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

Anonymous Referee comment #2

We would like to sincerely thank you for your further constructive and insightful comments on our manuscript. We greatly appreciate the time and effort you have taken to carefully review our work (both for the first and second reviews), which has clearly substantially improved the robustness and confidence of our study. We have implemented all your suggestions from your second review in the revised manuscript. In particular, changing the approach to compute basin-averages in response to your major comment #4 altered the results quite substantially, which necessitated another major rewrite (following the major rewrite from the first review) - but the fact the results were so sensitive to this aspect absolutely justifies that this was necessary. Consequently, the revised manuscript includes

- Revised title, which now mentions East Antarctica.
- Revised Abstract
- Revised methods section, which now explains our new approach to computing basin-averages.
- Revised results section, as Figs. 3-6 and Table 2 for CESM1 (and equivalent SI figures for CESM2) have all changed, i.e., the detection and attribution sections of the results. We have also added a new Table (Table 1 in manuscript), which gives the ERA5-based basin-averaged annual linear trends in total and extreme precipitation for all the drainage basins.
- Revised Discussion and Conclusions section, which better discusses the initial CESM1 results, and the differences with CESM2.

Below, we provide detailed point-by-point responses to each of your comments. Your comments are shown in italics and our responses in blue. Any revised text is highlighted in bold.

I sincerely thank the authors for their valuable work. The results are clearer and have improved the overall quality of the paper. However, I still have major comments that were already raised in my previous review but have not been addressed, even though I believe they are important to consider.

Comment #1

Regarding the notion of extreme events, I maintain the same view on the use of this term in the text. I acknowledge that defining EPEs based on the 90th or 95th percentiles has been widely used in Antarctic literature, and I have also contributed to studies that adopted this terminology. However, I consider that this was an imprecise use of language, and it would have been more rigorous to acknowledge this limitation at the time. Referring to these events as "extreme" in previous publications was (1) reflecting a lack of rigor and (2) have significant implications for how results are interpreted and cited.

1) *Regarding the lack of rigor, my concern relates to the underlying probability distribution of the data and the associated mathematical assumptions and conclusions. Referring to precipitation exceeding the 95th percentile as "extreme" implicitly undermines the relevance of a GEV based framework. Would one consider a Gumbel distribution to be equivalent to a Gamma or a normal distribution? The answer is no; consequently, this issue should at least be acknowledged as an imprecise use of terminology.*

2) *If precipitation exceeding the 95th percentile is classified as "extreme," then subsequent studies may rely on this definition and claim that we already have a solid understanding of extremes. However, I would argue that the March 2022 event in Antarctica clearly showed that truly extreme events behave as outliers, with causes and impacts that are fundamentally different from those of events above the 95th*

percentile. Consequently, our current understanding of extremes in Antarctica remains very limited, which makes it all the more important to use precise terminology and to clearly distinguish between intense events and genuinely extreme ones.

Regarding the use of the term "extreme" in your study, I do not think that the reported trends reflect changes in extreme precipitation in Antarctica. To support my point, I downloaded ERA5 precipitation data for Antarctica and computed trends in annual maxima (This corresponds to a 2-year return level, i.e., the 99.86th percentile, meaning one event every 2 years, which is well beyond the "extremes" considered by the authors) and in the 10-year return level of daily precipitation using a non-stationary GEV framework (Figure R1). I applied a 5% significance level, which differs from the 10% significance level used in your Figure 2. Note that the scale is expressed in mm decade^{-1}, as I analyze the trend of individual events rather than the total precipitation from events exceeding the 95th percentile (i.e., approximately 18 events per year). Hereafter, I will refer to precipitation above the 95th percentile as EPEs, to clearly indicate that I am referring to the definition used in your paper.

