Answers to the reviews of manuscript "Brief Communication: The global signature of post-1900 land ice wastage on vertical land motion" by Riva et al. (2016), doi:10.5194/tc-2016-274.

We wish to thank the referees for their feedback on our manuscript.
Below we respond to each individual comment, where text by the referees is in bold.

On behalf of all authors,
Riccardo Riva

Referee #2

Abstract. "Deformation" should be replaced by "vertical displacement" here and in the rest of the paper. They are used as synonyms but they are not, in my opinion.

We agree that the two words are not synonyms, with "displacement" being a purely kinematic concept especially valid when talking pointwise (displacement is a change in the position of a point or of all points of a rigid object), while "deformation" better refers to the relative motion between sets of points (with the more general meaning of "change of shape"). Hence, we argue that it is appropriate to talk about "GPS measuring displacement" and "ice melt causing deformation". As such, we would rather keep using both words, though we have made an additional effort to use each of them consistently through the paper.

Line 23. Another less obvious effect that could be mentioned is the variation of gravitational potential \Phi that together with U give relative sea-level change according to the sea-level equation S=\Phi/\gamma + c -U where c is the notorious c-constant.

True, but maybe confusing, since the paper expressly only deals with vertical land motion.

L26. I think this is realised, indeed, also in the cryospheric community.

We are not sure whether the reviewer expects us to remove the sentence, or agrees with our viewpoint. In any case, we admit that it is difficult to quantify which portion of a community is aware of a specific concept. That is why we have originally opted for the wording "what is often not realised", which we believe we can defend based on our personal experience.

L28. Actually the SLE is more general and can also deal with the viscoelastic Earth's response.

True. Even though this paper only deals with elastic deformation, it is a good idea to have statements of more general validity in the introduction. We have changed "elastic" into "viscoelastic".

L30. In this brief communication. . . From what I have understood, the novelty here is the long time window considered (1900-now) for the computation of the elastic displacement, and the use of realistic ice sources.

Indeed. We now mention the long time window and of the use of realistic ice sources as an additional innovation of this study.

L40. Quantification is not so problematic if the melting histories are well constrained.

We did mean an accurate quantification of the melting histories. We now specify it.

L58. Adding the individual responses to obtain the total response is OK if you assume linearity. An indeed the SLE is linear as long as you do not allow for shoreline migration. But I guess that here the shorelines do not move.

Indeed. We have added a sentence explaining that our superimposition approach is allowed by the fact that the SLE is linear since we make use of fixed coastlines.

L58. Compressible is OK. But I imagine also layered and consistent with the seismic travel times.

Indeed. We now write "compressible and spherically layered". Consistency with seismic travel times is, to our knowledge, standard practice.

L59. 'period of interest' is vague. From the figures I see different rates at different times, that appears to contradict the use of a unique linear trend.

We meant to refer to the various time windows shown in Fig.2. We now say "over each time window under study".

L65ff. It can be worth to recall that these fingerprints have a vanishing global average.

We are afraid that such a statement will be obvious to people familiar with spherical harmonics, but confusing to many other potential readers. In addition, when sampled at discrete points (e.g., GPS stations or tide gauges) these fingerprints will probably still lead to non-zero global mean values due network geometry issues. Hence, we prefer not to add the suggested comment.

L68. I am not sure that 'pole tide' is appropriate. From e.g., http://www.navipedia.net/index.php/Pole_Tide I understand that the pole tide is related with the 14-months Chandler Wobble, which I am sure the authors have filtered out from their equations. What causes the lobes in the far field in the vertical displacements maps is the (non-oscillatory) secular component of polar motion.

We are sure that the terminology "solid earth pole tide" is appropriate. In the given link, the Chandler Wobble is only provided as an example. The term is mostly used within the geodetic community, that's why we had only mentioned it within brackets.
Nonetheless, we have removed the word "pole tide", since we reckon that the terminology may be misleading (the pole tide is not a "regular" tide, in the sense that it originates from Earth's rotation instead of from gravitational attraction by external bodies).
As a consequence, at line 112 we now write "earth rotational effects" instead of "pole tide".

L68ff. Where are these max values met?

These max values are met over Greenland, we now specify this in the manuscript.

L76. . . Has been subsiding. . . well, the actual subsidence stems from this component plus GIA, etc. etc

True. We thought this was implicit, but it may be better to specify it once more. We have added "because of contemporary ice mass change".

L85. Ditto. See L76. These subsidences are virtual, they only represent one component of total subsidence, and probably not the largest one.

Same as above. We now specify “due to continental ice mass loss, cities...”.

L84. Vertical displacement has certainly an effect on tide gauge. But also N =\Phi/\gamma + c has one. Is this negligible? Has this been computed? In a more in-depth study I recommend to show both S and N along with U, for the same sources considered in this study.

We acknowledge that, especially in the far-field, geoid changes and global mean mass changes can be as important as vertical land motion. However, those signals are a part of the tide gauge observations that researchers want to preserve. It is vertical land motion that often represents a nuisance signal, which is the reason why we have decided to make it the object of the current study.

L111. The coseismic displacement can be also modelled globally (see http://onlinelibrary.wiley.com/doi/10.1029/2003GL019347/full).

We have added the suggested reference to Melini et al. (2004).

L112. What is the signal identified therein? Unclear. Is the rate of solar motion driven by the ice sources considered? What is its amplitude and direction?

Indeed, we do not specifically quantify the size and direction of the pole tide (now “earth rotational effects”) driven by ice mass loss, because it is beyond the scope of this study.
We have clarified the sentence, which now reads “meaning that the decadal and secular signals contributing to vertical land motion as identified in this study are not considered”.

L130. I do not understand why the ‘far field signature’ is mentioned here. Viscosity also controls deformation in the near field.

The far-field signature is the main object of this study. Instead of “controlled by bulk viscosity values” we now say “controlled by viscoelastic relaxation mostly taking place deep in the mantle”. We agree that also the near field is controlled by viscosity, but near field relaxation is more sensitive to shallow mantle regions, where viscosity values could be much lower and provide significant responses even at decadal scales.

See http://journals.fcla.edu/jcr/article/view/80095/77355 for advice on how hyphenate “sea level”.

Thank you for the reference, we have harmonized hyphenation of “sea level”.