We thank the reviewer for acknowledging the ambition inherent to our article, namely developing a GIA model of reduced complexity, able to regionally mimic a 3D GIA model at much lower computational cost, for application to coupled GIA/ice-sheet modelling. We are disappointed that the reviewer felt that our goal was not achieved with FastIsostasy, since none of the concerns they raise argue against the code accuracy that we established through the benchmarks. We emphasise that eight experiments (Fig.7, Fig. 8, Fig. A2 and Fig A3) show differences relative to a 3D GIA model that are small (less than 5% on average, and maximum ca. 15% over space and time). In our view, this benchmarking effort establishes FastIsostasy as the most accurate approach to date for mimicking 3D GIA at greatly reduced computational cost. As such, we believe that it is a significant contribution to coupled ice-sheet/GIA simulations and that GMD is the ideal venue for communicating our results to both the ice-sheet and the GIA communities.

We believe that the reviewer's criticisms can be comprehensively addressed with a series of changes to the text, and that they stem in part from a misunderstanding of the model, which is not a 3D model, and from some confusing text in the original manuscript. As a first step in this regard, we start by proposing a modification of the title:

FastIsostasy v1.0 – An accelerated, regional 2D GIA model accounting for the lateral variability of the solid Earth

For the sake of clarity, we will now address each comment of the reviewer. In our opinion, some terms that appear in the review, including the words "comical", "haphazardly" and "author-touted", are not appropriate in a scholarly discussion . We have thus tried to answer the reviewer's concerns whilst maintaining a constructive tone.

1. *The introduction to the manuscript is poorly written. Perhaps, it's symptomatic of a larger issue. While Figure 1 might certainly be relevant to a follow-on paper, these frames are not relevant to this research paper. The heart of this GMD submission is to solve a generally time-dependent 3-D viscoelastic flow problem with complex 3-D distribution of material properties, and then show that the prediction of vertical time dependent deflection is reliable, albeit with some error. The pertinence of the problem to ice sheet stability and past and future sea-level change can be addressed in a single paragraph with appropriate referencing. What is needed in this manuscript are more meaningful descriptions of the approximations and the tests against the Seakon code. I elaborate more in item 2 below. The paper should give the details at the same level as might be seen in journals such as the Journal of Computational Physics (https://www.sciencedirect.com/journal/journal-of-computational-physics). In fact, such a journal might also be a logical option.*

   We believe that the introduction of the article in its current form is appropriate to introduce ice-sheet modellers - who are often unaware of the complexity of the GIA response and represent our main target audience - to the importance of including laterally-variable Earth structures in their simulations. We motivate this by invoking the GIA feedbacks exerted on marine-terminating glaciers, as depicted in Fig. 1, since their inclusion was shown to differentially impact 1D and 3D GIA models coupled to ice-sheet models over glacial cycles (e.g., Gomez et al. 2018, van Calcar et al. 2023). Furthermore, we emphasise the present work allows for modelling on time scales relevant for Marine Ice-Sheet Instability over low-viscosity zones, as we discuss below. We are certainly happy to provide more information on the FastIsostasy-Seakon comparison, and our revision will do so.

The present work does not solve a generally time-dependent 3D viscoelastic flow problem but rather approximates such a problem on a 2D grid. We hope that this is clarified, in part, by the change in the title and we will include an additional sentence to highlight this at the very beginning of the manuscript.

2. *Fluid behavior, and elasticity for that matter, are poorly described, both from the standpoint of physics and basic mathematics. The approximation for the lithosphere is that of planar elasticity, an approximation in which a reduced stress tensorial balance is assumed. This is never mentioned, and the severity of the approximation also goes without explanation. Equally, for the fluid part of the physical space the assumption of the layering is that it can be 'lumped' and furthermore each layer acts in a manner like the shallow water approximation in physical oceanography. This puts severe restrictions on the type of interactions a laterally heterogeneous sub-lithosphere mantle may possess (i.e., lateral flow may occur but not like in a 3-D Stokes flow that we are familiar with in ice sheet models and/or the nominal 1-D structure / 3-D GIA simulation of the poloidal flow that employs the full equation system governing the gravitational-viscoelastic response to surface forcing. This could be accomplished by writing out the full equations, then dropping out the terms that yield the theory that is then developed in Section 2.1.*

The approximations and assumptions in our methodology which concern the reviewer are well documented in fundamental citations that are included in our manuscript. For instance, thin plate theory is a widely-used scientific term, and the assumptions behind it, including the reduced structure of the stress tensor, are clearly stated in Ventsel & Krauthammer (2001).

