Review of "Recent intensification of extreme precipitation over Antarctica driven by increases in greenhouse gases"

Dear All,

I would like to sincerely thank both Referees for their time and effort in providing such thoughtful and expert feedback on our manuscript. Their insightful comments prompted substantial revisions, which have greatly improved the scope and overall quality of the manuscript as a result. One of the most significant suggestions from Reviewer #2 was to conduct the detection and attribution analysis with other models and compare the results. This led to us additionally analysing results from the large ensembles from CESM1, as this includes ensembles for all-forcings, greenhouse gases, stratospheric ozone, and anthropogenic aerosols - resulting in a complete rewrite of the Results section. The changes are outlined in red in the revised manuscript.

To summarise, the main changes are:

i) Revised title, which now mentions both greenhouse gases and stratospheric ozone.
ii) Revised Abstract, concluding that the precipitation trends can be attributed to both greenhouse gases and stratospheric ozone, but the impact of anthropogenic aerosols is unclear.
iii) Figure 1 has been revised to include only the locations used in the manuscript.
iv) In the Datasets section (2.1), a new description of the CESM1 model, as well as CESM2.
v) In the Methods section (2.2), an expanded explanation of the regression-based detection and attribution technique, as well as how we examine the role of Atmospheric Rivers (ARs).
vi) In the Results section (3) we now focus on results using the large ensembles from the CESM1 model (Figures 2 to 7; Table 1), especially section 3.2 on detection and section 3.3 on attribution. The original results from CESM2 are now included in the supplementary information (Figures S1 to S6), and discussed in section 3.4..
vii) The original Conclusion section has been merged with the Discussion section to form a 'Discussion and Conclusions' section, in order to reduce repetition. This section has also been revised to reflect the new results based on CESM1.

Best wishes,

Sai Prabala Swetha Chittella

Anonymous Referee comment #1

The study is focused on examining trends in extreme precipitation events (EPE) over Antarctica in ERA5 reanalysis and a global climate model CESM2 and their attribution. The authors identify which Antarctic drainage basins exhibit statistically significant trends in precipitation and EPE. The trends are then related to atmospheric rivers. Further, the trends are identified in three CESM2 runs – including all external forcings ('ALL') and then separately for the model runs forced only with greenhouse gases ('GHG') and only with anthropogenic aerosols ('AAER') – with the latter two considering changes since 1850. Historical runs as well future projections under the SSP370 scenario are considered for the period 1850-2100 with the present-time period 1979-2023 together with ERA5 reanalysis. The results demonstrate that for the period 1979-2023 greenhouse gas forcing is the primary driver of the positive precipitation trends in the ALL ensemble, including EPE trends. The paper is well written and is based on a new methodology using single-forcing applied to earth system models in order to attribute the detected trends in precipitation to a specific forcing beyond natural climate variability. Before the paper can be accepted for publication, several major concerns must be addressed:

We sincerely thank the referee for the constructive and insightful comments on our manuscript. We appreciate the time and effort taken to carefully review our work. Below, we provide a detailed point-by-point response to all comments.

1. *One of my major concerns is applying ERA5 and a global climate model for estimating trends in precipitation over Antarctica without demonstrating in the paper or citing previous studies how well the reanalysis and the model represent precipitation and particularly extreme precipitation events. I agree that ground-based observations are scarce and difficult but they do exist. There are also satellite observations. And regional climate models (such as MAR and RACMO2), which have been shown to much better represent Antarctic precipitation – not only due to their higher resolution (as noted in the Discussion, L354-360) but also improved physical parameterizations. I invite the authors to provide an evaluation of ERA5 and CESM2 before applying statistical analysis on trends and attribution – this will make the results of the paper more credible.*

   Here are some suggested references – the authors do refer to some of them already however lacking to present the conclusions about various biases found both in ERA5 and ESMs:

   Turner, J., Phillips, T., Thamban, M., Rahaman, W., Marshall, G. J., Wille, J. D., et al. (2019). The dominant role of extreme precipitation events in Antarctic snowfall variability. Geophysical Research Letters, 46, 3502–3511. https://doi.org/10.1029/2018GL081517

   Roussel, M.-L., Lemonnier, F., Genthon, C., and Krinner, G.: Brief communication: Evaluating Antarctic precipitation in ERA5 and CMIP6 against CloudSat observations, The Cryosphere, 14, 2715–2727, https://doi.org/10.5194/tc-14-2715-2020, 2020.

   Gossart, A., S. Helsen, J. T. M. Lenaerts, S. V. Broucke, N. P. M. van Lipzig, and N. Souverijns, 2019: An Evaluation of Surface Climatology in State-of-the-Art Reanalyses over the Antarctic Ice Sheet. J. Climate, 32, 6899–6915, https://doi.org/10.1175/JCLI-D-19-0030.1.

Gilbert, E., Pishniak, D., Torres, J. A., Orr, A., Maclennan, M., Wever, N., and Verro, K.: Extreme precipitation associated with atmospheric rivers over West Antarctic ice shelves: insights from kilometre-scale regional climate modelling, The Cryosphere, 19, 597–618, https://doi.org/10.5194/tc-19-597-2025, 2025.

Reply: We thank the reviewer for this important point. For a detection and attribution (D&A) framework we require a comprehensive, consistent, and gap-free record of daily precipitation over a multi-decadal period. Over Antarctica, satellite based precipitation datasets such as CloudSAT do not have complete spatial and temporal coverage and are also only available for a short period (Rousell et al., 2020; CloudSAT was launched in 2006, but experienced a battery failure in 2011). We did consider using precipitation outputs from high-resolution regional climate model simulations which dynamically downscale ERA5 to a horizontal resolution of 0.1°. Such datasets are generally better suited to represent the critical interactions between atmospheric flows and the complex coastal orography that influences precipitation, and especially extreme events. However, since our methodology required regridding to the coarser global model grids (horizontal resolution of 1°), the added value from the downscaling was effectively lost, and no systematic advantage was gained compared to ERA5. We therefore use ERA5 as our primary reference dataset, as it is widely used for Antarctic daily precipitation and extreme-event analysis (e.g., Vignon et al., 2021; Carter et al., 2022; Tewari et al., 2022; Yu et al., 2025). Additionally, given its wide use in previous Antarctic studies, we feel that a dedicated assessment of ERA5 precipitation is unnecessary.

Regarding the model choice, in the original manuscript we used large ensemble (LE) simulations based on the CESM2 global climate model. However, in the revised manuscript we have also added LE simulations using the CESM1 global climate model. These models were deliberately selected as prior evaluation studies showed that they have a good representation of the surface climate of Antarctica (Dunmire et al., 2022; Dalaiden et al., 2021, 2022; England et al., 2016; Landrum et al., 2017; Lenaerts et al., 2016). This is explained in section 2.1 in the manuscript. Furthermore, Fig. 2 of the revised manuscript compares precipitation trends over Antarctica from ERA5 and the CESM1 ensemble for ALL, which are in agreement from Ellsworth Land in West Antarctica to the Amery ice shelf in East Antarctica (see section 3.2).

2. *Methodology clarity: L129-131: as the method of scaling coefficients is central to the results, it has to be explained in more detail (can be also as a supplementary). Now to understand the methodology, the reader has to read Dalaiden et al 2022 paper and its supplement.*

   Reply: We thank the reviewer for this helpful comment. We agree that the scaling factors estimation is central to the paper and should be described more clearly within the manuscript. Accordingly, we have revised the Methods section (2.2) to provide a clearer description of how the scaling factors are estimated and interpreted.

