We thank the reviewer for their positive assessment of our manuscript and for their constructive suggestions for improvement. We provide below a detailed response to their comments.

Swierczek-Jereczek et al. present a new GIA model, FastIsostacy, based on the Fourier collocation method that can include lateral viscosity and lithospheric thickness variations in the mantle. The new model is compared to the existing 3D self-gravitating visco-elastic Earth model Seakon. They show a maximum error of 0.2 over a glacial cycle when FastIsostacy is used compared to Seakon. Although the error is not negligible, FastIsostacy has the potential to be coupled to ice sheet models because the runtime is very short and the model is open source.

The manuscript is well written. The introduction provides a great overview of current existing methods used in GIA models. The second part of the introduction, "FastIsostasy in the model hierarchy", provides a clear overview of the different GIA models. Table 1 also includes which numerical scheme is used for each type of GIA model, but the numerical scheme are not explained in the text of the section "FastIsostasy in the model hierarchy". For this manuscript, the FDM/FCM and the numerical scheme of Seakon are particularly important and should be described in more detail.

The numerical scheme of Seakon is extensively described in Latychev et al. (2005), whereas the combination of FDM/FCM adopted in FastIsostasy is described in Section 2.3. We will nevertheless extend the discussions around l. 240 to add details about both methods.

The method section explains quite well the details of the new method developed but should elaborate more on the effect of simplifications in FastIsostasy in order to reduce computational cost compared to Seakon. Line 503 states that tuning can be done easily but doesn't explain what tuning would exactly be required.

Tuning the density or the effective viscosity field could be performed by minimising the mean square difference of the predictions to the Seakon-derived displacement and would only require the use of an optimisation technique. See for instance Kalman inversion in the documentation of the code. Simpler techniques such as hand tuning can also be adopted.

Also, line 537-539 states that the difficulty of creating meaningful ensembles is decreased but even though different viscosity fields are available, it is not shown how FastIsostasy performs using different viscosity fields. It is necessary to discuss the implication of the use of effective viscosity, the characteristic wavelength, and alfa. Equation (6) suggests that the alpha is selected to improve the fit to a 3D model, which means the results depend on a particular 3D GIA model. Are alpha and W fixed for all comparisons? That results in conclusions that are dependent on this choice, which should be emphasized. Tests for different alpha and W are required to have robust conclusions on the accuracy of FI3D.

All tests use the same alpha and W choices, which demonstrates that this aspect of the methodology need not to be tuned by the user (although alternate choices may be appropriate if the characteristic wavelength of an application is significantly different than the cases treated in our manuscript). We will add to the manuscript a discussion regarding the choice of the characteristic wavelength, which influences the effective viscosity, and a comparison between the response time scale of an incompressible vs. compressible 1D GIA model, which is relevant to the parameter alpha.

FastIsostasy is presented in line 533-534 as a model that can greatly reduce the error of bedrock displacement compared to ELRA and ELVA and that is useful coupled to an ice sheet model due to the short computation time. However, there are no results presented in this manuscript that compare the performance of FastIsostasy with the most widely used GIA model in ice sheet modelling, the ELRA model and the laterally varying ELRA model so there could also be no conclusion about the reduction of the eror of FastIsostasy compared to ELRA. It is therefore not shown whether FastIsostasy is an improvement on what already exists. The focus of the paper should therefore be changed and the introduction rewritten with less focus on coupled ice sheet – GIA models, or a comparison with ELRA should be shown.

We agree with this suggestion and will include ELRA in the comparisons treated in the manuscript. We will also explore the possible inclusion of LV-ELRA - however, this is a more complex undertaking since viscosity fields in this case need to be converted into fields of relaxation time.

In line 361 the authors justify an error of 0.2. However, the error should ideally be smaller than the error introduced by different parametrizations. In some cases this error could be in the order of a hundred meters, which is significant and could have a large effect on ice dynamic models. In comparison, 1D GIA model benchmark study show much lower errors.

Most reviewers rightfully indicate that our choice of the threshold in relative error is arbitrary. We will therefore remove it from the manuscript and simply mention the different values obtained in the tests. We emphasise that the appeal of FastIsostasy is an error reduction compared to ELVA (and ELRA as we will show in the revised version of the manuscript), without any significant increase in the computation time. It will be up to the user to decide whether the error level is acceptable for their application. This point will be made more explicit in the manuscript.

Furthermore, from figure 8b it can be seen that the error between FastIsostasy 3D and Seakon 3D after 8 kyear is larger than the effect of 3D rheology itself.

This is correct, although the errors incurred by adopting FastIsostasy are overall quite small (only a few percent). We emphasise that adopting a 3D GIA model is substantially more computationally expensive than FastIsostasy, which makes the latter appealing for many ice-sheet modellers.

