13 February 2016
Authors' Final Response to Reviewers' Comments on

ISSM-SESAW v1.0: mesh-based computation of gravitationally consistent sea level and geodetic signatures caused by cryosphere and climate driven mass change

We thank both anonymous reviewers for many insightful comments and we believe the comments to have helped us reformulate many of our paragraphs and to place a little more emphasis on the (high-resolution) ice sheet specific aspects of the modeling. In the comment we posted on 16 January 2016 (http://www.geosci-model-dev-discuss.net/8/C3712/2016/gmdd-8-C3712-2016-supplement.pdf), we pointed out some of the misinterpretation by the Reviewer #2, as the review was focused on one section of the paper only. We obviously may not have properly conveyed some basic facts about the goals in our Introduction. Our newly revised version of the manuscript clearly describes ISSM-SESAW as an elastic representation of solid earth deformation with a dynamic pair of ice sheets capable of driving spatially variable changes in sea level and earth rotation. Reviewer #2 interpreted our model as being the one for viscoelastic solid earth problems and solving the classic GIA problem for paleo-sea-level since Last Glacial maximum. (The figures in the original submission, however, showed that this was not the case.) As a consequence, some of the comments of Reviewer #2 (although very insightful) are not entirely relevant or appropriate in the present context of an elastic rotating earth. Again, we bare some responsibility for not having made our assumptions clearer upfront in the original manuscript.

This document summarizes how we address specific questions in the Revised Manuscript (attached herewith). Our responses, below, are in blue.

Sincerely,

S. Adhikari, E. R. Ivins, E. Larour

Jet Propulsion Laboratory,
California Institute of Technology
4800 Oak Grove Drive, Pasadena CA, 91109.

Review #1

This work presents a method for solving the Sea Level Equation, which is proposed as an alternative to the more traditional approaches based on the pseudo-spectral technique. At this stage, the method is limited to the case of an elastic Earth and does not account for the migration of shorelines. Quite convincingly, the authors illustrate the details of the method and compare its efficiency with respect to previous studies. Some examples of applications are also given. Overall, the paper is well organized and written, the topic is perfectly fit for this journal and its science level is high.

We thank the reviewer for appreciating our work, and nicely summarizing the strengths and limitations of our model.

I have three major points, however, which require attention before the paper can be considered for publication.

We have carefully addressed each of these, outlined below, in the revised manuscript.

1) The method is not new. In Tushingham and Peltier, 1991, a discretization scheme was used (which the authors called "finite elements method") that essentially follows the same philosophy of the approach presented here (although the details of the implementation differ). There are traces of the same method spread in the works of Peltier and coauthors between the 80s and the 90s. I encourage the authors to discuss previous works so to put their own contribution into the right perspective.

The reviewer is correct on noting that the earliest developments for numerical solution of the sea-level equation involved discretization (e.g., Clark, 1977; Wu and Peltier, 1983; Tushingham and Peltier, 1991). In this context our method is not entirely "new". However, the truth is that these discrete methods were never fully examined for their potential computational advantages, nor were the generalizations to flexible adaptive meshing on the sphere examined or exploited. The motivation for this new scheme is in concert with the rapid developments in ice sheet modeling that have occurred over the past 5-10 years, wherein processes that occur at a kilometer scale must be captured along with local sea-level variability. Our method provides a systematic and efficient framework for capturing the kilometer-scale physics within the global scale computational framework for earth system models. On **Page 4** of the Revised Manuscript, we summarize this with appropriate citations. We have also dropped adjectives such as “new” or “novel” associated with our model. We instead write, for example, “a method” (**Page 2**) or “a mesh-based approach” (**Page 31**).

2) The quantity “N”, defined at page 9773, is not the “perturbation to the geoid radius…”, and should be called “absolute sea-level change” instead. The reason is explained in e.g., Tamisiea (2011, GJI, doi: 10.1111/j.1365-246X.2011.05116.x).

We agree that *N* is, strictly speaking, the “absolute sea level” or simply the “sea surface”, although it is commonly (but perhaps not correctly) referred as “geoid height” in the literature. We thank the reviewer for brining the details of this paper to our attention. We now cite Tamisiea (2011) on **Page 5** and define *N* as the “absolute sea level”. Necessary changes are made throughout the revised manuscript (e.g., Pages 5, 6, 17, 18, 24, and Fig. 4).

3) At page 9787 (?) the authors mention some tests that have been made to validate their solutions by comparison with Farrell and Clark (1976) and SELEN. But the results of these comparisons are not presented in this paper (at least, I am unable to find them). Since I think that these comparisons could add a value to the new method developed, I strongly encourage the authors to present and discuss them.