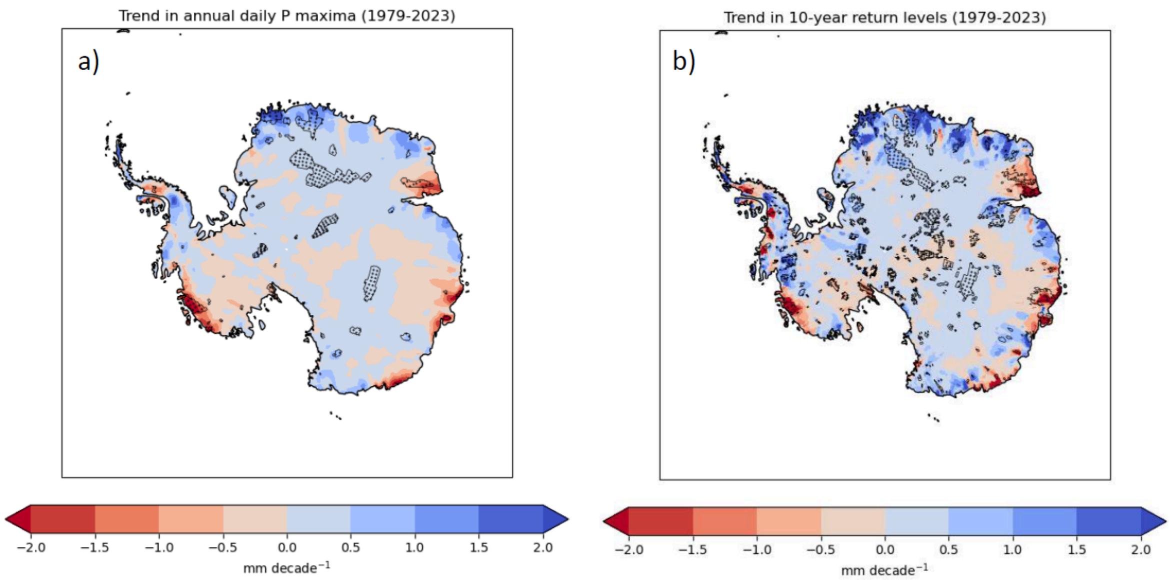

Figure R1: a) trends in ERA5 annual precipitation maxima over 1979-2023, b) same as a) but for 10- year return level precipitation. Stippling indicates where trends are statistically significant at the 95% confidence level.

In spite of an overall similar pattern, there are key differences. In your Figure 2a, regions showing strongly negative trends in mean precipitation, such as Wilkes Land, were progressively shifting toward positive trends when considering EPEs (Figure 2f). When examining annual maxima, and even more so the 10-year return levels, this shift becomes clearer. Conversely, regions such as Enderby Land or the Antarctic Peninsula shift from increasing trends to decreasing trends.

This is particularly concerning because these changes occur in the I–Ipp, J–Jpp, and especially Ap–B catchments, suggesting that trends derived from EPEs in key basins of your study are unlikely to be representative of true extremes. These shifts are actually indicating that the entire probability distribution is evolving, with notably different behaviors in the upper tail. Moreover, the F–G basin (along the coast of Marie Byrd Land) shows widespread regions with significantly negative trends, suggesting that the basin-scale trend may be statistically significant.

In my first review, I was only expecting a clarification in the text and an adjustment in wording, not a complete revision of the study. Again, I am not requesting a major change to the study. I am not suggesting a completely new analysis based on annual maxima or return-period events (e.g., 10-year events). I am simply asking that the *manuscript clearly states that precipitation above the 95th percentile should be described as intense rather than extreme.*

A sentence such as the following would be sufficient:

"In this paper, we consider intense events as those exceeding the 95th percentile, which have generally been referred to as extreme precipitation events in the previous literature. For simplicity, we will refer to them as EPEs, although, for the sake of scientific rigor, we note that these events should be considered intense rather than extreme."

Then, the definition of EPEs needs to be clarified in the very first sentence of the introduction. In the current manuscript, it is only explicitly defined in the Methods section, which makes several statements in the introduction unclear or misleading. For instance, the definition given in the first sentence of the introduction as: "Extreme precipitation events (EPEs, i.e., where the amount of precipitation at a location is substantially greater than normal)" is ambiguous. What exactly is meant by "substantially"?

Similarly, the statement "EPEs account for over 40% of the continent's total precipitation and almost 70% of the variance in annual precipitation" relies on the definition from Turner et al. (2019), but this is not clearly stated upfront.

I also suggested revising sentences such as: "EPEs over recent decades in Antarctica is hampered by such events being rare and difficult to separate from the substantial interannual precipitation variability." Here again, the term "rare" is not appropriate for events defined as exceeding the 95th percentile.

If EPEs were clearly defined in the first sentence of the introduction, it would immediately establish that you are using something similar to Turner's definition, and the subsequent statements would become coherent and meaningful, for example:

"Positive trends in the number of EPEs occurring over recent decades have been identified over parts of East Antarctica" or "Antarctic precipitation and EPEs are the major component of surface mass balance." Without an explicit definition at the beginning, these statements lack precision and may even be misleading or incorrect.

Reply: We thank the reviewer for this comment, and for their very helpful suggested changes. We have done our very best to revise the opening sentences of the Introduction as they suggested. In particular, in the opening sentence of the Introduction we now refer to these events as '*intense precipitation events*', and then later say that in the Antarctic literature they are commonly called extreme precipitation events (EPEs), which we shall also use.

