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 #1 (Alvaro Santamaría-Gómez)

L21: "the century-long trend" in ice-mass loss . . . Also, a reference to the Fig. 1 (right) would be appropriate.

Done.
We have added "in ice-mass loss" and a reference to Fig.1

L26: "what is often not realized" by who? I believe is quite common to deal with solid Earth deformation due to loading at global scale.

We have changed the sentence into "what those communities often do not realize".
From experience, we have the feeling that outside the geodetic community, to which Reviewer 1 belongs and which routinely deals with loading effects, scientists are mostly aware of the near field effects.

L56: while the secular or mean VLM trends are probably indistinguishable in a CM or CE/CF frame, the interdecadal vertical deformation may be different depending on the chosen frame, which, in turn, may have an impact on the short-term trends shown in figs. 2 and 3. This is what happens with other loadings (atm, ocean and hydro) at the interannual variations leaving the long-term trend unchanged. Maybe it does not happen with the spatial pattern of the ice-mass unloading, so I suggest adding a sentence explaining why the CM frame was chosen and whether it has any impact on the results.

We carefully considered whether to present vertical deformation in the Centre-of-Mass of the Earth System (CM) of Centre-of-Figure of the Solid Earth (CF) frame, after having computing both.
What we found is actually the opposite of what has been sketched by the reviewer: secular VLM trends are largely affected by the choice in reference frame, especially in the far-field, while the difference between secular and decadal trends is mostly significant in the near field, which means it is roughly reference-frame independent.
From the point of GPS observations, it would have made sense to use the CF frame, since CM-CF motion is accounted for by the underlying global reference frame (albeit the reference frame realization introduces uncertainties of its own).
However, from the point of sea level research, we believe that it makes more sense to look at vertical land motion in the CM frame, since that is the most natural reference (the sea surface at rest follows the geoid, which is centered at the CM).
We have added an explanatory sentence to the text about why we chose the CM, but we deem the discussion of the impact of the reference frame choice on the modelled signal to be too technical, hence possibly confusing, for the broader TC audience.

Fig. 2: if the format of the communication allows it, I would suggest to add two more maps showing the rate differences between the maps a) and c) and a) and d). This would support the discussion of the results and also fig. 3.

We agree with the suggestion and we have added two panels to Fig.2 (below), showing differences between the secular and the decadal trends. The practical need to use the same colour scale for all panels mostly highlights near-field differences, but it is luckily enough to highlight the larger mid-latitude trends in the last decade. This indeed supports the discussion of the time-variable trends in Fig.3, especially for New York, London and Sidney which experience a considerable acceleration in recent times.
We have added a brief discussion of the new panels while describing Fig.2 and a reference to them while discussing the right panel of Fig.3.

