We would like to thank the reviewer for their positive comments, as well as constructive suggestions for improving the manuscript. In particular, the reviewer points to section 2.4 and the need for a more explicit comparison to Coulon et al. (2021). As we outline below, we will address all their concerns in the revised version of the manuscript.

We now give a detailed answer to the reviewer's comment.

In this study, the authors present FastIsostasy, a regional GIA model capturing lateral variations in lithosphere thickness and mantle viscosity, as well as gravitationally consistent sea-surface changes. The key advantage of FastIsostasy is its ability to bypass the computational expense of 3-D full self-gravitating earth models. This makes it a very promising tool, particularly for ice-sheet modellers, as it facilitates the consideration of lateral variations in solid Earth properties often overlooked in sea-level projections due to computational constraints. FastIsostasy hence holds promise for significantly enhancing the accuracy of sea-level predictions, particularly for the Antarctic ice sheet. Spatial variations, especially beneath the West Antarctic ice sheet with its low mantle viscosity, have been identified as crucial in Antarctica. The potential for triggering negative feedbacks that limit and delay mass loss adds to the interest of this novel model.

Overall, I find the manuscript to be well-written and effectively presented. FastIsostasy appears to be a compelling tool, advancing beyond previous models that addressed a similar exercise (such as the Elementary GIA model). The benchmarking in section 4 against 1D and 3G GIA solutions is very interesting and provides valuable insights into the model's behaviour. The open-source and well-documented nature of the model adds immense value.

We thank the reviewer for acknowledging the development effort, including technical aspects such as the code documentation.

However, the manuscript could benefit from a few clarifications:

- If my understanding is correct, FastIsostasy is essentially a 2D model due to the lumping of the depth dimension. I think this needs to be emphasised more clearly in the manuscript. This aspect is sometimes presented in a misleading way, and a more explicit acknowledgement of this approximation and its implications is needed.

The reviewer is correct. FastIsostasy is a 2D model. Other reviewers shared this confusion and the issue will be clarified in the revised version of the manuscript.

- FastIsostasy allows to include gravitationally consistent sea-surface changes in a regional domain. Again, this allows for a significant improvement in ice-sheet projections, given that most ice-sheet models consider the sea surface to be uniform, missing important feedback influencing the stability of grounding lines. Unfortunately, the section introducing the sea-level model and its improvements compared to Coulon et al. (to account for time-varying ocean area) lacks clarity. A restructuring and clarification of this section, with a focus on explaining how the improvements offer a key advantage, would enhance the manuscript (see my suggestions in the specific comments below).

We agree that the implementation of the sea level theory was not sufficiently documented. A more precise description will be provided in the revised version of the manuscript following the suggestions made in the reviewer's specific comments.

- Table 1 provides valuable insights for comparing FastIsostasy with other existing approaches. However, the current manuscript could benefit from a more explicit discussion in the main text regarding the similarities and differences between FastIsostasy and the Elementary GIA model. This is particularly important since both models aim to address similar challenges, namely, offering a computationally-efficient approach to incorporate LV solid Earth properties and sea-surface changes. While it is evident that FastIsostasy is more complex, there are noteworthy similarities between the two approaches. One notable advantage of FastIsostasy, briefly highlighted in the conclusion section, is its ability to avoid translating viscosities into relaxation times. Expanding on this point in the main text, in addition to the information presented in Table 1, would contribute to a more comprehensive understanding of the strengths and distinctions of FastIsostasy compared to the Elementary GIA model.

We agree with the reviewer's comment and will ensure that the revised manuscript provides a more thorough comparison between the two approaches.

Once these clarifications are addressed, along with the specific comments provided below, I believe this paper is suitable for publication and would make a very valuable contribution to the field.

Specific comments :

- Introduction, l.24-26: I would suggest reformulating this, as it implies that enhanced melting is the driver of bedrock uplift and sea-level drop, not the grounding-line retreat itself.

Agreed. This point will be clarified.