However, we found it difficult to exactly follow their suggestion of '*the definition of EPEs needs to be clarified in the very first sentence of the introduction'* as those exceeding the 95th percentile. This was primarily because although the studies we refer to used the term EPE, they used different definitions of this, including exceedance of the 95th (Gorodetskaya et al., 2014) and 90th (Turner et al., 2019) percentile thresholds, as well as increases to the surface height of the Antarctic ice sheet (Adusumilli et al., 2021), and Rx1day and Rx5day (Sun et al., 2021). Therefore as it was not possible to have a fixed upfront definition of what an EPE was, we decided for clarity to keep the EPE definition used in this study to the Methods section and remove any statements in the Introduction that are subsequently unclear or ambiguous from not stating this upfront. For example, we removed the text *"Extreme precipitation events (EPEs, i.e., where the amount of precipitation at a location is substantially greater than normal)"* and *"EPEs account for over 40% of the continent's total precipitation and almost 70% of the variance in annual precipitation"* from the opening paragraph. Additionally, we removed the word 'rare', following the reviewer's advice that this was not appropriate for events defined as exceeding the 95th percentile.

Therefore, to address this comment we have revised the opening paragraph of the Introduction to the following (revised text highlighted in bold): '***Intense precipitation events have increased in intensity and frequency globally in recent decades (Donat et al., 2013; Sun et al., 2021; Dunn et al., 2020; Li et al., 2024), which has***

been attributed to anthropogenically-induced climate change (Min et al., 2011; Zhang et al., 2013; Paik et al., 2020; Kirchmeier-Young et al., 2020; Dong et al., 2020, 2021; Estrada et al., 2023; IPCC 2023). In Antarctica, these events have generally been referred to as extreme precipitation events (EPEs; Gorodetskaya et al., 2014; Turner et al., 2019; Adusumilli et al., 2021; Wille et al., 2021, 2024, 2025; Gehring et al., 2022; Maclennan et al., 2022, 2023; Simon et al., 2024). We adopt the same nomenclature for consistency with existing literature, while recognising that the events analysed here represent high-intensity precipitation rather than extreme. In Antarctica, EPEs are also often associated with intense atmosphere rivers (ARs; Gorodetskaya et al., 2014; Adusumilli et al., 2021; Wille et al., 2021, 2024, 2025; Gehring et al., 2022; Maclennan et al., 2022, 2023; Simon et al., 2024). However, identifying statistically significant trends in EPEs over recent decades in Antarctica is hampered by such events being difficult to separate from the substantial interannual precipitation variability that characterises its climate, as well as by limited observations (Connolley, 1997; Hosking et al., 2013; Yu et al., 2018, 2025). *For example, Intergovernmental Panel on Climate Change (IPCC) assessments of observed extreme precipitation changes largely exclude Antarctica (Seneviratne et al., 2021). Nevertheless, positive trends in the number of EPEs occurring over recent decades have been identified over parts of East Antarctica (Yu et al., 2018; Simon et al., 2024).*'

Comment #2

Overall, the manuscript is primarily a D&A study and already contains substantial and valuable material for discussion, including uncertainties, and discrepancies between CESM1 and CESM2, as well as comparisons with previous studies. In my view, the abstract would be better concluded by referring to the differences between CESM1 and CESM2, which are more robustly supported by the analysis presented. I believe the discussion should also be slightly extended and focus more on these aspects, in particular on the robustness of the results.

Indeed, current final sentence of the abstract (referring to ARs) is based on conclusions that are not sufficiently robust and should only be treated as a discussion point. ARs currently represent only a secondary and still debatable element of discussion and key questions remain unresolved on AR impact: do AR precipitation intensities increase more than non-AR precipitation? Or could the apparent change simply result from the thresholds used in Wille's ARDT methodology? Concluding the abstract on this point would require a more solid basis.

Indeed, in my first review I suggested computing trends in AR intensity during AR days and non-AR days, and then comparing both, because the current figure does not allow us to draw robust conclusions about the actual role of atmospheric rivers in EPE trends. Here, you assume that $\sum (EPE) = \sum (EPE_{ar}) + \sum (EPE_{nonar})$*. Then if the sum of EPE increased by a rate of α in mm* y^{-1}*, and if* $d(\sum (EPE_{ar}))/dt = \alpha$*, then* $d(\sum (EPE_{nonar}))/dt = 0$ *implying that 100% of the change is attributed to ARs. While mathematically correct under this decomposition, this does not allow one to conclude on the actual contribution of ARs, because the number of* EPE_{ar} *and* EPE□$_{o}$□$_{-ar}$ *events is itself evolving over time.*

