We have submitted a revised version of the manuscript titled "Mapping geodetically inferred Antarctic ice height changes into thickness variations: a sensitivity study" by Valencic et al. The manuscript received two positive reviews that listed several suggestions for improvement. In the material below, we respond comprehensively to each point, and indicate the associated changes to the text. The reviewers' comments appear in blue, and our responses are in black, with text quoted from the original or revised manuscript indented. No changes were made to the manuscript beyond those required to address the reviewers' comments.

Reviewer #1 (Anonymous):

"Mapping geodetically inferred Antarctic ice height changes into thickness variations: a sensitivity study" by Natasha Valencic and co-authors

In this study, the authors investigate uncertainties in determining Antarctic ice volume change from satellite altimeter-based observations of ice height. They focus on 1) the GIA signal, and 2) the scaling factor used to convert ice height change to ice volume change (after GIA has been removed). Results are forward looking, reporting errors for future projections. This is a nice concise study exploring some of the uncertainties in determining Antarctic ice volume and is worthy of publication. The article is very well written, and I have a few comments that may help to clarify the text to broaden the reach.

We thank the reviewer for their positive comments.

Major Comment:

1) Some further descriptions are needed about the theory:

The paper of Groh et al., (2012) is mentioned many times, I think it would be useful to have a short summary of that paper or the methods from that paper. How are the equations in Section 2 different to those in Groh et al., (2012)?

We agree that our paper should more clearly state how the derivation in Section 2 compares to the treatment – and assumptions - of Groh et al. (2012). To this end, we will make the following changes to the text:

On line 53, we will delete the following text since it implies, incorrectly, that our derivation follows the approach of Groh et al. (2012):

> Following the discussion in Groh et al. (2012)

Next, after equation (8) we will add the following explanatory text:

> This expression relates ice thickness changes to ice surface height (or surface elevation) changes (ΔH) and ocean thickness changes (ΔS) and makes two assumptions. First, that viscous effects can be ignored and, second, that Earth structure varies with depth alone. The latter introduces negligible error (Mitrovica et al., 2011).

After equation (10) the following text in the original manuscript:

> This expression indicates that the mapping between ice surface height and ice thickness changes is dependent on spatial scale (via the degree l), as shown in Figure 1.
>
> Another assumption in the above derivation is that the response of the solid earth to modern ice loss in the Antarctic region can be represented by an elastic Earth model. The inaccuracy this introduces will be a function of the timescale of ice loading that is considered and the viscosity of the underlying mantle. In the results section, we investigate this issue by considering both elastic and viscoelastic Earth models. Our predictions also include the impact of water height changes ΔS_{lm} on the mapping.

will be revised to read:

> Precisely the same expression was derived by Groh et al. (2012), although their derivation was based on considering the gravitational effects of ice mass changes and crustal uplift, following results in Wahr et al. (1998). The expression indicates that the mapping between ice surface height and ice thickness changes is dependent on spatial scale (via the spherical harmonic degree l), as shown in Figure 1.
>
> As noted, Equations (9) and (10) assume that the response of the solid earth to modern ice loss in the Antarctic region be represented by an elastic Earth model. The inaccuracy this introduces will be a function of the timescale of ice loading that is considered and the viscosity of the underlying mantle. In the results section, we investigate this issue by considering both elastic and viscoelastic Earth models. Our predictions also include the impact of water height changes ΔS_{lm} on the mapping.
>
> Although Groh et al. (2012) also derived equations (9) and (10), they assumed that the second term in brackets in these equations could be replaced a simple constant (.0205) that they computed by considering the value this term would take on if one used a spatial scale consistent with the mean scale of Antarctic drainage basins. Their assumption, in this case, that $\alpha = 1.0205$ (red line, Fig. 1), removes the dependence of α on spatial scale.

Line 40: Before the sentence starting “In previous work”, it would be useful to have further description to explain what this ratio is, I think this would help reach a broader audience.