- Introduction, l.25: Maybe it is worth clarifying what is meant by 'sea-level' here, as a panel of definitions exist. I suspect you mean the relative sea level, i.e., the difference between the bedrock and the sea surface.

Yes, we do mean the sea surface elevation relative to crustal elevation. We will make this explicit.

- Introduction, l.31: Maybe clarify here that it is the creation of pinning points that explains the influence of GIA on the buttressing ice shelves?

Agreed. This point will be clarified.

- Introduction, l.34: Replace ‘parameters’ with ‘properties’? To check throughout the manuscript.

This change will be made throughout the manuscript.

- Introduction, l.56: I would remove ‘of the ice-sheet and GIA models’ from the sentence as you do not discuss uncertainties in ice-sheet models themselves. For example, ‘Such uncertainty thus needs to be propagated…

We will remove this text.

- Introduction, l.59-60: Coulon et al. (2021) addressed this parametric uncertainty by running large ensembles of simulations with a computationally efficient GIA model. This is worth mentioning here.

This will be indicated in the revised text.

- Introduction, l.62-64: This is not entirely true, as Coulon et al. (2021) proposed a way to account for the lateral variability in Antarctic rheological properties at a low computational cost. I know that it is presented later, but I think that, even though their model is of lower complexity than FastIsostasy, it is worth mentioning their work here, and more generally in this section, especially as their motivation was very similar to the one presented here.

This will be indicated in the revised text.

- Introduction, l. 65-69: Some of these concepts have not yet been introduced or only very briefly, e.g., the depth dependence of the mantle viscosity (only shown in Fig.2 but not mentioned in the text), or the dependence of the response time scale on the spatial scale of the load. Either introduce these before or simplify the description here and give these details later.

The concepts described on these lines will be more thoroughly introduced in a revised Introduction.

- Introduction, l.78-79: I am not sure what is meant by ‘sufficiently constrained in literature’.

Although uncertainties remain in estimates of upper-mantle viscosity, the value adopted in published articles is always prescribed in the case - as in our article - of adopting a Maxwell rheology, The situation is more complicated in the case of a Burgers rheology, which requires specification of two viscosity fields. We will clarify this point in the revised manuscript.

- Introduction, ELRA description: I think this section lacks a few references, for example, to illustrate studies that use such a model.

These references will be added to the manuscript.

- Table 1: Why does the Elementary GIA model have a ‘~’ symbol in LV? This would be worth explaining in the main text or caption. Same for the ‘sea-level’ for LV-ELRA and FastIsostasy, clarification in the caption might be useful.

In deriving a field of relaxation time, Coulon et al. (2021) apply a Gaussian smoothing over a binary field. This is an approximation of the structure of LV in comparison to adopting a field such as that depicted in Fig. 2. Deriving *a-priori* estimates of the relaxation time from Fig. 2 and using these in LV-ELRA is possible but tedious and does not consider the dependence of the adjustment time scale on the load wavelength. We will add text to justify our use of the “~” symbol at this point in the manuscript.

- Section 2.2, 198: While I find the idea of lumping the depth-varying viscosity profile at a location into an effective viscosity interesting and understand its value for the models’ computational efficiency, I think that it remains important to discuss the limitations in more detail. In particular, it is important to acknowledge that (similar to previous LV-ELRA attempts) because you end up with a unique effective viscosity value, it does not allow to capture the full multi-normal mode response of the Earth to surface loading which is accounted for in 1-D and 3-D GIA models due to their viscoelastic layering. In fact, the larger the load, the deeper the deformation reaches into the mantle. The ease with which the mantle relaxes thus depends on the radial viscosity profile, with, e.g., the shallower layers being the more relevant at the local spatial scale.

This reviewer is correct on all these points. The connection between load size and depth sensitivity is well described in the literature. For example, the response to the ocean load or a much larger ice sheet (e.g., the Laurentide Ice Sheet) will depend on deeper mantle structure than we have considered. However, the near-field displacement in the vicinity of the West Antarctic, for example, will be dominated by viscosity within the depth region we account for, which is reflected in the accuracy we've established for FastIsostasy relative to a fully 3D GIA simulation. In any case, we will discuss the issues the reviewer raised in the revised manuscript.