It is true that there are restrictions in the type of interactions ("lateral flow may occur but not like in a 3D Stokes flow") that may occur in FastIsostasy. We agree that this limitation should be explicitly stated and will do so in a revised version of the paper. However, in none of the benchmark calculations we summarise, and, we believe, none of applications to ice-sheet modelling we envisage, will this limitation be disqualifying.

We emphasise on l. 232-235 that we did not succeed in deriving Eq. 9 from first principles and can therefore not fulfil the reviewer's request expressed in the last sentence of this paragraph. Alternatively, we will add an appendix that emphasises which approximations are made in related work (Cathles 1975, Bueler et al. 2007, Coulon et al. 2021) and how Eq. 9 is inspired from these studies.

3. *The referencing is poor. This is usually a problem that can easily be sorted out: referee asks for a new reference, or to delete one, and the author response is simply*

to agree or argue the contrary. This is not what I mean. The referencing as it stands in the submitted manuscript is quizzical, to the point at which it is clear, in my opinion, that the authors might not have read the papers that they so haphazardly refer to. For example, Section 2.4 discusses the sea-level computations. There is reference to the nonlinearity in the system that is required to be solved but has nothing to do with the traditional problem of nonlinearity treated, for example, by Spada and Stocchi (Computers & Geosciences 33, 2007, 538–562) and that is treated by Mitrovica and Milne (2003) and Kendall et al. (2005). The latter authors also treat, in an elegant way, the problem of migrating boundaries, which, to my reckoning, is NOT a nonlinearity but non-perturbative part of the global GIA problem, i.e., the ocean boundaries need to be updated, and this is simply a trial-and-error process. Mixing the reference to the method of Goezler (2020) and the references in the last three sentences on Page 12 is both confusing and wrong.

As always, we are open to including references that we may have missed in our discussion, as we describe below. However the suggestion that we are “haphazardly” citing references, some of which were written by co-authors of this paper (Mitrovica et al. 2003, Kendall et al. 2005), is, in our view, unfair.

The reviewer questions our use of the term nonlinear in describing the sea-level theory and prefers the term “non-perturbative”. Both terms are accurate. Migrating shorelines on a realistic topography introduces a nonlinearity with respect to the ice load in sea-level physics. We therefore see this concern as one of semantics and will make clearer that the nonlinearity is introduced by the use of a realistic topography.

Finally, as was pointed out by the second reviewer, Section 2.4. needs to include more details about the sea-level treatment. We believe that this addition will resolve the reviewer’s concern about references to Goelzer et al. (2020).

Another glaring example of confusion, is the fact that Table 2 shows values for Young’s modulus and Poisson’s ratio, implying that we are dealing with a compressible model. But then the Appendix A discusses necessary corrections for the assumption of incompressiblity and claims that the Maxwell time needs to be adjusted to account for this.

We understand the confusion of the reviewer and will make clearer in the revision that Eq. 9 assumes incompressibility but that, as described in Appendix A, an adjustment of the Maxwell time is made to match the more realistic case of a compressible model.

The Maxwell time (incorrectly written with Young’s modulus) is not influenced by compressibility, since it is a material parameter of the constitutive equation. It is difficult to understand what the authors are up to, but as I interpret what is written in Appendix A, there is confusion between Maxwell time and relaxation time(s), the *latter of which can only be derived by solving the eigenvalue problem in the full inverse Laplace transform (though there are alternative approaches to obtaining the decay spectra). I am afraid Appendix A is simply wrong. Some justification is needed as to why this is implemented in the author-touted Julia code.*

The Maxwell time depends on the viscosity and Young's modulus. In fact, the latter depends on the shear modulus and the Poisson ratio. Hence compressibility influences the Maxwell time.

Some additional remarks

Some parts of the paper are well-organized, and I encourage the authors to strengthen these in a revision. The section "FastIsostasy in the model hierarchy" (which seems not to have a number ??) is well written and informative. While the benchmark cases and comparisons in Section 4 are also good, but they are also incomplete. I think there is an underlying contradiction, and I allude to this in the opening sentence of item 1 above. The revised manuscript needs a table to define all acronyms (BSL, etc.).

