Reviewer #2

We would like to thank the reviewer #2 for its constructive comments that will improve the paper. We have responded to all of them and will modify our paper accordingly. The majority of the highlighted issues of the paper come from its writing style. The paper will undergo a restructuring and will be sent to a native english speaker after rewording to reduce confusions. Our point-by-point answers (in bleu) follow as a supplement.

Major comments:

I miss the applicability of this assimilation technique for Antarctica. In Antarctica melt-rates are relatively unimportant in the contemporary climate, and for future simulations this assimilation technique obviously does not work. What will this technique provide us with? Some extra words/paragraphs, either in the introduction, or in the conclusions, or both, should be spent on this to improve the relevance of this paper.

> In strictly accounting terms, it is true that melting and associated runoff are "second-order magnitude" components of the mass balance computation. However, the surface hydrology triggers other processes (e.g. water loading, ponding, hydrofracturing) that destabilize ice shelves and, therefore, continental ice. In addition, it may not be the case in the future (Gilbert & Kittel, 2021). Unfortunately, we do not have a long time and large-scale observations of the surface melt production in Antarctica that can be used to calibrate models. Remote sensing data are the only dataset for detecting the presence of water we can integrate into the model to constrain the presence of liquid water in the model. The purpose of the paper will be more explicitly integrated into the text of the revised version by concluding the introduction with a paragraph explaining the purpose of the applied technique. The paragraph will be in the style of: "*Assimilation of data into the model is a crucial step in quantifying the uncertainties associated with the model's output without assimilation. The assimilation process helps to identify areas of the model where the modelisations are not consistent with the observations. This can help us to better understand the underlying physical processes and their interactions. In this way, data assimilation provides a powerful tool for improving the reliability of models. In our case, it is an essential step in the process of model refinement, leading to improved predictions of future scenarios.*"

Slightly related to point 1, I miss a recommendation based on the results of this study. Would the authors advise to use this technique on all future simulations, or is the main aim to provide better uncertainty estimates? I advise the authors to take a stronger stance on what is the main take-home message of the study.

> As Reviewer 2 mentioned in the first point, remote sensing datasets have only become available in the past few decades, and until now, the Antarctic melt rate may

be considered relatively unimportant. The main aim of the study was to study the sensibility of the model to assimilation to obtain an idea of the uncertainties of the model and intra variability of the assimilation technique itself and modeled melt amount. By obtaining an "ensemble model" that can be compared to MAR, it would be possible to quantify, or at least better estimate, the uncertainties of the model regarding the liquid water content of the snowpack. This would enable the correction of hydrological processes within MAR (without assimilation) to improve its accuracy. This refined mode would enable long-term simulations with a better estimate of the liquid water content of the snowpack in the future. With regards to point 1, a paragraph stating the main message of the paper will be included in the revised version. This paragraph will summarize the relative sensitivity of MAR to the presence of liquid water in the first centimeters of the snowpack and its current predisposition to make water percolate while remote sensing data still observe water.

I miss a thorough evaluation of the actual modelled surface melt. There are several AWS on the AP that close the SEB (Jakobs, C. L. et al., (2020)) and enable a much more detailed and independent evaluation of simulated melt production. In turn, these can then be used to actually provide (a part of) the uncertainty calculation that the authors hint at in the last sentence of the abstract, which would really improve the papers conclusions and applicability (see point 1).

As also pinpointed by the other reviewer, the MAR evaluation will be explained in further details in the revised version and the dataset with which the evaluation is performed will be better described. The surface melt can also be evaluated by comparing the results with Jakobs et al. (2020). The results of this evaluation still need to be made carefully. Jakob's dataset also remains a modeled-based estimate, with its own biases and limitations, and therefore cannot be used as if there were in situ measurements, and so a reference for the modeled estimations. However, a short comparison of MAR_{ref} with the AWS used by Jakobs available on The Antarctic Peninsula could be added to the paper.

Nevertheless, it is important to remind that the study does not aim at providing better melt estimates but rather testing the sensitivity of the model. We only evaluate MAR without assimilation as we know that the value given after assimilation may differ from the observations. However, while the MAR simulations without assimilation were conducted for the period of 1980 to 2022, the "assimilations" were only performed from 2019 to 2021, which renders their comparison with the AWS dataset results presented in Jakobs et al. (2020) impossible.

