We thank the reviewer for their positive comments in regard to the manuscript, and their constructive suggestions for improving the discussion of our methodology and results. We detail below our response to each of the reviewer's comments.

Summary:

Swierczek-Jereczek and colleagues present a regional Earth model that improves previous regional approaches by adding capabilities to account for the heterogeneous structure of the Earth as well as for barystatic and gravitational feedbacks on relative sea level. FastIsostacy is mainly build on the fast Fourier transform (FFT) formulation (Bueler et al., 2007) of the Lingle-Clark bed deformation model (Lingle and Clark, 1985), where two linear models for purely elastic and viscous displacement were superimposed. This results in a single time-dependent differential equation for the combined vertical displacement (Cathles 1975). Numerically, this equation is discretized in time using a finite difference scheme, while spatial derivatives are computed in Fourier space (Fourier spectral collocation method), allowing for using explicit integration methods instead of solving for large systems of linear equations.

This is correct, excepting that some spatial derivatives arising in Eq. 9 are evaluated by a finite difference method. The hybrid nature of our solving technique arises from this choice rather than from the time integration.

The proposed Earth model also applies a regional sea level model introduced by Coulon et al., (2021), which here additionally accounts for barystatic sea-level dependent ocean area (shoreline migration), which is required in the conversion of ice volume changes into sea-level changes. The authors show that on glacial time scales this correction can explain about 4% of the sea level change.

The presented Earth model is of intermediate complexity between the simplified models used in current ice-sheet simulations and the full spherical GIA models, nicely categorized in Table 1 by the authors. FastIsostacy (or LV-ELVA in their notation) has not been coupled to an ice sheet model (at least not shown here) but from an ice modeling perspective it potentially fills a gap. The authors raise the problem, that GIA models are rarely open source and computational expensive due to the numerical integration against the global load history. Parameter ensemble simulations, as often used to estimate uncertainties in sea-level projections, require computational costs of the GIA model comparably to the ice sheet model or lower. As standalone model, FastIsostacy makes use of Julia programing language allowing for GPU parallelization for the FFT-related operations and additional functionalities. The main speed-up of FastIsostacy compared to full spherical GIA models, however, is due to the Fourier spectral collocation method, as already used in ice sheet model coupling (e.g. PISM, https://research-software-directory.org/software/pism).

General assessment:

FastIsostacy combines the previous ELVA approach (Bueler et al., 2007) with a regional sea level approach (Coulon et al., 2021) and extends those by adding a correction for ocean area and for projection (distortion factor K) and by including hydrostatic force induced by elastic displacement. The authors suggest to account for ocean and sediment loads, which has not been treated in the previous ELVA, as this may influence the boundary conditions (load perturbations should vanish at the boundaries assuming spatial periodicity) and therefore the FFT solution, even for an extended computational domain. For ice load changes in the domain center with distance to the lateral boundaries this assumption is more likely fulfilled.

Most importantly, laterally-variable solid-Earth structures can be treated without introducing major errors compared to a fully 3D GIA model. As mentioned at l. 295 of the manuscript, accounting for changes in sediment and liquid water column is only performed within a masked region to fulfil the vanishing boundary conditions mentioned by the reviewer.

The authors claim to account for a higher vertical resolution in Earth structure using a “lumped” weighted average defining an effective viscosity, a concept commonly used in terms of 2D Shallow Approximations in ice sheet modeling. I am not sure if the channelized (lateral) viscous flow of mantle material (e.g. in a low viscosity channel) can be really represented this way, as the systematic underestimation of peripheral forebulges compared to the 3D GIA model suggests. Also self-gravitational effects are ignored in the approach. The viscosity (Maxwell time) tuning, which is motovated by representing compressibility and shear modulus variations, may compensate for the effects of reduced complexity. The comparison to the 3D GIA effects also shows the effect of the missing rotational sea level feedback in FastIsostacy, which can be of the order of 10s of meters for glacial cycles simulations. This can be significant in terms of marine ice sheet instability and the onset of deglaciation in coupled ice sheet – GIA models.