We thank the reviewer for pointing out the missing numbering and will add it. We believe a table of acronyms is not necessary, nevertheless the revised version of the manuscript will introduce each of them with more clarity. In our comments below, we argue that the benchmark cases cover the main issues of relevance but we are always open to further suggestions.

The authors, it appears, are striving to have an efficient way to compute the coupled ice-sheet solid Earth problems. It appears they may have made progress toward this goal. I mention a 'contradiction'. It has to do with the time scales involved. The opening discussion gives a description of an ice sheet retreating on a retrograde slope (Figure 1, which I recommend be deleted). The time scale relevant to such analysis is comparable to the time scale of melt-water pulse 1A, or about 50-500 years, yet the benchmarks of the analysis involve time scales that are more than 1-2 orders of magnitude larger than this. So, this needs to be clarified. In my detailed remarks and conclusion to this review I suggest a way to better address this. As it stands the new code is tested against an FE model on time scales of a full glacial cycle. It's not clear that any of the faster time-scale phenomenon has been addressed. Again, my detailed remarks are designed to help the authors better address this.

The reviewer's assertion on time scales is incorrect. The time scales of the forcing applied in Test 1-3 (Heaviside in time at t = 0, order of 1 year) and Test 4 (glacial cycle including a rapid retreat in West-Antarctica at t = -14kyr, order of 100-1000 years) correspond to those of meltwater-pulse 1A and more generally MISI. The long simulation times are achieved using short time scales of forcing. Finally, we emphasise that Test 3 as well as Test 4 include low-viscosity zones that produce fast deformational responses, as is evident in Fig. 7c, or in an animation of the glacial cycle that can be found at https://github.com/JanJereczek/FastIsostasy.jl.

Detailed Comments

Abstract

I find the phrase "solve its underlying partial differential equation" odd because Section 2.1 just jumps immediately to the approximations.

This concern will be addressed by the additional section in the appendix mentioned above.

Introduction

It appears from the 1st paragraph that the authors are interested in ice-solid Earth coupling. In the next three paragraphs the authors are interested in Antarctica, vertical deflection of the surface, lateral heterogeneity and run time per simulation. It's not clear that the title fits the goals. Secondly, it is odd that the authors choose not to reference Sasgen (2017), Kachuck (2020) or Weerdsteijn (2022), since they have goals that align tightly with goals of this paper, especially the former since he attacks lateral heterogeneity in a very simple way that is analogous to what is set up in FI3D.

We point out that Weerdsteijn et al. (2023) is cited in the paper and that it offers a more complete approximation than the work in Weerdsteijn et al. (2022). We will add the two other references suggested by the reviewer. We must emphasise, however, that none of these approaches offers a large benchmark suite against a 3D GIA model, nor an open-source code that can be used by ice-sheet modellers to account for the lateral variability of the solid-Earth parameters.

In any revision, there needs to be an explanation of the disparity of modeled time scales (instability vs glacial cycle).

Figure 1 needs to be removed since this geometry and time scale does not exist in the remainder of the paper. The physics is an important background to the motivation for the study. This manuscript is about developing an efficient code strategy and finding how much errors exists in the simplifications needed to speed up the run times.

This section is well organized, but here the authors need to provide a table of acronyms employed. In describing ELRA it is an oversight not to mention that using the relaxation time to estimate topography change is a method assumed by ice sheet models for a long time now (George Denton, for example).

These points are answered above and below.

Line 82. ‘as e.g.’ -> ‘for example as in the case of’

Agreed. The change will be performed.

Line 88. I am confused by the ‘neighboring points’. It’s a continuous medium.

This text will be revised to “vicinity”.

Line 93. Cite a few ice sheet modelers that use ELRA.

References will be added.

Line 106. How is tau (x, y) determined? As a relaxation time it will generally always longer than the local Maxwell time, and at any x , y position there are multiple relaxation times, each with their own wave number dependencies.

Text on tau(x,y) will be added. We will also mention that in Coulon et al. (2021), one value is used for East Antarctica and another for West-Antarctica (within a range of possible relaxation time scales), with a Gaussian smoothing applied.

Line 110 ‘vertical ..’ -> ‘gravity, vertical …’

The text will be revised accordingly.

