Volker Klemann

Review of https://doi.org/10.5194/gmd-2021-126

The manuscript is very well written and I rate it as an important contribution to GIA and coupled ice sheet–solid earth modelling. Nevertheless I have a number of concerns regarding the setup of the study and the findings phrased between.

In principle there are two results presented. First the authors show that the impact of loading events fades away with age relative to present day. This aspect is well known as fading memory for difusion processes like the solid earth deformability considered as a viscoelastic gravitating continuum. Accordingly, the loading details in the past are of less impact on the present-day deformation state, here discussed as topography change. In consequence, the authors consider this fact for the design of their convolution algorithm which they apply to integrate the viscoelastic field equations.

One drawback of the normal-mode approach, which is usually applied in GIA studies, is that for each additional time step computed with a further load change, the summation over the whole loading history has to be repeated. This results in a quadratic increase of computing time and storage of previous loading steps, making it rather unatractive for dynamic coupling. To overcome this problem, the authors suggest a scheme in which the number of considered loading steps is tapered, applying a skipping scheme where loading steps between are not considered. With the procedure presented here, they can reduce the integration time markedly, almost reaching a linear increase of integration time. But they only implicitly mention, that this problem is restricted to models using the normal mode approach, whereas in codes solving the field equations in the time domain, this problem does not appear. This aspect should be discussed already at the beginning of this study in order to show, where the method is applicable. In models capable of considering non-linear rheologies or lateral variations in the earth structure, the viscoelastic field equations are solved in the time domain and the problem of quadratic increase in integration time does not appear.

The second result, addresses the problem of the coupling interval between ice-sheet and solid-earth models. There, they find suitable values of 200 y for a standard global viscoelastic earth structure, and about 1 y for a structure representing the low viscous region of West Antarctica. They show, that coarser resolutions by a factor of 5 or beyond result in markable deviations in the resulting dynamics of the ice sheets. What they did not discuss are shorter coupling intervals. This would be a nice add on, as they start from the interval already suggested by Gomez et al., and so only confirm what those authors already found.

In a first experiment they consider an idealised rotationally symmetric problem in order to discuss the problem of sampling a predefined glaciation history. The motivation is not clear to me. It shows that the considered integration scheme results in a distinct delay of the forcing with increasing sampling interval. From the discussed deviations, the author could already consider a different integration strategy where load distributions between are averaged. For instance in case of $dt = 20$ ky for a predefined coupling interval of 0.2 ky, the authors could calcuate the response at time t to the load interval t_j from the two load heights

$$H(t_{j-1} = t_j - dt) \;=\; H(t_j) \;=\; 1/N \sum_{i=i(j)-N}^{i(j)} H_i$$

where $i(j)$ is the load index of the j-th considered loading step for the integration up to coupling time step t and $N = dt/dt_c$. This algorithm should be applied of course only to time steppings larger than the coupling interval dt_c. With such a method I think, the delay could be reduced markedly and the information loss due to skipped load distribtutions can be avoided.

In summary, I would suggest 'minor revision', mainly with regard to the setup of the study in the introduction the authors should make clear from the beginning that their method is only applicable to 'standard' 1D GIA modelling.

Regarding additional modelling I would rate my points to be considered as suggestions, which from my point of view would improve the proposed algorithm markedly.

Some further remarks in the order of the text

1. I suggest to replace time window by something like adapted time stepping algorithm. Time window somehow implies that you shift the integration domain.
2. l. 8 ff: $\sim O 10^{0-6}$, I would write $O\,10^{0-6}$, the tilde makes no sense here.
3. l. 12ff: Callling this classic would only be understood by GIA experts. Instead you should be more specific here, that in classical GIA the problem is solved in the Laplace domain considering a normal mode approach.
4. l. 26: 'improve' → 'reduce'. More important here is that the cumulative integration time of a coupled model becomes almost linear.
5. l. 40: 'slower' → 'retarded'.
6. l. 40ff: You discuss elastic and viscous effects, but what is about the shear relaxation process between, where viscoelasticity takes place.
7. l. 43ff: Write simply 'lithosphere and mantle' as the elastic lithosphere cannot have a rheological structure.
8. l. 52: Remove 'in' in front of 'influencing'
9. l. 83: Write 'annual to decadal scale resolutions'
10. l. 94ff: As stated in the abstract, also here it becomes not clear that the quadratic increase is a consequence of the applied convolution, to solve the linear viscoelastic problem. So, I suggest the authors to clearify this.
11. l. 105ff: Skip 'what they called'
12. l. 109ff: Skip 'classic' as this algorithm is only 10 yr old, and so, not classic. May be, use standard instead.