- Section 2.3, l.209-210: Is this similar to what is performed in Bueler et al. (2007)? If yes, this should be acknowledged. If not, maybe explain how it differs.

Yes, it is. This will be made explicit in the revised manuscript.

- Section 2.3, l.215: refer to section 4.3 here for clarity.

This will be done.

- Section 2.4: I find section 2.4 along with Figure 4 a little confusing. In particular, I find the motivation for the extension to Goelzer et al. (2020) unclear. Overall, your regional sea-level model is largely based on Coulon et al. (2021), except that you propose an improvement to account for the time-varying ocean surface. The motivation and significance of this improvement are not clearly expressed. I would suggest that you start this section by providing more detail on what influences sea-surface changes, i.e., explaining that you calculate the regional sea-level field using equation (20). This will give the reader more context. I would then introduce the gravitationally consistent sea-surface changes, i.e., the sea-surface height perturbation, which is what has been emphasised so far in the manuscript and especially in the introduction, and which will dominate the sea-level signal. Unless my understanding is wrong, equations (17-18) are required to improve the estimation of s(t). The section would also benefit from a better introduction to s(t) and how it is defined.

- Section 2.4, l. 276, and Figure 4: I believe that SLC has not been introduced so far.

The concern raised by the reviewer is similar to comments by one of the additional reviewers. We agree that our treatment of gravitationally self-consistent sea level should be discussed in more detail and we will provide a more substantive discussion in the revised manuscript.

- Section 4, l.356-362: Is it really necessary to define 'acceptable' error bounds a priori? Unless you can infer them from actual studies using the same 3D GIA model with different viscosity fields (if so, please provide a reference), the values proposed here seem rather arbitrary. I think it is sufficient to say that larger errors comparable to parametric uncertainties are acceptable.

This is an opinion shared by other reviewers, which we agree with. We will modify the manuscript accordingly.

- Section 4.2, l.412-414: What do you think is the influence of the regional versus global domain? Could it influence the larger differences in N towards the edge of the load? It might be worth mentioning it.

The use of a regional domain introduces errors associated with large scale gravitational effects that can only be captured in a global geometry. We will include additional explanation of the limitations of a regional domain in the revised manuscript.

- Section 4.2, l.415: I think I would use ‘comparable’ instead of ‘excellent’.

This revision will be made in the revised manuscript.

- Section 4.3, l.429: I find the name FI3D misleading. If my understanding is correct, the parameter fields in FastIsostasy are 2D and not 3D, given that the depth dimension is lumped. Please clarify.

We agree and will revise the label to avoid confusion (for more details, see the summary of answers to the reviews).

- Section 4.3, l.440: To what do you attribute this underestimation of the forebulge?

Our hypothesis: Analyses of the resolving power of data related to glacial isostatic adjustment have shown that forebulge dynamics may be sensitive to relatively deep mantle viscosity (e.g., Mitrovica et al., JGR, 1993). We suggest that the inaccuracy arises because our mapping of 3D viscosity structure into 2D does not capture this sensitivity.

Section 4.3, l.453: I find the reference to ELRA misleading given that it also has a computationally-efficient LV version (Coulon et al., 2021).

We will resolve this confusion in the revised version of the manuscript.

- Section 4.3, l.455-459: This is an interesting point, but I do not understand the reference to Le Meur and Huybrechts here since (i) they do not only look at the final uplift map but also at the transient evolution (for the mean bedrock evolution) and (ii) they do not consider heterogeneous solid Earth configurations.