We agree. The original text (line 40):

> Correcting altimeter data for crustal deformation due to modern-day melting is generally based on elastic one-dimensional Earth models. In this case, elastic Love number theory (Farrell and Clark, 1976) has been applied to approximate the ratio of ice thickness to surface elevation changes. In previous work, this ratio was fixed at a value of $\alpha = 1.0205$ by considering the average spatial scale of various Antarctic drainage basins (Groh et al., 2012). However, the full expression for the scaling derived from Love number theory indicates the ratio is dependent on spatial scale and will thus be geographically variable (see the theory section)

will be revised to read:

> Correcting altimeter data for crustal deformation – i.e., mapping altimeter observations of ice surface height changes into ice thickness changes - due to modern-day melting is generally based on elastic one-dimensional Earth models. In this case, elastic Love number theory (Farrell and Clark, 1976) has been applied to approximate the ratio of ice thickness to ice surface height changes. In previous work, this ratio, which we denote by α, was fixed at a value of $\alpha = 1.0205$ by considering the average spatial scale of various Antarctic drainage basins (Groh et al., 2012). That is, the field of firn- and GIA-corrected ice surface height change was multiplied by this constant to estimate ice thickness change. However, the full expression for the scaling derived from Love number theory indicates the ratio is dependent on spatial scale and thus the mapping between ice surface height changes and ice thickness changes will be geographically variable. Moreover, the adoption of a constant scaling neglects both crustal deformation due to ocean loading and viscous effects (see the theory section for full details).

Minor Comments:

Line 24: Perhaps change “and the response to modern-day melt” -> “and the solid-Earth/elastic/viscoelastic response to modern-day melt” ?

We will revise the text to read:

> and the viscoelastic response of the solid Earth to modern-day melt

since “viscoelastic” incorporates “elastic”.

Line 33: “various levels of complexity” it might be worth mentioning that many of the 1D models only have 3 layers.

We will revise this text to read:

> various levels of complexity, from 1D models with as few as three layers to full 3D variability

Line 85: It would be useful to provide justification in using the IJ05 model, since it is fairly old and an updated version exists.

In fact, we have adopted an updated version of the model recently provided to us by Erik Ivins. We think the model remains relevant because it was the first to reduce the excess ice volume at Last Glacial Maximum well below values adopted in ice histories developed by Profs. Peltier and Lambeck. In any case, we will revise the text (line 84):

> the Antarctic model of Ivins and James (2005) (referred to as IJ05)

to read:

> an updated version (Erik Ivins, pers. comm.) of the Antarctic 85 model of Ivins and James (2005) (referred to as IJ05)

Line 96: what is the resolution of the 3D Earth model – laterally and with depth?

The spatial resolution varies from a value of ~6 km near the surface to ~25 km above the core-mantle boundary. We will add text to the manuscript to make this explicit.

Line 100: This first sentence could be clearer, the phrase "based on" makes this a little confusing. I think you mean the calculation of the Earth's deformation response to mass flux use ice change input from the fETISH projections. Just make it clear this is the ice input and you are calculating the response using the 3 earth models. Why did you choose this projection from the 180 available? Is it an upper bound? How does it change the results if you use a different projection?

To avoid the confusion the reviewer alludes to, we will revise the text (line 100):

> The calculations of the Earth's response to modern mass flux are based on the fETISH32 (EXP A1) projections to 2055 CE of the Antarctic ice sheet (Pattyn, 2017).

to read:

> Our calculations of the Earth's response to modern mass change adopt, on input, the fETISH32 (EXP A1) projections to 2055 CE of the Antarctic ice sheet (Pattyn, 2017).

As noted on the original line 103 (and in the revised text), this projection is characterized by the upper bound on ice mass loss in all projections within ISMIP6. We considered one other projection and the main conclusions (which are based on the dimensionless ratio α, which scales magnitudes out of the problem) are unchanged.