In that case, differences between FastIsostasy and global GIA models are outside of the range of parametric uncertainties, which contradicts the conclusion in line 534-535. The *conclusions on the performance of FastIsostasy should therefore be more considerate of the large error rather than accept them compared to an arbitrary standard, especially since the values hold for certain choices of the resolution.*

We agree with the comment. We will emphasise that the effect of LV (and therefore the parametric uncertainties) is only important for large transients displayed during the (last) deglaciation.

Specific comments

Line 3-4: The impact of 3D GIA on ice sheet dynamics has only been shown for glacial cycles and not yet for projections. This sentence is suggesting that it has been shown. Please include in this sentence that the impact has only been shown over a glacial cycle.

We agree with this comment, although we are aware of a manuscript by Natalya Gomez and colleagues that is in review that involves projections. We will cite this work if it has appeared online before this review process continues, but will revise the sentence if it has not.

We believe that it is still appropriate, later in the text, to point out that accelerated mass loss in Antarctica has been observed in the Amundsen region, where lower viscosities of the upper mantle have been robustly inferreded. Additionally, most studies showing a collapse of the WAIS display an onset of the instability in this very region. Therefore, 3D viscoelasticity can be expected to play an important role in future ice loss of the AIS.

Line 6: An iterative coupling scheme is required when simulating a glacial cycle but it hasn't been studied yet whether iterations are required to simulate projections. Projections have been performed without an iterative coupling scheme using a 1D GIA model and there are no published projections using a 3D GIA model. The need for an iterative coupling scheme is therefore not an argument why 3D GIA models are not used in ice sheet models. I would suggest to, instead, include that 3D GIA models are not used in ice sheet modelling because the effect of 3D GIA is not known, 3D GIA models are computationally expensive and the coupling scheme is complex to apply.

We strongly believe that 3D GIA models are not used in ice-sheet models because of their prohibitive computational requirements, not because there is a presumption that 3D variations in Earth structure are not important. Gomez's work, for example, amply demonstrates that such variability alters ice sheet evolution and so it is reasonable to assume that it impacts projections - an issue that is the purpose of the new Gomez article mentioned above. Nevertheless, we will revise the manuscript to avoid speculation.

Line 12-13: Please include the value of error here instead of mentioning that the agreement is very good.

We agree. The value will be cited in the revised manuscript.

Line 15: The Fortran version is not provided yet, according to the data availability section. I suggest to include in this sentence that it will be provided.

Since the submission of the paper, significant work has been invested in developing the Fortran version of the code, which is now ready to be used.

Line 44-47: The impact of 3D GIA over a glacial cycle doesn't necessarily mean that the impact is also large over projections of a much shorter time scale. In multiple places in the introduction, the distinction between what has been studied over glacial cycles and what has been studied over projections is not clear. There are studies that show a significant effect of using 3D GIA compared to 1D GIA over a glacial cycle using a coupled ice sheet – GIA model (for example Gomez et al., 2018 & van Calcar et al., 2023) and from recent history till present day using a GIA model with a prescribed ice history (for example Blank et al., 2021). However, in this manuscript, results from Gomez et al. and van Calcar et al. are presented as if they show the impact of 3D GIA in projections as well, which is not the case. There are studies that show the importance of 3D GIA in projections using uncoupled models, such as Yousefi et al. (2022) but this study is not referenced in the manuscript. Currently, there are no publications on coupled ice sheet – 3D GIA models used for projections. This distinction should be made more clear throughout the introduction and the references to Yousefi et al. and Blank et al. should be added.

Again, we are aware of new work that does demonstrate the importance of incorporating 3D structure in coupled GIA/ice-sheet projections. As mentioned above, if this work appears in print then we will cite it, but if it does not appear we will revise the text accordingly.

Line 47: Include that sea level contributions from the basins are 19.2 and 3.4 m at present day.

The values are mentioned in the manuscript and we understand that the reviewer wants to emphasise that they refer to the present-day state of Antarctica. We believe this is already unambiguous in the text.

Line 58: Include references of the 3D GIA models that you are referring to.

These references will be included in the revised manuscript.

Line 62-64: Whether or not the ice-sheet modelling community is well aware of the how important 3D GIA is, is subjective. There are only a few studies showing the importance of 3D GIA over a glacial cycle and in projections and there are no published studies simulating projections using coupled ice dynamic – 3D GIA models. It can therefore be argued how well informed the ice sheet modelling community is up to this point and how well aware they could be without so many studies. I suggest to only mention that 3D GIA models are computationally expensive and complex to couple to an ice sheet model.

We will revise the manuscript accordingly.

