Reply to comments of REVIEWER 1: Volker Klemann

Dear Editor, dear Dr. Klemann,

Hereby we respond to the comments (in blue). In the annotated revised manuscript, the modifications made are marked in bold face and a label "R1-N" is found on the margin of the manuscript (where this is permitted by LaTeX), where R1 stands for Reviewer 1 and N is the point made by Reviewer 1 (labels are defined, in blue, in this letter).

We are also grateful to Dr. Klemann for having pointed out that the RSL data we have used in this work are given in uncalibrated time units, and should be calibrated before drawing any comparison with GIA models outputs. We have mentioned this issue in the text.

We hope that we have responded satisfactorily to the constructive comments received, and we are looking forward to have your feedback.

Note that we have also corrected a few typos, added some references, improved the text in a few places, and re-edited some of the figures and tables.

Kind regards

Giorgio Spada & Daniele Melini

Urbino, 20 September 2019.

Interactive comment on "SELEN4 (SELEN version 4.0): a Fortran program for solving the gravitationally and topographically self-consistent Sea Level Equation in Glacial Isostatic Adjustment modeling" by Giorgio Spada and Daniele Melini

Comments by Reviewer: Volker Klemann

General comments

Point R1-0

The authors present in this manuscript an update of numerical code which enables to calculate the gravitationally consistent interaction between a surface mass load and water mass load which compensates the total mass change. Previous versions of this code were presented already on a number of workshops dealing with glacial isostatic adjustment, GIA. In contrast to these versions, the authors improved the code in a number of aspects which are currently discussed in GIA, and extended the portability of the code a lot. These improvements, to my point of view justify a new publication in a method and code-oriented Journal like GMD.

As I don't understand this review as a testing of the code, I will focus my review purely on the presented manuscript, and will not consider the supplement in this regard, especially for the derivation. In general, the manuscript is clearly written, the focus lies on the considered theory which the authors present in the theory section, and, as an application, the authors chose a published and established forcing for which they discuss the output in detail. Due to the fact, that the manuscript presents a methodologically oriented study, they do not discuss deviations from their results to those presented in the original publication of this forcing. From my point of view, this is reasonable strategy.

R. We thank Volker Klemann for his positive evaluation and for the suggestions made. We have made efforts to address all his comments; when not, a justification is given. See the details given below.

Point R1-1

As a validation of the method they refer to the benchmark study Martinec et al., 2018, which was recently published, and to which they contributed with a preliminary version of this code version. Of course the question may arise, why the authors did not present the results for the benchmark, and discuss the addition of the rotational effect which was not considered in that benchmark this would be a nice extension to that study. If they won't do this, at least they should state, that the results of that version are identical with the current one, if rotation is switched off. Consulting that study, the SELEN code deviates a bit more than the order models when considering moving coastlines and floating ice. But this is only a suggestion, to establish more a benchmark study, than the quite complicated ICE6G vm5a applied here.

R. The benchmark study in Martinec et al., 2018 was conducted between codes that do not account for rotational feedback (or in which these effects have been switched off). While we certainly could recompute benchmark results taking into account rotational effects, they cannot be directly compared with other results; actually, we could only discuss the difference between SELEN results with and without rotation. To some extent, this is actually done in the manuscript; moreover, in a recently published review paper (Spada and Melini, Water, 2019) the effect of rotational feedback on GIA fingerprints has been discussed in greater detail. In the revised manuscript, we now refer explicitly the reader to this paper. We think that a benchmark between SLE solvers that include rotational feedback would be of great interest, however at this stage we believe that recomputing the 2018 benchmark results with the inclusion of rotational feedback would not add insight to the discussion. As suggested by the Reviewer, we explicitly remark in the revised manuscript that the SELEN4 results, without rotation and in the same conditions of the 2018 benchmark, coincide with the results published in that context. See also point **R2-1**.

In the following, I focus on individual aspects of the presentation.

to Introduction

Point R1-2
P. 2, L 16: Elasticity is material law, but not a rheology.

The reviewer is right, we have rephrased and we do not use “elastic rheology” in the revised paper.

to Theory

Point R1-3
To present a reduced version of a derivation is every time dangerous, especially if this is not put into an appendix but a supplement which is not part of the manuscript. So, a number of questions arose, which partly come up in the following comments.