Nevertheless, the modeling work done by the authors is remarkable, and add another piece to bridging between ice sheet model and GIA model community. The performed standalone test simulations cover different aspects of the implementation (verification, 1D benchmark, 3D and glacial cycle application for Antarctica), which makes sense to me. The presented misfits are below 20%, which can still be of the order of 100m. The authors argue that this is comparable to parametric uncertainties, but those misfits can be amplified in coupled simulations, especially for a more realistic deglacial ice sheet grounding line retreat (with faster time scales).

We thank the reviewer for their positive comments which acknowledge that FastIsostasy represents a major step forward compared to current simplified GIA approaches that are standard in ice-sheet modelling, and that it provides a computationally highly efficient alternative to fully 3D GIA models. The limitations of FastIsostasy mentioned by the reviewer are systematically described in the manuscript.

We have coupled FastIsostasy to the Yelmo ice-sheet model but the evaluation of its performance requires a detailed benchmark comparison against a coupled ice-sheet–3D

GIA model. This effort will be the focus of future work. A detailed response to the reviewer's comment on the effect of lumping the depth dimension is given below.

This model approach may still fill a gap, but ice sheet – GIA coupling is just improving significantly with reduced computational costs and comparably small coupling time steps (e.g. up to 10yr with a GIA model based on spherical harmonics as in Albrecht et al., preprint), while providing a more comprehensive GIA response covering all implied GRD (gravitational, rotational, deformational) feedbacks, with a globally conserved water budget, considering different sea level contributions and their interaction (e.g. Greenland) and global relative sea level fingerprints (geoid), also accounting for horizontal displacements (for validation against GNSS data). Hence, sea level projections and uncertainties could be directly linked to potential impacts along the global coastlines. This is what decision makers want to know in the end, and maybe it is time for ice sheet modelers to do this step forward, to think their models more globally.

We fully agree with this point and believe that 3D GIA models and FastIsostasy have complementary goals. 3D GIA models are the appropriate tool for studying the global, space-time variation in sea level across glacial cycles and into the modern world. On the other hand, the development of FastIsostasy was motivated by the application to coupled ice-sheet/sea-level modelling since it avoids the prohibitive computational costs associated with 3D GIA models while dramatically improving accuracy relative to simplified GIA models currently in use. It can therefore be a very useful tool for ensemble simulations exploring model sensitivities. We will include a few sentences in the revised manuscript to highlight this complementarity.

Detailed comments:

l. 58 I think that SELEN (Spada & Melini, 2019) come with a BSD3 open source software license, which allows commercial usage. This is comparable to the permissive MIT license used for FastIsostacy, both without copyleft.

This is indeed the case, however SELEN is a 1D GIA model. Our statement refers to 3D models. We nonetheless agree that mentioning Spada & Melini (2019) here is a good point, since it is the first open-source effort in GIA modelling to our knowledge.

l. 162 What would be the induced effect of neglecting the distortion factor, just a rough estimate? In terms of estimating sea-level contributions, the same weighting terms should be used (Goelzer et al., 2020) in the regional sea-level model (Eq. 16). I encourage the authors to double-check for double-accounting?

The distortion of the projection is, in fact, accounted for in Goelzer et al. (2020) but only in the computation of the sea-level contribution. Our approach extends its meaning to the derivatives of the displacement field arising in Eq. 9. Neglecting the distortion factor has an only marginal effect for the simulations performed in the present work. However, we believe that the factor can play an important role in simulations of the past evolution of larger ice sheets, e.g. Laurentide + Greenland on one domain.

l. 251 Does u_i,j in Eq. 15 also depend on time t?

Yes! This dependence will be made explicit.

l. 252 To my knowledge this requirement in the current PISM implementation has been improved by using an extended computational domain for the Earth model, at least 4 times the size of the ice domain. The authors also mention an extended domain in l. 292, but without any details.

We will include this additional information in the revised manuscript.

l. 258 Maybe refer to Gregory et al., (2019) for definition of 'barystatic sea level' change. I think that sea level change induced by vertical land movement (V_pov) is treated separately.

