Climatology and Surface Impacts of Atmospheric Rivers on West Antarctica

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REVIEWER COMMENT #3:

General Comments

This is a really interesting paper that investigates the role of atmospheric rivers on the surface mass balance of the West Antarctic ice sheet, for which there is a clear knowledge gap. The authors have obviously put in a lot of work into this and strived for high standards. The Introduction is nicely written/researched, and the study nicely put into context with previous work. However, Section 2 on data and methods is difficult to follow as it is disorganised / disjointed and contains sometimes unnecessary text – the paper would really be improved if this section could be organised better. Section 3 is a well written description of the comprehensive analysis. The figures are clear and appropriate. I was slightly uncertain about Figs 2 and 3, as the results mentioned in the text did not seem to be in the same range as Fig. 2, and the justification for the trend 1995-2015 was not clear in Fig. 3 – also a possible explanation for these trends seems to be missing. But despite that the authors have obviously put a lot of work into this analysis. The study includes a very comprehensive, well researched, and well considered Discussion section which does a very good job of contextualising the results. To summarise, I think this is an excellent study, but would benefit from addressing some of the comments below, especially related to Section 2 which really needs to be much clearer / linear – especially given the complexity of the analysis and the number of data sets and the incorporation of both climatological and case study analysis.

> **We thank the reviewer for taking the time to review this manuscript and providing recommendations to improve the structure and organization of the text. In response to the reviewer's recommendations below, we have revised the methods section to stick with describing each dataset in its respective subsection. Furthermore, we provide explanations and context to clarify the information presented in Figures 2 and 3. Responses are written in bold, and excerpts from the manuscript are *italicized*. Changes to the text are *italicized and in blue*. Line numbers refer to track changes in the revised manuscript. Our responses are as follows:**

Specific Comments

+ The motivation for the case study in the Introduction is not that clear. I understand that it is included as it can be investigated in more detail using the in-situ observations, and so complements the more broader scale climatology work. But this is not that well explained and comes across as rather disjointed. Please strengthen this justification.

> **Reviewer Comment #1 mentioned a similar need to improve motivation and clarity in the introduction, and we have revised the last two paragraphs as a result. The final paragraph of the introduction (starting on line 70) explains the motivation for our climatology and case study and highlights the gap in prior research that our study addresses. We explain that based on previous studies, the spatial variability of extreme precipitation associated with ARs over West Antarctica is poorly understood and fails to capture the local accumulation associated with AR events. We highlight that our analysis of a case study event provides key indications of small-scale spatial variability in AR-driven accumulation and surface melting on Thwaites Eastern Ice Shelf. Finally, we emphasize that placing this case study within the broader context of the climatology of West Antarctic ARs enables us to better understand the characteristics and impacts of ARs on the surface mass balance.**

+ Section 2.1 is labelled 'observations' but has quite a few sentences describing the method, including the SNOWPACK model which is mentioned before described in its own dedicated subsection later on. I find this rather unstructured/confusing/disorganised and would suggest that a dedicated methodology section would help the reader. And in general, please choose appropriate sub-headings and stick to the appropriate content for these headings.

> **We have removed references to the atmospheric reanalyses MERRA-2 and ERA5 and SNOWPACK modeling in section 2.1 to improve clarity and reduce confusion. We now stick to discussing the available observations and how we use them for the AR case study event in February 2020. In general, we have revised the Data and Methods (section 2) to ensure the content of each of the four subsections is consistent with their respective sub-headers.**

+ Lines #98 - #100: More details on the reanalysis are required such as their appropriateness / representativeness of the AIS, and even just spatial resolution are necessary. Also, the reanalysis are compared with the in-situ observations on Thwaites, but there is no explanation for whether this is appropriate. For example, whether the in-situ observations are representative of a wider area that is comparative to the reanalysis grid boxes.