Indeed, let us consider that the intensity of both EPE_{ar} *and* EPE□$_{o}$□$_{-ar}$ *changes at the same rate. In that case, all events would be intensifying, not only those associated with atmospheric rivers. In your calculation, both* $\sum (EPE_{ar})$ *and* $\sum (EPE_{nonar})$ *would increase. However, if you also suppose that the number of AR events increased over time. In this situation, the changes in* ∑EPE□$_{o}$□$_{-ar}$ *could decrease or remain constant, while* $\sum EPE_{ar}$ *would increase even more strongly, even though the intensity of both types of events evolves similarly. One could then conclude that the observed change in EPEs is driven by an increase in the number of AR events rather than by changes in their intensity. However, this interpretation would still be misleading, because non-AR events would be experiencing the same underlying changes in intensity. The actual driver of the signal would therefore not be atmospheric rivers per se, but rather a general increase in atmospheric moisture transport affecting all events. In the present example, since both AR and non-AR event intensities would increase at the same rate, the difference in* $\sum EPE_{ar}$ *and* $\sum EPE_{non-ar}$ *trends would primarily reflect how events are classified by the ARDT method rather than a true change in the physical properties of the two categories. Actually, this is possible because Wille's ARDT algorithm mechanically classifies a greater number of moist atmospheric events as "AR"* *under conditions of overall increasing atmospheric moisture. This can generate an apparent AR-related signal even if the underlying change is not specifically driven by atmospheric rivers themselves.*

In my first review, I already noted that the authors did not assess whether the observed changes in trends result from changes in frequency, intensity, or a combination of both. The authors' write "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.". My comment was unclear, because additional calculations were not what I was expecting and do not resolve this issue.

At this stage, I suggest analyzing whether trends in precipitation intensity differ between the EPE_{ar} *and* $EPE_{\square o \square -ar}$ *subsamples. For this task, the authors have to compute the trend considering the mean intensity of EPE event and not the sum of EPE events. Atmospheric rivers could be considered a driver of the observed change only if non-AR events present (significantly) weaker trends. It would thus be necessary to test the significance of EPE intensity trends during AR days and non-AR days using a bootstrap approach.*

Reply: We thank the reviewer for this detailed and thoughtful comment. The reviewer suggests that '*the abstract would be better concluded by referring to the differences between CESM1 and CESM2, which are more robustly supported by the analysis presented.* We have implemented this change by and now conclude the abstract with mention of the differences between CESM1 and CESM2, as well as limitations of the D&A approach. The revised now concludes with the following: '***Applying the same analysis to CESM2 large ensembles confirmed that the ERA5 precipitation signal was detected in the all-forcing simulations but unlike CESM1 did not provide robust attribution of total or extreme precipitation to any individual forcing. These findings provide evidence that external drivers have already impacted East Antarctic total and extreme precipitation, while demonstrating that uncertainties in attributing individual forcings remain a key limitation in understanding future changes to the Antarctic surface mass balance.***'

The reviewer also suggested that '*the discussion should also be slightly extended and focus more on these aspects, in particular on the robustness of the results.'* We have implemented this suggestion, which was included as part of a major rewrite of the Discussion in response to the substantial change in the results from modifying how we calculated basin-averages. In particular it was valuable to extend the Discussion to focus more on differences between the initial attribution analysis based on trends and the regression-based analysis, as well as on differences between the CESM1 and CESM2 results. For example, the Discussion now includes the following paragraphs three paragraphs focusing on these aspects:

"Our initial attribution analysis based on comparison of basin-averaged trends using the CESM1 ensembles showed that increases in greenhouse gases, anthropogenic aerosols (excluding anthropogenic influences on biomass burning), and stratospheric ozone depletion are all important drivers of the positive trends for total precipitation for our (five) basins of interest (Figs. 2 and S1, and Table 2). However, for extreme precipitation, the role of greenhouse gases and stratospheric ozone depletion are less important than anthropogenic aerosols for driving the positive trends for our (three) basins of interest (Figs. 2 and S1, and Table 2). For example, for extreme precipitation, only basin K-A (Dronning Maud Land) showed that greenhouse gases, anthropogenic aerosols, and stratospheric ozone depletion are all important drivers of the trends (Fig. 2 and Table 2).