Line 81: It is worth mentioning that there might be no asthenosphere at certain locations in Antarctica, and that ELRA includes that mantle, but that does not weigh against the confusion that it could cause to change a name that has been used in numerous papers since 1996. I suggest to leave the name as it is.

Line 86: Add the constant "flexural rigidity" and "lithospheric thickness" in the text, as these are other important parameters in the ELRA model.

The lithospheric thickness is already mentioned and the flexural rigidity is derived from it, as pointed out later in the text.

Line 101-102: To improve the readability, provide a short explanation about the difference between a viscous channel and a viscous half space.

We agree. This explanation will be included in the revised manuscript.

Line 109-112: Include that 1D GIA models also include the buoyancy effect of the core on the mantle and the mantle on the lithosphere.

The impact of mantle buoyancy on the lithosphere (which is, by definition part of the mantle) is also captured by other models. Buoyancy effects of the core on mantle is not included in most 1D GIA models.

Line 118: Include reference A et al. (2012), and Huang et al. (2023) for the finite element method.

These references will be included in the revised manuscript.

Line 119-121: The referenced models in this sentence (Gomez et al. and van Calcar et al.) are coupled ice sheet – GIA models, which require a much longer simulation time than 3D GIA models by itself. Since this section is solely about 3D GIA models, a simulation time of weeks is not applicable.

Correct. Instead, we will refer to computation times of Seakon (4.5 days for glacial cycle as pointed out later).

Line 123-124: The 3D GIA model in van Calcar et al. uses timesteps varying from approximately 1 to 1000 years, depending on the ice loading and the deformation rate, so the lower limit of the timestep of 3D GIA models is not accurate. Furthermore, it is not clear *why it is relevant in this context that GIA models sometimes have a larger timestep than ice sheet models.*

We agree.

Line 126-139: The manuscript mentions two regional models specifically (Coulon et al., 2021 & Weerdesteijn et al., 2023). However, it is not clear why these two are picked out, since there are multiple other 1D and 3D GIA regional models (Nield et al., 2018; Book et al., 2022). I suggest to move line 126-129 to the section about LV-ELRA, and line 129-135 to the section about 1D GIA models. Also explain why Coulon et al. and Weerdesteijn et al. are mentioned specifically, and not other regional models. Some other important references are missing, such as Book et al. (2022), who used a similar method as this manuscript for a regional model focused on Thwaites glacier, and Kachuck et al. (2020).

We believe that l.126-129 and 129-135 should not be moved since they build upon some of the listed models in the context of regional modelling, and particularly relevant for this publication. We will, however, include references to the papers by Nield, Book and Kachuck.

Line 136-137: The available GIA models have a runtime acceptable for modelling ice sheets over glacial cycles, the runtime is only not acceptable to perform ensemble studies with a wide parameter space. Please include this nuance in the manuscript.

We agree. This nuance will be included in the revised manuscript.

Line 137-138: Could you define what is meant by "complexity gap" since there are regional 3D GIA models.

We will define this more explicitly in the revised manuscript.

Line 155-159: To improve readability, include a sentence to explain why a placeholder field is used and what the pseudo-differential operator represents.

A placeholder field is cited here to illustrate the operation and it can be replaced by any other field. The pseudo-differential operator has no straightforward explanation in intuitive terms and we prefer here to refer to Bueler et al. (2007) on this issue.

Line 168-169: Add whether the viscous half space have a variable or constant thickness.

A half-space has no thickness since it is infinite. We believe that this is sketched in Fig. 3 and already included in the name "half-space".

Line 180: Clarify in the text whether R is computed at each time step.

Any computation related to the parameters, including the computation of the effective viscosity, are performed only once at initialisation. We will include this explanation in the revised manuscript.

Line 248: Please include why it is required that the far-field displacement should be zero.

This will be discussed in the revised manuscript.

Line 307: It is not clear what “in-place” means.

In-place refers to the fact that computations are performed without any memory allocation. This is of importance when programming in julia and is commonly used in other GMD publications.

Line 340: To improve readability, include reference to Spada et al. (2011).

This reference will be included in the revised manuscript.

Line 415: Given the negligible maximal difference in displacement between the 1D GIA models of Spada et al. (2011), a maximal difference of 0.16 between FastIsostasy and the 1D GIA models of Spada et al. is relatively large. Also, purely based on this idealized test, it cannot be stated that FastIsostasy can replace 1D GIA models. This is also shown by figure 8, showing a maximal error of about 0.8 around -4000 years between the SK1D and FI1D, which is relatively large for a benchmark test.

We emphasise that the mean error is very low (mean(e) < 0.05) and that the metric used in the manuscript (maximum over time and space) is the strictest possible metric. In particular the error is much lower for the cap load (max(e) < 0.07, mean(e) < 0.03), which, from an ice-dynamics perspective, is more coherent in space and therefore far more plausible than the disk load.