> **We have moved the introduction of the reanalysis data from section 2.1 to section 2.2, titled "Reanalysis Products: MERRA-2 and ERA5". We have re-organized section 2.2 to improve the flow and clarity of this section. First, we introduce both datasets. Then we describe how we use the data in order of**

how the results are presented (first the West Antarctic AR climatology, and then the February 2020 case study).

Following the introduction of each reanalysis product and their spatiotemporal coverages, we added a sentence to justify our use of MERRA-2 to analyze the drivers and impacts of West Antarctic AR events (line 114):

We primarily use MERRA-2 analyze the large-scale synoptics and impacts of AR events in West Antarctica, as MERRA-2 explicitly represents ice sheet hydrological and energy budgets and compares best to ice core records of snow accumulation in Antarctica among multiple reanalyses (Gelaro et al., 2017; Medley and Thomas, 2019).

In the last paragraph of section 2.2, which discusses the use of MERRA-2 and ERA5 in the case study event, we have added the following (line 125):

While AMIGOS observations reflect local conditions at the Cavity and Channel Camp sites, MERRA-2 and ERA5 data represent grid-cell averages, meaning local values for temperature, surface pressure, wind speed and wind direction can deviate from those grid-cell averages. In the near-surface temperature comparison, MERRA-2 and ERA5 use 2 m temperatures while the observed temperatures are from approximately 6 m above the surface. We include ERA5 in this analysis because there are differences between MERRA-2 and ERA5 in 2 m temperature and snow accumulation during the event.

The spatial resolution of each reanalysis is mentioned in the first paragraph of section 2.2.

Finally, we have revised the introduction to include that comparing atmospheric reanalyses to the observations during the February 2020 event is a goal of this study - we want to know how well reanalyses are able to reproduce this event (line 77):

Then, we use in-situ observations and a firn model to examine the specific impacts of a series of three successive ARs that made landfall on TG in February 2020, as well as the ability of reanalyses to reproduce those observations.

+ Section 2.2: Please see comment above about discussing reanalysis data before it is properly introduced. Another comment here is that you state that the datasets are ‘regularly gridded’, so is that in terms of lat/lon? Also, much of the text in this section again seems rather inappropriate and better placed elsewhere. For example, mention that ‘this region’ has experienced large acceleration in recent years should surely have been clarified in the Introduction and no need for repetition. Finally, its not really clear why MERRA is used for one purpose (as opposed to ERA5) and ERA5 only used for comparison with MERRA during the case study.

> **"Regularly-gridded" refers to the spatial coverage of the data, which are on a latitude-longitude grid.**
>
> **We have removed the text about the accelerating mass loss from West Antarctica, which was already discussed in the introduction.**
>
> **We have added several sentences throughout section 2.2 explaining why and when we use MERRA-2 reanalysis vs ERA5; please see the previous comment for details.**

+ Section 2.4: This section is labelled SNOWPACK firn modelling but the opening sentence discusses precipitation from reanalysis. Please restructure these sections much better.

> **We have removed references to MERRA-2 and ERA5 reanalyses from the opening sentence of section 2.4 (line):**
>
> ***We use observed snow height and temperature from the AMIGOS to force the firn model SNOWPACK (Lehning et al., 2002a, b) to reconstruct accumulation and surface melt during the AR case study event in February 2020.***
>
> **Overall, we have revised the section to ensure we focus specifically on SNOWPACK firn modeling and how we use it in the study.**

+ Section 2.5: Its not clear why surface height changes using interferometric reflectometry is necessary given that the in-situ observations also mention snow height. Can you please clarify?