   L169: "Over the Antarctic Peninsula, the ERA5 trends in total and extreme precipitation differ in direction, with total precipitation increasing and extreme precipitation decreasing, although only the trends over the northern Antarctic Peninsula are significant." – this can be related to the increased occurrence of rainfall instead of snowfall. The -variable "precipitation" used in the *study – is this a total precipitation or only snowfall? Please specify this in the Data&Methods section*

Reply: The precipitation variable used in this study refers to total precipitation (rainfall + snowfall). We have now clarified this in the Datasets section (2.1).

Also, as Fig 2e shows there seem to be a positive trend in extreme precipitation over the I-Ipp basin however difficult to see because of a very thick line denoting the basin. Maybe making the basin contour line thinner? This positive trend in the I-Ipp region is then mentioned on the next page (L180) so this contradicts the sentence above.

Reply: As suggested, we have reduced the basin-outline thickness in the plots showing the spatial distribution of precipitation trends over Antarctica (Figs. 2 and S1 in the revised manuscript). Over the Antarctic Peninsula, the I-Ipp basin was the only one to show significant basin-averaged positive trends in both total and extreme precipitation (see Figs. 2 and 3), which is clearly explained in section 3.1.

L90: The authors refer to Simpson et al 2023 paper, which describes a single-forcing methodoloy applied to CESM2 model. It will be very useful to provide key details which should help to better understand the results of the present study, especially how GHG forcing is defined in the CESM2 model.

Reply: We thank the reviewer for this helpful comment. In response we have carefully rewritten the description of the LE simulations produced using the CESM2 model to provide the information necessary to better understand the results of the study. This includes referencing a more appropriate study by Simpson et al. (2023). We have also added a description of the LE simulations produced using the CESM1 model. This text now clearly explains both the all-forcing and single-forcing simulations used. Please see the Dataset section (2.1).

Simpson, I. R., Rosenbloom, N., Danabasoglu, G., Deser, C., Yeager, S. G., McCluskey, C. S., ... & Rodgers, K. B. (2023). The CESM2 single-forcing large ensemble and comparison to CESM1: Implications for experimental design. Journal of Climate, 36(17), 5687-5711. https://doi.org/10.1175/JCLI-D-22-0666.1

The analysis in the present paper is done for 1979-2023, while the authors mention future simulations under SSP370 scenario (Lines 87-89). According to Simson et al (2023): "The CESM2 large ensemble, referred to hereafter as LENS2, is a 100-member ensemble of simulations run under CMIP6 historical forcings between 1850 and 2014 and forcings of the Shared Socioeconomic Pathway 3–7.0 (SSP3–7.0; Meinshausen et al. 2020) thereafter. " - Were the same periods used in the present study, meaning 1979-2014 were from historical simulations and 2014-2023 from SSP370 scenario? This and other relevant details have to be clearly described in the Data/Methodology section.

Reply: Thank you for noting this. In the present study we analyse LEs for the period from 1979 to 2023, using 1979-2014 from the CESM2 historical simulations and 2015-2023 from the SSP370 simulations, consistent with Simpson et al. (2023). We have now stated this clearly in the

Datasets section (3.1). For example, it now states: '*CESM2 includes a 50-member ALL ensemble that spans the period from 1850 to 2100 based on the CMIP6 (Coupled Model Intercomparison Project Phase 6) historical forcings from 1850 to 2014 and the SSP370 (Shared Socioeconomic Pathways, representing additional radiative forcing of 7 W m^{-2} by 2100) scenario from 2015 to 2100.*'

3. *L278-286: As the authors suggest, the presented results lack consistency stating "the signals from the GHG and AAER ensembles (and OTHERS) cannot be considered statistically significant for either total or extreme precipitation. " - does this undermine the entire study conclusion regarding the GHG signal in positive precipitation trends? This differs from the results obtained by Dalaiden et al (2022) who found positive and greater than 1 scaling factors for snow accumulation for GHG forcing. Can the authors put their result in perspective and analyze why there is such a difference?*

   This is also highlighted in the Discussion section: "However, the three-signal regression-based D&A analysis using the ALL, GHG and AAER ensembles (resulting in scaling factors for GHG, AAER, and OTHERS) was unable to provide robust attribution for total or extreme precipitation to any specific single-forcing, including increased greenhouse gases (Fig. 7). This inconsistency between these results demonstrates that formally detecting the fingerprint of anthropogenic forcing on recent Antarctic precipitation changes remains highly challenging (Previdi and Polvani, 2016; Dalaiden et al., 2022)." – this statement is conflicting with the paper title and its conclusions that intensification of extreme precipitation is due to increases in greenhouse gases. I invite the authors to carefully revise the methodology and/or rethink major conclusions of the study as well as better discussing the results in comparison to previous publications (including Dalaiden et al 2020).

   Reply: This is a very valuable comment, which we have addressed by adding results based on the LE simulations from the CESM1 global climate model, which was also used by Dalaiden et al. (2022). This includes all-forcing (ALL) simulation, as well as single-forcing simulations for greenhouse gases (GHG), anthropogenic aerosols (AER), and stratospheric ozone (O3). In fact the CESM1 results are now the mainstay of the manuscript as they show using the regression-based D&A approach that the precipitation changes are robustly attributed to increases in greenhouse gases in four basins and stratospheric ozone depletion in three basins, and increases in anthropogenic aerosols in only one basin (see section 3.3). In contrast, although the CESM2 based results also indicate that greenhouse gases are an important driver of the precipitation trends over these basins, the D&A analysis was unable to provide robust attribution for greenhouse gases (Fig. S6). These inconsistencies between the CESM1 and CESM2 results do demonstrate that detecting the fingerprint of anthropogenic forcing on recent Antarctic precipitation changes remains challenging, which we think is an important point in the manuscript, and discussed further in the Discussion and Conclusions section (4). Consequently, we have also revised our major conclusions, which are (see revised title and abstract):

   - A combination of both anthropogenic and natural forcings are responsible for the precipitation trends in our five target basins.

- Using CESM1 LE, the precipitation changes are robustly attributed to increases in greenhouse gases in four basins and stratospheric ozone depletion in three basins, and increases in anthropogenic aerosols in only one basin.
- Using CESM2 LE, also indicates that greenhouse gases are an important driver of the precipitation trends over these basins. However, these results suggest that increases in anthropogenic aerosol emissions can oppose / offset the increases in precipitation from increased greenhouse gases over our five basins, which contradicts the findings based on CESM1.

Minor comments:

4. *L39: It is difficult to understand which statement is supported by which reference with 12 references… In this and other instances with more than 5 references listed, I would recommend to break it into separate statements each supported by several key references.*

   Reply: We have considerably reduced the number of references in this sentence. It now states: 'Furthermore, mass gained from precipitation and EPEs over Antarctic ice shelves can partially compensate for mass-losses caused by either basal or surface melting (Paolo et al., 2015; Pattyn et al., 2017; Gardner et al., 2018; Rignot et al., 2019; Nakayama et al., 2021; Johnson et al., 2022).'