In the original manuscript, we did not include those benchmark/validation experiments partly because (we thought) the paper was already too long. However, we now include some useful model comparison in the appendix “Appendix B Model Validation”. We provide the description of the validation experiments on **Pages 33-35**, and include three figures (**Figures B1-B3**) in the end of the manuscript. We hope that this will add a value to our model and the manuscript. We appreciate the suggestion.

Review #2

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.

In the first paragraph above, the reviewer correctly noted that we deal with elastic earth problems. However, based on some of the comments listed below, it is not difficult to judge that the reviewer has misunderstood our model as being the one for viscoelastic solid earth problems. This may be partly because the reviewer seemed to have avoided Section 4 (Figures 3-7) of the original manuscript wherein it is very clear that our goals to be realized with this version of code is to capture short time scale (elastic) effects. As a consequence, **some of the comments listed below are not relevant:** For example, any comparison to the glacial isostatic adjustment (GIA) benchmark papers (e.g., Hagedoorn et al., 2007; Spada et al., 2011) is not appropriate. Similarly, incorporation of the altered linear momentum balance in rotational feedback identified in the new theoretical GIA paper by Martinec and Hagedoorn (2014) is not possible. Other requests (referencing and some clarifications) are straightforwardly accommodated in the Revised Manuscript.

The reason that we have taken this simple (elastic) approach in this first paper (v1.0) is that a good part of the ice sheet modeling community, quite broadly speaking, now calls for this feature to be explored. This is especially true as the higher-order ice-flow models (including full 3D Stokes and higher-order 3D Blatter/Pattyn) attempt *to simulate present-day observed change*, a feature that is extraordinarily difficult to do. This is now clearly stated on **Page 3** of the Revised Manuscript. The original exposition of the theory in James Clark's PhD thesis of 1977 (and subsequent work) was to explore the sea level equation (and later rotational feedback in Glenn Milne's thesis) in the context of relative sea level data that would constrain GIA models. Consequently, many of the references to the existing literature in our Discussion paper demand reference to GIA theory. Perhaps this was confusing for Reviewer #2 and this is unfortunate. We bare some responsibility for not having made our assumptions more explicitly clear in Section 1 of the Discussion paper, which is now clarified in the revised manuscript (**Pages 3-4**). We clearly, and repeatedly, state our goals and assumptions in "Abstract" (**Page 2**), "Introduction" (**Pages 3-4**), "Methods" (**Pages 12-13**), "Conclusions" (**Pages 31-32**), and "Appendix B" (**Pages 33-34**). We hope that these revisions will help clarify our goals and assumptions.

One of the major strengths of our model is to be able to resolve the kilometer-scale physics (e.g., ice-flow mechanics) and predict its local and global geodetic signatures (e.g., relative sea level), which is clearly stated, for example, on **Pages 2 and 4**. In order to demonstrate high-resolution capability of our model, we now provide sea-level fingerprints driven by high-resolution models of polar ice sheets and dedicate a whole new section "**Sea level fingerprints of high-resolution ice sheet forcing**" in the Revised Manuscript (**Pages 29-30** and **Figure 8**). Such a high-resolution computation is not included in Sect. 4 wherein results are based on relatively low resolution GRACE solutions (on the order of a few hundreds kilometer). We hope that this adds a value to our model and the manuscript.

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 functions 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).

We thank the reviewer for comments on this particular point regarding the degree one term. This comment is pertinent to any finite element method (FEM) in which a continuous media deformation is to be computed by applying surface tractions as a boundary condition at the outer boundary of the region. The same problem can occur in FEM computation of ice flow. The reviewer is correct that one solution that involves the degree-one term loading (which we include) can involve both a rigid translation and rotation of the entire spherical volume.

However, we also state quite clearly that our variable mesh system is designed to cover the spherical earth surface, coupled to global climate (atmosphere, ocean or ice) modeling system framework. In fact, the point of this method is to explicitly avoid having to solve the finite element/volume problem, much as in the same spirit as Love's original monograph on the subject published in 1911. We now state this clearly on **Page 13** of the revised manuscript. The comment is, therefore, somewhat odd as being a major issue. It does not affect, *in any way*, any computational example performed in the manuscript (see, for example, **Figures B1-B3** that are considered to validate/test our model performance).

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.

We agree that there are a variety of ways to solve the sea level equation (SLE), and Reviewer #1 also pointed this out. We now acknowledge some other methods, including the so-called spectral/finite-element methods (e.g., Martinec, 2000; Hagedoorn et al., 2007) and discrete methods (e.g., Clark 1977; Tushingham and Peltier, 1991) on **Page 4**, and full-blown 3-D finite-element methods (e.g., Wu, 2004) on **Page 12**. We may have still missed some other methods, but our purpose, of course, falls quite short of attempting a full review.