13. l. 137ff: Remove the '(18)' of this equation, and discuss that the term in braces implies a summation over the loading steps from 0 to j. Then, the quadratic nature of the integration time of one coupled cylce becomes more clear. ω is not explained.

14. l. 139: change to 'ΔS_{j-1}'.

15. l. 12ff: 'where the change in topography is defined [...]'

16. l. 152ff: Again, remove 'classic'. Later you also phrase it 'standard' which is somehow better.

17. l. 162ff: How do you motivate the choice of just four time intervals?

18. l. 172ff: Here it is better described than in the caption of Fig. 1.

19. l. 180ff: Can you give a formula how many ice load files have to be considered by this scheme? This might be also interesting for the cumulative integration time. My first guess was that a quadratic nature nature remains, but in the later figures it becomes clear that the integration time is dominated by the shortest time stepping of the last interval which is of constant length and, so, becomes almost linear.

20. l. 182: Rephrase 'amount of surface loading history', as it is not clear what you mean.

21. l. 191ff: I doubt that you you adopt your scheme to a 3D earth model. Most 3D codes are solving the equations in the time domain. Accordingly, I rate the statement 'We adopt 1-D Earth models [...]' as rather misleading, as your method is designed for 1-D Earth models.

22. l. 193 ...: The unit should be abbreviated as 'Pa s'.

23. l. 196ff: Regarding the Antarctic experiment in Sec. 3.3, which lithosphere thickness and upper mantle viscosity do you consider?

24. l. 207ff: Can you specify the cross section, I guess it is a lying parabola, and the relation between thickness and radius? Only during the text it becomes clear that the radius is varying with thickness, and not everybody has the book you refer to at hand.

25. l. 226ff: The vaning experiment again starts from a hydrostatic equilibrium state, and the load is considered to be negative?

26. l. 232ff: From the model setup, I would rate 20 ky as rather unrealistic, as it expresses only a delayed heaviside forcing. More interesting in view of the later discussions would be to discuss changes between 0.2 and 1 ky as such variations you disuss with respect to the coupling interval. If the changes are much smaller here, you can directly show that the deviations in the coupled runs are due to the ice sheet interaction.

27. Sec. 3.1: The experiments nicely show the fading memory effect with regard to the delayed response. But for the further discussions I would rate them as less helpful; also see my general statement above. I really suggest, to shorten this section markedly.

    The outcome of this section I would comment as: Interesting view, that a delay in loading is of less impact on the displacement when ongoing in time. Spada and Stocchi I think also discussed this. To my understanding this is a direct consequence of the linear viscoelastic relaxation process you consider.

    In Figs. 2b-2 and 2b-2 I would plot instead the topography change, as the topography is otherwise dominating and the dela yed response becomes much clearer.

28. l. 250ff: I would address this due to the longer wavelength of the water loading, at which the relaxation process is slower.

29. l. 261ff: Again I would adress this as a consequence of your considered integration scheme.

30. l. 272ff: Why not specifying this at the beginning describing the model setup. I suspected this during reading of this discussion, but never could be sure. At the beginning you could write how r_load and V_load are related to h_load which you change linearly.

31. l. 284ff: This is an important aspect which you do not further consider in Sec. 3.3.

32. l. 287ff: I would skip this paragraph, as it is clear from what you stated above.

    In total, I suggest to shorten this subsection, and concentrate on a more realistic time stepping change of 5 ky and smaller, as the given ones are obviously much too coarse.

33. l. 315ff: Not sure, if this is also the case for any dynamic process. Comparing dt 0.1 0.2 and 1 ky, 0.2 ky results in slightly larger volume, whereas 1 ky results in smaller volume.

34. l. 323ff: 'unstably' I would rephrase as specific dynamics of the coupling is not resolved.

35. l. 323ff: Is this not a repitition of Gomez et al. 2013?

36. l. 330ff: This is not clear to me. Does this mean, that in the ice model an iterative time integration scheme is implemented?

37. l. 335ff: So, 45 hr @ 0.2 ky and 98 hr @ 0.1 ky. From here it is not clear if these integration times only represent the sea-level calculation or also that of the ice sheet. Furthermore in Fig. 3a, you discuss the sensitivity on the coupling step.

38. l. 342ff: Well, this plot is quite obvious, more interesting would be also here to plot the cumulative integration time, as you do in Fig. 5.

39. l. 352ff: Change 'causes less [...]' → 'is inadeqate to use as a coupling interval'.

40. l. 355f: Remove 'In the same way [...] infeasable' as main aspect remains that such a scheme is only applicable in diffusion like processes like linear viscoelasticity in case of a normal mode approach.

41. l. 416: Set 'and' between in roman.

