## Reply to Review of egusphere-2024-3200 from Volker Klemann

We thank Dr. Volker Klemann for his careful review. In the following, we respond to the comments in a point-to-point manner and the original comments from the reviewer are italicized and in blue fonts.

*The authors present the publication and benchmark of the open source FE software package CitcomSVE 3.0, which allows to solve the GIA problem for a viscolestic continuum with lateral variations in material poperties considering elastic compressibility and the usual requirements for a GIA solver which are rotational deformations due to polar wander, geocenter motion and the sea level equation.*

*They benchmarked the code against a spectral 1D code following a similar benchmark of the incompressible precursor. 2.1.*

*The method to solve the equations for a compressible continuum with CitcomSVE2.1 was already presented by A et al. (2013) but without the SLE solver of the incompressible version and so lacking a comparable benchmark for GIA problems. Due to lack of suitable 3D benchmarks the authors were forced to test their model against the established spectral normal mode theory for 1D problems. This is in agreement with the testing of further 3D codes. To my knowledge only Martinec 2000 presented a benchmark against an analytical not spherical symmetric solution.*

*In summary, the presented method sounds reasonable and the results show a rather good agreement with the provided 1D solutions. Nevertheless I have a small number of suggestions which might improve the discussion and the reliability of the code:*

*1. discussion of spectral loads at least up to d/o 128,*

*2. transfer of the indepth discusssion of the applied new SLE solver into the supplement,*

*3. discussion also of the geoid displacement for the GIA example.*

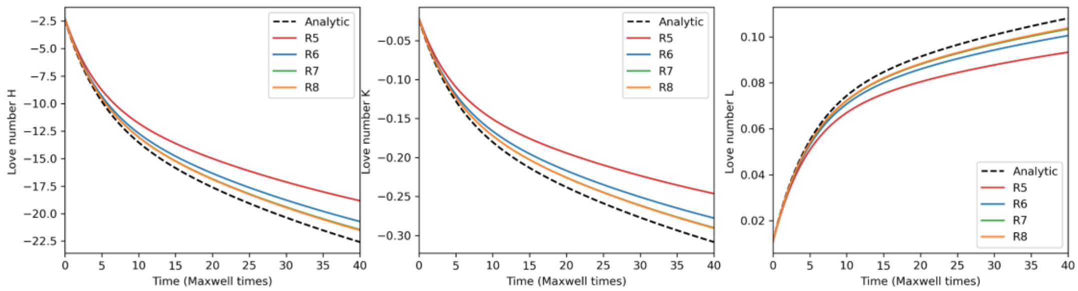
*Otherwise this paper is set up clearly and I suggest its consideration for GMD. Volker Klemann*

**Response**: We appreciate the three suggestions listed above and revised the manuscript accordingly.

First, we added a new case of spectral load of degree 64 instead of 128 for the following reasons. To calculate Love numbers for a spectral load of degree *N*, it is necessary to calculate gravitational potential up to at least degree *N*. As CitcomSVE-3.0 calculates gravitational potential in spherical harmonic domains and displacements in finite-element domains and needs transformations between those two, it becomes significantly more expensive (both in efficiency and memory requirements) with increasing maximum spherical harmonic degrees in potential field calculation. Although the maximum harmonic degree of potential calculation affects the calculated potential and geoid, it has a much smaller effect on the surface displacement and relative sea level, as shown in Table 3. In practice, having a maximum degree larger than 32 or 64 is usually unnecessary for displacement and relative sea level calculations. So, for spectral load cases, we chose to add a case with loading at degree 64.

For spectral load of degree 64, four resolutions are tested: 12x80x128x128 (R5), 12x80x192x192 (R6), 12x80x256x256 (R7), 12x96x256x256 (R8).

The following figure shows the Love numbers from CitcomSVE-3.0 and from the semi-analytical solution (also in figure 1).




The results of this new case are also added into Table 2 in the revised manuscript.

Second, we simplify the discussion of the new SLE solver in the main text and put extra discussion into the supplement.

Third, we added more results and discussion for geoid calculations. For example, in the revised Table 3, we included the misfits for geoid rate at different stages (and we also added misfits for RSL). And we also added the geoid rates and its misfit in a figure along with displacement rates.

We also included benchmark results for a Heaviside tidal load case with degree 2 and order 0 in Table 2.