We are aware that balancing detail and conciseness in a theoretical section is not an easy task. We thank the reviewer for his suggestions on this point. We agree that the supplement is not strictly part of the manuscript, but it is nevertheless available to the reader after publication of the manuscript.

Point R1-4
P. 4, L 7: You refer to SSM19, here you should at least specify on which principles this expression (Eq. 2) is based.

We have introduced an explicit expression for the load (L) in terms of mass per unit area, which should illustrate better the physical meaning of L. Eq. (3) (formerly Eq. 2) is now better framed since we have made explicit the ocean function, following the suggestion in **R1-5**.

Point R1-5
P. 4, L 10: The meaning of OF should be defined explicitly.

This has now been done, since it effectively helps a lot the understanding of our reasoning. See also **Point R1-6.**

Point R1-6
P. 4, L 12: You introduce here the term bedrock topography, but do not specify what it means, especially as you further down use this quantity, to derive changes in sea level.

This section of the manuscript has been reorganised a bit, in order to define topography (T) in terms of sea level (B), which is a more intuitive definition. We refer more explicitly to Kendall et al. (2005), who follow the same definition on topography. We also define the ocean function explicitly (see also **Point R1-5**), which should help to better understand our reasoning. See also **Points R2-7 and R3-1.** Due to these rearrangements, we have modified the beginning of Section 2.2. accordingly.

Point R1-7

P. 4, L 16: Here and in the following, you use the 'cal' symbol to specify variations with respect to a reference state. If so, you can of course reduce the number of equations, e.g., Eq.s 6, 11, 13 and 15 become redundant. Furthermore, you do not speficy the reference state itself.

We understand the point, but we prefer to keep these equations as they stand, since they define very fundamental quantities that should be 'lost' if embedded in the text. We are now more explicit about the meaning of the reference state, also quoting the paper of Kendall et al. (2005), who uses the same approach to the SLE.

Point R1-8

P. 4, L 24: I don't htink that you have to refere to SSM19 to introduce the definition (7), but simply if follows from $\cal M / \int_e dA$.

We agree. The text has been modified accordingly.

Point R1-9

P. 5, L 2: This is somehow abrupt, and do you really need it here?

No, we do not need it here. We have removed the equation and a few lines of text.

Point R1-10

P. 5, L 4: What do you understand as a plausible surface load. At least you should guide the reader a bit more than referring to Bevis et al. 2016.

As we state, for plausible surface loads we mean surface loads that conserve the mass of ice+water. We better focus on Bevis et al. (2016) realization of plausible loads, by noting that the SLE is not relying upon a-priori definitions of the load distributed over the oceans, acting to compensate the ice load.

Point R1-11

P. 5, L13: Is 'stem' used here synonymously to 'is associated'? If so, why then using a different word.

We use ‘associated’.

Point R1-12

P. 5, L 5ff: How do you ensure in (Eq. 9) that you considered all terms with respect to $\cal M$? In Eq. 5, I ike that \cal O can be one of \{-1, 0, 1}\.

In (previous) Eq. (9) we are confident we have included all the terms wrt $\cal M$, based on the analysis we perform in SSM19. We are not suspecting that anything is missing. Probably here the reviewer is referring, in his second observation, to Eq. 15 (not 5). In this case, we agree, ${\cal O}$ can (only) take the values -1, 0, and +1. We note that, in the revised manuscript.

Point R1-13

P. 6, L 20ff: The definition of seal level from topography is a bit tricky to understand, as you did not define topography itself properly, also sea surface is not defined her properly. So, why not motivate it from Gauss definition of the geoid as that equipotential surface to which a static ocean surface would adjust? Then, the jump from N to rˆss is not as large. Also at the beginning you should specify what the sea level equation expresses. Only then you can obtain its most basic form as U-N. Also the relation to the water column might help the reader to understand its relation to loading more intuitively.

In **Point R1-6**, we have responded to the issue about the definition of topography, also with the aim of responding to similar points of **R2** and **R3**. In the revised manuscript, we have made a short and clear statement about the meaning of the SLE at the very beginning of this Section. We also make a reference to the water column just after having written the basic form of the SLE, to help intuition.