The reviewer is correct to point out that LeMeur and Huybrecht (1996) present a time series of the mean bedrock elevation across different models. Our point is the fact that the spatial comparison is only performed for the present-day uplift map. We believe that this is not sufficient and that the spatial pattern of the error should be analysed for various time steps, or alternatively for the time step of maximal error, as we do in Fig. 8. LeMeur and Huybrecht do not consider heterogeneous Earth structures but a heterogeneous response will nevertheless arise because the spatial scale associated with retreat in the Ross and Ronne regions is different from mass flux associated with the comparatively small retreat in (for instance) the Amery region. Since none of the regional GIA models included in LeMeur and Huybrechts incorporate a dependence on the load wavelength, there may be substantial differences in the spatial pattern of displacement that are “smoothed out” by simply studying the time series of the mean displacement. Our objection would be milder if the time series showed the spatial mean and maximum error over time, since these are stricter metrics.

- Section 4.4, l.473: observed where? Please provide context or a reference.

The revised manuscript will provide more details by discussing results obtained by running simulations with various maximal depths.

- Figure A3: the colorbar is missing.

Thank you for noticing this error, which will be corrected.

- Section 4.4, l.477-479 (Figure 8): I find this choice of the masking corresponding to the LGM extent questionable, especially as the area that matters for marine ice sheets is the area around the grounding line. I would suggest taking the whole domain, or an area larger than the ice sheet extend to include the vicinity of the grounding line. Typically, you have shown in the previous tests that FI underestimates the peripheral forebulge. The masking applied here may therefore ignore this signal.

Since the LGM only represents a few kiloyears of the full glacial cycle, the peripheral forebulge and grounding-line vicinity are included in the mask for the vast majority of the simulation, although admittedly not for the LGM. Furthermore, the error in the peripheral forebulge is relatively small (about 10 metres for all cases of Test 3), albeit systematic, and is therefore likely to have only marginal significance in a glacial cycle that exhibits displacements of more than 550 metres. This issue will be resolved by extending the mask, as we indicated in our summary of the answers to the reviews.

- Figure 8: The panel subscripts are arranged in a confusing way.
- Section 4.4, l.501: I believe that t=-14kyr is not shown on the figure.

Thank you for pointing out both issues, which originate from a late modification of Fig. 8 shortly before submission and will be corrected.

- Section 4.4, l.502: I am not sure where to look at. From Figures 8e and 8h, it seems to me that FI3D underestimates around West Antarctica and overestimates around East Antarctica.

The revised version of the manuscript will include an additional sentence to make clear that panel (h) shows that over most of the mask the vertical displacement is underestimated, with the exception of the Eastern margin. This is likely to be due to the rotational feedback and the associated increase in ocean load in this region, which is captured by Seakon but not by FastIsostasy.

- Section 4.4, l.507: But does FastIsostasy take into account the loading influence of sea-surface changes? I don't think this is mentioned in the manuscript?

It does. This inclusion is evident in Eq. 1. However, we concede that section 2.4. misses this point, which will be included in the revised version of the manuscript.

- Conclusion, l.525-532: I am not sure whether these limitations have their place in the conclusion, as I do not think they (or at least for some of them) have been discussed before. Perhaps instead in a 'discussion' or 'limitations' section, or in section 2, which presents the model?

This is a sound point. The revised version of the manuscript will follow this advice.

Minor comments/typos:

- Introduction, l.44-48: This long sentence is a bit hard to follow, maybe try to split it for clarity?
- Introduction, l.56: 'ensemble simulations' -> 'ensemble of simulations'.

- Figure 1, caption: ‘from (Whitehouse et al., 2019)’ -> from Whitehouse et al. (2019). I spotted this issue at other locations in the manuscript.
- Figure 1, caption: ‘enhanced melting at the grounding line, leading to larger thickness…’ -> ‘enhanced sub-shelf melting, leading to grounding-line retreat, and therefore larger thickness and increased outflow at the grounding line’.
- Section 2.2, l.179: ‘the following scaling factor’?
- Section 2.3, l.232: ‘a an ad-hoc’
- Section 4, l.346: ‘ice loading history’, or even instead ‘by an ice loading history over a full glacial cycle’?
- Section 4.1, l.393: ‘present the following differences’
- Section 4.3, l.421: ‘in (Gomez et al., 2018)’

Thank you for pointing out these errors. They will all be corrected in the revised manuscript.