5. *L62 and L187: the section titles 3.1 Identification of precipitation trends and 3.2 Detection of precipitation trends seem to say basically the same thing – identification or detection. The difference is that section 3.1 shows trends detected in ERA5 and section 3.2 then shows trends detected in CESM2 model and continues with a quantitative trend analysis focusing only on selected basins with positive trends according to ERA5. This is confusing. My recommendation will be to separate into "Spatial distribution of precipitation trends" or along the lines discussing Fig 2 for both ERA5 and CESM2, and then the next section "Analysis for basins with positive precipitation trends" or similar.*

   Reply: We thank the reviewer for this helpful comment. These sub-section headings were actually very carefully chosen, in particular to reserve the term 'detection' to the D&A approach. Thus the heading for section 3.1 deliberately used the term 'identification' and not 'detection', as it focused on identifying and describing the precipitation trends in ERA5 (Identification of precipitation trends in ERA5). While the heading for section 3.2 used the term 'detection' as it was focused on examining whether the CESM1 LE detects the observed/ERA5 precipitation trends (Detection of precipitation trends in CESM1). We are also very careful to not use the term 'detect' in section 3.1 to avoid confusion.

6. *Figs 5&7: yaxis label: "sacling" – should be "scaling"*

   Reply: We thank the reviewer for pointing this out. The y-axis label in Figs. 5 and 7 has now been corrected from "sacling" to "scaling".

7. *L656: Typo: reference Wille et al 2021 listed twice*

Reply: We thank the reviewer for noticing this. The duplicate citation to Wille et al. (2021) has now been removed.

Anonymous Referee comment #2

This paper investigates whether recent trends in total and extreme precipitation over Antarctica are detectable and, if so, whether they can be attributed to anthropogenic forcing. Based on ERA5 precipitation data from 1979–2023, they perform a detection&attribution study using CESM2 large-ensemble simulations forced with all external forcings (ALL), greenhouse gases only (GHG), anthropogenic aerosols only (AAER). Because the precipitation trends over six basins where similar in both the ALL and GGH ensembles, they conclude that greenhouse gases are the primary driver of present-day trends in total and extreme precipitation in these regions. However, they note that robust attribution to any single forcing is complex because of the relatively small ensemble size. Finally, they also provide a brief discussion of the potential influence of atmospheric rivers on the observed precipitation changes even though this step is quite disconnected from the rest of the study.

The paper is well written overall, though some parts could read more smoothly. I expected this study to serve as a follow-up to previous work using the CESM1 and CanESM2 models (Dalaiden et al., 2022), with a particular emphasis on intense precipitation. Dalaiden et al (2022) concluded that: "We show that surface climate changes since the 1950s were driven by anthropogenic forcing, in particular the greenhouse gas forcing and stratospheric ozone depletion". Since the contribution of the Antarctic ozone hole to changes in snow accumulation is well established (e.g., Lenaerts et al., 2018), I found it surprising that the authors rely directly on CESM2 simulations, given that no CESM2 simulations including stratospheric ozone depletion are currently available (Simpson et al., 2023). This choice effectively neglects the influence of the ozone hole, leading the analysis to focus solely on other forcings. Since the authors themselves acknowledge that a robust attribution to any single forcing is not possible, the resulting conclusions on GHG are neither particularly novel nor fully convincing. This is disappointing, as the research group has a good expertise for a D&A analysis, and the study could have been substantially strengthened by including a comparison with CESM1 or CanESM2 simulations. Given that Q. Dalaiden is a co-author of the present study, such a comparison would seem feasible.

Therefore, I have difficulty in identifying the main new contribution of the paper. The link with atmospheric rivers could have been interesting, but it is curretly too limited, and providing a D&A analysis on the changes in atmospheric river frequency and intensity would constitute an entirely new study.

I believe that this paper cannot be published in its present form and requires either an additional study or a clear comparison with previous work to properly account for ozone depletion in the analysis. The simplest approach would likely be to directly apply their method to the simulations presented by Dalaiden et al. (2022) and compare the results with the current findings. I am aware that this represents a significant amount of work, but as it stands, the conclusions are comparatively fragile relative to those of earlier studies.

Reply: We thank the reviewer for this very helpful and constructive assessment. We appreciate the concern regarding the role of ozone depletion and agree that this is a key component of Antarctic precipitation changes. In response, we repeated the analysis using the CESM1 LE simulations (which includes ozone forcing simulations) used in Dalaiden et al. (2022) to investigate whether we could generate much more robust results. We were pleased to find that this additional analysis showed that the precipitation changes can be robustly attributed to both greenhouse gases and stratospheric ozone. Accordingly, in the revised manuscript we now present the CESM1 results in the main text, and move the CESM2 analysis to the Supplementary Material for comparison. Please also see the reply above to Reviewer #1 / Major comment #3, which explains in more detail the revised conclusion of our work in response to this change. Additionally, we also investigated using the CanESM2 LE simulations. However, as these simulations have a relative coarse resolution of 2.8 degrees (250-300 km), they were unable to clearly resolve the target basins, and thus proved unsuitable. By contrast, both CESM1 and CESM2 LE have a resolution of 1 degree.

Regarding ARs, we agree that a formal detection and attribution analysis of AR frequency or intensity is beyond the scope of the present work. However, our AR analysis is included to provide physical context for the precipitation trends, since ARs are known to be one of the major atmospheric drivers of Antarctic extreme precipitation (as mentioned in the Introduction). We genuinely feel that this additional analysis is of interest to the Antarctic atmosphere research community and readers of The Cryosphere, and provides a more complete / holistic understanding of the main results. We have also very deliberately kept this section (3.5) short and concise, consisting of one figure (Fig. 8) and one paragraph of explanation.

Major comments:

1. *A primary concern is the misuse of the term 'extreme' in this paper. This issue is not unique to the present work but is widespread in the Antarctic literature. For instance, Turner et al. (2019) or Adusumilli et al. (2021) used this term for precipitation exceeding the 95th percentile, but it should not be considered as extreme events. This is misleading from a statistical perspective, as true extremes follow different statistical laws. Events at the 95th percentile are not rare, they occur roughly 18 times per year. In contrast, annual maxima could reasonably be considered extreme. I recommend replacing the term "extreme" with "intense". Including a remark on this aspect in the text would also be welcome.*

   Reply: We thank the reviewer for this important comment on terminology. We agree that "extreme" can be defined more narrowly in an extreme value theory sense, for example using annual maxima, but in climate science it is also routinely used in a peaks over threshold framework where events are defined by exceedance of a high percentile at a given location. This broader usage is explicitly recognised in the IPCC AR6 glossary, which notes that heavy or extreme precipitation can be characterised either using block maxima or using threshold exceedances above a high percentile. In this paper, we use "extreme precipitation events" in this percentile based, peaks over threshold sense, because it is widely adopted in the Antarctic precipitation literature and is well suited for analysing event frequency and drivers over multi decadal periods. For instance, Turner et al. (2019) define extreme precipitation events using a 90th percentile threshold of precipitation days and demonstrate that these events dominate Antarctic snowfall variability. Similar terminology and percentile based definitions continue to be used in various Antarctic studies linking threshold based precipitation, including Wille et al. (2021), Adusumilli et al. (2021), Simon et al. (2024), Jackson et al. (2023), Wauthy and Dalaiden (2025), Gilbert et al. (2025), sui et al., 20205, Yu et al., 2018, Welker et al., 2014, Wang et al., 2024, Swetha Chittella et al., 2022. We therefore retain the term "extreme" to remain consistent with this established Antarctic literature.

In addition, we note that the statement that 95th percentile events occur "roughly 18 times per year" implicitly assumes that precipitation occurs on almost every day. In our framework, the percentile threshold is computed at each grid point using precipitation days only (defined as days with precipitation ≥ 0.02 mm day^{-1}), so the expected number of above threshold events is approximately 5% of the local precipitation days per year, and therefore varies across Antarctica. This spatial pattern is shown in the figure below, which presents the mean annual number of EPE days during 1979-2023: the largest values are concentrated mainly along coastal regions where precipitation days are common, while the interior plateau has markedly fewer events above the threshold per year. Because our results are interpreted at basin and continental scales, this definition ensures that extreme precipitation is characterised across both coastal and interior regions, rather than focusing only on coastal events.

