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Geoscientific Model Development Discussions

Interactive comment on "ISSM-SESAW v1.0: mesh-based computation of gravitationally consistent sea level and geodetic signatures caused by cryosphere and climate driven mass change" *by* S. Adhikari et al.

Anonymous Referee #2

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This paper deals with solving the sea-level equation (SLE) on a two dimensional mesh spanning the solid Earth surface. The response to ice-ocean loading is considered only for an elastic isotropic spherically symmetric earth model.

My comments are related to theoretical and modelling part of the paper, I do not comment section 4 on geodetic signatures of ice sheets.

My first comment concerns the solvability of the linear elastic problem if forcing is represented by a mesh-parameterized load. Since the authors consider that Green's func-

tions contain degree 1 Love numbers, the response of an elastic sphere to a mesh-based load will contain also degree 1 terms causing that the elastic model will rotate and translate as a rigid body. This is wrong since we do not observe such a rigid-body motion of the Earth. (The Earth rotates and translates as a rigid body due to other reasons, not by loading by ice and ocean). There are various ways to prevent the elastic body from rotating and translating. For instance, an elastic membrane can additionally be included in an elastic model with the aim to fix up a rigid-body motion under external forcing (this way has been used by e.g. L. Fleitout), or exclude degree 1 harmonics from the load (e.g. Martinec, 2000).

My second comment is related to the references on solving the SLE. The authors only consider the so-called pseudo-spectral method (Mitrovica and Peltier, 1991) of solving SLE. This is not the only way to solve SLE. For instance, Hagedoorn et al. (Pure Appl. Geophys., 2007, 164, 791–818) developed another way to solve SLE, which is more efficient than the pseudo-spectral method and, I guess, is comparable (at least in computational time consumption) to the method proposed by the authors. On top of that, Hagedoorn et al. (2007) method allows considering the effect of moving coastlines on the viscoelastic response of the Earth under surface loading. The author should compare their method with this existing and published method. My third comment relates to the rotational response of an elastic rotating body under surface loading. Martinec and Hagedoorn (Geophys. J. Int., 2014, 199, 1823–1846) recently published the improved theory of the rotational feedback on linear momentum balance. The improvement concerns the change of the centrifugal force in linear momentum balance due to the change in rotational dynamics. This feedback mechanism contributes, in turn, to change of the rotational response of the Earth. The theory is derived for a gravitating viscoelastic body both in time domain and the Laplace domain. Inspecting the Laplace-domain improved solution, the rotational feedback to the linear momentum balance contains an elastic term. This term is not considered by the authors. I recommend to include this term in the rotational response in their modelling. On top of that, when the authors review the literature what has been achieved in the theory of rotational deformation (on line 74 and elsewhere), the paper by Martinec and Hagedoorn (2014) should be cited.

Comment 4 to the sentence on l.373. *There are no standard benchmark (or model intercomparison) experiments available in order to test and validate new postglacial sea level models such as the one presented here.* This is not true. The authors should have a look at the recently published benchmark paper by Spada et al. (Geophys. J. Int., 2011, 185, 106-132), or the synthetic benchmark by Martinec and Wolf (Geophys. J. Int., 1999, 138, 45–66). In addition, the authors should contact the researchers in GIA community if they want to run the benchmark on solving SLE. Such a benchmark has been carried out by V. Barlette, J.Hagedoorn, Z.Martinec, G.Spada and others. Unfortunately, the results has not been published (though submitted for publication), but various numerical codes have been tested and validated.

Comment 5 to the sentence on l.218. '*Therefore, evaluation of SLE that is based on the viscoelastic Love number theory using the pseudo-spectral method in a SH domain has been the standard approach for both standalone modeling of postglacial sea level and coupling of sea-level and ice-sheet models.*' This is not true again. I recommend the authors to inspect papers by I.Sasgen and others (J. Geodyn., 2012, 59–60, 49–63, and Cryosphere, 2013, 7, 1499–1512) on GIA modelling of North America, Greenland and Antarctica, where the SLE is carefully considered and solved.

Comment 6 to the sentence on l.233. '*Here we present a simple mesh-based computation of SLE that bypasses the need for SH discretization.* This is not true. By considering eq.(6) in eq.(7), one can see that load L is projected onto the SH domain. Hence, the mesh-discretized SLE is projected onto spherical harmonics.

In summary, I cannot recommend the paper to be published in the current form. The authors should consider the above comments in the next step, research the existing literature on GIA more carefully than presented in the manuscript, and be more careful in the statements on their method when comparing with existing approaches.

Interactive comment on Geosci. Model Dev. Discuss., 8, 9769, 2015.