time accumulates ~ 58.4 hr."*

47. l. 443: Change 'that' ! 'than'

Change has been made.

48. l. 437{448: Based on these results, I would still keep at least 5 ky, as nsteps is only slightly reduced, especially visible in Fig. 5c. Figure 5d has the problem, that the above mentioned fluctuation of 10-17% shows the results being not representative.
Furthermore, it would be interesting, if the deviation at 5 ka is due to a still too coarse timestep, and would reduce if you would choose 1 or 2 ky instead.

We agree that one can choose 5 ky instead of 10 ky without compensating too much CPU time. However, we would like to emphasize that the integrated RMSE in topography using the 10 ky is still very low, remaining below ~ 0.35 m, even when considering the regional rmse could be different (higher) than this value. We think our choice is sufficiently good enough for our application, as we have responded in the comments above regarding the relative insensitive to the coupled model results to the specific choice of the time window profile.

49. l. 450{457: I would expect that the integration of one ice-modelling step is constant. Then it would be easy to derive a simple equation to calculate the cumulative integration time of your sea-level model.

The reviewer is correct that the cumulative integration time shown in the figures are only considering the sea-level model calculations. And the cumulative time, as shown in the last column of Figs. 5 and 6 is dependent on the number of time (ice history) stepping at t_j shown in the third column of Figs. 5 and 6. We think that showing these two columns together show the relationship between the total computation time and the number of time steps and thus demonstrates the effectiveness of the time window algorithm already very well. We do not think the derivation of an equation to calculate the cumulative integration time of your sea level model is unnecessary especially given that we alternatively provide the number of ice history files that the sea-level model will consider based on the prescribed time window profile.

50. l. 465ff: As stated above, this statement comes rather late and should be clearified also in the caption of Figure 5.

In the revised manuscript Line 359 (Section 3.3), we have modified the sentence to read as follows: *"For each scenario, we perform a suite of* ***standalone sea-level*** *simulations in which…."*.

We also have clarified the caption of both Figure 5 and Figure 7.

51. l. 467: Change to 'sea level simulation based on a normal mode approach'. [Applying a time-domain code, such an algorithm is not necessary.]

Please refer to our response to the first major comment where we have made it clear that the sea-level model in which we implement the time window algorithm is 1D model that takes normal mode approach in the beginning and end of the manuscript. Given this added clarification, we leave the text here as is to avoid to much repetition throughout the text.

52. l. 470: Change to 'rapid growth of integration time'.

We have deleted the last part of the sentence to read as follows: *"Moreover, the reduction will grow for longer simulations as the CPU time in the standard simulation will increase quadratically whereas the time window simulation will suppress the rapid growth."*

53. l. 475ff: You should repeat here that you consider a low viscous 1D earth structure which you discussed in Sec. 2.

We have included the following new sentence in the revised manuscript Lines 474-475: ***"For the Earth model, we adopt a profile of thin lithosphere and low mantle viscosity as described in Methods (Section 2)."***

54. l. 498: Add 'along the S-N profile shown in Fig. 6d. [You should also mention the relation to Fig. 6 in Fig. 8.]'

We now mention the "the cross-section shown by the red line in Fig. 6d" throughout this section.

55. l. 500 vs. l. 511f: The two sentences are not consistently phrased, as you cannot state you foundan optimal internal time step by choosing dt=5 yr as being appropriate.

In the revised manuscript, we use "preferred" as opposed to "optimal".

56. l. 521ff: You consider an acceptable rms of topography change, how do you choose such a value, and do you consider here a global rms or a regional one? I have no idea what such an rms would mean in topography change at the grounding line you show in Fig. 8.

Our choice of suitable time window parameters depended more on the relative differences between the simulation results rather than imposing an absolute value of the RMSE. We acknowledge that the absolute value of the RMSE in predicted global topography does not tell us much about what is happening regionally (e.g, in grounding lines). To address this concern, we have performed (as mentioned above) a new suite of coupled simulations for the future AIS scenario using the time window profile derived from the standalone sea-level simulations: In Fig. 9 and 10, we show the differences in modelled ice thickness (rather than global topography RMSE) in the West Antarctic region and in grounding lines. These figures demonstrate that our time-window profile derivation approach based on the RMSE analysis works well.

57. l. 532: I won't call this 'ideal' but 'appropriate' in reducing the integration time by a factor of two if compared to the standard run.

We agree. We now use "appropriate" instead of "ideal" in the revised manuscript.

58. l. 534ff: Again the last paragraph discusses a different aspect than the former. Here again the coupling step between ice sheet and sea level model is discussed. Considering the rather short response times of the considered earth structure, I wont rate dt = 5 y to be relatively fine. Nevertheless, the result that one need 1 y time resolution in coupling is a rather important finding, although the fact that this was predefined as the shortest time step applied.

In this last paragraph, we are still discussing the temporal resolution of the standalone sea-level model rather than the coupling time step as we've discussed in the previous paragraphs in the section. We are comparing the results from standalone sea-level simulation incorporating the time window profile we derived in Fig. 7 to results of standalone sea-level simulation not incorporating (i.e., standard, uniform) temporal resolution of different sizes (dt = 5, 10, 50 yr), and these differences between the two results are shown in Fig. 8, as the first sentence of the paragraph writes (Line 534 of the initial manuscript) , *"Having chosen the time window profile for the future AIS retreat scenario, we compare predicted topography from this time window simulation to that from the standards simulations that incorporate coarser uniform temporal resolution of 5 yr, 10 yr and 50 yr."*

In the revised manuscript, we have extended our analysis to include results from coupled ice-sheet – sea-level simulations to demonstrate satisfactory performance of the time window we derived in this section. We have included a new last paragraph that discusses this new analysis and new Figures 9 and 10 as already mentioned above. Even though we pre-defined the coupling time interval of 1 yr in deriving a suitable time window profile for the future WAIS application, we think that this is appropriate given the previous literature on fast ice sheet variability and the low mantle viscosity in the West Antarctic region suggests such short coupling time interval.