Line 111: For clarification could add here that each it calculated with its own Earth model e.g. "for each ice model paired with it's own Earth model"

Agreed. As suggested by the reviewer, the text will be revised to read:

> for each ice model paired with its own Earth model

Line 113: Can you add a line to say how you worked out the Antarctic ice volume at LGM from uplift rate – or are these values taken from the individual studies?

We will revise the text (line 113):

> Antarctic ice volume at LGM in each model

to read:

> Antarctic ice volume at LGM as prescribed in each published model

Line 118: It would be useful to have these locations labelled on the figure.

We will add labels to one frame of Fig. 3.

Line 137: “largest changes in this scaling factor” compared to the fixed value of α = 1.0205?

Yes. We will revise the text (line 137):

> changes in this scaling factor

to read:

> variation in this scaling factor from the value of α = 1.0205

Once again, we thank the reviewer for their constructive comments which have improved the manuscript.

Reviewer #2 - Martin Horwath:

The manuscript addresses the effects of vertical crustal deformation on surface height changes of the Antarctic Ice Sheets as measured by geodetic techniques such as satellite altimetry. These effects must be accounted for when inferring changes of ice-sheet thickness and ice mass from measured surface height changes. Corrections for crustal deformation effects have been implemented (at least partly) by previous analyses of satellite altimetry. However, limited attention has been usually given to the uncertainties of such corrections.

Given that the various sources of uncertainty in altimetry-based mass balance estimates are shrinking due to improved sensors and improved analysis, it is relevant and timely to take a fresh look on the uncertainty associated to crustal motion.

The study addresses two types of crustal deformation: glacial-isostatic adjustment (GIA) in response to past load changes; and the deformation induced by "modern" load changes. This latter deformation is usually conceived as purely elastic, but here, the importance of viscoelastic response to multi-decadal load changes is also investigated, and this proves important for regions of low mantle viscosity.

The investigation of the GIA-related uncertainty consists in comparing the results from three GIA models based on three ice histories (IJ05, ICE-6G_C, G18) coupled with Earth viscosity structures (1D for IJ05 and ICE-6G_C, 3D for G18). Differences "IJ05-minus-G18" and "ICE-6G_C-minus-G18" of the crustal motion correction are interpreted as uncertainties of IJ05 and ICE-6G_C and quantified to be 0.028 mm/yr and 0.046 mm/yr sea-level equivalent, roughly 10% of the present-day actual Antarctic Ice Sheet mass change.

The investigation of the crustal deformation due to "modern" ice mass change takes the approach of identifying a ratio (alpha) between ice thickness change and surface height change. A simulated pattern of ice thickness changes over 40 years is employed to calculate this geographically dependent ratio. It is stated that the spatial variability of alpha amounts to 10% of an approximate single value (1.02) that has been adopted previously by Groh et al. (2012). This result refers to the consideration of purely elastic load deformation. When implementing two viscoelastic Earth models with 3D rheologies, alpha becomes considerably higher than for the purely elastic case, so that using the elastic-based alpha would underestimate ice mass change. The associated error is quoted as "4.5 mm GMSL equivalent forty years after present" in the abstract.

The topic merits publication in The Cryosphere, the calculations are sound, the language and the figures are clear.

We thank the reviewer for these positive comments.

However, some important aspects need clarification and more elaboration. This concerns background on how previous altimetry analyses treated the problem; clarity of conclusions; some confusion between the approaches of space-dependent versus average "alpha" ratios; discussion of the distinction between GIA and viscoelastic response to "modern" load change; and maybe terminology;

The study is brief in terms of data analysis and phenomena addressed, so I believe it should shine with clarity. In this way it may become frequently cited when it comes to crustal deformation corrections in altimetry analyses.

Below I specify my major comments.

(1) The review on how crustal deformation has been accounted for previously is limited to reference to a single study (Groh et al. 2012). No context is given on how the numerous Antarctic altimetry studies have addressed the problem. For example, line 130 writes "alpha_l [....] is commonly assumed to be a single scale factor (Groh et al., 2012)". No substantiation is given on who (other than Groh et al. 2012) assumes this mean value.

