

The Cryosphere Discuss., referee comment RC3  
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## Comment on tc-2022-101

Anonymous Referee #2

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Referee comment on "Climatology and surface impacts of atmospheric rivers on West Antarctica" by Michelle L. Maclennan et al., The Cryosphere Discuss.,  
<https://doi.org/10.5194/tc-2022-101-RC3>, 2022

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### 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.

### 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.

+ 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.

+ 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.

+ 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.

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

+ 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?

+ 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.

+ 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)?

+ 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?

+ 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.

#### Minor / Technical Corrections

+ Line #9: 3 -> three

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

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

+ 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.

+ Line #28: This statement requires a reference.

+ Line #33: TG is undefined.

+ 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.

+ 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.

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

+ 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.

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

+ 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.

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

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

+ Line #291: 2 -> two

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