As represented in the Figure here under (Figure R2.1) we compare the melt estimates from the AWS described in Jakobs et al. (2020). to the surface melt production of the 4 closest MAR pixels of the AWS. MAR has a tendency to overestimate some extremes of melting while simultaneously underestimating or overestimating the duration of periods during which the ice shelves are experiencing melting. Even if it is important to note that there can be a difference in altitude between the AWS and MAR pixels that explains the differences between the two datasets, this comparison also highlight the importance of nudging MAR to correspond to the remote sensing observation of wet snowpack.

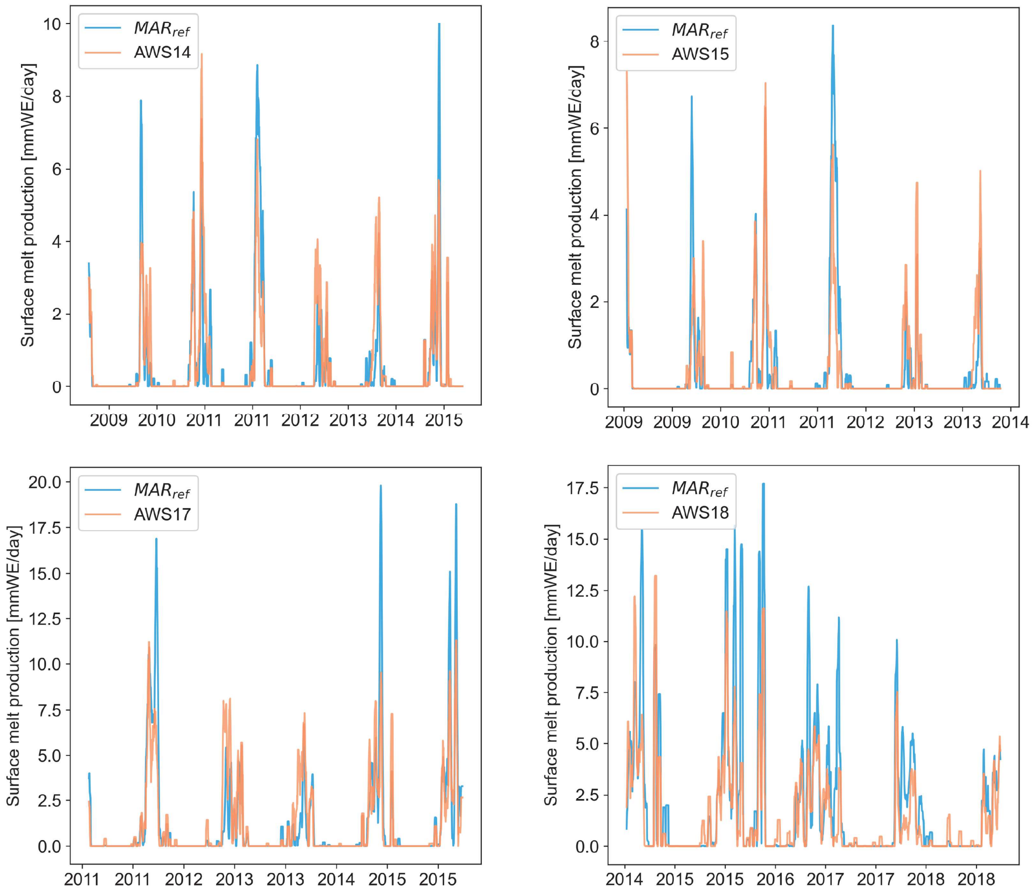

Figure R2.1 : Comparison of surface melt production as modeled by MAR without assimilation and estimated surface melt production from AWS 14, 15, 17, and 18 described in Jakobs et al. (2020).

Minor (line by line) comments

The paper will be restructured, especially the introduction and methodology, with a stronger contribution of the co-authors and the help of a native English speaker.

L2-5: unclear. Too much detailed and lengthy information for an abstract, can be considerably shortened by just writing something like the following: "However, RCMS are *subject to biases, which Remote Sensing (RS) products can help solving. Here, we assimilate several satellite products that detect surface melt into the RCM MAR…" etc.*

The abstract will be rewritten to be simplified and focused on essential points. In parallel, other important information such as the problematic related to *in situ* observations and remote sensing will be included in the revised version.

L10-11: This seems ambigiuous, are the previous two methods not also assimilations?

The confusion results from a poor choice of word. The first two parameters are thresholds on assimilation depth and liquid water content used for the assimilation and the third is the choice of assimilated sensors.