We will incorporate this suggestion in the revised manuscript.

l. 262 For gradual sea level changes the slightly different approach by Adhikari et al., (2020) may be a valid alternative.

Thank you for pointing this out. We will include a reference to Adhikari et al. (2020) in the revised manuscript.

l. 263 the sea level depends on the ocean area as function of the sea level, which is implicit, but not necessarily nonlinear.

It becomes nonlinear when using a realistic topography, as shown in Figure 4. This will be made explicit in the revised text.

l. 289 Eq. 20 is an approximation of gravitationally consistent geoid changes, allowing to approximate near-field relative sea-level changes, but the complete sea-level equation (Farrell & Clark, 1976) is not solved here, as the deformation of the whole Earth surface is not considered.

This is correct. We will emphasize this point in the revised manuscript.

l. 295 This predefined mask should be shown somewhere. Does this also apply to the ocean mass changes in Eq. 1? Peripheral forebulge effects at the edge of the LGM ice sheet extent can provide important feedbacks and should be represented (see Albrecht et al., 2023). Does “extended domain” then mean, without the mask?

We agree and will show the mask in the supplementary material. This addition will make clear that the peripheral forebulge and relevant ocean mass changes are included in it. The mask is only applied to the pressure term of Eq. 9 and is introduced to prevent a nonzero load close to the boundary, which allows one to solve Eq. 9 with a Fourier collocation. Note that this issue is only relevant for Test 4, since the other tests present a load that is concentrated at the centre of the domain (⅓ of the total length).

The term “Extended domain” refers to a domain with a doubling of the dimensions, which naturally arises when computing a matrix convolution numerically. The mask does not apply to any of these computations and is only included on the right-hand side of Eq. 9.

l. 306 What are FFT plans?

FFT plans include all precomputable operations of an FFT and can therefore save a substantial amount of time. We will include this explanation in the revised manuscript.

Fig. 5: It would be helpful to indicate in the figure where the load is located (vertical dashed line, or illustrated at top of panel a) at 1000km. Similar in Fig. 6 for the margin (10°) or shape and for Fig. 7 for the anomaly extent.

We agree. These additions will appear in the revised version of the manuscript.

l. 440 Albrecht et al., (2023) highlight a possible forebulge feedback for ice sheet growth.

This is a relevant citation and we will include it in the revised version of the manuscript.

Fig. 7: It would be nice if the experiment names (150→50km, 150→250km, 21→20, 21→22) were put into a subtitle, legend or figure caption. Also the forebulge region could be highlighted (arrow?). I like the detailed error statistics.

This is a very good that will make the figure easier to read and interpret.

l. 462 Doesn’t ‘stiffer’ mean a larger Maxwell or relaxation time?

Stiff here refers to the mathematical vocabulary of ODEs. We will resolve this confusion in the revised version of the manuscript.

l. 466 How is the forcing applied, the model uses the ICE6G ice history ΔH_ice in Antarctica and the barystatic sea level change from the northern hemisphere (or outside of the computational domain) from ICE6G as s(t)?

This point was not sufficiently described in the manuscript and Section 2.4 will be extended to add the required discussion. We note that the barystatic sea level used here is obtained by regionally averaging the sea-surface height obtained in Seakon, which differs significantly from the global mean. We believe that using external barystatic sea level is the best option for simulations of the past, since real-world data is available. Updating the barystatic sea level as described in Section 2.4 will only be used for projections. This point will be highlighted in the revised version of the manuscript.

l. 472 This overestimation suggest that the weighted mean (Eq. 4) may not be valid for n layers.

This overestimation can be understood to be a consequence of the lumping process, which requires setting a characteristic wavelength, as mentioned on l. 193. The wavelength chosen here relies on the characteristic extent of the load in ice-sheet modelling, which is sensitive to structure in the top-most layers of the sub-lithospheric mantle - at least for load sized considered in the manuscript. We will include an explanation of this point in the revised version of the manuscript.