As noted earlier, we think that there must be some misunderstanding by the reviewer. Our paper concerns present-day changes in earth rotation, sea level, and geodetic quantities related to computations of relatively small-scale (~1-2km) ice changes coupled to the instantaneous response of a seismologically realistic earth model and geopotential field (as noted, for example, on **Page 2**). In contrast, the paper by Hagedoorn et al. (2007) is for a treatment of global GIA and the response field that can be compared to global tide gauges (and a nice paper!). The effects like moving coastlines (as in Glenn Milne's PhD thesis) is important for certain regions when computing present-day relative sea-level at coastlines that respond to the Last deglaciation over the past 120,000 years (see Hagedoorn et al, 2007, Figure 4). Although, it might be a moot point considering the substantial difference in the governing deformation equations and time scales between our model and the work of Hagedoorn. It would be quite *impossible*, therefore, to perform any comparison to Hagedoorn's solutions for the simple fact that the latter is solving a profoundly different initial-boundary value problem relative that is being proposed for publication in GMD in our submitted manuscript.

In the spirit of the reviewer's comments, however, we further clarify on **Page 12** of the Revised Manuscript as to why we choose to compare our results against the more common pseudo-spectral methods. A part of the reasons is that contemporary software is available, which makes it straightforward for us to perform some sort of model inter-comparison analysis (as shown in **Figure B2**; read "Appendix B" as well).

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 24 and elsewhere), the paper by Martinec and Hagedoorn (2014) should be cited.

We are quite happy to cite Martinec and Hagedoorn (2014) on **Page 9** of the Revised Manuscript. For reasons explained on page 4 of this document, however, the comments and suggestion to include the elastic term in the rotational feedback are inappropriate/irrelevant. We will consider this implemented in the future version of the ISSM-SESAW wherein we will treat longer time scales and viscoelasticity. We thank the reviewer for bringing this paper to our attention.

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.

To our knowledge there are no benchmark experiments available for relative sea level model comparison (at least for those that operate on elastic, rotating earth). The papers that the reviewer have listed (e.g., Spada et al., 2011) present the benchmark study for GIA codes. (It is unclear where a purely elastic limit comparison can be developed by using the materials provided in the Spada et al., 2011, paper.) Nevertheless, we dedicate a new Section in the appendix "Appendix B Model Validation" (**Pages 33-35**) that includes three figures (**Figures B1-B3**) to validate key components of our model by reproducing the relevant published results. Minor changes are made on **Page 19** as well.

We are happy to contribute to the benchmark study, but we are not yet in a position to provide the viscoelastic solutions using ISSM-SESAW v1.0. Any benchmarked code will be placed into GMD Discussion for dissemination to the wider community as a part of ISSM-SESAW v2.0 wherein we will treat viscoelasticity.

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.

We opt to drop this sentence, although pseudo-spectral methods are far widely used than other methods and for which contemporary software (Spada and Stocchi, 2007) is also available. (Perhaps, Reviewer #2 is simply unaware of the wide use now found at Durham, BAS, McGill, and other institutions, wherein the non-GIA modeling community uses such software.) We have noted this explicitly on **Page 12** of the Revised Manuscript. Recent works of Hagedoorn et al. (2007), Sasgen et al. (2013), and Konrad et al. (2015), which are not based on the pseudo-spectral methods, are properly acknowledged in Introduction (**Pages 3-4**).

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.

We opt to drop this sentence as well, although it is not entirely incorrect. Equation (6) presents the Green's function representation of perturbation in gravitational potential and elastic deformation of solid earth evaluated at the earth's surface. We map the Green's functions on the mesh system with great accuracy, retaining kilometer-scale resolution properties (see **Figure 2c**). The mesh architecture affords control on the number of elements and is thus computationally efficient. This is clearly stated on **Pages 12-13** of the Revised Manuscript.

Note that our computational technique (Section 3) does not operate on spherical harmonic (SH) domain. Only places where SHs appear in our formulation are Equations (19) and (24), while accounting for the rotational feedback. Since we need to deal with degree 2 SHs only (that have large wavelengths), there is no inconsistency as to the unstructured nature of our mesh (as noted on **Page 15**).

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.

We hope that our response and the Revised Manuscript help the reviewer to understand the goals and assumptions of our model. In summary, our main goal is wed computations of global scale (elastic) solid earth deformation and sea level variation that are driven by kilometer-scale (high-order) ice-flow mechanics on time scales less than once century when viscous effects can largely be ignored over most of the globe. This is clearly stated on **Pages 3-4** of the Revised Manuscript, and further justified by inclusion of high-resolution solutions provided in Section 5 "Sea level fingerprints of high-resolution ice sheet forcing" (**Pages 29-30** and **Figure 8**).

Thank you!