To include additional context for the use of α = 1.0205 ratio from Groh et al. (2012), we will add the following text to the manuscript:

> Subsequent studies of Antarctic ice elevation measurements have applied the scale factor used in the Groh et al. (2012) paper as a correction in the conversion of ice surface height changes to ice mass changes. For instance, in Schroeder et al. (2019), a study using four decades of altimetry data from multiple satellite missions, surface elevation changes are multiplied by a value of α = 1.0205 to account for elastic solid earth rebound effects. The use of this ratio is not restricted to Antarctica: in Kappelsberger et al. (2021), the same α = 1.0205 is applied to surface elevation changes in northeast Greenland.

While the use of this constant α across Antarctica is accurate in most parts of the continent, the error introduced by this approximation is magnified in the regions where ice mass change is largest and which most impact future GMSL rise.

(2) Please clarify your concept of a pointwise calculation of alpha. Let me elaborate a bit on this comment:

Groh et al. (2012) established alpha as the ratio between mean ice thickness changes and mean surface height changes over certain regions. They were aware of its dependence on spatial scale.

In response to Reviewer #1, we will revise the text to elaborate on the details of the Groh et al. (2012) in the following manner:

On line 53, we will delete the following text since it implies, incorrectly, that our derivation follows the approach of Groh et al. (2012):

> Following the discussion in Groh et al. (2012)

Next, after equation (8) we will add the following explanatory text:

> This expression relates ice thickness changes to ice surface height (or surface elevation) changes (\Delta H) and ocean thickness changes (\Delta S) and makes two assumptions. First, that viscous effects can be ignored and, second, that Earth structure varies with depth alone. The latter introduces negligible error (Mitrovica et al., 2011).

After equation (10), the following text in the original manuscript:

> This expression indicates that the mapping between ice height and ice thickness changes is dependent on spatial scale (via the degree l), as shown in Figure 1.
>
> Another assumption in the above derivation is that the response of the solid earth to modern ice loss in the Antarctic region can be represented by an elastic Earth model. The inaccuracy this introduces will be a function of the timescale of ice loading that is considered and the viscosity of the underlying mantle. In the results section, we investigate this issue by considering both elastic and viscoelastic Earth models. Our predictions also include the impact of water height changes \Delta S_lm on the mapping.

will be revised to read:

> Precisely the same expression was derived by Groh et al. (2012), although their derivation was based on considering the gravitational effects of ice mass changes and crustal uplift, following results in Wahr et al. (1998). The expression indicates that the mapping between ice surface height and ice thickness changes is dependent on spatial scale (via the spherical harmonic degree l), as shown in Figure 1.

> As noted, Equations (9) and (10) assume that the response of the solid earth to modern ice loss in the Antarctic region be represented by an elastic Earth model. The inaccuracy this introduces will be a function of the timescale of ice loading that is considered and the viscosity of the underlying mantle. In the results section, we investigate this issue by considering both elastic and viscoelastic Earth models. Our predictions also include the impact of water height changes \Delta S_lm on the mapping.
>
> Although Groh et al. (2012) also derived equations (9) and (10), they assumed that the second term in brackets in these equations could be replaced a simple constant (.0205) that they computed by considering the value that this second term would take on if one used a spatial scale consistent with the mean scale of Antarctic drainage basins. Their assumption, in this case, that $\alpha = 1.0205$ (red line, Fig. 1), removes the dependence of α on spatial scale.

The manuscript remains unclear about the question whether previous studies have applied alpha in a spatially resolved manner per grid cell. If "yes", then there is reason "to explore the inaccuracy" (line 130) of assuming a constant value for each grid cell. If "no", then there is no one to accuse for such inaccuracy.

As we note above, Groh et al. (2012) made the approximation that $\alpha = 1.0205$ despite their derivation of our equation (9). As we also note above, studies such as Schroeder et al. (2019) and Kappelsberger et al. (2021) apply this constant scaling when converting ice surface elevation changes to ice volume changes, meaning that is it indeed applied to spatially resolved measurements.