L14-15: Way too detailed for an abstract. Shorten

As stated in comment L2-5, the abstract will be shortened and simplified in the revised version. L14-19 will be shortened to only state the sensitivity of MAR to the assimilation depth and its impact on surface melt production.

L17: A refreeze of what?

A (night)refreeze of the meltwater produced during the day. It will be clarified in the text of the revised version.

L22-23: Good to end the abstract with this (but I expect you to end the conclusions section likewise). Can you extend slightly on this?

Conclusion will be extended with a more detailed study of the change in LWC that will be conducted in the result section in the revised version. The study of the evolution of LWC and the saturation of the snowpack will enhance the message of this paper.

Abstract overall: Please shorten and simplify the abstract!

L25: Here you mention both polar ice sheets, and in the following sentence you immediately move to Greenland. This transition can be improved.

Sentence will be revised into: "*More than two-thirds of the Earth's freshwater is held in the polar ice sheets (Church et al., 2013), with the majority of it trapped as ice on the ground at the south pole, forming the Antarctic Ice Sheet (AIS). According to Fretwell et al. (2013), if all the ice in the AIS were to melt, it would result in a sea-level rise of 56 meters.*"

L27: Here you should distinguish between grounded ice mass loss and actual mass loss, especially if later in the introduction you want to emphasize the importance of surface melt (i.e. hydrofracture and grounded ice acceleration)

The distinction will be made by changing the sentence to: "*Currently, the Antarctic Ice Sheet (AIS) is primarily losing mass due to grounded ice flowing into the ocean. There, the ice is lost mainly through a combination of basal melting and calving.*"

L28: Why is surface melting not yet a big concern now?

In strictly accounting terms, it is true that melting and associated runoff are "second-order magnitude" components of the mass balance computation. However, the surface hydrology triggers other processes (e.g. water loading, ponding, hydrofracturing) that destabilize ice shelves and, therefore, continental ice. In addition, it may not be the case in the future (Gilbert & Kittel, 2021). Unfortunately, we do not have a long time and large-scale observations of the surface melt production in Antarctica that can be used to calibrate models. Remote sensing data is the only dataset for detecting the presence of water we can integrate into the model to constrain the presence of liquid water in the model. The purpose of the paper will be more explicitly integrated into the text of the revised version by concluding the introduction with a paragraph explaining the purpose of the applied technique. The paragraph will be in the style of: "*Assimilation of data into the model is a crucial step in quantifying the uncertainties associated with the model's output without assimilation. The assimilation process helps to identify areas of the model where the modelisations are not consistent with the observations. This can help us to better understand the underlying physical processes and their interactions. In this way, data assimilation provides a powerful tool for improving the reliability of models. In our case, it is an essential step in the process of model refinement, leading to improved predictions of future scenarios.*"

L36: RCMs are not yet introduced, rephrase

Noted and it will be changed to "*Regional Climate Models (RCMs) are nowadays one of the effective tools to monitor the ice shelf evolution by enabling to model their past, present, and future climate. For example, MAR (for "Modèle Atmosphérique Régional" in French) has been developed to monitor the polar ice sheets. However, RCMs still have some limitations.*" in the revised version.

L38: what do you mean with "other independent sources of uncertainties". Vague!

We wanted to state that the data included should not have the same source uncertainty as the model. That is to say that we do not include a dataset that is based on MAR and/or already included in MAR. We will rephrase it "*These* *uncertainties can be mitigated by employing external data, which is not already indirectly incorporated into MAR, to improve the model's accuracy at specific points in space and/or time.*"

L39-41: How does this assimilation technique compare to other common techniques such as reanalyses?

Reanalyses such as ERA5 use much more complex assimilation techniques than nudging. Reanalyses assimilate observations in a forecast model by taking into account temporal and spatial variability of the observations. Here, nudging only consists in slightly adjusting the model at each time set to match the observations.

L42: What do you mean with sequentially? Reword

Will be rephrased to *"we assimilate satellite-derived surface liquid water presence over the Antarctic Peninsula (AP)"* in the revised version. The world *sequentially* means that, for each time step of the model, we try to match the models and the remote sensing observations. The ontologies related to data assimilation techniques will be explained in subsection 2.3 dedicated to the assimilation algorithm.

L45: what is a complex surface hydrology?

Complex in a sense of the variety of the hydrological structure and related processes, and the fact that the water streams do not directly come out of the ice sheet in visible rivers. This part will be reworded to "*These ice shelves undergo most of the surface melt of the AIS. Their surface and subsurface processes are poorly understood due to challenges in making direct observations caused by their complex surface hydrology. (Barrand et al., 2013; Datta et al., 2019; Johnson et al., 2020). "*

L46: isn't it more like 10km scale?

