Response to Reviewer 2.

We thank the reviewer for his or her comments.

Comment 1:
1) The correction of the data for the recent signal is calculated without considering its large variability over the last two decades. More specifically GPS time spans are not uniform and, as I understand, the elastic correction is not computed for each station coherently with its time span. The elastic correction is not constant in time. Greenland mass loss for example has accelerated in the last decades.

Response 1: It is true that the elastic corrections for the GPS are not computed for each station within its time span – the trend is rather computed over the time interval 1993-2014. There is an acceleration for Greenland mass loss considered when the displacements for the elastic effect are computed (see text, Section 2.3). It is true that the computed elastic effect would be larger over shorter or more recent time spans. The full time span that we have used could therefore be considered a moderate elastic correction. It would be nice to try to compute the elastic corrections for each station for the appropriate time span – this would be possible for the data taken from Blewitt et al. (2016) but not possible with the Kierulf et al. (2014) dataset (as given) since the begin and end years of the trends were not provided. It is also this dataset that is centred over Scandinavia, where the elastic correction is largest.

Comment 2:
2) As for the GRCE correction of the mass loss in Svalbard and the Russian arctic, the "large" discrepancies in Table 1 are mostly due to the different time spans and to the fact that the mass loss there has not been constant at all. I understand that from Cryosat is still hard to derive mass changes, so I wouldn't include the range of possible estimate. The most reliable estimates for Svalbard and Russian arctic come from ICESat and GRACE and over the same period they agree well enough. Since you need to extract a long term signal I would simply use the GRACE data over the period for which you have the most reliable corrections.

Response 2: There are some large discrepancies in Table 1, even over the same time spans. For example, the glaciological estimates for Svalbard differ from each other over 2003-2009 and again from the IceSat and GRACE estimates over the same time period. That the mass loss has not been constant means that it is natural that the estimates for different time spans differ from each other. So it may be natural that the Cryosat estimate is larger than the IceSat estimate due to accelerated mass loss, and not due to unreliability of Cryosat estimations. If only IceSat is reliable, then this would mean that we stop the GRACE time series after 2009 and we prefer to use a longer time series.

Comment 3:
3) The re-scaling procedure of the mass loss in Svalbard and Russian is questionable and shows that the filter applied to the GRACE data is way too heavy. In fact Root et al. 2015 (doi.org/10.1002/2015GL063769) perform the same kind of correction on the GRACE data in the Barents Sea without the need to rescale. The authors also recognize that they cannot properly invert for the gravity data and that the initial filtering could have been too strong. So what if more a suitable filter were used on the GRACE data instead? How and how much would the result change? Is the gravity signature of the a priori GIA filtered with the same filter?

Response 3:
We have indicated that the treatment for mass loss in this region was problematic. Note that we applied altimetry-derived corrections, whereas Root et al. (2015) use a correction based on mascons which are smaller than the altimetry estimates. The *a priori* gravity information is unfiltered. The filter may have been too strong in the Barents Sea region, but less aggressive filters show comparable results over Scandinavia, so it is not clear to what extent the use of a different filter would result in a different prediction.

Minor comments

Comment 4: It is not explicitly said that is a semi-empirical study. It is called explicitly "inversion" which is quite misleading at first glance.

Response 4: In the introduction we now refer to the model as a semi-empirical model.

Comment 5: The use of the word “posterior”: I suggest the use of "a posteriori" (if that is what the authors mean), but it is not necessary, it just sounds better to me.

Response 5: Ok, we have changed occurrences of posterior to a posteriori.

Comment 6: L45-46. Forward models are supposed to have formal uncertainties only when the models parameters are well (known and) constrained. The model parameters can have uncertainties depending on the error on the constraints (for the inversion). If a model parameter is unknown or have too large uncertainty then the error on the forward model is meaningless. The sentence is misleading (or incorrect), so I suggest rephrasing it.

Response 6: The inferred model parameters can have uncertainties that depend on the error on the constraints and the model uncertainties can be well or poorly constrained depending on the model’s sensitivity to the data. What we meant also here is that GIA predictions themselves are often provided/discussed/used without uncertainties. The text has been reworded here: “The majority of GIA models are however forward models which can be limited by uncertainties in both the ice sheet model and Earth model. Furthermore, because a best-fit forward GIA model is generally a single Earth-ice model combination, their predictions of GIA deformations are typically provided without uncertainties”.

Comment 7: L153-156. While this can be true, I think the GIA signal from LIA cannot explain large differences. The large differences come from computing the trend over different periods.

Response 7: We have suggested an LIA signal as a possibility, not as a certainty, as indeed there could be other explanations. That the GRACE signal differs from glaciological estimates and to a lesser extent altimetry estimates suggests that the GRACE signal may contain a solid Earth signal in addition to a mass loss signal (which would originate either from paleo GIA or LIA GIA, and spatially a signal from LIA would more likely be centred over the currently glaciated regions than the central Barents Sea region).

Comment 8: L241-244. The sentence is difficult to understand. Mostly because here the use of "... ’tuned’ ice sheet history ..." is rather confusing. At first I believed it referred to the previous sentence so the following didn’t make any sense. ICE5g and ANU for example are in fact ’tuned’ ice histories. Anyway I believe the authors are referring to something else.

Response 8: Yes, we agree that the ICE-5G and ANU models are both in their way tuned ice sheet histories, and that is what we were referring to in this and the previous sentence. The text here has now been somewhat reworded, hopefully this clarifies the meaning. Our meaning was that if an ice sheet history is best fit with a particular viscosity profile then varying the viscosity profile over a wide range of values may make the predicted response variations larger than appropriate for a particular model; however, uncertainty in other parameters not considered would also likely make the uncertainties larger.