> **As shown in Fig. 1, the GNSS-interferometric reflectometry sites are located inland from Thwaites Eastern Ice Shelf, at Lower and Upper Thwaites Glacier. The records from these sites provide more information on spatial variability and local accumulation over Thwaites Glacier, not only on the ice shelf. To clarify, we have revised the text in section 2.5 on lines 175 and 182:**
>
> ***We supplement the record of surface height change estimates observed by the AMIGOS on Thwaites Eastern Ice Shelf with surface height change measurements from the grounded TG, observed with the global navigation satellite system (GNSS) using interferometric reflectometry (Larson et al., 2009, 2015; Roesler and Larson, 2018).***
>
> ***The addition of GNSS-IR snow accumulation records enables us to compare spatial differences in snowfall on Thwaites Eastern Ice Shelf and TG during the AR case study event.***

+ Section 3.1, first paragraph: 1) The value given is 3.2% but Figure 2 only shows AP frequency values from 0 to 0.8%? So its not at all clear how this value was calculated. 2) Please clarify how the uncertainty value is computed? 3) Similar to the above, its not clear where the value of 28.7% comes from as this is not the range in Figure 2.

> **We have added a sentence to the beginning of the paragraph to introduce AR frequency and how it is calculated, and reference Fig. 2 later in the paragraph when we discuss local AR frequency (line 195). 3.2% refers to the total AR frequency over the whole region (i.e., there is an AR making landfall somewhere in the region 3.2% of the time, on average). 0.2-0.8% refers to local AR frequency at a given point within the region, with 0.2% being the lowest value and 0.8% being the highest in the region. The uncertainty values refer to one standard deviation from the mean, which we have added into the text the first time it is used. We use r-squared from a linear regression to compute the 28.7% percent interannual variability in the total precipitation explained by AR precipitation.**
>
> ***To determine the frequency of ARs over the Amundsen Sea Embayment and Marie Byrd Land region, we divide the number of AR times by the total time from 1980 to 2020. Our analyses show that ARs exhibit a total frequency of 3.2% over the whole region from 1980 to 2020 (i.e., there is an AR making landfall somewhere in the region 3.2% of the time, on average) (~~Fig. 2~~). ~~This represents the total frequency of ARs over the region, calculated by dividing the number of AR times by the total time from 1980 to 2020.~~ Within the region, localized AR frequencies range from 0.2 to 0.8% of the time, with the highest frequencies over the Abbot Ice Shelf and the Getz Ice Shelf (Fig. 2a). Integrated over the entire region, ARs contribute 59 +/- (one standard deviation) 24 Gt precipitation annually (out of 550 +/- 63 Gt total annual precipitation, Fig. 2b and c), and explain 28.7% of the interannual variability in precipitation (linear trends removed).***

+ Figure 3: Is the large variability of AP events connected to the large variability in the Amundsen Sea Low / large interannual variability in cyclone frequency in this region (Simmonds and Keay, 2000)?

> **While the semi-annual zonal migration of the Amundsen Sea Low drives strong seasonal variability in the total amount of snowfall on Thwaites Glacier (Maclennan and Lenaerts, 2021), we state in our results (line 211) that ARs in this region do not exhibit statistically significant seasonality in their number nor in their duration. The interannual and multi-decadal variability in the number of AR events may be explained by variability in the strength and positioning of the Amundsen Sea Low and multiple modes of atmospheric variability.**

+ Figure 3: 1) Can you please justify why the range 1995 to 2015 was chosen? Bluntly, was this cherry picked to get a significant correlation? What if you shifted the range by 1 or 2 years, how does the trend change and its significance? 2) There doesn't seem to be any mention of what could be causing the positive trend in AR events – this is also noticeably absent from the Discussion. For example, could this be due to decadal changes in the Madden-Julian Oscillation (Hsu et al., 2021; Science Advances) which occurred in the late twentieth century and early twenty-first century?

1) **Yes, we can justify the range of 1995 to 2015 which was chosen to highlight multi-decadal variability in the number of AR events within the longer-term positive trend from 1980 to 2020. The long-term trend from 1980 to 2020 is 0.12 +/- 0.06 events per year squared (p = 0.055, standard error = 0.0595). This represents a statistically significant increase in the annual number of AR events over time. Within the 1980 to 2020 period, however, there is shorter-term variability in the number of AR events over time as well. We selected the 1995 to 2015 period to highlight this short-term variability because it exhibits a statistically significant positive trend that is notably higher than the total trend from 1980 to 2020. From 1995 to 2015, the trend in AR events is 0.32 +/- 0.16 events per year squared (p = 0.059, standard error = 0.1598).**