It depends on the size of the model domain. The model could have been run at 5 km instead of 7.5 km. It will be changed to "10 km scale" to be more realistic with the diversity of spatial resolution used in literature.

L47: rephrase.

It will be rephrased to "*Phenomena such as melt induced by the Foehn effect can occur at a smaller spatial scale than the spatial resolution of RCMs and thus may not be correctly represented the model (Datta et al., 2019; Chuter et al., 2022; Wille et al., 2022).*"

L49: "multiple" comes out of the blue and confuses me, rephrase.

It will be rephrased to "I*n this study, we use four remote sensing datasets. This enables us to perform assimilation over the entire studied zone every day for two melt seasons (2019-2020 and 2020-2021), even if one of the datasets is missing acquisitions for one or multiple days.*" in the revised version.

L50-51: Is it, or will it be, a promising technique? Outside of Kittel 2022, there is not really any other study doing this right?

Assimilation in general is a promising technique, especially with the increasing amount of spaceborne sensors, as well as the longer and longer Earth Observation time series available. Multiple studies are already comparing models and remote sensing data. But assimilating remote sensing data in models is not new, Navari et al. (2016,2018) are examples of *posterior* data assimilation in MAR. But in the case of assimilation of wet snow retrieval into MAR with nuddging, my current knowledge only encompasses the work of Kittel et al. (2022).

L52: Again, vague, and repeat of the previous.

This line will be removed in the revised version.

Figure 1: It's George VI, not Georges.

Noted and it will be changed accordingly in the revised version. Same for other mentions of George VI in the text.

L54-55: you already mentioned this in the beginning of the introduction.
This paragraph needs to be rewritten or completely removed; most info is repeated or obsolete. Your paper is about assimilating melt, so spend time on explaining melt and why it is important to improve melt simulations.

With the restructuring of the introduction, the paragraph will be moved and better integrated at the beginning of the introduction to be merged with the line where this is already stated.

L67-72: This paragraph is all over the place, again repeating previously introduced information. Rephrase it and make it more concise by just writing: "Here, we assimilate different satellite observations of melt in the RCM MAR, etc". The Methods section is to explain the actual details, pros and cons of the products.

These lines are supposed to explain the main advantages/disadvantages of the passive/active sensor and that both can be used together to benefit from the advantages of the two types of sensors. We still believe this paragraph contains important information as a remote sensing point-of-a-view, but requires a complete rebuild. Section 2 will include subsections describing the remote sensing products where the pros and cons will be explained.

L78: Introduce what a binary melt mask is, I did not know.

"*Binary melt masks*" is an unwise choice of word that means we created masks of 1 and 0 from the satellite observations. 1 signifies wet snow will be detected and 0 the negative. "*Binary melt masks*" will be reworded to "*wet snow masks*" in the revised version and will be introduced in L78.

L78: Three sensors? Sensors on the satellites? The three satellites? Unclear.

It is true that the remote sensing part may be confusing. The spaceborne carriers and their sensors are often interchanged, making the information difficult to follow. Subsections will be created for each remote sensing dataset employed during the study. Subsections will include a description of the mission, sensor, and its characteristics. Better describing the datasets should decrease the confusion around them.

L79: radiometer is a new word, it should be introduced. Are all satellites equipped with microwave radiometers?

A radiometer is a sensor for measuring the radiant flux of electromagnetic radiation. Here only AMSR2 is a radiometer. The other two are active sensors, which means that they do not only observe the radiant flux, they send energy as pulsed electromagnetic waves to the Earth and record the backscattered portion of the signal.

L84: this is not correct. Liquid water can't be melting. Rephrase to something like: "Here,we relate subsurface liquid water with subsurface melting". Although I am still confused how this works, how do you distinguish between percolated surface meltwater and subsurface meltwater, or meltwater that has not yet refrozen after a previous melt event?

In the paper, the presence of liquid water observed in the remote sensing dataset but not in the model is considered meltwater. The wording was chosen because we force melt in the model to match the observed presence of liquid water in the snowpack from the satellites. However, this wording is confusing as we do not observe melting with satellites but the presence of water. We will rework the wording by changing the ontology:

- "*observed melt*" → "*observed wet snow*"
- "*binary melt masks*" → "*wet-snow masks*",
- "*assimilated melt state*" → "*assimilated liquid water content state*",

and so on.