For a while, I misunderstood Section 4.2 in the sense that it suggested to define and apply a space-dependent alpha for "mapping" ice surface height changes to ice thickness changes (as the section title suggested). I now think that this is not the authors' intention. The space-dependent alpha is just presented for diagnostic purposes. (Please clarify.)

In that case, the authors may want to discuss recommendations how the "mapping" should be actually done. I believe that the kind of forward-modeling exercised in the study is what you would recommend. If so, please spell it out.

The reviewer raises a very important issue here that was not sufficiently addressed in the original manuscript.

As defined by our equations (8-10), the ratio α is dependent on spherical harmonic degree, that is $\alpha = \alpha(\ell)$, and thus it could not, in that form, be defined per grid cell. However, in addressing the accuracy of the assumption that $\alpha = 1.0205$ we were, as the reviewer notes, interpreting α as a “space-dependent” variable – this is certainly the most direct way to assess the issue of accuracy. But it does not really address the second point the reviewer is making, namely “how should the mapping actually be done”. We have revised the manuscript to deal with both points. First, we have added the following text to the end of the Methods section (beginning with text discussed above):

> Although Groh et al. (2012) also derived equations (9) and (10), they assumed that the second term in brackets in these equations could be replaced a simple constant (.0205) that they computed by considering the value that this second term would take on if one used a spatial scale consistent with the mean scale of Antarctic drainage basins. Their assumption, in this case, that α = 1.0205 (red line, Fig. 1), removes the dependence of α on spatial scale. In the results described below, we assess the accuracy of this assumption by also treating α as a spatial variable and computing it from forward modeling of the crustal response to a projection of modern ice mass change in the Antarctic.

However, while this comparison does quantify the spatially variable error in assuming that α = 1.0205, it doesn't provide an obvious way forward. To this end, we will add the following text to the end of the manuscript:

> To reduce this error, we advocate performing a spherical harmonic decomposition of the altimeter-derived ice surface height and scaling the harmonics using the α (ℓ) filter (Figure 1) to estimate ice thickness changes. While this approach ignores both viscous effects and ocean loading on the isostatic adjustment, we have found that it reduces errors in the estimate of ice thickness changes relative to assuming α = 1.0205. As an example, Supplementary Figure S3 shows a map of the error in the estimated ice thickness change over the next 40 years in the vicinity of the Amundsen Sea when ice surface height changes computed using the H17 viscoelastic Earth model are scaled using the α (ℓ) filter. The peak error of 6.2 m is less than 1% of the peak ice thickness change of ~830 m. The RMS error in the α (ℓ) case is a factor of 2.3 times smaller than the error incurred in adopting the assumption α = 1.0205.

Supplementary Figure S3 is attached as "fig03s.pdf" in the .zip archive and included as Figure 9 in the revised manuscript.

(3) Following from the previous comment: What information can the reader really get from the results (in the abstract) that alpha varies spatially by 10%, given that this is based on arbitrarily masking out alpha values over most of the ice sheet surface?

We feel that citing this spatial variation of α is an important point to make but we agree that we should have better motivated our choice of the ice height cutoff. Applying a cutoff is necessary because in its absence there will be large variations in α in areas where ice surface height changes are small and contribute relatively little to the ice mass balance. Our choice of a 10 m cutoff yields a loss of only 10% in the integrated ice mass change of the fETISH32 (EXP A1) projections to 2055 CE.

In any case, the original manuscript should have specified the cutoff in the abstract, so we will revise the following sentence (line 13):

> We adopt modeling results based on a projection of Antarctic ice mass flux over the next 40 years to demonstrate a spatial variability in the scaling of up to 10% across the ice sheet.

to read:

> We adopt modeling results based on a projection of Antarctic ice mass change over the next 40 years to demonstrate a spatial variability in the scaling of up to 10% across the ice sheet over areas in which ice surface height changes are greater than 10 m.