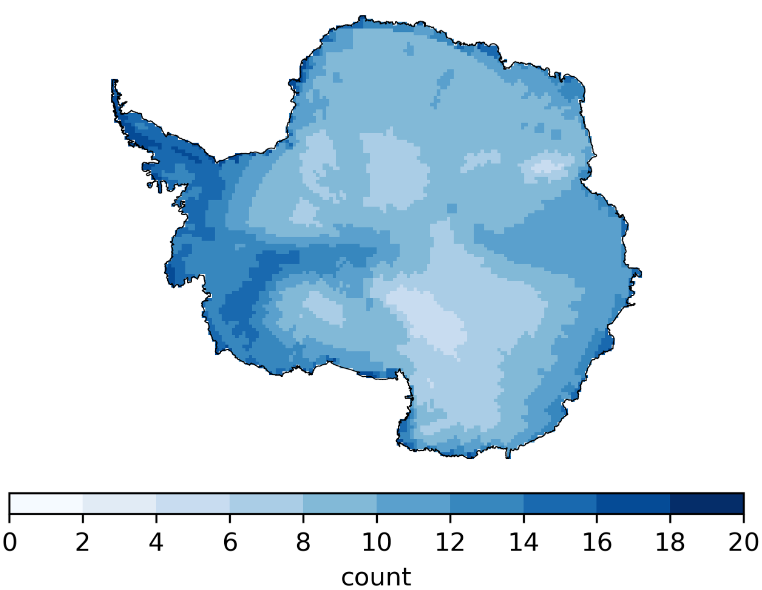

Figure: Spatial distribution of the mean annual number of EPE days (1979–2023) from ERA5. EPEs are defined days with precipitation exceeding the 95th percentile of all precipitation days (days with precipitation ≥ 0.02 mm day^{-1}) over the period 1979 to 2023.

IPCC. (2022). Annex II: Glossary (V. Möller, R. van Diemen, J. B. R. Matthews, C. Méndez, S. Semenov, J. S. Fuglestvedt, & A. Reisinger, Eds.). In H.-O. Pörtner, D. C. Roberts, M. Tignor, E. S. Poloczanska, K. Mintenbeck, A. Alegría, M. Craig, S. Langsdorf, S. Löschke, V. Möller, A. Okem, & B. Rama (Eds.), Climate Change 2022: Impacts, Adaptation and Vulnerability.

Contribution of Working Group II to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change (pp. 2897–2930). Cambridge University Press. https://doi.org/10.1017/9781009325844.029

Turner, J., Phillips, T., Thamban, M., Rahaman, W., Marshall, G. J., Wille, J. D., ... & Lachlan‐Cope, T. (2019). The dominant role of extreme precipitation events in Antarctic snowfall variability. Geophysical Research Letters, 46(6), 3502-3511. https://doi.org/10.1029/2018GL081517

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Adusumilli, S., A Fish, M., Fricker, H. A., & Medley, B. (2021). Atmospheric river precipitation contributed to rapid increases in surface height of the west Antarctic ice sheet in 2019. Geophysical Research Letters, 48(5), e2020GL091076. https://doi.org/10.1029/2020GL091076

Simon, S., Turner, J., Meloth, T., Deb, P., Gorodetskaya, I. V., & Lazzara, M. (2024). An extreme precipitation event over Dronning Maud Land, East Antarctica-A case study of an atmospheric river event using the Polar WRF Model. Atmospheric Research, 311, 107724.

Jackson, S. L., Vance, T. R., Crockart, C., Moy, A., Plummer, C., & Abram, N. J. (2023). Climatology of the Mount Brown South ice core site in East Antarctica: implications for the interpretation of a water isotope record. Climate of the Past, 19(8), 1653-1675. https://doi.org/10.5194/cp-19-1653-2023

Wauthy, S., & Dalaiden, Q. (2025). Role of precipitation and extreme precipitation events in the surface mass balance variability observed in three ice cores from coastal Dronning Maud Land. *EGUsphere*, *2025*, 1-20. https://doi.org/10.5194/egusphere-2025-192

Gilbert, E., Pishniak, D., Torres, J. A., Orr, A., Maclennan, M., Wever, N., & Verro, K. (2025). Extreme precipitation associated with atmospheric rivers over West Antarctic ice shelves: insights from kilometre-scale regional climate modelling. The Cryosphere, 19(2), 597-618. https://doi.org/10.5194/tc-19-597-2025

Sui, C., Yu, L., Zhong, S., & Feng, L. (2025). Trends in extreme precipitation events across Antarctica. *Advances in Atmospheric Sciences*, *42*(12), 2471-2484.

Yu, L., Yang, Q., Vihma, T., Jagovkina, S., Liu, J., Sun, Q., & Li, Y. (2018). Features of extreme precipitation at Progress Station, Antarctica. Journal of Climate, 31(22), 9087-9105. https://doi.org/10.1175/JCLI-D-18-0128.1

Welker, C., Martius, O., Froidevaux, P., Reijmer, C. H., & Fischer, H. (2014). A climatological analysis of high‐precipitation events in Dronning Maud Land, Antarctica, and associated large‑scale atmospheric conditions. Journal of Geophysical Research: Atmospheres, 119(21), 11-932. https://doi.org/10.1002/2014JD022259

Wang, S., Ding, M., Liu, G., Li, G., & Chen, W. (2024). Blocking events in East Antarctica: Impact on precipitation and their association with large-scale atmospheric circulation modes. *Journal of Climate*, *37*(4), 1333-1345. https://doi.org/10.1175/JCLI-D-23-0419.1

Swetha Chittella, S. P., Deb, P., & Melchior van Wessem, J. (2022). Relative contribution of atmospheric drivers to "extreme" snowfall over the Amundsen Sea Embayment. Geophysical Research Letters, 49(16), e2022GL098661. https://doi.org/10.1029/2022GL098661

2. *This comment is related with my first remark in the introduction of this report. The detection step leads to the consideration of only six basins where CESM2 and ERA5 trends are in agreement, whereas basins with negative trends are excluded. However, this exclusion could reflect the model's inability to reproduce the real processes driving the spatial pattern of accumulation changes in Antarctica. What result woould be obtained if the same approach was based on simulations from Dalaiden et al. (2022). in the latter publication, they show clearly more negative and significant trends in these regions. Overall, the current approach gives the impression of cherry-picking, or that CESM2 misrepresents the impact of anthropogenic forcings on recent Antarctic changes. The latter could reflect that the model induces an artificial overestimation of the precipitation/temperature relationship in CESM2 (Clausius–Clapeyron)? If so, could this in turn bias or exaggerate the inferred link with greenhouse gases? The overall increase in Antarctic snowfall (in figure 2) could reflect that. Could such an effect potentially mask the impact of circulation changes? How are precipitation changes correlated with temperature changes? This issue is further more complex to interpret because, although the trends in the selected basins are similar in absolute value, they occur in basins where the average precipitation is roughly twice as high as in ERA5. If the data were standardized, the trends would appear approximately half as large as those in ERA5.*