59. l. 565: What do you mean by 'viscous signal'?

We mean viscous signal as non-elastic signal that appears after the firs- time step of a loading event in the sea-level calculation as shown in Fig. 2 (the idealized experiment) and explained in Line 244 of the original manuscript. However, to make this sentence concise, we have re-written in the revised manuscript to read as follows (Lines 575-578):

*"Our results show that sea-level simulations with coarser temporal resolution do not accurately capture the timing and geometry of ice loading, **which leads to** an underestimation of topography changes…"*

60. l. 566ff: I would consider this as a common fact in GIA modelling since Peltier (1974).

We have modified the sentence to now read as follows:

"**Our results also show** stronger sensitivity to more recent loading **(as suggested in earlier literature, e. g., Peltier, 1974),** indicating that higher temporal resolution is required close to the current time step in a simulation."

61. l. 572ff: Again, I won't call this an unstable

We changed "unstable ice volume fluctuations" to "less-smooth ice volume fluctuations"

62. l. 572ff: In short I would summarise this statement as for a 1D standard earth structure usually applied in global GIA studies.

We have edited the sentence to read as follows in the revised manuscript (Line 584): "*Our results also identify that 0.2 ky is the appropriate coupling time interval for glacial-cycle **simulations with 1D Earth structure typically adopted in global sea-level studies***"

63. l. 580: 'suitable time window parameters' I would rate as the correct phrase describing what you have achieved. I miss a more rigorous calculus in which, you can estimate how much you can increase the integration time step based on a given ice sheet variability.

Okay. Thank you for this comment.

64. l. 614ff: The larger timestepping in de Boer is likely due to the different dynamic behaviour of their applied ice sheet/shelf model. One aspekt also is the coarser spectral resolution of only j_{rmmax} = 128 those authors applied. From my point of view, the proper time stepping has to be based on the the individual model setup. So, I would suggest, to write, that de Boer found in their model setup, 1 ky to be sufficient whereas in the present coupling with PSU a 0.2 ky coupling model is more appropriate. Konrad et al. (2015, EPSL) applied a coupling step of 0.05 ky considering an asthenosphere viscosity of 1019 Pa s. So, I would rate the coupling interval being a combination of spatial resolution, relaxiation behaviour and induced loading changes.

We agree that the coupling time interval for different applications would depend on ice sheet variability and Earth Structure, and may be ice sheet model dependent as well. It is a good point that the coupling time interval would also depend on the spatial resolution of sea-level model (and likely of the ice model resolution as well). To incorporate this point, we have modified the last sentence of this paragraph to read as follows (Lines 632-640):

"***This difference in conclusions of ours and de Boer et al. (2014)'s may be attributed to different spatial resolution of the sea-level model incorporated in each study: our sea-level model uses three-times finer spatial resolution than theirs, which uses spherical harmonics expansion up to degree and order 128. Furthermore, the sensitivity of ice dynamics to bedrock elevation changes may also be ice sheet model dependent dependent (e.g., Larour et al. 2019, Wan et al., in review, GMD).***

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 the ***spatial and temporal scale of ice sheet and sea level variations as well as*** *the adopted Earth structure model."*

65. Section 4: In the summary, the findings are presented much better than throughout the text, where you use phrases like 'optimal' or 'ideal', whereas introducing a set or providing applicable time window parameterisations are more appropriate.

In the revised manuscript, we use 'preferred' and 'appropriate' instead of 'optimal' and 'ideal' throughout.

Figure 5b, shows that your approach can be improved, as the *L ITW1* dt of 5 and 10 kyr does not match all variability, seen in the increased RSME at ~ 5 ky. As a future aspect, I would suggest to improve this strategy in order to allow the time stepping to depend on the variability of subsequent loading intervals. May be, the averaging over those loading intervals instead of skipping might allow to keep such information.

We thank the reviewer for this suggestion. We agree that there is room to refine our approach to developing the time window profile in future work and while this would be outside of the scope of the current paper, we have considered the idea of developing an adaptive time window scheme that varies its temporal resolutions according to ice sheet variability. However, we do feel that our coupled model results (both the original results for the last deglaciation and the added results for future Antarctic evolution) demonstrate that our current approach is sufficient to capture the effect of sea level on the ice sheet while greatly improving computational efficiency, especially considering other sources of uncertainty. We have added a sentence in the last paragraph of our conclusion section in the revised manuscript (Lines 662-667)***: "A next step in algorithm development could be to implement an adaptive time window scheme in the sea-level model such that the time window profiles self-adjust to ice-sheet variability within the simulation. Meanwhile, we have shown that our time window algorithm achieves the goal of overcoming computational challenges introduced in coupled ice sheet-sea level modelling, while broadly capturing ice-Earth feedbacks, especially considering the range of other sources of uncertainties in the ice sheet and sea level model components."***

66. l. 675: I am puzzled a bit, as multiplied by 0 would mean they are considered as a zero load. I think you mean that those indices are not considered.

We have made the sentence clear in the revised manuscript:

"The ice load files shown as blue vertical bars are multiplied by a template element with a value '1' ***(considered by the sea-level model)****, and grey bars are multiplied by '0'* ***(ignored by the sea-level model)****."*

67. Fig. 3d: Here and in Fig.5 I would write only Nj at the y-axis.

Changes have been made – we now write Nj on the y-axis of Figs. 3, 5 and 7.

68. l. 739: Add '(see Fig. 8)'.

We have added "(see also Fig. 8)" in the last sentence of the caption.