## Details

*L3 Although discussed in the paper the applicability to solve the GIA problem is not stated in the title.*

We now mention GIA in the title: CitcomSVE-3.0: A Three-dimensional Finite Element Software Package for Modeling Load-induced Deformation and Glacial Isostatic Adjustment for an Earth with Viscoelastic and Compressible Mantle

*L34 not clear if also compressibility can vary laterally.*

We clarified that the compressibility (actually both $\lambda, \mu$) can vary laterally in the abstract.

*L38 Is the SLE solver is part of published software?*

Yes. It is included in CitcomSVE-3.0.

*L40 Only at the end I found an explanation of what a second-order accuracy means. But, I am not convinced if this criterion holds heres see there.*

"Order of accuracy" is commonly used in numerical analysis and computational science to quantify the rate of convergence of a numerical approximation of a differential equation to the exact solution. Fig. 2 shows the error trends to horizontal resolution for Love numbers, and the slope is about 2 on the log-log scale, indicating errors are roughly proportional to the square of the grid sizes (i.e., second-order accuracy).

*L42 An assessment of the computation time is given. May be you can add that it is three times slower than the incompressible version*

That is mentioned in the main text, and in this revision we also mentioned it in the conclusion section. The increased computation time is mainly caused by calculations of gravitational potential with the spectral method currently used in CitcomSVE.

*L70 You should add here Tanaka et al. (2011, doi:10.1111/j.1365-246X.2010.04854.x), where like in A et al. (2013), compressibility is considered. Here also see the discussion of L200ff. you should also discuss there, which codes are compressible and which are incompressible.*

We added Tanaka et al. 2011 here and discussed which codes are incompressible.

*L115 Your code works in the Lagrangian domain. Then I would state, that the density increment is considered as being in the Eulerian domain, first as its advection in Eq. 2 is of second order, and second that in this way the Poisson equation (Eq. 3) holds. But you could also state that in case of small perturations and the resulting linearisation the Eulerian and Lagrangian density increment do not differ.*

It is true that our code works in the Lagrangian domain, and the density increment in Eq.2 and Eq.3 is Eulerian density increment; there will be no problems as long as we correctly calculate density in terms of Eulerian increment when solving the equations. We have acknowledged that $\rho_1^E$ used in those equations is Eulerian density perturbation, and the definition of $\rho_1^E$ (Eq. 1) includes effects of both volume variation and advection of the initial density field, as it should for Eulerian increment. The Eulerian and Lagrangian density increments differ by an advection term, which is $u_i\rho_{,i}$, a first-order term, not a second-order term. Detailed description of $\rho_1^E$ can be found in A et al., 2013.

*L126 The boundary condition at the CMB (Eq. 5) is important (and also goes back to Wu and Peltier 1982). L 127 According to Zhong et al. 2003 the equation holds for an incompressilbe core.*

We make it clear that, in our case, the core is incompressible. Also, the core is not part of the computational domain in CitcomSVE, and the core's influence is introduced through the boundary condition. Interestingly, the analytical solutions by John Wahr that includes the core as an compressible medium and that we used here are in excellent agreement with CitcomSVE, suggesting that the core's compressibility may not play an important role.

*L 132 Here and in the following I would prefer 'continuum' instead of 'medium', due the continuum mechanical formulation of the problem.*

We adopted the suggestion.

*L149 Small suggestion: 'Maxwell rheology (6)' should be sufficient to write.*

We adopted the suggestion.

*L156 For the time integration of the field equations you apply an explicit time differencing scheme. Is this correct? I would then specify this.*

The discretization in time space is required for the Maxwell rheological equation (Eq. 6 in manuscript). As we mentioned, it is discretized in time by integrating it from $t - \Delta t$ to $t$ with a second-order trapezoid rule. The resulting formulation is essentially an implicit time differencing scheme, more specifically, a Crank-Nicolson scheme (see also Martinec 2000, Eq. 13).

*L176ff Can you state that this coincides with Tanaka et al. (2011).*

Although Tanaka et al. (2011) also used weak formulation, we are not able to find a clear similarity between the equations presented there and ours, due to significant differences in numerical methods between those two studies. However, we mentioned that Tanaka et al., 2011 also used an FE method in the introduction. Of course, our original FE formulation was in Zhong et al., (2003).

*L200ff You should place this discussion to L70ff.*

We moved this discussion into the introduction section (line 88-96).

*L255ff This is a recap of Kendall et al. 2005. May be you can reduce this section and refer to them. Also in Spada and Melini (2019, doi:10.5194/gmd-12-5055-2019) a nice overview is given.*

*One further aspect you do not discuss is, how you treat the inner iteration between subsequent integration steps. Is this omitted here similar to Hagedoorn et al. (2007,* *doi:10.1007/s00024-007-0186-7), where also the field equations are solved explicitly in the time domain?*

We think this summary of the approach use in Kendall et al. 2005 is necessary to make our description of SLE completed, so we prefer to keep it in the main text.