Point R1-14

P. 6, L 12: This is also known as Bruhns formula. As $\cal .$ is not applied to Phi, I suggest to write this equation a bit different: \cal G = \frac{\Phi - \Phi}{g_0}, to remain in the used schema of formula symbols.

Yes, we use this notation, now, here and elsewhere. We quote the Bruns formula.

Point R1-15

P. 6, L 13: One important reference in your discussion of the SLE is missing which is Martinec and Hagedoorn, 2014, who discuss in detail the SLE of a rotating body. They, for instance, distinguish between gravity and gravitational potential, where the latter only considers the surface loading, i.e. variation due to surface mass change and solid earth deformation. The

gravity potential, MH14 use for gravitational potential plus rotational effects. This is in accordance to Helmut Moritz, 1990, The figure of the earth.
So I would use gravity potential instead of geopotential.

Yes, we agree on all these points. We use the same terminology as in MH14, and we quote this important reference.

Point R1-16
P. 6, L 14: The meaning of c only depending on t, can easily be explained simply by the fact, that the displacement of the reference potential surface, N_0, does not represent the potential level to which the water surface will adjust. This is not only due to mass conservation but also due to the changes in the ocean basin. So, it is a bit more complicate.

We believe that a nice and simple explanation of the existence and of the meaning of the ‘c constant’ is given by Tamisiea (GJI, 2011), to which now we refer more explicitly.

Point R1-17
P. 7, L 10: Here and in the following, O is dangerous to use, as it is easily mistaken for the reference state 0.

It should not be the case, since the reference state label “0” is used as a subscript, while “o” in this equation and in the following is a superscript.

Point R1-18
P. 7, L 12: From type setting it is nicer to use text abbrevations in formulas with an unslanted font, e.g., ${\cal S}ˆ\text{ave}$.

This is somewhat subjective, and since **R2** and **R3** did not make a similar observation, we keep all the equations as they stand.

Point R1-19
P. 7, 14: For the definition of the different sea-levels see below. For me, equivalent is more a renormed quantity, but here you are using it with respect to a model dependent quantity, depending on the current ocean area Aˆo (\gamma, t) and \mu (gamma, t).
Both terms depend on the $\cal S$ and $\cal Lˆ{[abc]$.

This is another nice observation. We prefer to keep the abbreviation “equ” but it is clear that something must be said about the fact that S^“equ” is associated to model-dependent “dynamic” quantities, like A^o and \cal O. We now mention this important point explicitly in the revised version of the paper.

Point R1-20

P. 7, L 8ff: This derivation is quite interesting, and I have needed some time to understand this alternative expression, especially why in Eq. 33 T_0 appears instead of T. May be, a specification what ‘using for c the expression found in SSM19’ does mean, might help, as c does not appear anymore. The interesting aspect is that this form is independent of the syphoning, which dominates <S>ˆe, and is put into <G-U>ˆO. May be this might help to understand Table 4 a bit more. So I guess the expression, $- <G-U>ˆO + Sˆave = c$ is missing.

For that part of the question that concerns “c”, we mean that to obtain eq. (30) of the submitted manuscript we have used the expression obtained for c by the imposition of mass conservation, given in the supplement. We are now more explicit on the equation used for c, to avoid any ambiguities. We are not sure we have captured the meaning of the other issues raised in this point. In particular, we do not understand why the syphoning is mentioned in the Reviewer’ comment. So we ask for a clarification, if possible.

Point R1-21

P. 8 L 3: Wikipedia defines the ‘eustatic change’ as the alteration of sea level due to changes in volume of water or ice, and also due to changes in ocean topography, which goes back to Suess. The article to my point of view is based also on Rovere et al. (2016, https://doi.org/10.1007/s40641-016-0045-7), that is, also steric changes are considered. I would call Eq. 36, the equivalent sea level, as you also do at P. 10, L. 14ff, expressing an ocean equivalent of the ice, which is simply a renormation of water mass expressed as water height. Sˆave, I like. Your Sˆeus, I would call \bar{\cal S}ˆice, what is the equivalent of ice at current state. Your Sˆequ is a help quantity, I would keep its definition. FC76 phrase Eq. (36) to be the eustatic sea level although the ocean basin might slightly change. The authors are aware of the fact and “tried to avoid [. . .] the word (eustasy) has received so many qualifications since it was introduced by Suess”.
So, I would suggest not to use eustasy at all. It is a deprecated definition of sea level and mainly leads to misunderstanding.