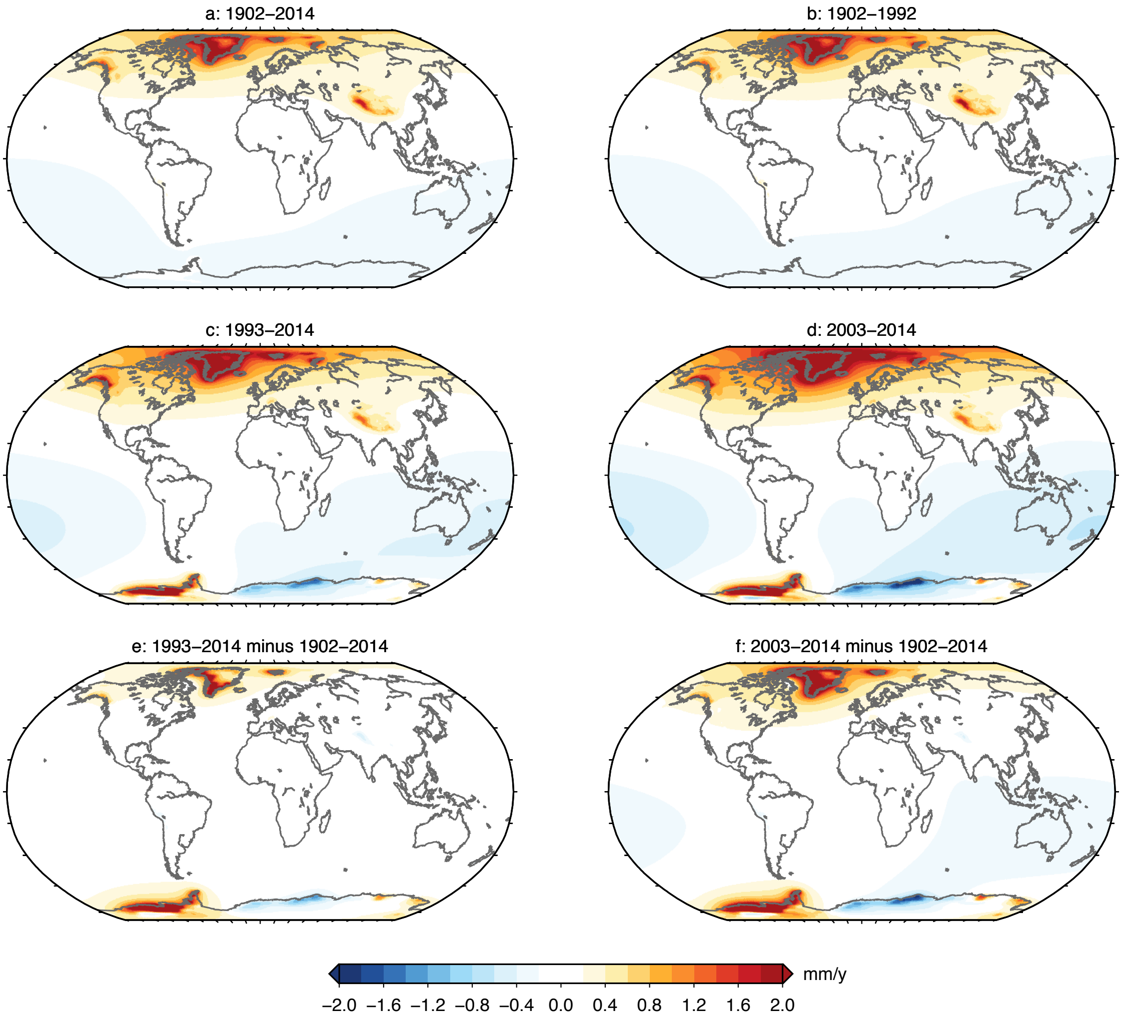

Figure 2: Maps of average vertical deformation rates over different time spans. a: full time span covered by this study; b: pre-satellite era; c: the GPS era; d: the GRACE era; e: panels c-a; f: panels d-a.

Note: from this point on there seems to be an offset of 5 lines between the numbering used by the reviewer and the online version of the manuscript (e.g., L66 below should be line 71).

L66: accuracy of both, the melt distribution and the regional mass loss values.

Agree, added "and the regional mass loss values".

L71:"most of Australia has been subsiding at rates larger than 0.4 mm/yr" this has been observed by GPS estimates since long ago without any plausible explanation thus far (see for instance Altamimi et al 2016). I suggest emphasizing this point.

We have added a sentence to highlight this issue, but we cannot accommodate the suggested reference due to limitations of the Bref Communication format.

L71: This is a very interesting spatial pattern in which northern TGs are uplifted faster in the last decade (captured by the GPS VLM corrections) compared to the last century, while southern TGs have subsided faster. This could partially explain the hemispheric difference in sea-level rise found by Wöppelamn et al. 2014. At the time that paper was published, this ice-mass loss fingerprint was unknown and it seems to me from your Fig. 2 that the average difference between the northern and southern TGs used by Wöppelman et al. 2014 could accommodate part of the hemispheric difference that was not explained by the uncertainties.

Wöppelmann et al. (2014) indeed found a hemispheric difference of about 0.9 mm/yr in sea level rise at GPS-corrected tide gauge stations, with larger values in the Northern Hemisphere. From the new panels of Figure 2 (e and f) it can be seen that GPS trends in the last 1-2 decades might overestimate the secular hemispheric difference by more than 0.4 mm/yr (e.g., by comparing New York with Hobart, which in the cited paper show trends close to the corresponding hemispheric means). Indeed, this could potentially explain a large part of the hemispheric difference discussed by Wöppelmann et al. (2014).
However, an exact estimate of this effect would require repeating their experiment by making use of all the 76 tide gauges used in that study. Hence, we have added a comment and the suggested reference in the discussion section (after line 122 in the original manuscript), but not given any hard number on the size of the bias potentially induced by non-linear VLM (simply referred to as "up to a few tenths of mm/yr").

L71: In relation to my comments above. Similar to the GIA effect on the deepening of the ocean basins and the resulting global mean sea-level change (of about 0.3 mm/yr), is there any ocean basin effect due to recent ice-mass loss to be accounted for in the sea-level trend?

This point is actually already discussed in the discussion section, at lines 123-126. The effect is about -0.1 mm/yr: noticeable, but within the uncertainty of global mean trends based on, e.g., satellite altimetry.

