Response to comments on TC-2022-44, second round.

We thank Bert Wouters and the two anonymous referees for their further attention to this manuscript. We have taken almost all of the suggestions into account, and are glad to have the opportunity to clarify some of the remaining confusing aspects of our study. I have interposed our responses to Dr. Wouters and to the referees in blue: Our dialog is shown in times new roman font, and any quotes from the revised manuscript are shown in *italics*.

--Ben

Editor's comments:

Comments to the author:
Dear Ben and co-author,

The referees have commented on your revised manuscript and are satisfied with the changes that were made. Your manuscript is now almost ready for publication, but I would like you to make the following, final changes:

• Be consistent in use of "ice sheet" when not an adverb (e.g. line 283: "For the ice-sheet as a whole" -> "For the ice sheet as a whole"). Use height changes for temporal height changes in the observations/models, and height differences when comparing observations and models (R3)

We corrected three instances where we had "ice-sheet" in place of "ice sheet." Thanks for noticing

• Split the sentence starting on line 26 in two (second sentence starting with "The third model…"

Done.

• Provide references for the statements on lines 37-39.

Please see our note on R1's comment. It appears to be standard practice in the literature to state that vertical stretching of ice associated with horizontal flux divergence is part of the local mass balance of the ice sheet without providing a reference. We added a parenthetical note to explain what we meant.

• Fig. S3: use "GSFCv1.1 and v1.2" instead of "GSFC v1.1 and v1.2.1"

Done

• Lines 243-246 was unclear to R1, who seems to interpret the changes in point density to be dependent on elevation. It may help to explain that there are more measurements in the North than in the South due to the converge of the ground tracks, i.e. latitude dependent (unless I'm misinterpreting the sentence…)

It sounded to us as if R1 was reading this as a paragraph about snow density. We made sure to refer to "measurement density" to help avoid further confusion.

• Add units on line 292, and explain what z_melt is (this may also address the comment of reviewer 3 concerning not using accumulated melt from the models).

Fixed

• Figure 2: explain in caption why some of the heights are (not) connected by solid lines (I assume the unconnected dots are from the first two cycles?)

You are correct about the crossovers, plus there are data gaps later on. We added a note to the caption explaining this.

• Check if reference on line 370 to figure 4 is correct. Include references to figures 4C and 4K, and to figure 4E/M on line 372, as suggested by R1.

Fixed

• Line 373: mention explicitly which season you are referring to.

Fixed.

• Clarify what the difference is between “total height” and “surface height”, or stick to one of the two.

Fixed.

• R3 notices that the accumulated melt is computed but not used in the analysis. If z_melt is indeed equal to the accumulated melt (see previous comment), this problem is solved. Otherwise, remove reference to accumulated melt on line 236.

We fixed table S1 and made sure to define z_melt as ‘accumulated melt’ in the text.

• The start of section 2.3 was confusing to R3, I suggest to change this to:

We reduce the full set of 57 million height-difference measurements from ICESAT-2 to a more compact sample with a more even spatial distribution by calculating a block-median set of height differences for each cycle-to-cycle (~91-day) epoch. For each epoc, we group the height differences into 2.5 km grid cells for each ICESat-2 pair track. For each such cell in each epoch, we identify the measurement (or measurements) that matches (or bracket) the median height difference. For each model, we then record the model parameters (i.e. surface height, model total height, model SMB anomaly, model235 FAC, and model accumulated melt) corresponding to the median value. For each median difference measurement, we sample each of our model fields at the location and time of the height measurements, and calculate their differences. This gives a set of model-field-difference values that are precisely collocated with the measured differences.

We made (approximately) this modification. Thanks for offering a clear rewrite of the paragraph.

Furthermore, the referees have made suggestions which I believe would further strengthen the paper, but I leave the choice on implanting these to you.

Please see our responses to the referees below.

- L32: May be more succinct to say “mass changes of ice sheets”.

Fixed

- Section 2.3.2: You can take this or leave it, but it may be helpful to make the two dh’s clearer by calling one dh_meas and the other dh_mod. I know currently you distinguish them by dh and dh_m, but it may be more helpful for readers to keep track with more descriptive subscripts? If you choose to implement this change, make sure it is changed throughout (e.g. Figure 5, 7, 8).

This is a good idea, but would require substantial modifications to the text, the equations, and the figures. With regret, we are not following this suggestion

- The results for the high-elevation subset of your data suggests that there is an overestimation of snowfall in the interior part of the ice sheet (fig 7A-C and 8A-B) in GSFC 1.1 and MAR (and possibly also GSFC 1.2) because the regression leads to a lower dhm and dhSMB. This may be worth pointing out in the text.

Please see my response to referee 3’s point 5. The residual reductions for the dhSMB rescaling are very small, and don’t provide good evidence one way or the other.