   Reply: We thank the reviewer for this important comment. We would like to clarify that our approach does not begin by selecting basins based on model behavior. In the first step, we identify basin-averaged trends from ERA5 (Fig. 2 in the manuscript) and retain only those basins where the observed (ERA5-based) trends are statistically significant, for both total and extreme precipitation. In our study, six of the 18 Antarctic drainage basins show significant ERA5 trends, and all of these are positive (Figs. 2 and 3 in the manuscript). We then examine whether CESM1 (i.e., the model used by Dalaiden et al., 2022) and CESM2 reproduce the same positive and statistically significant trends in these ERA5-identified basins (Figs. 2, 3, S1, and S2), and only then proceed with the detection and attribution analysis. In fact, both CESM1 and CESM2 show broadly similar precipitation trends (Figs. 2 and S1) and show the same five domains in agreement with ERA5 (Figs. 3 and S2). We do mention in the revised manuscript that there are patchy areas that show significant negative trends over Marie Byrd Land, Victoria Land, and Wilkes Land. However, when we examine the basin-averaged trends, none of the basins showed significant negative trends - so please be reassured that we are definitely not cherry picking. To avoid any confusion in the revised manuscript, we have added an additional sentence in section

3.1 (Results) that clearly states this:: '*Note that within the regions showing negative ERA5-based trends, none of the drainage basins showed significant negative basin-averaged trends.*'

Additionally, with regard to the final part of this comment, the new CESM1 precipitation amounts for the five target basins are actually consistent with the ERA5-based trends, i.e., the ensemble-mean value of ALL is close to the ERA5 value, and the interannual variability of the observed precipitation amounts is within the ensemble spread of the CESM1 model (Fig. 4). This is mentioned in the Results section (3.2) by the sentence: '*Additionally, for all basins the ensemble spread of ALL encompasses the ERA5-based precipitation amounts, indicating confidence in the ability of CESM1 to capture regional internal variability, as well as the trends.*' By contrast, the CESM2 precipitation amounts are appreciably higher than ERA5 (Fig. S3).

We also note that our results are not directly equivalent to Dalaiden et al. (2022), because the studies differ in an important way in the analysis period. Dalaiden et al. 2022 analyze an earlier period (1959-2000), whereas our study focuses on 1979-2023. This difference in period can affect both the sign and magnitude of regional trends, and likely contributes to the differences in the spatial pattern of trends between the two studies.

Regarding the reviewer's broader points on possible model biases (including the precipitation–temperature relationship, mean-state precipitation biases, and the possible role of circulation changes), we agree these are important questions. However, a detailed evaluation of model biases and process-level mechanisms is beyond the scope of the present study, which focuses on detection and attribution of observed precipitation trends.

3. *The spatial distribution of observed SMB variations is largely linked to trends in the Southern Annular Mode (SAM; Meddley and Thomas, 2019), which is itself strongly influenced by the Antarctic ozone hole. Lenaerts et al. (2018) and, later, Dunmire et al. (2020) also suggested this role of the ozone hole. Moreover, in ALL, if I am not wrong, the impact of ozone is considered, but its effect on precipitation distribution is weaker than in ERA5 ; it is also the case if compared to the simulations from Dalaiden et al. (2022). Could you justify why these significant differences are observed ? I suggest performing the same D&A analysis using CESM1 or CanESM2 simulations provided by Dalaiden et al. (2022), particularly since Q. Dalaiden is a co-author. At least providing more accurate comparisons with previous studies is necessary.*

   Reply: We thank the reviewer for this very valuable suggestion. As already outlined above in response to this comment, we have now included the CESM1-based results in the main manuscript (with the CESM2 results moved to the Supplementary Material).This revision allows a more direct comparison with previous work and helps place our results in better perspective, and undoubtedly improves the relevance and importance of the manuscript. We also explored the use of CanESM2 simulations. However, the native horizontal resolution of CanESM2 is much coarser (about 2.8°), which would require regridding ERA5 (0.25°) to a much coarser grid for the spatial comparison. Given the strong spatial gradients in Antarctic precipitation, especially near the coast and over topographically complex regions, such coarse regridding can substantially smooth the signal and reduce the reliability of basin-scale trend analysis. For this reason, we did not include CanESM2 in the final analysis. In the revised manuscript, the CESM1 results show that ozone forcing plays an important role in the ALL precipitation trends, and the attribution analysis indicates that greenhouse gases and stratospheric ozone contribute to the positive precipitation trends. We have revised the Data and Methods and Results sections accordingly.

4. *Atmospheric rivers (ARs) are only briefly addressed in the paper, which focuses on quite different aspects. Important methodological information on AR precipitation calculations is missing in the text. Calculation of their role in precipitation trends should be improved (or better explained). Moreover, the authors did not analyze whether changes in trends resulted from changes in frequency, intensity (or both), and what was the cause of these changes. Consequently, in its current form, the study of ARs in the paper appears somewhat artificial. It would have been interesting to perform a D&A analysis on AR changes, but this would constitute a significantly different paper. I rather suggest to remove this part of the study.*

   Reply: We respectfully disagree with the Reviewers suggestion to remove this part of the study. As we explained earlier, we are not in any way suggesting that this analysis is complete. However, we do feel that it's of interest to many readers and complements the main results based on the D&A analysis. The observed / ERA5 based precipitation trends that are statistically significant are rather patchy, which is likely consistent with these coastal regions being characterised by large inter-annual variability, which is entirely consistent with the importance of extreme precipitation events and ARs. It's therefore of interest that the areas that show that the relative contributions of the AR trends to the ERA5-based precipitation trends have increased substantially across all five basins. Additionally, as it is placed at the very end of the Results section and only takes up a single figure, we do not feel that it disrupts the main results in any way. However, we did refine the method that we contribute the percentages for the relative contributions of AR-associated precipitation (Fig. 8), as this could be spuriously large if the ERA5-based values are close to zero. This was therefore avoided by only calculating this for grid cells where the ERA5-based trends exceed a threshold of 0.05 mm per year.

Other specific comments:

5. *Line 27: "Extreme precipitation events (…) have increased in intensity and frequency globally in recent decade (…) which has been attributed to anthropogenically-induced climate change". I find this sentence a bit too affirmative. In chapter 11 from the IPCC, AR6, they write that "Regional increases in the frequency and/or intensity of heavy precipitation have been observed with at least medium confidence for nearly half of AR6 regions" they also write that "The assessment focuses on land regions excluding Antarctica.". This indicates that there is still considerable work to be done, particularly in regions where the time series are too short to clearly demonstrate changes or trends in extremes, more particularly in Antarctica.*

   reference : Seneviratne, S.I., X. Zhang, M. Adnan, W. Badi, C. Dereczynski, A. Di Luca, S. Ghosh, I. Iskandar, J. Kossin, S. Lewis, F. Otto, I. Pinto, M. Satoh, S.M. Vicente-Serrano, M. Wehner, and B. Zhou, 2021: Weather and Climate Extreme Events in a Changing Climate. In Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change[Masson-Delmotte, V., P. Zhai, A. Pirani, S.L. Connors, C. Péan, S. Berger, N. Caud, Y. Chen, L. Goldfarb, M.I. Gomis, M. Huang, *K. Leitzell, E. Lonnoy, J.B.R. Matthews, T.K. Maycock, T. Waterfield, O. Yelekçi, R. Yu, and B. Zhou (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 1513–1766, doi: 10.1017/9781009157896.013.*

Reply: We thank the reviewer for this helpful comment. We agree that the original wording could be read as applying to Antarctica, which was not our intention. We have therefore revised the text to clarify that the IPCC AR6 assessment of observed heavy precipitation changes largely focuses on land regions and excludes Antarctica due to limited observations (Seneviratne et al., 2021). This makes clear that the statement refers to the global evidence base and not to observed trends in Antarctic extremes.