The inner iteration between subsequent integration steps is similar to that of Kendall et al., 2005 and Zhong et al., 2022, where the iteration is considered converged when the changes of potential and displacement are smaller than a certain threshold. We added the description of inner iteration in lines 282-286.

*L293 The main reason to run the outer iterations is to approximate a consistent initial topography. I did not find this explicitly stated.*

This was stated in line 281: "the unknown initial topography $T_0$ needs to be determined iteratively to keep the modeled present-day topography consistent with the observed present-day topography."

*L298ff This first iteration is an interesting suggestions.*

Thanks.

*L304 The efficiency is not shown in the next section but later in 3.2.1. Nevertheless as stated there, I would shift this discussion to the supplement as it interrupts the benchmark discussion in this section.*

We follow the suggestion to put section 3.2.1 into supplementary materials.

*L313 Why not call this subsection 'Spectral surface load with step-function in time' ?*

We adopted the suggested section name.

*L317 You can also here specify that you vary the load between (1, 0) and (16, 8).*

We now describe that the tested cases range from degree 1 to degree 64 in line 322.

*L321 Why do you considere only the cosine term and not the complete representation of the spectral load distribution?*

A single real harmonic load can be either a cosine or sin term, which are essentially identical except with a phase difference. Hence, there is little difference between using either one. For simplicity, we always use the cosine term.

*Table 1 The reader would help if you list here the reference Maxwell time used for normalisation, also in view of Fig. 1 and the following discussion. Furthermore I wonder why the viscosity in the upper mantle is higher than in the lower mantle, this does not look like vm5a.*

We list reference Maxwell time in Table 1 as suggested. The viscosity in the upper mantle is $4.853 \times 10^{20}$ Pas, not $4.853 \times 10^{21}$ Pas as it was listed in the manuscript. We corrected the typo.

*L335ff Can you state something regarding the radial discretisation? How many elements are considered in the lithosphere, upper and lower mantle, respectively.*

The radial discretization was mentioned in the next paragraph. We now moved the description into this paragraph as it fits this paragraph better (around line 360).

*With respect to the considered spectral representations did you check if the derived load love numbers deviate for different orders, I think you have checked this but it would be interesting how much they vary also in view of the spectral solutions. The reader might also wonder why the lln of (2, 1) differ so much from (2, 0). Obviously it is due to the polar motion term. You should state this here.*

We make it clear that (2,1) case considers the polar wander effects at line 345. For loads of same degree but different orders, the Love numbers from numerical solutions could be slightly different, although Love numbers from semi-analytical solutions would be the same. However, it is beyond our scope to investigate this in this study.

*Figure 1. Here you chose a different nomenclature to specify the degree/order forcing. In the next you describe the (l, m) nomenclature. Easiest would be to keept it in Figure 1 but change it in Table 2 and throughout the text.*

We modified fig.1 to use l1m0 nomenclature.

*Furthermore there is a big step from degree 16 presented here to degree 128 usually considered in GIA (see Spada et al., 2011 or Tanaka et al., 2011). So it would be interesting to show the deviations also at such high degrees (see main points).*

We addressed this comment in our response to the main points above.

*Figure 2. Form the figure and the caption it is not visible where R5 is applied. In the lower or in the upper triangle, although it should be the lower one of course.*

We made it clear in the caption that “R5 has smaller relative errors compared to R4”.

*L422 '[...] and (12) with the floating ice criterion' ?*

We added “floating ice criterion” here.

*L424 'multiple' sounds like at least 10. Also Kendall et al or other authors usually only consider 3 to 4 iterations.*

We made it clear here that 3-4 iterations are usually used.

*L428 as stated at L304 I would shift the whole discussion of 3.2.1 to the supplement. This as you also only refer to figures there. May be you can summarise the main output there. Here it disrupts the benchmark section (see also main points).*

We moved section 3.2.1 into supplementary materials, see line 452-456.

*L433 here and throughout the text I would replace 'kybp' by 'ka BP' as used in literature, see also Figure 4 vs. Figure 6.*

We modified the text and figures according to this suggestion.

*L531 If you shifted 3.2.1 you do not have to repeat the setup of the problem here, as this as given already before.*

We restructured this section by moving most of this paragraph into the paragraph above it (line 440-451), since it is more of a background than a direct discussion of model setup.

