Review of Adhikari et al. 2025: Enabling ice sheet models to capture solid Earth feedback with ease and accuracy

The authors propose in this manuscript a new method to consider the viscoelastic response of a viscoelastic earth to surface loading. The method is designed to consider the solid-earth feedback mechanism in coupled dynamic ice sheet models. In order to reach an efficient set up, they made some simplifications with respect to the usual solution of the glacial isostatic adjustment problem, here called GRD.

The main advantage I see in the consideration of pre-calculated Green's functions. They suggest an efficient algorithm for the convolution, although I would argue that such an algorithm should be state of the art. Furthermore, the main aspects of the algorithm only appear in the Appendices.
Only after reading it becomes clear that this algorithm is not sufficient to analyse the SE-IS feedbacks for longer time scales, when efficiency breaks down and also the accuracy is reduced. Furthermore, the sea level equation is not considered regarding mass conservation, ocean loading and also the rotational feedback are not considered in this approximation.

Whereas the authors rise the impression that the GRD response is considered, at the end their approach turns out to solve the sea level equation in its simplest form (Spada et al., 2019), i.e., without mass conservation, ocean loading and rotational feedback, providing the local relative sea level change due to radial displacement and gravity change from the ice-mass change only. They should set this premise at the beginning of the study and motivate it from the specific application to solid earth--ice sheet model coupling on decadal to centennial time scales.

In particular, they show that these simplifications are sufficient for modelling the future evolution of the West Antarctic ice sheet during the coming centuries giving an error of up to a few %.

I see this approach valid for the applications described, but I question its help when working over longer time scales, like a glacial cycle, when interpreting geodetic observables, or when considering lateral variations in earth structure consistently. In so far, I see this model as a small step towards a more realistic coupling of the solid earth to ice sheet dynamics, but not sufficient in order to achieve a physically consistent solution.

Mz summary is minor, as the presentation of the results can be be improved. Furthermore I recommend to transfer this manuscript to GMD, which I see more on the side of discussing software developments (the reason why I set novelty to Fair). In particular for TC, I do not see a major impact, also due to the fact that the applicability of this method was discussed only for the future evolution of West Antarctica. Otherwise, the setup is ok, and my further recommendations remain valid also after a possible transfer.

Further recommendations:

1. L. 7: I would skip 'virtually additional software development'. My interpretation is, that only an offline coupling is necessary, where only the interface has to be set. But such approaches were already applied in a number of coupled models like PISM (Albrecht et al., 2024), and MPI-ESM (Mikolajewicz et al., 2025 https://doi.org/10.5194/cp-21-719-2025). Only to list those in which I was involved.
2. In the Introduction I miss a more structured discussion to which time and spatial scale the method is

designed for.

3. L. 22ff: Regarding the solid earth response, I miss the discussion of ELRA and Lingle and Clark, which are both considered for instance In PISM, with more realistic viscoelastic displacements but no gravity change.
4. L. 28: I miss here the reference of Konrad et al., (2015, http://dx.doi.org/10.1016/j.epsl.2015.10.008).
5. L. 36: Konrad et al. (2016, https://dx.do,org/10.2312/polfor.2016.005), compared ELRA against a global GIA model representing the solid earth response in a coupled system.
6. L. 38: Coming back to my 1. suggestion.
7. I don't see the requirement of adapting a GIA module in an existing code. Regarding VILMA it was done for CLIMBER-X (Willeit et al. 2024, https://doi.org/10.5194/cp-20-597-2024) but for PISM and MPI-ESM it was done off line, by only exchanging the RSL and Ice thickness.
8. L. 55: Also, I would reserve an own equation for the definition of R.
9. L. 74: p was not introduced.
10. L 82ff: The authors should specify the sampling and set up of the viscosity structures. Furthermore, they state that the time range extend from seasonal to hundreds of years. So, why do they not consider anelastic effects in their Green's functions in this study?
11. L. 99ff: The discussion regarding the number of integrations comes a bit late, and results in a further problem which weakens the statement of 'ease' a bit. Can the author give realistic integration times? That would help the reader to estimate which further approximations have to be made.
12. L. 105: I would expect 'validate' instead of 'scrutinize' the proposed method.
13. L. 113ff: I wonder why the authors do not consider the floating condition here. And is the rotational feedback only neglected in this experiment or everywhere. This is only clarified in the last paragraph of this study.
14. L. 121: I would not translate 'minor differences' into 'high accuracy', but into 'sufficient accuracy'.
15. L. 123: the neglect of ocean load is only insignificant when considering small changes in sea level.
16. Figure 3: I miss the error in R as this is the main quantity when coupling is considered.
17. L144: Although it becomes clear from the reference, a short note what PICOP means would help.
18. L 147: A transient rheology is considered in the fully coupled setup. Does this mean this is also considered for the Green's functions applied for the simpler model, and so is also considered in the provided set of Green's functions, or alternatively additional transient effects are not necessary for the coupling.
19. L 153: They state that for the year 2350 AD a cumulated error bias only reaches 2%, for their experiment which is rather small. But may it not affect the timing of tipping points. Also, what about local effects, which are not represented in discussing the GMSL.
20. In Figure 5: I miss a comparison with LC or ELRA which are further coupled set ups.
21. L 152ff: For the process I agree, that the effect is small. What about the impact on far-field aspects? Do we need a different model approach then? So, my impression is that the rotational potential is not considered and so, the degree 21 pattern in the far-field sea level is not modelled correctly. Furthermore, the ocean loading is not considered. Which results in an additional difference.
22. L 163ff: I read this between the lines: This approach does not allow a straight forward representation

of a self-consistent GRD. Additional effort would be necessary to reach this, but for the considered case of analyzing centennial coupling and near field processes it is sufficient.

23. L. 166ff: Here it becomes more clearly, where the method is applicable. Nevertheless, I wonder regarding 'accurately' and 'high accurate'. That the method holds only when coupling on decadal to centennial, I agree. At L. 83 you state also 'seasonal', so be consistent.
24. L. 170: You capture the GRD by a number of simplifications, which you should summarize here in view of what terms do not have to be considered in the intended scenarios.
25. L. 176: Why does the neglection of a number of processes result in a strategic advantage. Migration of grounding lines, and bedrock ridges, are rather important in the discussion of grounding line evolution, especially when reaching spatial resolutions of the ice sheet models.
26. L. 178: Love numbers of radially symmetric viscoelastic earth models are also accessible with public domain software like TABOO. Can such sets be implemented in your setup?
27. L 182: Is the extension to more comprehensive linear rheologies necessary regarding the number of simplifications you apply and also the statement on L. 154, that the largest uncertainties are in the ice-sheet models themselves?
28. L.185ff: This final statement appears rather late to my understanding. It reads rather like a premise for the development of the code. So, I would expect this motivation and also the disclaimer at the beginning of the manuscript. This would make the discussion of the study much more transparent.

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