   We performed the same statistical analysis on 20-year periods within the five years before and after 1995 to 2015 (i.e., 1990-2010, 1991-2011, …, 1999-2019, 2000-2020). Among the periods tested, 1995-2015 exhibits the lowest standard error, a low p value, and a high trend combined. 1996-2016 similarly exhibits a high trend and low p value, but with a slightly higher standard error (still statistically significant). That is why we selected the 1995 to 2015 range. We have revised the text as follows to highlight the role of shorter-term variability within the overall trend (line 203):

   From 1980 to 2020, there is a positive trend in AR events of +0.12 +/- 0.06 events per year squared (p = 0.055), similar to the results from Wille et al. (2021), which also showed an increasing trend in AR frequency from 1980 to 2018 over the WAIS region. From 1995 to 2015, there is a marked trend of +0.32+/- 0.16 events per year squared (p = 0.059), indicating multi-decadal variability in the number of AR events embedded within the longer-term positive trend (this 20-year period is selected based on its high trend, low p value, and low standard error of 0.16 events per year squared).

2) **There are a number of modes of variability, both decadal and interannual, that impact this region, most notably the PSA2 - for references on phase sign for each year in the record, see supplemental figure S2 in Shields et al. (2022). However, the rarity of AR events, combined with the interaction of multiple modes of variability, makes it challenging to link specific trends in AR activity to individual modes (Wille et al., 2021; Shields et al., 2022). We have revised the Discussion to include this point and highlight that future research on how modes of variability and anthropogenic forcing will be critical to understanding how ARs and their impacts may change in the future (line 410):**

   While AR events occur slightly more frequently over the Antarctic Peninsula and Dronning Maud Land than over the Amundsen Sea Embayment and Marie Byrd Land (Wille et al., 2021), the vulnerability of the latter region to ocean-induced ice mass loss and ice sheet instability amplifies the importance of quantifying accumulation and the interannual variability of AR events, as well as the modes of atmospheric variability driving their long-term trends (Shields et al., 2022), as a compensation mechanism for the mass loss. The long-term positive trend in the number of AR events and the shorter-term variability identified in this study underlines the importance of understanding how modes of atmospheric variability, especially the PSA2, and anthropogenic forcing are impacting AR activity in this region (Dalaiden et al., 2022).

   Shields, C., Wille, J., Collow, A., Maclennan, M., and Gorodetskaya, I. (2022): Evaluating Uncertainty and Modes of Variability for Antarctic Atmospheric Rivers. Geophysical Research Letters., doi: 10.1029/2022GRL09957

+ Could the pressure patterns / anomalies responsible for Ars be compared to the analysis of Scott et al. (2019; Journal of Climate) , which uses ERA5 and a cluster technique to identify dominant circulation patterns. Perhaps this would be appropriate for the Discussion section.

Scott et al. (2019) identifies the PSA2 signature consisting of a high-low pressure couplet off the coast of West Antarctica, with anomalously high 2 m temperatures pushing towards the Amundsen Sea Embayment from the Southern Ocean. Maclennan et al. (2021) found that snowfall events on Thwaites Glacier are moderately correlated with the PSA2 pattern as well. We have added a sentence on this topic to the discussion (line 324):

We find that AR events making landfall in the Amundsen Sea Embayment and Marie Byrd Land are driven by the coupling of a blocking high over the Antarctic Peninsula with a low-pressure system known as the Amundsen Sea Low. This pressure pattern is similar to the Pacific South-American patterns identified by (Scott et al., 2019) as drivers of marine air intrusions and West

Antarctic surface melting, and consistent with geopotential height anomalies identified by (Adusumilli et al., 2021) during WAIS AR events in 2019.

Minor / Technical Corrections

+ Line #9: 3 -> three

Done.