*L555 Further down you apply a nearest neighbor algorithm for the interpolation of the displacement field. Did you apply the same algorithm here or did you use a mass conserving algorithm?*

The interpolation here (that is, reading ice load from a regular grid and interpolating it into CitcomSVE grid) is done by (bi-)linear interpolation from regular grids to arbitrary points (i.e., irregular grids in CitcomSVE-3.0).

*Table 3. It would be great to see here also the error statistics of the RSL for the presented epochs further down.*

We added the error statistics for RSL in Table 3, see the modified Table 3 in the revised paper.

*L582 May be you can state at the beginning that in this subsection you present surface displacement rates, and RSL. What I miss is the gravity change signal at pt, as a further prominent observable (see also main points).*

We added figures for geoid rate at present day in figure 3 and the error statistics for geoid rate in Table 3, see the table and figure in the revised manuscript. And see also line 545-550 and 558-561.

*L627 Here you mention the gravity change and change rate of geoid height, but you do not show results, also what about RSL for this specific case?*

We added the results for geoid rate and error in RSL for those cases, as mentioned in our response to the main points above.

*L640ff Is the required higher resolution for (2, 1) only an observation, or can you give an explanation for this deviating behaviour? The relative difference between -120 and 0 ky is much larger for this term in comparison to the other coefficients. May be Cambiotti et al. (2010, 0.1111/j.1365-246X.2010.04791.x) helps.*

The required higher resolution for (2,1) is more of an observation and reflects the difficulty in accurately modeling polar wander, which is also discussed in A et al., 2013 and Zhong et al., 2022. The relatively large difference between -120 and 0 kr means a relatively large total displacement on degree 2 and order 1 after a full glacial cycle. Cambiotti et al., 2010 (Fig. 5-7) is informative on this topic, showing the effect of the non-hydrostatic correction ($\beta$) on total polar motion after a full glacial cycle. The detailed discussion of the nature of polar wander in ice age is beyond the scope of this study. However, it is an interesting topic that deserves future investigations.

*Figure 6, You should discuss the offset between the FE and S solutions at the far field sites. Is this due to coastal levering especially at Geylang or mismatch in L2m1?*

Figure 6 shows that the offset between FE and S solutions of RSL reduces with increased numerical resolution for near field sites, but not for far field sites. This is reasonable because the RSL at near field sites is controlled by load-induced crustal motion and potential change, whose accuracy is highly sensitive to numerical resolution, whereas RSL at far field sites is more controlled by total ice/water volume and ocean areas, which are less sensitive to numerical resolution. The offset in RSL between FE and S solutions at far field sites is not likely caused by l2m1, since the accuracy of l2m1 increases with numerical resolution. The offset is more likely related to factors other than numerical calculations, such as the interpolation of ocean function from the regular grid to the CitcomSVE grid or the interpolation of results on the CitcomSVE grid to RSL sites. We added some descriptions for the offset at far field sites in line 604-607.

*L691 I would not call the presented comparison 'extensive', as you discuss rather low degree spectral loads and only one GIA realisation.*

Now, after adding a spectral load of degree 64 and a benchmark for tidal load, and considering we have at least 4 numerical calculations with different resolutions for each case (seven calculations for the GIA case where different scenarios were considered), we think it is fair to say the benchmark is extensive.

*L694ff I do not follow this calculus. Considering the errors in Table 3. There, from R1 to R2 the error reduces by a factor of 2, whereas you increased the number of elements by a factor of 2.7.*

The error is proportional to the grid size dx or dy to some power, not to the total number of elements. With increased number of elements by a factor of 2.7 in horizontal directions, the horizontal grid spacing is only reduced by about sqrt of 2.7 or a factor of 1.6. The second order accuracy would lead to error reduction of a factor 2.7, which is slightly larger than the actual error reduction of a factor of 2. That we did not get the full error reduction with increased resolution here could be caused by other factors in GIA calculations, for example, we did not solve the gravitational potential at the same resolution as FE grids. Also the GIA case with SLE makes determining level of accuracy more difficult since other factors affect the comparison between CitomSVE-3.0 and semi-analytical solutions, for example, the different representation of ice load and ocean functions between CitcomSVE-3.0 (on irregular grids) and the semi-analytical code (on regular grids and spherical harmonic domains).

We clarified that the accuracy discussed here is mainly from the single harmonic benchmark (spectral loads); see line 632.

*L699ff You can also state here that the integration time for a compressible continuum is three times larger than for the incompressible solution.*

We add one sentence in the last paragraph to address this comment.