L85: I am unfamiliar with remote sensing so have no idea what you mean by acquisition capabilities.

Here we stated that by using sensors operating in microwave frequencies, it is possible to obtain images even at night or when there are clouds. This confusion can be clear out by mentioning what "*capability*" we're refering to, e.g. *"day-and-night capabilities"*.

L86: specify "small scale".

Small scale is used to talk about melt events with an extent under $100km^2$ which is smaller than the spatial resolution of radiometers.

L97: Rephrase to "A dry snowpack has a lower emissivity than a wet snowpack".

It will be rephrased accordingly in the revised version.

*Equation 1: Is it epsilon^*, or is * a multiplicator? Anyhow epsilon is not defined in the text.*

It is a multiplicator. Epsilon is defined at L97 as the emissivity of the snowpack. It will be stated under the equation as well in more general terms.

L108: I don't understand, what's "dominant melt"? rephrase

It will be rephrased to "*The grids are superimposed, and the melting state for each pixel in the MAR is determined based on the most prevalent melting or non-melting condition observed in the corresponding area of the satellite mask.*"

L111: Please group the three satellite production per subsection.

Subsections will be created in the revised version for each remote sensing dataset employed during the study. Subsections will include a brief description of the mission, the sensor, and its main characteristics. Better describing the datasets should decrease the confusion around them.

L143: "pixel-wise"? huh? Typo?

"*pixel-wise multiplication*" refers to a mathematical operation where each pixel in one image or gridded data set is multiplied with the corresponding pixel in another image or gridded data set. In algebra, the corresponding operation is called the Hadamard product. Pixel-wise can also be used in other contexts such as pixel-wise comparison, pixel-wise classification, pixel-wise cross entropy, as examples.

Figures 2-4: Is there a way to combine these in one graph, or something? They seem rather obsolete to this study (using 3 figures to show something that's not main result of the paper).

Comments from anonymous reviewers #1 and #2 tend to suggest opposite directions (either simplify or on the contrary go into more detail). Consequently, the figures may change. Considering this viewpoint, having 3 figures (Fig. 2,Fig. 3, and Fig. 4) on melt detection, which is not the main subject of this paper, may be redundant. Fig 3 will be removed as well as its reference line 104. Also, Figure 4 will be reworked in order to add a panel showing the b) panel after normalization.

Section 2.3.1: So, what threshold do you finally choose? This is unclear.

Both 0.1 and 0.2 thresholds are used in the different assimilations. It will be added to the end of the subsection. They are also mentioned during naming conventions for the different assimilations. We plan to add "*Currently, there is no clue to identify the best fitting threshold for this study. Both thresholds will be used to test the sensibility of the model.*"

L235: It's unclear for me what you are presenting here. Are you evaluating your melt assimilation simulations, or are you repeating evaluations from previous studies? It the latter, this entire paragraph is obsolete.

The evaluation of MAR prior to its assimilation is a mandatory step to optimize its hyper-parametrization. From this benchmarked model, we can then add the assimilation module whose sensitivity is the object of the paper. The parametrization used here is based on Kittel (2021), but over a different region, resolution, and time period, thus needs to be assessed. Section 3 thus presents MAR_{ref} evaluation.

Section 3: The evaluation should be more detailed. AWS observations exist that are used in a SEB model so explicitly calculate melt. This can be perfectly used to evaluate the model, especially the later sensitivity experiments, and assess the models performance in simulating surface melt production. See for instance Jakobs et al., 2020.

See our earlier comment about this in major comment number 3.

L280: rephrase.

l279 - 281 will be reworded to "*Despite the fact that the relative increase in surface melt and runoff is almost similar for $\overline{Assim}$ and MAR_{ref} (66.7% and 63.8%, respectively), their absolute increase in Gt y-1 is not the same (+95 Gt y−1 and +21 Gt y−1, respectively). This suggests that the snowpack can still absorb liquid water unless it reaches its maximum capacity*."

Results overall: Several sentences in the results section are better suited in the introduction or methods section; please increase the focus of this section on the actual results.

As the introduction and methodology section will be reworked, some sentences from the results section will be moved there, such as the presentation of the different simulations.

L295: What's the global zone? And what evolution?

"*Global zone*" refers to the whole Antarctic Peninsula and the "*evolution*" to the change of the variable with and without assimilation. This will be reworded to "*For the three highlighted ice shelves (Larsen C, Wilkins, and Georges VI), the effect of the assimilation follows the same general trend as for the whole peninsula but at different orders of magnitude (Table 4).*" in the revised version.