Next, we use observations from automatic weather stations on Thwaites Eastern Ice Shelf with the firn model SNOWPACK and interferometric reflectometry to examine a case study of three ARs that made landfall in rapid succession from February 2 to 8, 2020, known as an AR family event.

+ Line #9: Please give the year of the case study.

Done - please see previous comment.

+ Line #13: I assume the accumulation value is water equivalent. Maybe state this.

We have added "or millimeters water equivalent" after the units.

+ Line #26: As written this states that all mass loss is from the WAIS, which is not the case as the Peninsula region has surely also lost mass.

We have revised the sentence to suggest that most, but not all, mass loss is from the WAIS (line 18):

In the last four decades, the AIS has experienced increased mass loss, from 40 +/- 9 Gigatons per year (Gt yr^{-1}) between 1979 and 1990 to 252 +/- 26 Gt yr^{-1} between 2009 and 2017, ~~due~~ most of which is attributed to increasing discharge across the grounding line of the West Antarctic Ice Sheet (WAIS, Rignot et al., 2019).

+ Line #28: This statement requires a reference.

We have added a reference (line 22):

Although it covers only 17% of the AIS, the WAIS accounts for 34% of ice discharge (Rignot et al., 2019).

+ Line #33: TG is undefined.

We have revised the sentence to define TG as Thwaites Glacier (line 27):

> ***In particular, Thwaites Glacier (TG), which borders the Amundsen Sea, is at considerable risk for continued grounding line retreat in the future because it is grounded on inward sloping bedrock, which may lead to a rapid positive feedback for increasing ice flow and retreat, termed 'marine ice sheet instability' (Weertman, 1974; Schoof, 2012).***

+ Line #40: What about evaporation? With increasing surface melting this will become increasingly important. For example, Bromwich et al. 2011 J. Climate showed that sublimation and evaporation combined accounted for around 25% of the precipitation term.

> **Here we discuss the contributors to surface mass balance from 1979 to present day. During this period, evaporation is less important than sublimation on the Antarctic Ice Sheet because there are only a few small regions where standing water is present, including some ice shelves (Kingslake et al., 2017; Langley et al., 2016; Lenaerts, Lhermitte et al., 2017). Melt water produced on snow surfaces can propagate into the firn, which prevents a significant amount of evaporation from occurring (Lenaerts et al., 2019). Bromwich et al. (2011) combines the evaporation and sublimation terms. We have changed "sublimation" to "sublimation/evaporation" to include the evaporation term in our description of surface mass balance (line 33).**
>
> ***The SMB represents the balance between mass gained at the surface through precipitation, and mass lost by sublimation/evaporation and surface meltwater runoff (Lenaerts et al., 2019).***

Bromwich, D., Nicolas, J., and Monaghan, A. (2011): An Assessment of Precipitation Changes over Antarctica and the Southern Ocean since 1989 in Contemporary Global Reanalyses. Journal of Climate, doi: 10.1175/2011JCLI4074.1

Kingslake, J., Ely, J., Das, I., and Bell, R. (2017): Widespread movement of meltwater onto and across Antarctic ice shelves. Nature, doi: 10.1038/nature22049

Langley, E., Leeson, A., Stokes, C., and Jamieson, S. (2016): Seasonal evolution of supraglacial lakes on an East Antarctic outlet glacier. Geophysical Research Letters, doi: 10.1002/2016GL069511

Lenaerts, J., Lhermitte, S., Drews, R. *et al.* (2019): Meltwater produced by wind–albedo interaction stored in an East Antarctic ice shelf. Nature Climate Change, doi: 10.1038/nclimate3180

Lenaerts, J. T. M., Medley, B., Broeke, M. R., and Wouters, B. (2019): Observing and Modeling Ice Sheet Surface Mass Balance, Reviews of Geophysics, doi: 10.1029/2018RG000622

+ Line #56: Mention of ‘on the order of the Amazon River’ is confusing. Do you mean the actual river? Is this a type of AR? Are you referring to spatial size? I’m afraid that this comparison is not that helpful so please revise.