By contrast, the two-signal regression-based D&A analysis using the CESM1 ensembles for ALL and GHG, ALL and AER, and ALL and O3 (resulting in scaling factors and confidence intervals for GHG, AER, and O3) found robust attribution for the increases in total precipitation to greenhouse gases in one basin (Ap-B situated within Enderby Land) and stratospheric ozone depletion in two basins (Ap-B situated within Enderby Land, and K-A situated within Dronning Maud Land), and the increases in extreme precipitation to greenhouse gases and stratospheric ozone depletion in one basin (Ap-B situated within Enderby Land) (Fig. 6), i.e., for one basin (within Enderby Land) the increases in total and extreme precipitation are robustly

attributed to both greenhouse gases and stratospheric ozone depletion. The other results for total and extreme precipitation using the two-signal regression-based approach have confidence intervals that include zero, indicating that any increases in total or extreme precipitation cannot formally be associated with these external forcings, including all results related to anthropogenic aerosols. These other results (with confidence intervals that include zero) are therefore inconsistent with the initial assessment of the contributions from different external forcings to the trends for ALL (Table 2), suggesting that although the basin-averaged trends are the same for both approaches, the statistical significance are different.

Results based on the CESM2 ensembles were also included to enable us to identify the responses that are consistent for both CESM1 and CESM2, and which are sensitive to the choice of model (Simpson et al., 2023). The results based on the CESM2 ALL ensemble identifies the same basins as the CESM1 ALL ensembles that are associated with positive trends in both total and extreme precipitation, which are in agreement with ERA5 (A-Ap, Ap-B, J-Jpp, Jpp-K, and K-A for total precipitation, and A-Ap, Ap-B, and K-A for extreme precipitation) (Fig. S3), i.e., confirming that a combination of both anthropogenic and natural forcings are responsible for the positive trends. However, repeating the three-signal regression-based D&A analysis using the CESM2 ensembles ALL, GHG and AER ensembles (resulting in scaling factors for GHG, AER, and OTHERS) was unable to provide robust attribution for total or extreme precipitation to any specific single-forcing, including greenhouse gases (Fig. S7). The reason why the CESM2 increases in total or extreme precipitation cannot formally be associated with external forcings, unlike CESM1, may be because the three-signal analysis used for CESM2 has more degrees of freedom than the two-signal analysis for CESM1, making it more uncertain. Nevertheless, repeating the initial attribution analysis based on comparison of basin-averaged trends using the CESM2 ensembles also agreed with the equivalent CESM1 analysis that greenhouse gases are an important driver of the precipitation trends over these basins (c.f. Figs. 2 and S2). However, these initial results also suggest that increases in anthropogenic aerosol emissions in CESM2 can oppose / offset the increases in precipitation from increased greenhouse gases over our selected basins, which contradicts our findings based on CESM1 (c.f. Figs. 2 and S2)."

Finally, the reviewer suggested that the '*current final sentence of the abstract (referring to ARs) is based on conclusions that are not sufficiently robust and should only be treated as a discussion point'*. Our intention was not to establish atmospheric rivers as the physical driver of the ERA5 precipitation trends, but rather to examine how much of the ERA5 precipitation trends occurs during AR conditions. We absolutely recognise that our analysis of ARs (Fig. 7) could be extended. However, as our manuscript is primarily a D&A study we feel a more comprehensive investigation of AR characteristics and their evolution is beyond the scope of this study. Accordingly, we have removed the reference to ARs from the concluding sentence of the Abstract and revised the Discussion to ensure that the AR analysis is presented as supporting evidence rather than a definitive conclusion. Additionally, we have revised the final paragraph of the Discussion to explain that a more comprehensive investigation of ARs would be valuable future work, especially focusing on applying the D&A framework. The final paragraph of the Discussion is now this: ***'Although detecting the fingerprint of anthropogenic forcing on recent Antarctic precipitation changes remains extremely challenging (Previdi and Polvani, 2016; Dalaiden et al., 2022), our results demonstrate that the effects of greenhouse gases and stratospheric ozone are particularly important for driving the present-day increases in total and extreme precipitation over drainage basins within the Filchner-Ronne sector, Dronning Maud Land, and Enderby Land, while the role of anthropogenic aerosols remains uncertain. Additionally, given the importance of ARs for Antarctica's surface climate, applying the D&A framework to changes in their properties (e.g., intensity, duration, and spatial extent) in future work would represent an important step toward identifying the contribution of external forcing versus internal variability in the AR changes, as well as their associated impacts.'***

Comment #3

Figures 2 and S1 show substantial differences between CESM2-AAER and CESM1-AER, and this point deserves much more discussion in the manuscript. In particular, while I understand that the sum of trends from GHG, AER, and O_3 forcings is not expected to reproduce the trends from the ALL simulation, the spatial patterns in

CESM1 appear remarkably similar across all forcings. What could explain this? This is especially puzzling given that trends associated with AER in coastal regions of Antarctica are clearly distinct from those retrieved over the Southern Ocean in Figure 4 of Deser et al. (2020), which describes the CESM1 Large Ensemble. Exploring, and ideally explaining, the origin of these differences and those with CESM2 would be highly valuable for the community, as it could provide important insights into the limitations of detection and attribution analyses in Antarctica.