Furthermore, we will expand on the relevance of the cutoff by adding the following text to the manuscript:

> We note that regions with at least 10 m of ice height change yield 90% of the total Antarctic contribution to GMSL rise in the fETISH32 (EXP A1) projection to 2055 CE.

(4) Including viscous deformation in response to "modern" load change makes results hard to interpret. In the manuscript, a simulated 40-year ice mass change starting from 2015 is implemented. I assume the viscous part of the response assumes no load change prior to 2015. But in fact, there has been load change prior to 2015. Hence, much of the numerical result is determined by the arbitrary onset in 2015.

The reviewer raises an important issue that we will address in the revision. In the original manuscript we considered two sets of simulations and their impacts on altimeter estimates of modern ice thickness changes in the Antarctic. The first involves GIA simulations in which ice sheet changes cease several thousand years in the past and the second considers projections over the next 40 years. The reviewer is correct to point out that ice sheet changes in the centuries prior to 2015 will also lead to ongoing crustal displacements that should be corrected for in estimating ongoing ice mass changes. To this end, we have added an additional calculation in the GIA section of the manuscript that explicitly addresses this issue. The new text reads as follows:

> To end this section, we augment the GIA calculation to consider the ongoing impact of Antarctic ice mass changes over the past century. We constructed the ice model by first adopting the ice history from 2003-2015 of Schroeder et al. (2019). This loading history has a 10 km spatial resolution and a melt geometry largely focused on the Amundsen Sea Embayment Region. To extend the loading history back into the 20th century, we follow the method described in Barletta et al. (2018). We computed the average ice thickness change per year of the Schroeder et al. (2019) ice model, scaled it by 25% and applied it across the period 1900-2003. This history yields a total GMSL rise of 10.8 mm. The mean crustal displacement rate over the period 2015-2055 computed using the 3D viscoelastic Earth model is shown in Supplementary Figure 2. The integral of this field over the Antarctic maps into a correction to any altimeter derived estimate of ice volume change from 2015-2055 equivalent to 0.008 mm yr^{-1} of global mean sea level.

Supplementary Figure 2 is labeled as "fig02s.pdf" in the attached .zip archive and included as Figure 5 in the revised manuscript.

Rather than puzzling the reader with a few single numbers (such as in the abstract: "4.5 mm GMSL equivalent") I would find it instructive to sea a curve that shows how the alpha factor evolves over time after the assumed onset of mass change, that is, after the onset of a viscous reaction to recent mass change.

First, we don't feel that our calculation that the "error in the projected net ice volume change of up to 4.5 mm GMSL equivalent forty years after present" would be misinterpreted by the reader. The number is rigorously defined in the manuscript as the upper bound on the error in estimating the net ice volume change, in units of GMSL, incurred by adopting the constant α = 1.0205.

However, we agree that it would be instructive to see how the value of α varies across the 40-year period. To this end, we will add a new Supplementary Figure 1 to the text that explores the (spatial) variation of α at 10-year intervals along a profile through the Amundsen Sea sector of the Antarctic Ice Sheet. We will introduce this figure with the following text at the end of Section 4.2:

> To explore the time history of the spatial variation in α, Supplementary Figure 1 shows the variation of this ratio, as well as total ice thickness change, in 10-year intervals along a profile through the Amundsen Sea sector of the Antarctic Ice Sheet (see Figure 5) computed using the H17 viscoelastic Earth model. (The choppiness of the profiles reflects the ~20 km spatial grid of fETISH32 Exp. A1 ice history.) A trend in the profiles is apparent in the first 30 years, but this trend reverses by the end of the simulation. The value of α comes closest to 1.0205 in the vicinity of the largest ice thickness change, but systematically diverges from this number moving away from this location in areas of significant ice mass change.

Supplementary Figure 1 is attached as "fig01s.pdf" in the .zip archive and included as Figure 8 in the revised manuscript.