Furthermore, we want to emphasise that the bulk of the error budget in the FastIsostasy simulations arises from lumping the third dimension of a Cartesian domain (Fig. 3). As shown in Fig. A2, relative errors that are comparable to those displayed in Fig. 8 arise even when only including 3 layers (a lithosphere, a channel and a viscous half-space - similar to Lingle & Clark (1985) and Bueler et al. (2007)).

Fig. 8: panel labels may be twisted. Panel e) and f) clearly show the order of magnitude of the rotational sea level feedback of the order of 10s of meters (as discussed in l.506ff).

The inverted labels will be corrected.

l. 500 80m maximum error sounds quite a lot, even though it is below the 20% acceptance limit.

This is correct. The main achievement here is the level of error reduction (at almost no computational cost) compared to a configuration with homogenous solid-Earth parameters.

l. 501 mentions peak maximal error of FI3D at -14kyr, while the figure subtitle and panel b) says -16kyr.

Yes, this will be corrected in the revised manuscript.

l. 515 The authors may confuse model time steps with coupling time steps or coupling intervals. The more comprehensive 3D GIA models likely use time steps lower than 10 years as well.

The studies cited in l. 515 uses model time steps of more than 100 years.

l. 518 The factor 70000 for Seakon vs. FastIsostacy might be a bit exaggerated, as it is only valid if you compare CPUh with GPUh, while GPU also use internal parallelization. If only wall clock time or energy consumption was compared (GPU: 175W, 128 CPU core node: 280W), it would be still a factor of around 1000. So more importantly here is the fact, that computational costs for the GIA model should be comparable to the ice sheet model (or lower). And this is already the case (see Albrecht et al., 2023, Table 1, which can be associated with a factor of still around 100 for PISM-VILMA compared to FI3D).

We partially agree with this comment and will simply mention an order of magnitude for the difference in run time in the revised manuscript. We nonetheless draw attention to the fact that the GPU used in the study has a significantly lower power consumption (80 W) and the total energy required for the computation is 80W * 10 mins vs. 280W * 4.5 days, which is substantial, to say the least. Most importantly, the acquisition price of the GPU (<1000 euro) is much lower than that of a 128 CPU cluster. We include these additional remarks in the revised manuscript.

Fig. A3 colorbar is missing

Yes, this will be corrected.

The affiliation of Konstantin Latychev states 'Seakon', or should it rather be the Physics Department of the University of Toronto?

The affiliation of Konstantin Latychev changed recently and is correct as it is in the current version of the manuscript.

Typos are covered by the other reviewers already.

References not mentioned in manuscript:

Adhikari, S., Ivins, E. R., Larour, E., Caron, L., and Seroussi, H.: A kinematic formalism for tracking ice–ocean mass exchange on the Earth's surface and estimating sea-level change, The Cryosphere, 14, 2819–2833, https://doi.org/10.5194/tc-14-2819-2020, 2020.

Albrecht, T., Bagge, M., and Klemann, V.: Feedback mechanisms controlling Antarctic glacial cycle dynamics simulated with a coupled ice sheet–solid Earth model, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2023-2990, 2023.

Gregory, J. M., Griffies, S. M., Hughes, C. W., Lowe, J. A., Church, J. A., Fukimori, I., Gomez, N., Kopp, R. E., Landerer, F., Cozannet, 610 G. L., Ponte, R. M., Stammer, D., Tamisiea, M. E., and van de Wal, R. S. W.: Concepts and Terminology for Sea Level: Mean, Variability and Change, Both Local and Global, Surveys in Geophysics, 40, 1251–1289, https://doi.org/10.1007/s10712-019-09525-z, 2019.

Spada, G. and Melini, D.: SELEN4 (SELEN version 4.0): a Fortran program for solving the gravitationally and topographically self-consistent sea-level equation in glacial isostatic adjustment modeling, Geosci. Model Dev., 12, 5055–5075, https://doi.org/10.5194/gmd-12-5055-2019, 2019.

We thank the reviewer for suggesting these additional references and we will include them in the revised version of the manuscript. Spada and Melini (2019) is actually already included, but it will be referred to in the discussion about open-source GIA code.