6. *Line 34: In Yu et al. (2018) and Simon et al. (2024), the analysis focuses on the 95th, or even the 90th, percentile. These values should not be considered as "extreme" but rather as "intense".*

   Reply: Thank you for this comment. We acknowledge that a 90th or 95th percentile threshold can also be described as "intense"; however, in this study we retain the term "extreme precipitation events" because percentile-based definitions at these levels are widely labelled as "extreme" in the Antarctic precipitation literature, and using the same terminology improves consistency and comparability with previous work. Please see the reply to your earlier comment on this for further information.

7. *Line 35: Antarctic precipitation and EPEs are the major component of surface mass balance (SMB) => This remark depends strongly on the threshold used. The 95th percentile represents a substantial contribution to the total SMB (Turner et al., 2019), but if the 99th percentile is considered, the contribution becomes much smaller. I agree that the March 2022 heat-wave event contributed to "up to 90% of the March 2022 total precipitation in local areas of the East Antarctic Ice Sheet" (Wille et al., 2024b), indicating that such events can have a critical impact. However, in that case, please cite appropriate references (e.g., Wille et al., 2024b; Clem and Raphael, 2023).*

   references :

   Clem, K. R. and M. N. Raphael, Eds., 2023: Antarctica and the Southern Ocean [in "State of the Climate in 2022"]. Bull. Amer. Meteor. Soc., 104 (9), S322–S365, https://doi.org/10.1175/BAMS-D-23-0077.1

   Wille, J. D., and Coauthors, 2024: The extraordinary March 2022 East Antarctica "heat" wave. Part II: Impacts on the Antarctic ice sheet. J. Climate, 37, 779–799, https://doi.org/10.1175/JCLI-D-23-0176.1.

   Reply: We thank the reviewer for this comment. We agree that the contribution of extreme precipitation events (EPEs) to SMB depends on the threshold used to define events, with higher-percentile definitions (e.g., 95th percentile) yielding a larger fractional contribution than rarer thresholds (e.g., 99th percentile). To avoid implying a threshold-independent statement, we revised the sentence to clarify that the contribution of EPEs depends on the event definition, and we now cite Turner et al. (2019), Clem and Raphael (2023), and Wille et al. (2024b).

8. *Line 47: "Moreover, an enhanced understanding of the drivers of these trends, such as anthropogenically-induced changes (i.e., external climate forcing), is also of critical importance to better highlight how EPEs" => the authors are attempting to address two distinct questions. Each question would warrant a dedicated study. The first part (on drivers) concerns potential changes in atmospheric circulation and their causes, such as links with lower latitudes or major modes of variability. This question is approached through an analysis of atmospheric rivers (ARs), but in the present paper the treatment of ARs is too superficial to adequately address it. I suggest to remove this analysis on ARs. The second part of the sentence pertains to D&A techniques, which is the actual focus of this study.*

   Reply: We thank the reviewer for this comment. Undoubtedly the main aim of this study is to investigate the physical origins of the precipitation trends that are observed, and particularly the role of external forcing - this is absolutely clear from the title, abstract, Introduction, etc. This alone warrants its own study, which is more or less what this paper has done. We agree that explaining the large-scale circulation changes and their links to lower latitudes or modes of variability is an important topic, and it has already been addressed in previous studies. Our paper does not aim to re-assess those dynamical drivers. Instead, our AR analysis is included for a narrower purpose: since ARs are a major source of heavy precipitation over Antarctica, we use the AR analysis only to show how much of the precipitation and EPE trends in our basins occurs on AR days; the main aim of the paper remains the detection–attribution analysis.

9. *lines 51-57: The list of potential drivers of change provided here is far from being exhaustive and this reflects my earlier comment on the complexity of addressing the question of the drivers. This point could be introduced, as noted in line 47, but it would also be reasonable to exclude it from the study's scope, provided this is clearly justified in the text.*

   Reply: The introduction explains that increased greenhouse gases, increased anthropogenic aerosols, and stratospheric ozone depletion are some of the potential drivers (external forcing) of changes over Antarctica, as well as natural variability (internal forcing). These are also the external and internal forcings that are available using the CESM1 LE simulations, i.e., ALL, GHG, AER, O3. The list of potential drivers therefore seems justified, and consistent with the datasets and methods applied.

10. *Line 57-59: I don't fully understand this sentence. Could you the directions of trends and the sign of feedback between the various interacting factors?*

    Reply: This sentence has been deleted as it was unclear. Instead, the proceeding sentence has been modified to read: "*However, the strong internal variability prevailing in the Southern Hemisphere high latitudes makes it challenging to detect the signal of anthropogenically-produced climatic changes from the observed changes during the past decades (e.g., Fyke et al., 2017), as well as when this signal will emerge from natural variability in the future (Trusel et al., 2015; Previdi and Polvani, 2016; Morioka et al., 2024).*"

11. *Line 60: "although some studies have investigated the drivers of trends of extreme temperatures over Antarctica (e.g., Blanchard-Wrigglesworth et al., 2023; Wille et al., 2024b)" => to my knowledge, these papers do not present trends in extreme temperatures.*

    Reply: We thank the reviewer for pointing this out. We agree that Blanchard-Wrigglesworth et al. (2023) and Wille et al. (2024b) primarily investigate the drivers of heatwaves. We therefore felt that these references were not appropriate and have been removed. The revised sentence is: '*This study addresses the knowledge gap around our understanding of the drivers of changes in EPEs over Antarctica by performing a detection and attribution (D&A) analysis to investigate the influence of external forcings and natural variability on annual trends in total and extreme precipitation across Antarctica from 1979 to the present-day*."

12. *Line 75: (Min Xu et al., 2024) is not in the reference list.*

    Reply: We thank the reviewer for pointing this out. The in-text citation was incorrect and has now been corrected to Yu et al. (2025). This reference is already present in the reference list.

13. *Line 78: "As ARs are an important contributor to EPEs" => please add a reference*

    Reply: We thank the reviewer for this comment. We have added supporting references. They are Wille et al., 2025; Maclennan et al., 2022; Gorodetskaya et al., 2014

    Wille, J. D., Favier, V., Gorodetskaya, I. V., Agosta, C., Baiman, R., Barrett, J. E., ... & Zhang, Z. (2025). Atmospheric rivers in Antarctica. Nature Reviews Earth & Environment, 6(3), 178-192. https://doi.org/10.1038/s43017-024-00638-7

    Maclennan, M. L., Lenaerts, J. T., Shields, C., & Wille, J. D. (2022). Contribution of atmospheric rivers to Antarctic precipitation. Geophysical Research Letters, 49(18), e2022GL100585. https://doi.org/10.1029/2022GL100585

    Gorodetskaya, I. V., Tsukernik, M., Claes, K., Ralph, M. F., Neff, W. D., & Van Lipzig, N. P. (2014). The role of atmospheric rivers in anomalous snow accumulation in East Antarctica. Geophysical Research Letters, 41(17), 6199-6206. https://doi.org/10.1002/2014GL060881

14. *Line 79: If I am not wrong, Wille et al. (2025) do not define a unique ARDT. Which ARDT did you use? How did you attribute precipitation to an AR? i.e., in which contour did you compute the precipitation attributed to an AR?*

    Reply: We used the polar AR detection catalogue based on ERA5 from Wille et al. (2021). Wille et al. (2025) also uses and refers to this same Wille et al. (2021) detection product in their data availability section. To avoid confusion, we have corrected the text to cite Wille et al. (2021) directly, since this is the AR detection dataset we use. We define 'precipitation attributed to an AR' as AR-associated precipitation, meaning ERA5 precipitation that occurs within the atmospheric river footprint provided by the Wille et al. (2021) AR detection catalogue. In practice, we overlay the AR footprint masks on the ERA5 daily precipitation and precipitation at grid cells flagged as being inside an AR footprint. We have now stated this clearly in the revised manuscript.