The authors may also want to clarify the distinction between GIA and the viscoelastic response to modern load change. (I no this is an ongoing discussion.)

This is an important point. We have separated the Antarctic ice history into a GIA component, which includes ice age cycle loading – the three models we consider, G18, ICE-6G and IJ05 do not have ice mass change over the past few thousand years – and projected loading from 2015-2055 CE. As we note above, we now also include calculations based on an ice history model from 1900-2015. We feel it is most appropriate to include this latter ice history in the GIA section of the manuscript since it represents a "past" evolution of the ice sheet.

Next, I add a number of additional comments:

I find the manuscript title confusing in some aspects. (That is said with a big portion of humility by a non-native English speaker.) I would prefer to straightly mention "crustal deformation effects", or something alike, in the title. Otherwise the reader could also think about a treatment of the firn-versus-ice density problem, for example. I am also confused about the use of "changes" versus "variations" in the title. This may sound like addressing two phenomena of somehow different spatio-temporal characteristics, which need to be "mapped" one onto the other.

We will revise the title to read:

> Mapping geodetically inferred Antarctic ice surface height changes into thickness changes: a sensitivity study

Some aspects of terminology:

- You use "ice mass flux" where I would prefer "ice mass change". "Ice mass flux" (through surface mass balance and glacier flow) can be large where the net effect, that is, "ice mass change", is zero.

Agreed. We will revise all uses of the term "ice mass flux" to "ice mass change".

- As a matter of taste: I would prefer "ice surface height" or "ice surface elevation" to "ice height". "Ice height" is simple to confuse with "ice thickness".

Agreed. We will revise all uses of "ice height" to "ice surface height".

- The authors themselves suffer this confusion when, in line 57, they explain Delta I as the coefficients of "ice height change", where it should be "ice thickness change".

This error will be corrected.

- line 21f "to convert [..] surface elevation [..] into [..] ice mass" should rather read "to convert [..] surface elevation change [..] into [..] ice mass change", I believe.

This text has been revised to read:

To convert these measurements of surface elevation change (henceforth referred to as changes to ice surface height) into estimates of ice mass change, several corrections must be applied.

- line 126 and line 169: the difference between two versions of the GIA correction (e.g. IJ05 minus G18) is termed "uncertainty". To my mind, "uncertainty" is a statistical description of "error", where "error" is the (unknown) deviation of the estimate from the truth (according to https://www.bipm.org/documents/20126/2071204/JCGM_100_2008_E.pdf). Do you mean in that sense? Or do you follow a different concept and terminology? Just clarify.

As we note in responding to the next comment by the reviewer, we believe that the G18 is the more accurate of the three models. One might ask, why are we considering the other two models in that case? The reason is that results based on GIA simulations using 1D Earth models and independent of any ice physics constraints (e.g., ICE-6G or its predecessor, ICE-5G) are, to our knowledge, the only ones used in correcting altimetry-determined ice surface height measurements for GIA – Groh et al. (2012) used ICE-5G and the original IJ2005. In any case, we understand the reviewer's comment that our use of "uncertainty" is not meant in the strictly statistical sense. We will thus revise the text beginning on line 121 of the original manuscript:

> For example, let us assume that the G18 model, derived from a GIA calculation based on a more realistic, three-dimensional Earth model, and consistent with ice sheet physics, provides

> the most accurate prediction of uplift rates. Then, the differences in uplift rates in Figure 4 represent a relatively accurate proxy for this uncertainty given that the ratio of uplift rate to ice thickness change is close to 1 for the case of modern ice mass flux (see below).

to read:

> We assume that the G18 model, derived from a GIA calculation based on a more realistic, three-dimensional Earth model, and consistent with ice sheet physics, provides the most accurate prediction of uplift rates. Then, the differences in uplift rates in Figure 4 represent a reasonable proxy for this uncertainty (though we are not using this term in a rigorous statistical sense) given that the ratio of uplift rate to ice thickness change is close to 1 for the case of modern ice mass change (see below).