Section 4.1: This section is completely unclear to me and contains several unphysical explanations (e.g. a cold snowpack producing melt??). And, I don't understand Figure 10. What are the curves? Not all curves are explained in the legend and as most of them overlap I also can't distinguish them at all. Improve the figure and try to extend the caption.

Section 4.1 will include a discussion about the comparison between the evolution of melt production, refreeze, and liquid water content that should clarify section 4.1. L310 - 314 will be reworded to: "*First, the available energy in the system is consumed by melting processes, preventing the layer under 1m from heating up. A colder snowpack constantly needs larger nudging to reach the melt threshold. The second point is that due to the lower saturation of water in the lower layers, the upper layers become saturated with less water because of densification during melt events, resulting in increased runoff and faster percolation of the water into deeper layers, outside of the assimilation depth range. If the model were to retain liquid water in its top snow layers for a longer duration, it would require less nudging to match the RS datasets.*"

As for Figure 10, its purpose is to show that curves of the same colors (same assimilation depth for AMSR2 data but different assimilation depth for Sentinel-1 data) have mainly the same outputs. This figure will be reworked to make it clearer in the revised version.

L306: the penetration depth of what? Be a bit more explicit.

It will be reworded to "*Assimilation depth*".

L311-313: Rephrase. What do you mean here?

The end of the paragraph will be reworded.

L337-338: How is that calculated? As the melt season starts in November of the previous year?

"*Melt season length*" is here defined as the number of days between the first and last day where liquid water is observed in the snowpack during the period 01/06/YYYY to 31/05/YYYY+1.

L355-368: I think this can all be moved to the Methods section (and in fact, it already contains several things already introduced in the methods).

The beginning of section 4.3 will be shortened and simplified as most of its content is already stated before. L335-336 will be removed as it is a repetition of information already stated.

L372: is this the sum of three ice shelves, or the whole AP? This is unclear. Also elsewhere in the next paragraphs. Be very consistent with these numbers.

The numbers are referring to the whole Antarctic Peninsula. "*The two assimilations gave similar numbers of melt days and close surface melt production on the Peninsula ice shelves" reworded to "The two assimilations gave similar numbers of melt days and surface melt production on the Antarctic Peninsula for the studied period*"

L388: uncomplete sentence.

It will be reworded to: "*We identified the assimilation depth (Δz) to be the most influential parameter when applied for low penetrating sensors*."

L390-391: rewrite.

L390-391 will be reworded to "*The uppermost layer of the snowpack is considerably denser than the underlying layers, owing to refreezing caused by the exceeding liquid meltwater from assimilation, as well as low night-time temperatures*" in the revised version.

References:

Jakobs, C. L., Reijmer, C. H., Smeets, C. J. P. P., Trusel, L. D., Van De Berg, W. J., Van Den Broeke, M. R., and Van Wessem, J. M. (2020). A benchmark dataset of in situ Antarctic surface melt rates and energy balance. Journal of Glaciology, 66(256), 291–302. doi:10.1017/jog.2020.6.

Kittel, C., Fettweis, X., Picard, G., and Gourmelen, N. (2022a). Assimilation of satellite-derived melt extent increases melt simulated by MAR over the Amundsen sector (West Antarctica), Bulletin de la Société Géographique de Liège, 78, 87–99, 2022.

Kittel, C., Amory, C., Hofer, S., Agosta, C., Jourdain, N. C., Gilbert, E., . . ., Fettweis, X. (2022b). Clouds drive differences in future surface melt over the Antarctic ice shelves. The Cryosphere, 16 (7), 2655–2669. https://doi.org/10.5194/tc-16-2655-2022

Navari, M., Margulis, S. A., Bateni, S. M., Tedesco, M., Alexander, P., & Fettweis, X. (2016). Feasibility of improving a priori regional climate model estimates of greenland ice sheet surface mass loss through assimilation of measured ice surface temperatures. The Cryosphere, 10 (1), 103–120. doi: 10.5194/tc-10-103-2016.

Navari, M., Margulis, S. A., Tedesco, M., Fettweis, X., & Alexander, P. M. (2018). Improving greenland surface mass balance estimates through the assimilation of modis albedo: A case study along the k-transect. Geophysical Research Letters, 45 (13), 6549-6556. doi: 10.1029/2018GL078448.