We have re-worded and discussed in more detail the meaning of the definitions given, also quoting in particular the recent paper of Gregory et al. (2019) about the terminology to be used in the context of sea level change. We agree that misunderstanding could easily arise in this context, we hope the modified text can be considered satisfactory. We keep “eus” but we better specify his meaning, quoting Gregory et al. (2019) about the new term “barystatic”, which should be preferred to “eustatic”.

Point R1-22

P. 8, L 17: ‘essentially’ is a bit enigmatic, do you mean terms of d/o = 2/0 and 0/0 can be neglected as they are too small? See also MH2014.

Yes, it is what we mean. We revised the text to make this point clearer.

Point R1-23

P. 8. L 7: From here to the end of this section, you discuss the numerical implementation into SELEN. As the whole article is about SELEN, I would expect a bit more detail, how the equations are solved also with respect to the iterations you discuss in the following. So, this part I suggest to extend.

No, this part is not meant to discuss the numerical implementation into SELEN. As quoted in the text, details are given in SSM19 and we do not intend to duplicate that material here. We add a few lines, however, in which we briefly describe in a qualitative way how the solution is approached.

to A test run of SELEN

Point R1-24

In the introduction to this section you should mention that you also consider changes in rotation in addition to N, U and S.

Yes, right. We have done that.

Point R1-25

P. 10, L. 23: Why do you use an interpolation. To go from a fine to a coarse grid by interpolation is everytime a bit dangerous. I would expect here a filtering or a binning algorithm.

This is an important observation and we thank the Reviewer for pointing it out. Indeed, we obtained the pixelized topography through an interpolation by means of the GMT ‘grdtrack’ module. However, in our numerical experiments, we used also binned topographies obtained through a sequence of ‘blockmean’ and ‘grdtrack’ GMT modules. The two realizations of topography turn out to be different in regions where small-scale relief features are present. However, we verified that all the relevant numerical results do not change within the numerical precision we used in the manuscript. In the revised text, we explicitly warn the user that for regional analyses focused on areas with small-scale topographic features, particular care shall be devoted to the realization of topography on the Tegmark grid.

Point R1-26

P. 11, L 10: 'Not agreed results on ve. Love numbers'. Do you have a reference for this statement, and why this is an argument to neglect compressibility?

We have rephrased (in LATEX) as <<\textbf{Since we \marginpar{R1-26} are not aware of published, community-agreed sets of Love numbers for a multi-layered compressible viscoelastic model}, in the test run we rest on \textbf{an incompressible profile, for which agreed results have been obtained \citep{spada2011benchmark}.}>> This is not an argument to neglect compressibility, it is just a remark. In the case Dr. Klemann (**R1)** can provide a reference contradicting this remark, we would change this statement accordingly. Beside this, SELEN4 can of course be used with any set of Love numbers, either community agreed or not agreed.

Point R1-27

P. 11, L 15: I would simply start the sentence: 'Numerical values of [. . .] in Table 3 and its caption, respectively', to avoid using two times reference, and the reader to search for the tidal love numbers in the table rows.

We agree; these changes have been done.

Point R1-28

P. 11, L 17 and P. 14, L 9: For the captions use 'Glacial isostatic adjustment in the past' and '[...] at present day' or alternatively 'Paleo glacial isostatic adjustment' and 'Present-day [...]'.

We use "GIA at present" as a title for Section 3.3 and "GIA in the past" for Section 3.2.

Point R1-29

P 11, P 27: I would in one sentence introduce the three configurations you discuss in the following.

We have been more specific and careful to describe the three configurations.

Point R1-30

P. 13, L 2: Why do you mention the small region around Patagonia with a reference only. Why not citing Klemann et al. (2015, DOI 10.1007/s41063-015-0004-x) for the East Siberian shelf or Lambeck for the Sunda Shelf. Furthermore, the light blue areas may represent locations covered with grounded ice, but also floating ice might be present there. The ice extent is not shown in Fig. 5.