Line 110 The phrasing ‘by means of spherical harmonics’ is comical. Better to say: ‘by solving the partial differential equations of the time dependent boundary value problem after spherical harmonic expansion of the dependent variables.’

This text will be modified.

Lines 119-121. There is no mention of van der Wal’s 3-d Finite Volume method. Also, perturbation methods are not really used in practice, though they can provide insight.

We will include a reference to van der Wal in the revised version of the manuscript.

Line 125. No mention is made of the ease with which a 1-D model can be structured in the context of a radially stratified earth model (just as in a seismic model) versus the great difficulty of parameterizing a Newtonian viscosity from a 3-D seismic model.

We will add this material to the revised manuscript.

Line 141. ' … against analytical, 1D …'. I don't know of any analytical 3-D solutions. Is this just a typo?

The reviewer might have missed the important comma here. To prevent this, we suggest rephrasing the text as "against analytical solutions, as well as 1D and 3D numerical solutions".

Section 2.1

Equations (2) and (3) need to be derived from the full governing equations in their vector and tensor forms.

Eq. 2 and 3 express finite difference rules and numerical differentiation via a Fourier transform. There is therefore no governing equation associated with them.

Section 2.2

Line 170. If eta sub l (x,y) is confined to a layer, then the model really is not 3-D. In the real earth we should imagine that flow from a weak zone centered at x1, y1, z1 can flow into/out of another neighboring (weaker/stronger) zone cantered at x2, y2, z2. This is kind of fundamental to a laterally heterogenous mantle under conditions of gravitational disequilibrium.

The manuscript repeatedly states that FastIsostasy is not a 3D model. We believe that the use of FI3D as an acronym may have led the reviewer to think otherwise, although "3D" refers to the viscosity field used for benchmarking and not the dimensionality of FastIsostasy's domain. To prevent any confusion, we will change the title of the manuscript as mentioned above, and will respectively rename FI1D and FI3D to FI (ELVA) and FI (LV-ELVA).

Line 180. It is unclear how this equation applies to layers at different depths sine the wave number dependencies should vary as a function of depth.

As stated in l. 472, shallow depths (about 300 km) are typically used to compute the effective viscosity. Over shallow depths, the wave number does not change significantly and we believe that the approximation is therefore valid.

Line 196. As stated above I do not understand this scaling. I don't think this is rationalized correctly.

This is answered above and below.

Line 215. The reference to Farrell is incomplete. What Green's function? A spherical model? Agreement with Test 3? That hasn't been introduced to the reader at this point in the manuscript.

The text "Agreement with tests below" will be used instead and the reference will be made with more specificity.

Line 221. A Stokes flow will always have a dynamic pressure term. The logic presented is flawed.

We do not solve Stokes flow equations (which is in any case impossible to do on a 2D grid). Using LV-ELVA to approximate 3D solid-Earth deformation resembles the shallow ice approximation of Stokes flow in ice-sheet dynamics in the sense that a 3D problem is approximated by a 2D one. As stated above, we plan to clarify this point in the revised manuscript

Section 2.3

Line 225. We don't normally see a partial u / partial t term in a momentum balance equation of GIA. Please derive this equation and Eq. (14). This is my first time to read a reference to a 'place holder matrix'. This perhaps this illuminates the problem with this paper for me as a reviewer. Perhaps it needs to be submitted to a journal wherein this choice of vernacular is familiar. J. Comp. Phys, or Physical Review, or others might be options.

The partial u / partial t notation is standard in continuum physics and is used in the main references cited in or manuscript (Bueler et al. 2007, Coulon et al. 2021). A placeholder matrix is simply a variable used to illustrate an operation, as in Eq. 2. It can be replaced by any arbitrary matrix, as its name suggests. The rationale for the reviewer's suggestion to submit the present work to J. Comp. Phys, or Physical Review based on such arguments is difficult to understand.

Section 2.4

Comments on Figure 4 and discussion of Sea Level. See my remarks above.

Section 3.

Rationalize the use of Julia beyond flowery language like 'vast ecosystem' and 'good performance'. There must be something at the heart of this language that has advantages over C or C++.

The vast ecosystem is specifically discussed in the bullet points l. 302-330. In any case, it is not necessary for Julia to be superior to C or C++ to justify its use.

Section 4.1

Equation (23) has some assumption of a time history. Please state this assumption.

This equation assumes a Heaviside function in time for the load (which implies very fast time scales of the load, as discussed above). This point will be stated explicitly in the text.