- Instead of making an educated guess about what resulted in the reported overestimation of dhFAC (densification rate, amount of melt, surface density), this variable could tell us what which process is responsible. For example, from figure 2 it seems that MAR overestimates melt in the low elevation regions, but GSFC underestimates melt. It would be nice if you could discuss this.

This question refers to the z_melt variable. We didn’t include an explicit regression WRT z_melt because errors in z_melt do not have a consistent relation to surface-height changes. We had tried something like this in developing the study, but its interpretation was not straightforward.

Congratulations on a very interesting paper!

Kind regards
Bert Wouters

Comments from referee 1:

I would like to thank the authors for revising the manuscript to improve the clarity and allow readers to better understand the methodology used. I recommend this manuscript be published subject to minor technical corrections. Below, I list just a few suggestions that I think would help to further improve clarity of the paper. The authors can decide whether they choose to implement the changes or not – none of them will impact the scientific findings of the paper. Congratulations on this impressive study!

Thanks for the kind description of the study, and for the recommendations. We realize that this manuscript is a bit dense in places, and are glad that Dr. Wouters was able to find referees who were game to go through it carefully.

Throughout the text, you use both "ice-sheet" and "ice sheet". This should be made consistent. I would recommend using "ice sheet".

We had a look through the paper, and made sure that we were using "ice-sheet" as an adjective, and "ice sheet" as a noun. I don't think we ever used it as an adverb (which would have no hyphen).

L27: by using "and" here in "and the third model…" makes it seem like GSFCv1.2 exacerbates the overprediction. I would suggest changing this to something like "however, the updated high-elevation melt prediction in GSFCv1.2 avoids this overprediction." Or something along those lines.

We split this sentence into two:

This overprediction seems to be associated with the melt sensitivity of the models in the high-elevation part of the ice sheet. The third model, GSFCv1.2, which has an updated high-elevation melt parameterization, avoids this overprediction.

L32: May be more succinct to say "mass changes of ice sheets".

Fixed.

L37-39: Could you provide some references here? The SMB expected change is obvious, but may be nice to provide some citations for variation in the local stress balance.

To us, this statement seemed somewhat axiomatic (note that the statement is about flux divergence, not about stress) so we examined a few other papers that treat the relationship between horizontal velocity and ice-sheet mass balance, and didn't see that any of them offering a citation for a similar statement. We added a parenthetical example to help avoid confusion:

On an ice sheet in steady state, whose volume and mass are constant in time, snow accumulation and ice ablation at the surface are balanced by ice-flux divergence in the ice-snow column (i.e., by thinning or thickening of the ice column related to horizontal stretching of the ice),

L243-246: This new sentence is a little unclear to me. The distribution of density differences is not spatially uniform, but to me, it seems better to say that the density differences are spatially coherent (or elevationally-coherent, or something like this) – density increases are concentrated in the low-elevation ice sheet periphery, while density decreases are found in the high-elevation interior.

I think the referee misread this paragraph: The density in question here is the number of measurements per unit area, not the physical density of the snow. Reading the paragraph over, it seems that what's written is reasonably clear, but to further help avoid confusion, we made sure to always refer to “measurement density” or “measurement-density values.”

Fig S3: Should the caption use “1.2” rather than “1.2.1” to remain consistent with the main text?

Fixed.

Section 2.3.2: You can take this or leave it, but it may be helpful to make the two dh’s clearer by calling one dh_meas and the other dh_mod. I know currently you distinguish them by dh and dh_m, but it may be more helpful for readers to keep track with more descriptive subscripts? If you choose to implement this change, make sure it is changed throughout (e.g. Figure 5, 7, 8).

This is a good idea, but would require substantial modifications to the text, the equations, and the figures. With regret, we are not following this suggestion

Line 292: Add units denoting the subsets. E.g. (h < 2000 m).

Fixed.

Line 292: Maybe make it clear where the z_melt comes from (I assume it is the model accumulated melt). You have mentioned model parameters before, and that one is model accumulated melt, but then all of a sudden the z_melt variable appears. You could just say “into strong-melt subsets using the model accumulated melt parameter, z_melt, ….”

We now specify that z_melt is accumulated melt, and have fixed this in table 1.

Figure 2: Very minor comment – but why are some of the heights from different RPTs in panels D-O connected by solid lines, while others are not?

Comment added to the caption:

Time series for each RPT are joined by a solid line when derived from continuous repeat-track measurements. Broken lines or lone points indicate crossover measurements or missing values in the repeat-track measurements.

Line 370: I think you are now referencing Figure 4 here since you added a new figure above? Also, I would reference Fig. 4C, 4K after you say “resulting in positive corrected values”.

Fixed

Line 372: Add reference to Figure 4 (maybe panels E,M?) to illustrate the overestimates of thickening during colder seasons.