> **This is a standard analogy used to emphasize the importance of ARs in the hydrological cycle. It refers to the large quantity of water they transport, which is more than double the flow of the Amazon River. It is mentioned in Zhu and Newell (1998) and included in the American Meteorological Society's definition of an atmospheric river: https://glossary.ametsoc.org/wiki/Atmospheric_river. We have revised the text as follows (line 49):**
>
> ***ARs are associated with a low-level jet and moisture fluxes on the order of the flow of the Amazon River (Zhu and Newell, 1998).***
>
> **Zhu, Y. and Newell, R. E. (1998): A Proposed Algorithm for Moisture Fluxes from Atmospheric Rivers. American Meteorological Society Monthly Weather Review, doi: 10.1175/1520-0493(1998)126<0725:APAFMF>2.0.CO;2**

+ Line #78: Maybe clarify this sentence a little regarding ‘rely on reanalysis’. For example, by saying ‘In this study, we rely ….’

> **In response to Reviewer Comment #4, we have rewritten this section of the paper, including the rephrasing of "rely on reanalysis" (line 76):**
>
> ***First, we use atmospheric reanalyses to quantify the landfalls and accumulation impacts of ARs from 1980 to 2020 over Marie Byrd Land and the Amundsen Sea sector.***

+ Line #103: Its not clear whether by observations you are referring to the in situ observations or the reanalysis. See specific comment above. Please clarify your methodology/approach in a dedicated section.

> **In response to comments above, we have moved all references to SNOWPACK methodology to the appropriate section (2.4). The sentence now reads as follows (line 151):**
>
> ***We use observed snow height and temperature from the AMIGOS to force the firn model SNOWPACK (Lehning et al., 2002a, b) to reconstruct accumulation and surface melt during the AR case study event in February 2020.***

+ Line #146: Is there justification for the 12 hour threshold?

> **The 12-hour threshold is a parameter choice we made to define separate AR events. For this study, we tested different thresholds from 6 hours up to 36 hours,**

and found a range of 20 events per year (6 hour threshold) to 14 events per year (36 hour threshold) on average. We found that choosing a 12-hour break period enabled us to capture the case study AR family event as comprising of three AR events, which is consistent with the poleward movement and positioning of the ARs detected during that time by the Wille et al. (2021) algorithm. Using a window shorter than 12 hours can group ARs together that are part of the same synoptic system and are not necessarily unique. Using a window longer than 12 hours risks erroneously combining multiple, unique AR events, such as those shown in the case study. An additional constraint was the 3-hourly temporal resolution of our AR detection algorithm. There are many different ways of defining ARs and AR events (Shields et al., 2018) and some studies count events using a duration, rather than time break, threshold (Fish et al., 2021). However, given the large variability in the duration of Antarctic AR events in this region, which ranges from 3 hours to days, we decided a time break was the most appropriate method for counting AR events.

Fish, M. A., Wilson, A. M., and Ralph, F. M. (2019): Atmospheric River Families: Definition and Associated Synoptic Conditions. Journal of Hydrometeorology, doi: 10.1175/JHM-D-18-0217.1

Shields, C., et al. (2018): Atmospheric River Tracking Method Intercomparison Project (ARTMIP): project goals and experimental design. Geosci. Model Dev., doi: 10.5194/gmd-11-2455-2018