Reference : 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

Reply: We really appreciate this point being made. Consequently we have made several changes to the manuscript.

Firstly, we have added an additional paragraph in the Discussion that gives some possible reasons for the substantial differences between the response of anthropogenic aerosols in CESM1 and CESM2, which we think is likely due to differences in the model and experimental setups, as well as differences in non-linearity between the models. We have mentioned Fig. 4 of Deser et al. (2020), which was actually produced using simulations that combined anthropogenic and biomass burning aerosols, while our results excluded biomass aerosols - we suggest that this is the reason for the discrepancy. The additional paragraph in the Discussion is: ***'The differences in the CESM1 and CESM2 results on the effects of anthropogenic aerosols could be due to the differences in either model physics or experimental design. For example, Simpson et al. (2023) showed that the simulated climate response in response to external forcings, particularly for anthropogenic aerosols, was particularly sensitive to the different approaches that are used to produce the single-forcing large ensembles. For example, in CESM1 an "all-but-one" design is used (i.e., everything is time evolving except the external forcing of interest), while in CESM2 an "only" design is used (i.e., only the external forcing of interest is time evolving). Furthermore, Simpson et al. (2023) also highlighted that the climate response for CESM2 was substantially more nonlinear compared to CESM1, meaning that the response to external forcing in CESM2 is more non-additive. The combined and potential non-linear effects of different forcings is likely important for representing precipitation, and especially for extreme precipitation where linear assumptions are least valid (Meehl et al., 2003; Pope et al., 2020) Note also that Deser et al. (2020) showed that increases in anthropogenic aerosol emissions in CESM1 resulted in negative precipitation trends over coastal regions of Antarctica and the Southern Ocean (their Fig. 4), while our CESM1 results showed that this caused positive trends over coastal regions of Antarctica (Fig. 2). However, this disparity is perhaps due to the CESM1 anthropogenic aerosol simulations used by Deser et al. (2020) also containing biomass burning aerosols, which were excluded from our simulations.'***

Secondly, we have modified the paragraph of section 3.4 in the Results section to make clearer the differences between CESM2 and CESM1: '*For the attribution analysis, the results show that the ensemble mean trends in total and extreme precipitation from the CESM2 GHG ensemble are in good agreement with those from the CESM2 ALL ensemble over much of Antarctica (Fig. S2), i.e., indicating that greenhouse gas forcing is the primary driver of the positive trends in the CESM2 ALL ensemble, consistent with CESM1. For example, the CESM2 GHG ensemble captures the positive trends from Marie Byrd Land to the western section of Wilkes Land that are apparent in ALL.* ***By contrast, the trends of the ensemble mean of the CESM2 AER ensemble are mostly negative for total and extreme precipitation over Antarctica, although areas that show significant trends are rather patchy, i.e., indicating that the effects of increased anthropogenic aerosols tend to offset the positive trends from increased greenhouse gases, which is inconsistent with CESM1 results.*** *The importance of greenhouse gas forcing for producing the positive trends in total and extreme precipitation over the selected basins is also evident from probability distribution functions of the trends from the CESM2 ensembles for ALL, GHG, and AER, which shows considerable similarities between the distributions for the GHG and ALL trends (Fig. S6).* ***The distribution of the AER trends in CESM2 also confirms the offsetting effect of anthropogenic***

aerosols for total precipitation (Jpp-K and K-A) and extreme precipitation (Ap-B and K-A), with the AER distributions skewed towards negative values. For the remaining basins (both total and extreme precipitation) the AER distributions include some positive trends for both total and extreme precipitation, albeit the negative trends still tend to dominate (with the exception of J-Jpp for total precipitation).'

Thirdly, the end of the abstract has been revised to also mention the difficulties attributing the role of anthropogenic aerosols: **'*In contrast, none of the precipitation trends could be attributed to anthropogenic aerosols despite all-forcing and single-forcing simulations of anthropogenic aerosols exhibiting similar trend patterns.*** *Applying the same analysis to CESM2 large ensembles confirmed that the ERA5 precipitation signal was detected in the all-forcing simulations but unlike CESM1 did not provide robust attribution of total or extreme precipitation to any individual forcing.* ***These findings provide evidence that external drivers have already impacted Antarctic total and extreme precipitation, while demonstrating that uncertainties in attributing individual forcings remain a key limitation in understanding future changes to the Antarctic surface mass balance****.'*