Should remind the reader that wave number kappa is a cylindrical wave number that only at high value corresponds to the wave number of a Cartesian system.

We believe that this reminder is unnecessary in the manuscript.

Section 4.2

Figure 6. It seems 'sea surface' is the depth of water column which changes as a function of time. This needs to be explained much earlier in the paper, maybe even in the Introduction.

The exact term used in the caption is "sea-surface height", defined in Gregory et al. (2019). This does not correspond to the depth of the water column and the revised version of Section 2.4 will ensure that any such confusion will be avoided.

Section 4.3

Figure 7. The results are really mixed here. Sometimes FI can receive a 'pass' and other times a 'fail'.

We think the reviewer has misinterpreted these results. FI3D, as defined in l. 428-431, always passes the test by a large margin. FI1D yields an error that is typically made by using

ELVA (Bueler et al. 2007) and is only plotted for comparison. It is our intention in these results to demonstrate quantitatively how the comparison with Seakon improved in the implementation of FI3D relative to FI1D. This is the only criticism the reviewer raises that is related to the accuracy of the code and it appears to originate from misreading the figure and the text describing it.

Line 457. This is a good point about the final quasi-equilibrium snapshot being misleading.

We are grateful for this comment.

Line 462. Concerning shorter time steps: this is common knowledge among those of us who do these computations and formulate models.

We believe that not all ice-sheet modellers are aware of this and that it is advantageous to inform them when to expect shorter time steps.

Section 4.4

Line 486. I am confused about how to interpret 'mean'. "Spread around unity" leaves me in the cold.

The mean error is the average value of *e* (defined in Eq. 22) over the masked region. The text reads "spread around identity" (not "unity"), which is labelled in the legend of panel (a) that is referred to in the text. We believe these formulations are not ambiguous and ask the reviewer to be more specific about how we could make the point clearer.

Figure 8 is generally good. The differences in the 3D approaches might be interpreted by some as being unacceptably large. But this depends on the specific geological or glaciological question being asked of the geoscientific modeler.

As pointed out by the reviewer, it is up to the ice-sheet modeller to decide whether the error values are acceptable for their application. We emphasise that FastIsostasy is a computationally inexpensive regional model incorporating a treatment of sea-level and that our benchmarking against a fully 3D GIA model demonstrates that it will be useful for most ice-sheet modellers since it greatly reduces the error in computing vertical displacements compared to ELVA (FI1D) and ELRA (the latter comparison will be included in the revised version of the manuscript).

Again, there is no reference to the important time scale for instability: 50 – 500 years.

As stated above, the deglaciation includes a rapid retreat in West-Antarctica which is fully captured in the modelling, and that its great utility is established by the relatively small difference between the FastIsostasy and Seakon simulations, even for these such short time steps (around -14kyr).

Appendix A

See above comments.

Appendix B

If material in Appendix A has any logical rationale (and I doubt it), then it needs folding into the main text. Appendix B can be added as a Figure companion to Figure 8 in the main text. I recommend removing the Appendices altogether.

As answered above, the reviewer seems to have missed the influence of compressibility on the Maxwell time. The correction factor we introduce in Eq. A2 is justified by physical equations and is important to explore given that Eq. 9 relies on the assumption of compressibility.

In contrast, the correction factor introduced in Eq. A3, is of a mathematical nature, albeit one that relies on physical arguments. We believe that it is also an important parametrization that the reader should be aware of.

Finally, we believe that Appendix B has value since it presents the structure of the parametric heterogeneities of Test 3, two additional tests and a comparison of FI1D and SK1D. This additional information is not central to the paper but will be of interest for readers.

Suggestion for an additional test

To capture the physics of solid earth LV and stability of an ice sheet margin, I suggest the following test that samples shorter scales in both time and space.

Allow the x, y system at its various layers to have a set heterogeneity, as for example in one direction across the space. Then consider linear (in time) surface load as a half-torus (ring) that has negative changes in mass over 500 model years, testing the model prediction of vertical deflection at 50-year intervals at the surface across the entire space. This would excite both high and low values of kappa as the surface load would have both short and long spatial scales of loading.

As mentioned above, the glacial cycle presented in Test 4 includes all wavenumbers that are relevant to ice-sheet modelling. We therefore see this additional test as unnecessary.