15. *Line 86: "We use CESM2 ensembles that consider all external forcings (ALL; 50 members) and span the period from 1850 to 2100 under CMIP6 (Coupled Model Intercomparison Project Phase 6)"=> What is the difference between simulations used by Dunmire et al. (2022) and those selected in your study? Indeed, Dunmire et al. (2022) write that "AIS historical precipitation trends in CESM2 appear to be largely driven by the increasing SAM and intensifying Antarctic ozone depletion". Consequently, could you clarify why they find a significant role for the ozone hole, whereas your study does not*

    Reply: We thank the reviewer for this comment. The difference mainly comes from how the ozone-related influence is discussed. Dunmire et al. (2022) analyzed CESM2 simulations (which do not include an ozone LE) and noted that the CESM2 precipitation trend pattern appears consistent with increasing SAM and Antarctic ozone depletion, with reference to Lenaerts et al. (2018). However, the Lenaerts et al. study was based on CESM1 simulations (which include an ozone LE), whereas Dunmire et al. (2022) did not present a separate CESM2 ozone simulation in the same way.
    However, following the reviewer's suggestion, we repeated the analysis with CESM1 LE simulations that include ozone forcings, and we now find a significant ozone contribution to the positive precipitation trends.

16. *Line 92: "Here, the GHG and AAER concentrations are evolving in time in their respective simulations, while all other forcings are fixed at 1850 values" => It is written above that you consider prescribed stratospheric ozone depletion, please clarify.*

    Reply: This comment is in relation to CESM2. The atmospheric component of this model has a top near 40 km and uses prescribed stratospheric ozone depletion (Kay et al., 2015; Danabasoglu et al., 2020; Deser et al., 2020; Simpson et al., 2023). This is clearly explained in the Datasets section (2.1).

Kay, J. E., Deser, C., Phillips, A., Mai, A., Hannay, C., Strand, G., et al. (2015). The community Earth system model (CESM) large ensemble project: A community resource for studying climate change in the presence of internal climate variability. Bulletin of the American Meteorological Society, 96(8), 1333–1349. https://doi.org/10.1175/BAMS-D-13-00255.1

Danabasoglu, G., Lamarque, J. F., Bacmeister, J., Bailey, D. A., DuVivier, A. K., Edwards, J., ... & Strand, W. G. (2020). The community earth system model version 2 (CESM2). Journal of Advances in Modeling Earth Systems, 12(2), e2019MS001916. https://doi.org/10.1029/2019MS001916

Deser, C., Phillips, A. S., Simpson, I. R., Rosenbloom, N., Coleman, D., Lehner, F., ... & Stevenson, S. (2020). Isolating the evolving contributions of anthropogenic aerosols and greenhouse gases: A new CESM1 large ensemble community resource. Journal of climate, 33(18), 7835-7858. https://doi.org/10.1175/JCLI-D-20-0123.1

Simpson, I. R., Rosenbloom, N., Danabasoglu, G., Deser, C., Yeager, S. G., McCluskey, C. S., ... & Rodgers, K. B. (2023). The CESM2 single-forcing large ensemble and comparison to CESM1: Implications for experimental design. Journal of Climate, 36(17), 5687-5711. https://doi.org/10.1175/JCLI-D-22-0666.1

17. *Line 102: the definition of "extreme" precipitation should be presented sooner. Nevertheless, please replace extreme by intense.*

    Reply: We thank the reviewer for this comment. Our informal definition of EPEs is actually made in the opening sentence of the Introduction (where the amount of precipitation at a location is substantially greater than normal). We then make the formal definition of extreme precipitation used in the analysis in the Methods section (2.2; extreme precipitation as days with precipitation exceeding the 95th percentile of all precipitation days over the period 1979 to 2023). This seems appropriate. Please see our explanations above for preferring to retain the term 'extreme' precipitation and not 'intense' precipitation.

18. *Line 111: "can be detected".*

    Reply: We thank the reviewer for noticing this. We have corrected the text.

19. *Line 112 : "we summed the 3-hourly precipitation values from the AR detection dataset over 24-hour intervals to produce daily AR-associated precipitation" => Over which area is it calculated? Is it the over the AR contour produced by Wille's ARDT?*

    Reply: We thank the reviewer for pointing this out. This is calculated on a grid point by grid point basis using the Wille et al. (2021) ERA5 based AR detection catalogue. For each 3-hourly time step, ERA5 precipitation at a given grid cell is counted as AR associated only if that grid cell lies within the AR footprint mask, and these AR masked precipitation values are then summed over 24-hour intervals to obtain daily AR associated precipitation. The text in the Methods section (2.2) has been revised to make this clear.

20. *Line 118: "the relative contribution of AR-associated total and extreme precipitation trends to the ERA5-based total and extreme precipitation trends"=> how did you proceed? Did you compute precipitation trends with all the data including ARs, and then excluding ARs?*

    Reply: We thank the reviewer for this helpful comment. In the original analysis, ERA5-based total and extreme precipitation trends were computed directly from the full ERA5 precipitation fields (i.e., without explicitly decomposing them into AR and non-AR components), and therefore include precipitation from both AR and non-AR conditions.

    Following the reviewer's suggestion, we also tested an explicit decomposition of ERA5 precipitation into AR-associated and non-AR-associated components. In this approach, ERA5-based total precipitation is expressed as the sum of (i) AR-associated total precipitation and (ii) non-AR-associated total precipitation, where non-AR-associated precipitation is defined as total precipitation minus AR-associated precipitation. The same procedure was applied for extreme precipitation. This alternative calculation gave very similar ERA5-based total and extreme precipitation trends.

21. *Line 188 and line 210: I already wrote that, but would this agreement still be real if accumulation were standardized? Since CESM2 accumulation is roughly twice as large as in ERA5, I would expect the relative trends to be about twice as large as well. This would affect also Figures 3 and 4. How do you explain the disagreement in Wilkes land and Marie Byrd Land ? I do not understand why this disagreement is less pronounced in the results of Dalaiden et al. (2022). Could the authors clarify the causes of the differences between CESM1 and CESM2? Indeed, if CESM2 misrepresents the Antarctic climate, the attribution based on it could be unreliable.*

    Reply: Given that the mainstay of the results are now based on the CESM1 model, this comment is less applicable. Our new results show that the precipitation amounts simulated by CESM1 are similar to ERA5 (Fig. 4). Additionally, we would like to clarify that a larger mean precipitation amount in CESM2 does not automatically imply that the trend should also be proportionally larger when expressed in absolute units.

    Regarding the disagreement in Wilkes Land and Marie Byrd Land between our results and those from Dalaiden et al. (2022), we do not interpret this as a contradiction because the Dalaiden et al. (2022) study uses a different analysis period of 1959 to 2020, while we use 1979 to 2023. Therefore the spatial trend patterns are not directly comparable (especially in regions where decadal variability is strong).