Fourthly, we added an additional line to the end of the Introduction to justify our use of both CESM1 and CESM2, and not simply using one model, i.e., to check for robustness of results, etc. The paragraph is now as follows: "*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. Our approach uses precipitation from: i) ERA5 atmospheric reanalysis to identify trends, and ii) large ensembles performed using the Community Earth System Model Versions 1 and 2 (CESM1 and CESM2), which are fully-coupled global climate models, to undertake D&A of the ERA5-based trends and determine the effects of* ***greenhouses gases, anthropogenic aerosols, and stratospheric ozone versus internal climate variability. Results from both CESM1 and CESM2 ensembles are analysed because differences have been found in their responses to external forcing, due to differences in experimental design and model physics (Simpson et al., 2023).****"*

Comment #4

After reading the initial version of the manuscript, I did not understand that the EPEs were computed based on "precipitation days," defined as days with precipitation exceeding 0.02 mm day^{-1}. I thank the reviewers for their response, as they clarified a point that I had not noticed during my first reading, even though it was already stated in the paper. I apologize for having missed this detail.

This results in the use of a "location-dependent" percentile used to define extremes since the 95th percentile is computed over different number of days depending on the location. I find this approach debatable, as this threshold is excluding more days inland than at the coast. As a result, basin-scale precipitation also becomes "disproportionately" influenced by coastal events, rather than reflecting

the inland signal. It also implies that the number of days considered at each grid point, varies with the forcing and across simulations, which complicates the interpretation of trends.

A second issue concerns the methodology used to define 95th percentile precipitation at the basin scale. You identify precipitation days exceeding the 95th percentile at each grid point, and then aggregate these values annually to compute a basin-averaged "extreme" precipitation. This is not consistent with standard hydrological practice, where extreme precipitation is typically defined at the basin scale to allow meaningful comparisons with extreme discharge. From a hydrological perspective, each precipitation event should be treated independently rather than merged with others. I would suggest to first compute daily basin-averaged precipitation, then identify days when this basin averaged precipitation exceeds its 95th percentile, and finally sum these values over the year. Even if this should not fundamentally change the conclusions, this would provide *a more physically consistent probability distribution of precipitation at the basin scale.*

Reply: We are very grateful for this comment, and agree that our current method of calculating basin-averages is more sensitive to locally distributed extreme events. Thank you also for your suggestion on how to calculate basin-averaged trends, which we have adopted. To explain this revised approach in Section 2.2 in the Methods, we have revised the first paragraph to state: '*Hourly ERA5 precipitation outputs are firstly re-gridded onto the CESM1 grid (resolution of 1°) using an area-weighted averaging method, and then summed over 24-hour intervals to produce daily values. We then compute trends in total and extreme precipitation* ***(at each grid point)*** *for ERA5 and the CESM1 ensembles ALL, GHG, and AER for the period 1979 to 2023 and O3 for the period 1979 to 2005. Here, precipitation days are defined as days with precipitation exceeding a threshold of 0.02 mm day*$^{-1}$*, and extreme precipitation as days with precipitation exceeding the 95*th *percentile of all precipitation days over the period 1979 to 2023, following Swetha Chittella et al. (2022). These daily data are then annually aggregated to obtain an annual time-series of both total and extreme precipitation for ERA5, ALL, GHG, and AER from 1979 to 2023 and O3 from 1979 to 2005.* ***These time-series are subsequently used to calculate over Antarctica the spatial distributions of annual trends of total and extreme precipitation for ERA5, ALL, GHG, AER, and O3. Additionally, we also calculate analogous basin-averaged annual trends of total and extreme precipitation for Antarctica's 18 drainage basins (mapped using the Ice Sheet Mass Balance Inter-comparison (IMBIE) dataset shown in Fig. 1; Rignot et al., 2019). Here, we first compute the daily basin-averaged precipitation, with basin-averaged precipitation days defined as days with this exceeding a threshold of 0.02 mm day***$^{-1}$***, and basin-averaged extreme precipitation as days with this exceeding the 95***th ***percentile. These daily data are then annually aggregated to obtain a basin-averaged annual time-series of both total and extreme precipitation for ERA5, ALL, GHG, and AER from 1979 to 2023 and O3 from 1979 to 2005, which we use to calculate basin-averaged annual trends.'***

Moreover, this revised approach revealed different results, which required a substantial re-write of both the Results and Discussion sections. The new results show that for five of the six basins exhibiting positive trends in total precipitation (within the Filchner–Ronne sector, Dronning Maud Land, and Enderby Land) and three of the four basins exhibiting positive trends in extreme precipitation (within Dronning Maud Land and Enderby Land), the ERA5 signal was formally detected in the CESM1 all-forcing simulations, indicating that these trends are driven by a combination of anthropogenic and natural forcings. Our analysis further show that for one basin (within Enderby Land) the increases in total and extreme precipitation are robustly attributed to greenhouse gases and stratospheric ozone, while for another basin (within Dronning Maud Land) the increases in total precipitation are attributed to stratospheric ozone only. In contrast, none of the precipitation trends could be attributed to anthropogenic aerosols despite all-forcing and single-forcing simulations of anthropogenic aerosols exhibiting similar trend patterns. Applying the same analysis to CESM2 large ensembles confirmed that the ERA5 precipitation signal was detected in the all-forcing simulations but did not provide robust attribution of total or extreme precipitation to any individual forcing.