L89: The estimated changes in VLM rates appear to induce a periodic-like oscillation close to 60 years, especially in northern TGs close to the areas of ice-mass loss. Many of these TGs have very long records and were used to assess a global 60-year oscillation in sea-level by Chambers et al. 2012. I wonder how much of the observed 60-year oscillation is due to the ice-mass loss fingerprints shown here. A detailed analysis would be worth pursuing. A priori, the oscillation phase shown by Chambers et al. 2012 (Fig. 1) is consistent with your results.

We thank the reviewer for another suggestion about potential implications of our results.

However, we find it difficult to assess the impact of the VLM variability on the results by Chambers et al. (2012), for at least two reasons: first of all, it is not possible to quantitatively compare VLM with relative sea level changes, since the latter also include the effect of ocean mass changes and geoid changes; secondly, the oscillation found by Chambers et al. (2012) is centered around a zero mean, while our rates remain positive or negative (depending on the hemisphere), since the net cryospheric contribution never changes sign.
Hence, while glacial fingerprints might have modulated long-term oscillations in regional sea level, we prefer not to comment on the issue, considering the impossibility to assess the size of this effect on the basis of VLM fingerprints alone.

L99-101: Note that we didn't correct or encouraged correcting for continental water mass loading due to the significant differences amongst the model outputs in terms of secular, as you mention in the next sentence, but also interannual deformation.

We have added that models outputs are also uncertain in terms of interannual signals.

L113: "those approaches are limited by the fact that space geodetic observations are only available since the 1990's". Note that there exist alternative approaches in combining satellite altimetry and tide gauge observations that benefit from the longer TG series, thus reducing this limitation (see for instance Kuo et al., 2004 and Santamaría-Gómez et al. 2014).

Thank you for those references, we have added a reference to Santamaría-Gómez et al. (2014) at line 117. Considering that, to our knowledge, those alternative techniques are not yet widely used, we have further edited the sentence at line 118 by writing "the majority of those approaches", instead of "those approaches".

L115-117: This is probably the biggest limitation of using GPS for correcting long TG records (together with the lack of nearby GPS observations), especially when very short GPS series are used. However, it is not a limitation exclusive of the GPS VLM corrections, but also when using GIA corrections which neglect any non-linear VLM in addition to any other linear VLM that is not GIA.

True. The fact that using GIA models to correct of VLM does not solve all problems has already been mentioned earlier in the same section. The fact that several processes can induce non-linear VLM has not been mentioned explicitly, simply because those processes are not the object of this study.

L117: In relation to my comment above. The average VLM for the last 10 years for the 6 TGs shown in Fig. 3, does not seem to lie far from the average VLM over the last century. It would be interesting to have some statistics of the VLM deviation during the GPS era or the additional maps I suggested above.

We have decided to add two panel to Figure 2, as earlier suggested by the same reviewer. The new plots show that the non-linearity effect in the far-field, where all 6 cities are located, is mostly visible during the last decade.
At line 122 we have added "especially if the observations have been collected during the last decade".

L140: This is an interesting perspective, but one also needs to consider the uncertainties in the ice-mass loss fingerprints, which were not discussed in this brief communication. In addition, even after correcting for this effect, the VLM corrections (from GPS or GIA) will still be considered linear as a working hypothesis even if we have clues that they may not be (due to pole motion deformation, hydrologic loading, long-memory noise, etc.).

Indeed, we have not directly assessed uncertainties in the ice mass loss fingerprints, even though those are part of the previous study by Frederikse et al. (2016), on which the fingerprints are based. It is also true that many other unmodelled processes might induce non-linear motions. Nonetheless, it seems reasonable to assume that VLM induced by ice melt currently represents the largest signal at regional scales, and as such should be modelled as well as possible.
We have rephrased the last sentence, which now reads: "In particular, due to the recent acceleration in land ice melt, which represents one of the largest drivers of regional vertical land motion, the estimation of secular rates from GPS observations should account for the effect of glacial mass change".

Technical corrections:
L28: "position of every other point on the Earth's surface" with respect to the Earth's center of mass.

Actually, changes in surface load will always change the 3D position of every other point at the Earth's surface, while fixing a certain reference frame will determine the size and direction of that change. Considering that our statement is only qualitative, we don't see the need for specifying a reference frame. We have considered adding "with respect to their initial position", but that seemed implicit in the wording "change the position".

L48: "cumulative mass loss" should be "equivalent sea-level change" or "barystatic sea-level change".

Agree, we now write "equivalent sea-level change".

L121: "induce" I would suggest "reveal" here

We agree that "induce a bias" is possibly not the best phrasing and decided to change it into "cause a bias".