    Additionally, we have now added CESM1 results (as used in Dalaiden et al., 2022) to improve the comparison (section 3.4). We find that the results based on the CESM2 ALL ensemble are broadly consistent with those based on the CESM1 ALL ensemble, showing significant positive annual trends in total and extreme precipitation over the same regions, which agree with ERA5 (Fig. S1). Additionally, the results based on the CESM2 ALL ensemble also identify the same five basins that are associated with significant positive trends in both total and extreme precipitation, which agree with ERA5 (Fig. S2). This therefore suggests that there is quite a strong consensus between the CESM1 and CESM2 results, which undoubtedly strengthens the manuscript.

22. *Line 220: What would happen if you considered larger areas, as in Dalaiden et al. (2022), rather than restricting the analysis only to basins where the trends are similar?*

    Reply: We thank the reviewer for this suggestion. We tested a larger area analysis similar to Dalaiden et al. (2022) by combining two to three basins within each region. However, when considering broader regions, the detection and attribution results were not robust and we did not obtain detectable signals, likely because spatial averaging over larger areas reduces the signal to noise ratio and increases the influence of internal variability. For this reason, we focus on the basins where ERA5 and the ALL ensemble show similar trends, which provides a more consistent basis for the detection analysis.

23. *Figure 4: Caption and elsewhere in the text: the names of catchments are impossible to retrieve without figure 1. It would be helpful to include the name of the region each time you mention a catchment.*

    Reply: We thank the reviewer for pointing this out. Following this suggestion, we now specify the region associated with each catchment wherever it is mentioned, both in the text and the captions.

24. *Line 118 and section 3.4, Line 293: role of AR in precipitation trends. The current calculation yields relative AR contributions to precipitation trends of up to 100% in some catchments, even where ARs are generally rare. Is this because the AR-related trend is driven by a single event or by very few events? In any case, it seems unexpected that trends in intense precipitation would be 100% attributed to ARs when ARs do not account for all intense precipitation events*

    Reply: We thank the reviewer for pointing this out. We agree that the very large relative AR contributions (including values approaching 100% in some interior regions) in the earlier version were mainly caused by the calculation becoming unstable where the total precipitation trend is very small. In those regions, even a small AR-associated trend can produce an artificially large percentage because the denominator is close to zero.

    To address this, we have revised the calculation and updated the figure accordingly. We now express the ERA5 total precipitation trend as the sum of AR-associated and non-AR-associated precipitation trends (with non-AR precipitation defined as total precipitation minus AR-associated precipitation). The relative AR contribution is then calculated only at grid cells where the magnitude of the total precipitation trend exceeds a threshold of 0.05 mm year^{-1}; grid cells below this threshold are masked to avoid spuriously large percentages. We apply the same approach for extreme precipitation trends as well. We have added this clarification to the Methods section and revised the figure in the manuscript.

    With this correction, the spatial pattern is much more physically consistent: the largest AR contributions are concentrated mainly in coastal regions, while the unrealistically high contributions previously seen over low-precipitation interior regions are no longer present. We agree with the reviewer that the earlier result was influenced by the small denominator issue, and this has now been corrected.

    The revised figure and caption are provided below.

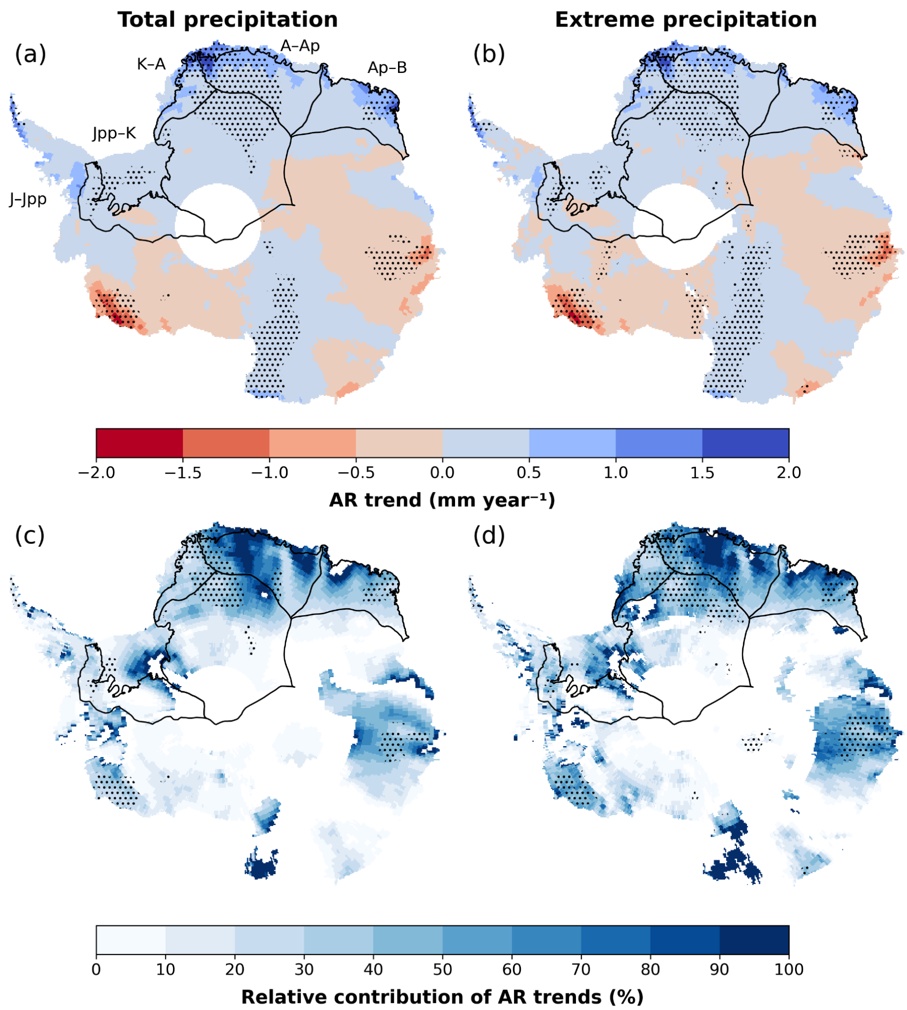

Figure 8: Annual trends in AR-associated (a) total and (b) extreme precipitation over Antarctica from 1980 to 2023 (mm year-1) from the AR detection dataset. Also shown are the relative contribution of the (c) AR-associated total precipitation trend to the ERA5-based total precipitation trend (%), and the (d) AR-associated extreme precipitation trend to the ERA5-based extreme precipitation trend (%). Stippling indicates regions where trends are statistically significant at the 90% confidence level. Also shown in panel (a) are the locations of drainage basins J-Jpp, Jpp-K (both part of Filchner-Ronne basin), K-A, A-Ap (both part of Dronning Maud Land), and Ap-B (Enderby Land).

25. *Line 318: "this agrees with Casado et al. (2023)"=> Could this result simply be due to coincidence and indicate that the model is incorrectly reproducing, or is masking, the effects of SAM variability?*

Reply: Perhaps, although it is also consistent with the argument that the strong internal variability prevailing in the Southern Hemisphere high latitudes makes it challenging to detect the signal of anthropogenically-produced climatic changes from the observed changes. Moreover, we now use two models (CESM1 and CESM2) to essentially examine model-dependency - yet both these models identify the same regions (five target basins) that have significant positive precipitation trends.

26. *Line 324: There is no D&A analysis for AR changes. Consequently, the study of ARs appears fully disconnected from the rest of the analysis.*

    Reply: As mentioned in the reply to other comments above, we genuinely don't feel this part of the results is disconnected from the rest of the analysis. The reviewer in their opening comments mentioned that this study was 'well written overall', and we feel that the AR analysis is evidence of how carefully and thoroughly we have undertaken this work.