Fixed

Line 373: “During that same season”… I assume you are talking about Q2 of 2019? I would explicitly say that here, because the previous sentence ends with you discussing the colder seasons, and I don't see readily apparent large discrepancies in the bare ice zones between models during this time.

Fixed

Comments from Referee 3

I thank the authors for the work they have done to address the comments from the other reviewers and me on the previous version of the manuscript. The current work is a clear improvement and I recommend that the paper can be published after some minor edits aimed mostly at improving the clarity of the text.

Thanks for the recommendation, and for the further attention to the manuscript. These changes will be quite helpful to the readers.

Comments:

1. Section 2.3's wording was confusing to me. Do the authors calculate the blockmedian for the ICESAT-2 measurements or for the model results? With the sentence 'we assign the height differences into 2.5 km bins', do the authors means that they combined the ICESAT measurements into a map of 2.5 by 2.5 km grid cells? I recommend rewording this paragraph to improve its readability.

Sorry for the confusion! We rewrote this paragraph as follows (with thanks to B.W. for his recommended changes):

For each model, we reduce the full set of 57 million height-difference measurements from ICESAT-2 to a more compact sample with a more even spatial distribution by calculating a block-median set of height differences for each cycle-to-cycle (~91-day) epoch. For each epoch in each model, we assign the height differences into 2.5 km cells for each ICESat-2 pair track. For each such cell in each epoch, we identify the measurement (or measurements) that match (or bracket) the median height difference. For each median difference measurement, we sample each of our model fields (i.e. model total height, model SMB anomaly, model FAC, and model accumulated melt) at the time and location of the height measurements, and calculate their differences. This gives a set of model-field-difference values that are precisely collocated with the measured differences.

2. At line 235, the authors mention that they collected the accumulated melt for each model, but never use it in their analysis of their regression experiments. Why is this variable not used? Instead of making an educated guess about what resulted in the reported overestimation of dhFAC (densification rate, amount of melt, surface density), this variable could tell us what which process is responsible. For example, from figure 2 it seems that MAR overestimates melt in the low elevation regions, but GSFC underestimates melt. It would be nice if you could discuss this.

In developing the paper, we did regressions against z_melt, but did not find a concise way to interpret the results. We added an excuse for not describing these regressions around line 300 in the revised manuscript:

We perform regressions for the total model change (dh_m), the height change due to SMB anomalies (dh_{SMB}), and the height change due to firn-air content (dh_{FAC}). We use the modelled total melt to segregate the data into strong-melt ($z_{melt} > 0.2\ |dh_m|$) and weak-melt ($z_{melt} < 0.2\ |dh_m|$) subsets, but do not perform explicit regressions between surface-mass-balance change and melt because melt does not have a consistent linear relationship with surface-height change. The height change associated with melt depends on the density of the snow or ice being melted, and on whether the meltwater runs off the ice sheet or is refrozen, *which makes the results of a regression between z_{melt} and dh more difficult to interpret than those for the other variables.*

3. In figure 2, line 235 and line 325 the model total height is mentioned, but also the surface height. Are these variables different from each other? If so, how are they different? If not, I recommend using only one or the other term.

Our inclusion of "surface height" in the list of parameters was a mistake. We looked through the rest of the paper, and where we were referring to something predicted to the model, used the term "total height."

4. The authors use 'height differences' and 'height changes' interchangeably. This was especially confusing while reading the methods section. Please use 'changes' to indicate measured or modelled surface elevation changes and reserve the word differences for when you are comparing ICESat-2 with the model data.

We acknowledge the confusion that using two different terms can cause, and made some wording changes to be more consistent. We now use "height differences" and "difference measurements" to refer to what we measured with the satellite, and use "height changes" to refer to what happened on the ice sheet, and what the models predicted. We refer to what is obtained when a modelled height change is subtracted from a measured height difference as a "residual." We hope that this makes our results easier to follow.

5. The results for the high-elevation subset of your data suggests that there is an overestimation of snowfall in the interior part of the ice sheet (fig 7A-C and 8A-B) in GSFC 1.1 and MAR (and possibly also GSFC 1.2) because the regression leads to a lower dhm and dhSMB. This may be worth pointing out in the text.

This is true, but we feel that the evidence for an overestimate of snowfall is not very strong. The changes dh_m in these conditions results in very small changes in the residuals compared to what we see when we vary the FAC component.

We have added a note at 538-541:

> *The small reductions in residual spread that result from rescaling the SMB alone in the high-elevation part of the ice sheet for MARv3.11.5 and GSFCv1.1 (Fig. 7, A-B, Fig. 8, A-B) might provide weak evidence that the models overestimate SMB variability in this region, but the reductions in spread are much smaller than those associated with rescaling the FAC, suggesting that our analysis is not strongly sensitive to SMB scaling in this area.*