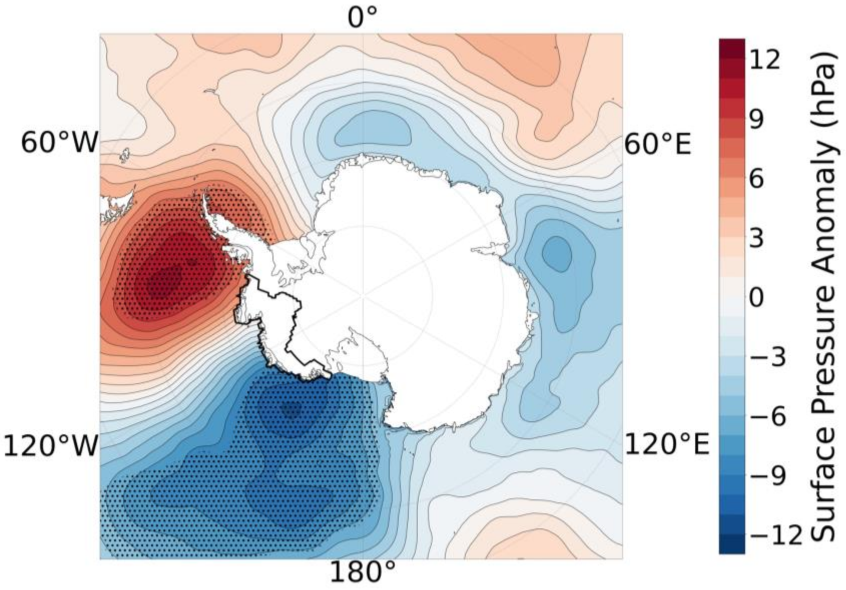
+ Line #206: Its not clear how these average surface pressure maps during AR events are computed. See comments above. Presumably you identified the ARs and then did calculated a composite of these events. But this really needs to be made clearer.

Line 117 of section 2.2 in the Data and Methods states how the surface pressure composite maps are generated:

We use MERRA-2 reanalysis to generate surface pressure and surface pressure anomaly (relative to 1980 to 2020 climatology) composite maps during the times of AR landfalls over coastal West Antarctica, including the Amundsen Sea Embayment and Marie Byrd Land.

+ Figure 4: The stippling wasn't really obvious. Could this be made clearer?

Yes - we have increased the size of the stippling to make it clearer:



+ Line #219: 1) So you are creating a distribution of the temperatures. Perhaps this needs a little more explanation. 2) What does ‘all seasons’ mean? Figure 5 only shows the seasonal breakdown?

> **On line 224, we state how the temperature difference is calculated:**
>
> ***To further examine the impacts of AR landfalls on TG surface conditions, we calculate the change in surface temperatures on Thwaites Eastern Ice Shelf during AR events (Fig. 5). To do this, we take the difference between the mean MERRA-2 2 m temperature 24 hrs before landfall, and the mean 2 m temperature 24 hrs after landfall.***
>
> **The distributions in Fig. 5 indicate the range of temperature differences we calculate for AR events, divided by season. We explain the results as follows. First, we discuss the overall temperature difference distribution of ARs among all seasons (not divvying up the ARs by season yet, line 226):**
>
> ***AR events are associated with a temperature increase of 1.4 K (first quartile) to 7.1 K (third quartile), with median 3.8 K, over Thwaites Eastern Ice Shelf over all seasons.***
>
> **Then, in the following sentence, we look at the seasonal breakdown, which is presented in Fig. 5:**
>
> ***In austral summer (December-January-February), the median temperature increase is the smallest at 1.5 K. In fall (March-April-May), winter (June-July-August), and spring (September-October-November), the median temperature increases associated with AR landfall are 4.3 K, 6.3 K, and 4.3 K, respectively.***

+ Line #225: Maybe state melting point of snow/ice.

The melting point is stated in the next sentence of the paper (line 233):

There are many more summer events where 2 m temperatures exceed the melting point of 273.15 K (6 events in 1980-2020) than in fall (2 events), winter (1 event), and spring (0 events).

+ Line #291: 2 -> two

We have changed "2" to "two" (line 299):

Overall, surface melt is nearly two orders of magnitude lower than the snowfall, indicating that the primary impact of this AR family event is to contribute snowfall to TG.

+ Line #317: Again, what is the uncertainty mentioned here. Is it one standard deviation? Please clarify.

Yes, "17 +/- 5 AR events per year" refers to one standard deviation. Please see response to previous comment on the uncertainty - we now introduce the uncertainty as one standard deviation the first time it is used.