For the comparison between SK1D and FI1D, see Fig. A3, which shows a maximal error of about 0.16 (and not 0.8). We believe that this justifies replacing a 1D GIA model by FastIsostasy for many applications. We will state this only later in the text (around l. 475) since an earlier appearance of this statement (l. 415) misses complementary information from Test 4, as pointed out by the reviewer.

Line 421-422: Please include the reference to the chosen viscosity.

The viscosity fields are theoretically derived and were defined within the present manuscript. The fields are shown in the supplementary material, which is referred to in the main text.

Line 438-439: The error of FastIsostasy compared to Seakon can be different when a realistic ice load with a realistic Earth rheology is used. Whether FastIsostasy can be used in regional ice sheet models should therefore be concluded based on test 4 as well and should not be stated based on only test 3.

We agree with this and we will revise the manuscript accordingly to emphasise this point.

Line 460-461: Could you quantify the error tolerance and adaptive time stepping.

We believe this goes beyond the scope of the manuscript, since it depends on the specific application being considered.

Line 472: Please include the results of this test in the manuscript and quantify what is meant with "better results".

We agree. This will be included in the revised manuscript.

Line 479: Quantify the offset in the forebulge and the implication of leaving the forebulge out of the presentation of the results.

As pointed out in the answer to the third reviewer, the mask used for the error metric includes the forebulge for most of the simulation, only excluding it at LGM. This point should be resolved in the revised manuscript, since the mask adopted in the revision will be larger.

Line 502: Define what is meant by "worst case".

This text will be changed to "time step of maximal error" in the revised manuscript.

Line 502: Define which region is meant by "near field".

By near field we here refer to the displacement field below the ice sheet. This definition will be included in the revised manuscript.

Line 505: The pattern of the rotational feedback is described as "a subtle dipole separated by a great circle" but when one doesn't know what the pattern of rotation looks like, the description is not so clear. It could be described as a gradient from east to west outside of the grounding line.

We will revise the manuscript to make the geometry of rotational feedback clearer.

Line 512: It would be useful to include what the runtime would be when a higher resolution is used.

We believe that Fig. 5 already provides a measure of how the run time scales with the dimension. In particular, we emphasise that the same equations are solved regardless of the test, meaning that the run times are comparable for the same problem size. The only difference in run time originates from the stiffness of the right hand side of the formulation, as exemplified in test 4 between the laterally constant and laterally variable setup of FastIsostasy.

Line 515: The timestep of the GIA model used in van Calcar et al. (2023) is dynamic and is about 1 year when the deformation rate is high. The convergence of the ice-sheet and GIA histories are there to reach a present day bedrock topography when simulation a glacial cycle. Those iterations would be needed by any ice sheet model coupled to FastIsostasy as well when a glacial cycle is simulated. The iterations are therefore not related to the time step of the GIA model. This should be corrected in the text.

We agree with this point and we will revise the manuscript accordingly.

Line 517: Include the resolution of Seakon.

The resolution will be included in the revised manuscript.

Line 520: It is unclear what is meant by “much richer”.

This terminology is explained in the text after “much richer”.

line 533-534: It should be stated more explicitly when FastIsostasy performs better than 1D GIA models, namely between -22 and -10 kyr of the glacial cycle.

We agree and will revise the manuscript accordingly.

Line 541-542: Clarify what is meant by “it minimizes the misrepresentation of the GIA feedback”.

We agree that this can be clarified by mentioning the deformational and gravitational response.

Line 560: A discussion should be included that compares the conclusion of this paragraph with literature that does compare incompressible and compressible models, such as Huang et al. (2023). Is this increase in viscosity consistent with literature? Describe the limitations of increasing viscosity instead of including compressibility.

See comment above. We will include a comparison between compressible and incompressible versions of the same 1D GIA code.

Technical corrections

We thank the reviewer for suggesting the corrections below. We agree with them and will address them, unless specified otherwise.

Line 20: impacting > altering
Line 169/Footnote 1: API is not defined.
Line 225: Define F.

F is defined by Eq. (9). It is simply a shorthand notation for the right-hand side of the equation.

Line 242: Define ODE

It is defined in the text (l. 200).

Line 268: A0 is not defined.
Line 276: SLC is not defined.
Line 365: Define parameters.
Line 501: t = -14 kyr > t = -16 kyr
Figure and table comments

- Table 1: includes a description of the rheology, but it is not clear what is meant by Maxwell-like.

Yes, this should be replaced by “relaxed”

- Table 2: Please include a short description of the parameters in the caption.
- Figure 4: To improve readability, include the definition of variables in the caption.