42. l. 419ff: But still, this has to be done a priori. The question is, can you assess what time stepping is necessary depending on the viscosity structure and the ice load variability?

43. l. 425: Change to 'because the time stepping of the time window profile coincides with the 0.2 [...] standard or benchmark simulation'

44. l. 427ff: Change to 'starts to increase for all'

45. below l. 435: Considering the deviation at 5ka, in the adjustment of the time stepping also ice thickness changes should be considered. It seems the larger deviations there, come from the ITW_2 reaching the ice retreat around 130 ka.

46. l. 441: Again 'simulation' is missleading here, I think you mean the integration time of the sea level part of the coupled simulation.

47. l. 443: Change 'that' → 'than'

48. l. 437–448: Based on these results, I would still keep at least 5 ky, as nsteps is only slightly reduced, especially visible in Fig. 5c. Figure 5d has the problem, that the above mentioned fluctuation of 10-17% shows the results being not representative.

    Furthermore, it would be interesting, if the deviation at 5 ka is due to a still too coarse timestep, and would reduce if you would choose 1 or 2 ky instead.

49. l. 450–457: I would expect that the integration of one ice-modelling step is constant. Then it would be easy to derive a simple equation to calculate the cumulative integration time of your sea-level model.

50. l. 465ff: As stated above, this statement comes rather late and should be clearified also in the caption of Figure 5.

51. l. 467: Change to 'sea level simulation based on a normal mode approach'. [Applying a time-domain code, such an algorithm is not necessary.]

52. l. 470: Change to 'rapid growth of integration time'.

53. l. 475ff: You should repeat here that you consider a low viscous 1D earth structure which you discussed in Sec. 2.

54. l. 498: Add 'along the S-N profile shown in Fig. 6d. [You should also mention the relation to Fig. 6 in Fig. 8.]'

55. l. 500 vs. l. 511f: The two sentences are not consistently phrased, as you cannot state you foundan optimal internal time step by choosing dt=5 yr as being appropriate.

56. l. 521ff: You consider an acceptable rms of topography change, how do you choose such a value, and do you consider here a global rms or a regional one? I have no idea what such an rms would mean in topography change at the grounding line you show in Fig. 8.

57. l. 532: I won't call this 'ideal' but 'appropriate' in reducing the integration time by a factor of two if compared to the standard run.

58. l. 534ff: Again the last paragraph discusses a different aspect than the former. Here again the coupling step between ice sheet and sea level model is discussed. Considering the rather short response times of the considered earth structure, I wont rate $dt = 5$ y to be relatively fine. Nevertheless, the result that one need 1 y time resolution in coupling is a rather important finding, although the fact that this was predefined as the shortest time step applied.

59. l. 565: What do you mean by 'viscous signal'?

60. l. 566ff: I would consider this as a common fact in GIA modelling since Peltier (1974).

61. l. 572ff: Again, I won't call this an unstable fluctuation.

62. l. 572ff: In short I would summarise this statement as for a 1D standard earth structure usually applied in global GIA studies.

63. l. 580: 'suitable time window parameters' I would rate as the correct phrase describing what you have achieved. I miss a more rigorous calculus in which, you can estimate how much you can increase the integration time step based on a given ice sheet variability.

64. l. 614ff: The larger timestepping in de Boer is likely due to the different dynamic behaviour of their applied ice sheet/shelf model. One aspekt also is the coarser spectral resolution of only $j_{rmmax} = 128$ those authors applied. From my point of view, the proper time stepping has to be based on the the individual model setup. So, I would suggest, to write, that de Boer found in their model setup, 1 ky to be sufficient whereas in the present coupling with PSU a 0.2 ky coupling model is more appropriate. Konrad et al. (2015, EPSL) applied a coupling step of 0.05 ky considering an asthenosphere viscosity of 10^{19} Pa s. So, I would rate the coupling interval being a combination of spatial resolution, relaxiation behaviour and induced loading changes.

65. Section 4: In the summary, the findings are presented much better than throughout the text, where you use phrases like 'optimal' or 'ideal', whereas introducing a set or providing applicable time window parameterisations are more appropriate.

    Figure 5b, shows that your approach can be improved, as the L_ITW1 dt of 5 and 10 kyr does not match all variability, seen in the increased RSME at ~ 5 ky. As a future aspect, I would suggest to improve this strategy in order to allow the time stepping to depend on the variability of subsequent loading intervals. May be, the averaging over those loading intervals instead of skipping might allow to keep such information.

66. l. 675: I am puzzled a bit, as multiplied by 0 would mean they are considered as a zero load. I think you mean that those indices are not considered.
67. Fig. 3d: Here and in Fig.5 I would write only N_j at the y-axis.
68. l. 739: Add '(see Fig. 8)'.