The manuscript assumes that the G18 model is considerably more accurate than the IJ05 and ICE-6G_C. This lead, e.g., to the statement in line 11 (abstract): "ice histories characterized by relatively high excess ice volume at the Last Glacial Maximum may be introducing significant error in estimates of modern melt rates." Could you give evidence that supports this assumption?

As indicated by the above quote, we do make clear that this is an assumption. However, we note that an independent reconstruction of the post-LGM Antarctic Ice Sheet based on geophysical modeling and a wide range of geological and glaciological constraints (Whitehouse et al., *Quat. Sci. Rev.*, 32, 1-24, 2012) is quite similar to G18. We chose to use the G18 model instead because it has the added benefit of being fully coupled to an ice sheet model.

The last sentence of the abstract sounds misleading as it talks about an "error in the projected net ice volume change", as though the projection itself was in error. Overall the fact that the simulated mass change is a projection is not too relevant for the study and needn't be stressed, I guess. It would be even more instructive to use some synthetic version of the recent past, that has been really observed by altimetry. This might by clarified in order not to mislead the reader.

We have deleted the word "projection" from the last sentence of the abstract.

Eq. 2 and line 59: Is Y_lm complex? Is this convention consistent with summing m from -l to l in Eq. 1?

Yes, the Y_$_{\text{lm}}$ are complex, as the text below equation (2) makes clear and this is consistent with the summation (e.g., see the Jackson textbook in electrodynamics).

Section 3 and 4: To my taste, the section titles "Theory" and "Methods" could be more specific. In particular Section 3 limits itself to some theory of the elastic load deformation.

We do feel that "Methods" is appropriate because the section covers the calculation of both the ice age signal and the response to modern melting. We will revise the title of the "Theory" section to read "Elastic Loading Theory".

line 125 "These differences, when integrated over the whole of Antarctica": Make cristal-clear what the integration region is.

This integration is performed over an area defined by grounded portions of the ice model. We will add text to make this explicit.

Fig. 3 and 6: You may ultimately want to save page space and align the three subfigure horizontally.

We will make this change to the manuscript.

Does the term "Mapping ice height changes into ice thickness changes" include the correction for GIA or does it exclude the correction for GIA? The former is suggested by the use of this wording in the manuscript title, while the latter is suggested by the use of this wording in the Section 4.2 title.

In Section 4.2 we focus entirely on the 2015-2055 ice history, and it excludes GIA. But the title of the manuscript reflects the fact that this mapping is investigated for both the GIA correction and the 2015-2055 case. These issues are treated independently in Sections 4.1 and 4.2, respectively.

line 130: the approach by Groh et al. is maybe better described as "approximating" alpha by a single scale factor, rather than "assuming" it was a single scale factor. Groh et al. mention the scale-dependence.

We will revise the text “assumed to be” to “approximated as”.

line 136: The cutoff at areas with less than 10m ice height change over 40 years must be motivated. This arbitrary cutoff affects the results reported in the abstract where no reference to this arbitrary cutoff is given.

As we noted above: applying a cutoff is necessary because in its absence there will be large variations in α in areas where ice surface height changes are small and contribute relatively little to the ice mass balance; a 10 m cutoff yields a loss of only 10% in the integrated ice mass change of the fETISH32 (EXP A1) projections to 2055 CE; and changes will be made to the text to make these points explicit.

In the second half of Section 4.2, errors of total volume change committed when assuming the constant alpha = 1.0205 are quoted for the case of viscoelastic deformation. It would be interesting to see the same assessment for the case of purely elastic deformation in the first half of Section 4.2. I guess alpha = 1.0205 does very well in this case. This might be worth mentioning.

We agree. We will note this value in the text, where we will write:

> The analogous value for the case of the purely elastic Earth model is 2.5 mm GMSL equivalent.

Fig. 6: The thin lines are unexplained.

A description will be added to the figure caption.

Once again, we thank the reviewer for their constructive comments which have improved the manuscript.