Response to Reviewer 2's specific comments

*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.

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 This comment is no longer valid.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 This comment is no longer valid.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: ***"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 140-145 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: ***"(We note that our model be easily be 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 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: *"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* ***1-D*** *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 20ky 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 (Line 418): ***"(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.

We have added the point and the text now reads as follows: *"This is because there is active water loading* ***(that has longer wavelength and thus causes slower relaxation process)*** *occurring at this site…"*

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

In this paragraph, we are describing what the results are showing rather than explaining reasons for why we see them. The explanation is provided the later paragraph (Lines 276-286 of the original manuscript.) No changes have been made.

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 Figs. 2a and d) changes linearly, but the actual volume change is nonlinear because of the changes in the ice sheet's 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.

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 protime 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): ***"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 where future ice retreat is most intense show the largest differences in ice thickness go***

up to hundreds of meters (Fig. 9, second and third column frames). On the other hand, results from the coupled simulations that incorporate the time window algorithm show that the differences in ice volume and thickness are substantially smaller compared to the benchmark simulation (fourth-column frames in 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 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 680: ***"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 the 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 tj 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 554 (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: ***"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 have addressed this comment by modifying the sentence to read as follows: *"... we first select a time window profile based on global RMSE in Fig. 7 and then we test the performance of the chosen time window at capturing deformation at the grounding line in Fig. 8* ***across the linear profile shown in Fig. 6d (red line)****."*

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) , *"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:

"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 902): "*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: "***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.*** *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***: "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.

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

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