The re-write was very carefully done to maintain the high quality of writing that both referees appreciated, and especially to prevent the writing from becoming laborious. Please refer to the revised manuscript for the full changes. Additionally, we have also slightly revised the title from '***Recent intensification of extreme precipitation over Antarctica driven by increases in greenhouse gases and stratospheric ozone***' to '***Recent intensification of extreme precipitation East Antarctica driven by increases in greenhouse gases and stratospheric ozone***'. This is because four of the five basins that we focus on are located in East Antarctica, and specifically the Atlantic sector.

Furthermore, we have also added further text to the Discussion section that highlights that most of the basins where precipitation changes are successfully detected are located in the Atlantic sector. The revised paragraph is now: "*We also showed that the trends in total and extreme precipitation over the selected basins can be linked to increasing ARs (Fig. 7). This provides physical support for the analysis, highlighting the consistency between the identification of the trends and the underlying dynamical drivers. This result is also consistent with previous results that suggested that ARs contribute up to 20% of total precipitation in East Antarctica (Wille et al., 2021,* *2025; Maclennan et al., 2022), and as much as 50% to 70% of extreme precipitation (Wille et al., 2021, 2025; Simon et al., 2024).* ***Furthermore, most of the basins where precipitation changes are successfully detected are located in the Atlantic sector, where surface ocean warming and a sea-ice loss in the Weddell Sea are reported (Goosse et al., 2024). These oceanic changes likely enhance atmospheric moisture availability and, together with a strengthening of the low-pressure system located off the East Antarctic coast (Goosse et al., 2024), create favorable conditions for increased precipitation over the continent.*** *Moreover, additional anomalous transport from the midlatitudes through Rossby-wave teleconnections could also be a factor (Wille et al., 2025). In addition, future increases in greenhouse gas concentrations are expected to strengthen Antarctic total and extreme precipitation by causing warmer atmospheric conditions (leading to more moisture, via the Clausius-Clapeyron relation; Dalaiden et al., 2020) and a reduction in sea-ice cover (Frieler et al., 2015; Lenaerts et al., 2016; Previdi and Polvani, 2016; Kittel et al., 2021; Vignon et al., 2021; Zhu et al., 2023; Nicola et al., 2023).”*

Minor comments

Line 116 vs. line 395: in the document and in the Supplementary material the term AAER is still used for CESM2.

Reply: Thank you for pointing this out. This has been corrected in both the main manuscript and the supplementary material.

Line 221 : “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.”=> if I am not wrong, extreme precipitation is increasing in the I-Ipp region.

Reply: We thank the reviewer for pointing this out. We agree that extreme precipitation increases significantly in basin I–Ipp. We have revised the sentence to clarify that the significant trends over the northern Antarctic Peninsula are positive. The revised sentence reads: *‘Over the Antarctic Peninsula, the ERA5 trends in total and extreme precipitation differ in direction, with total precipitation increasing and extreme precipitation decreasing,* ***except the northern Antarctic Peninsula, where the extreme precipitation trends are significantly positive.****’*

Line 387 : “indicating that greenhouse gas forcing is the primary driver of the positive trends in the CESM2 ALL ensemble, consistent with CESM1.” => In lines 344–363, it is stated that the scaling factors are generally associated with relatively small confidence intervals for all forcings, including GHG. Is this in contradiction with the present sentence?

Reply: In the revised results, the confidence intervals for the CESM2 ensemble GHG simulations included zero, indicating weak signal detection and that the increases in ERA5-based total precipitation cannot be formally associated with this external forcings. One aspect we have tried to highlight in the revised Discussion is that the initial attribution based on examination of basin-averaged trends can contradict the regression-based results. For example, in the revised Abstract we highlight that ‘***However, the increases in precipitation were not attributed to anthropogenic aerosols in any of the basins using the regression-based approach, despite the trends for the all-forcing and single-forcing simulations of anthropogenic aerosols showing good agreement.***’

Additionally, the Results and Discussion also now mention that the likely reason for this contradiction is that although the basin-averaged trends are the same for both approaches, the statistical significance is different.