We are aware that Fig. 5 is not including the ice. This is clearly stated in the second paragraph of 3.2.2. Paleo-maps also showing the ice sheets shall be included in future releases of SELEN4. The reviewer is right, some important references were missing; now we quote Klemann et al and some others with reference to GIA modeling in specific areas. SELEN can visualize the Ocean Function, in addition to paleotopography. OF maps are located in the /OFU output folder, also showing the distribution of the floating and grounded ice at every time increment. To limit the length of the main text, we do not show them here, where we focus only on topography (folder /TOP). However we now mention them, so the reader is aware of these maps in SELEN. The whole paragraph has been somewhat rearranged to fit the requirements of **R1**.

Point R1-31
P. 13, L 20. As stated before the MH14 paper, is in my point of view an important contribution to the discussion of rotation in GIA.

We agree and we quote the MH14 paper also in this context.

Point R1-32
P. 13, L 25: 'since' used twice.

Right. We use 'because' the second time, instead of 'since'.

Point R1-33
P. 14, L 8: For such statement, you should at least relate the order to that of other transient processes observed or proposed for rotational variations.

It is unclear to us, what the Reviewer is pointing to, here. We did not change the text.

Point R1-34
P. 14, L 11: Why 'shall', although you consider it.

Indeed. We now simply write "we consider".

Point R1-35
P. 14, L 15: Here you define $\cal S$ as relative sea level, I suggest you to use this definition from the beginning, what also would clearify what the SLE is solving for.

We define $\cal S$ as "relative sea level change", uniformly throughout the manuscript.

Point R1-36

P. 14, L 16: would repeat ‘frame’ also before b, c and d.

Done**.**

Point R1-37

P. 15, L 4: This is only clear from the fact that the syphoning effect is inside the definition of γ-depending part, otherwise the reader would wonder.

We are not sure we understand this comment. We did not change the text.

Point R1-38

P. 15, L 8: Instead of ‘decontamination’, geodesists speak about ‘correction for’ an effect.

We mention both terms.

Point R1-39

P 15, L 20: ‘then’ to ‘them’.

OK.

Point R1-40

P. 16, L 23–25: This is an interesting aspect. Can you give a formula to indicate which term has to neglected as direct rotational effect?

We now refer explicitly to Eq. S173 of the supplement, showing the 1+k structure that we quote in the main text. We do not think it is necessary to duplicate it here.

Point R1-41

P. 16, L 32: Is this the degree variance, and why not presenting the equation here?

Because there is no necessity to duplicate Eq. S477 here, in our opinion.

to Conclusions

Point R1-42

P. 16, L 23–25: This is an interesting aspect. Can you give a formula to indicate which term has to neglected as direct rotational effect?

This point has been made above already, see **R1-40.**

Point R1-43

P. 16, L 32: Is this the degree variance, and why not presenting the equation here?

This point has been made above already, see **R1-41.**

Point R1-44

P. 17, L 12: replace 'runs' by 'run'.

Yes, done.

Point R1-45

P. 17, L 15: Not sure, if one can speak about 'physical realism'.

Right. Only realism.

to figures and tables

Point R1-46

Figure 1: In this color scheme, the dark ice thicknesses make it difficult to identify the coast lines. In this caption and in the further ones, I would not state which plotting command you used.

Right. We now use a red contour for the coastlines. We prefer to leave the command we use, for the sake of ease of reproducibility.

Point R1-47

Figure 2: I would skip this figure.

We have been tempted to skip this figure, too. But after consideration that it represents an important quantity, i.e., the final condition of the SLE, we would prefer to leave it.

Point R1-48
Table 2: ‘Density, [...] values specifying the parameters of the homogeneous layers defining the adopted [...]’. ‘Some spectral [...]’ I would skip this sentence, as it does not contribute to the table content.

We have simplified a bit the second sentence, which was effectively too long.

Point R1-49
Table 3: The PMTF is not defined in the manuscript. Without it, the Aˆ[es] and [hkl]ˆT are not necessary. In this context it would be interesting, if you need also viscoelastic tidal love numbers.

We agree. We have skipped the PMTF information. We have left the tidal Love numbers information, as suggested by the Reviewer.

Point R1-50
Figure 8: Can you specify the extreme values reached in the respective plots?

Done.

Point R1-51
Table 4: I would skip the integrals over the whole earth, as these are clear from the definition of U and G.

We know that these are clear, but it is a nice indication of the precision of the SELEN4 numerical results. We prefer not to remove them.