Answer to Lan Wang-Erlandsson (editor) in the Interactive comment on "Simple physics-based adjustments reconcile the results of Eulerian and Lagrangian techniques for moisture tracking in atmospheric rivers" by Alfredo Crespo-Otero, Damián Insua-Costa, Emilio Hernández-García, Cristóbal López and Gonzalo Míguez-Macho

Dear authors,

Thank you for constructively and comprehensively addressing the concerns of the two reviewers. Your revised manuscript has been reviewed again by Harald Sodemann, but not the first reviewer, who unfortunately declined to review the manuscript again. In the referee report of Harald Sodemann, you will see that he is surprised by the lack of sensitivity to the specific humidity threshold. Could you please address this major concern of his, as well as the other minor comments and suggestions he provided? In addition, relevant data and code should be made accessible in accordance with ESD policy: "Data and model modifications used in the manuscript should be available in open-access repositories. See also the data policy web page on the ESD site for further information and options of repositories." The transparent sharing of code and data could potentially also help clarify questions regarding model code discrepancies, as raised by Prof. Sodemann.

In addition, as the first reviewer declined to review the revised manuscript, I took a detailed look at your revisions in his/her place. I think most of his/her comments are addressed really well. Further down, I would just like to follow up on a few of his/her concerns regarding clarity and provide some additional suggestions. Hope you find them useful.

Best wishes,
Lan Wang-Erlandsson

Thank you very much for continuing the work of the first reviewer and handling the interactive discussion. Following your suggestions and ESD policies, we will make our code accessible in an open-access repository. Please, find below the responses to your comments.

Introduction paragraph 1: Please consider explaining a bit more why understanding the moisture sources of atmospheric rivers (e.g., in the paragraph starting at L45) is important. This would help contextualise the paper and motivate its publication in an interdisciplinary Earth system science journal like ESD. The reason provided in L29-30 "how water is transported between different regions of our planet" is only a general and climatological rationale, and does not explain the focus on atmospheric rivers.

Thank you for the suggestion, we agree that the focus on atmospheric rivers (ARs) has not been completely justified so far. On the one hand, despite occupying only a small portion of the Earth's surface, ARs are responsible for ~90 % of the poleward water vapor flux across mid-latitudes (Gimeno et al., 2014; Zhu and Newell, 1998). In some regions of the world (particularly, extratropical and western coastal areas), they account for more than 30 % of the total precipitation (Ralph et al., 2020), and it has also been shown the close connection between ARs and extreme precipitation events (Ralph et al., 2006; Lavers and Villarini, 2013). On the other hand, although there are previous studies where they compute the precipitation sources for ARs, the problem is not completely closed. This is mainly because of the focus on specific regions and events, but also due to the use of different moisture tracking methodologies, which makes the comparison difficult. Thus, by focusing on ARs we are addressing an important and unresolved problem relevant to the climate of a substantial portion of the planet. To better explain this in the manuscript we will change the first part of the second paragraph:

"The aforementioned techniques have been particularly used to identify moisture sources in precipitation events associated with atmospheric rivers (ARs). ARs are structures of enhanced moisture and intense water vapor transport in the atmosphere, typically located in the pre-cold frontal region of an extratropical cyclone (Ralph et al., 2005), and are responsible for the majority of the poleward water vapor flux across mid-latitudes (Zhu and Newell, 1998). Their large-scale nature, together with their connection to extreme precipitation events (Ralph et al., 2006; Lavers and Villarini, 2013), has made them the focus of several studies aimed at determining the origin of the moisture they carry —specifically, whether it is primarily transported from remote regions or local sources. This issue has been addressed using Eulerian water vapor tracers (Sodemann and Stohl, 2013; Eiras-Barca et al., 2017; Hu and Dominguez, 2019), Lagrangian techniques (Liberato et al., 2013; Ramos et al., 2016) or both (Bonne et al., 2015)."

L55: The overarching objective statement formulation could be sharpened to better explain what the study is trying to achieve and what insights it is hoping to gain. 'Comparisons' and 'adjustments' are more methods than end-goals in themselves. At L88 there's a good example of a clearer explanation of 'why' comparisons are made and 'what' adjustments are meant to achieve, but currently not tied to the main objective statement.

We agree with the reviewer that the main objective of our study is not well stated here. We will reformulate "compare and adjust two Lagrangian methodologies for the computation of moisture sources for precipitation" to "assess the differences and reduce the discrepancies between two Lagrangian methodologies for the computation of moisture sources for precipitation".

L60: typo

We will delete the extra punctuation mark in the revised version of the manuscript.

L56-L94: These paragraphs currently both describe and motivate the aims and methodology. For clarity and flow, you could consider adding a paragraph or subsection immediately after the Section 2 heading to provide the details of the methodological flow. This would allow the final paragraph of the introduction to focus on explaining the study aims and the overall structure.

Thank you very much for the suggestion. We agree that the three paragraphs between L56 and L94 both describe the objectives and briefly introduce the methodology used in this study. However, these paragraphs are closely connected: the second paragraph (L75-L85) discusses previous studies that employed a similar approach (specifically, the use of water vapor tracers as reference to evaluate a Lagrangian methodology), and the third builds directly on this by detailing which Lagrangian tools we assess and what is our ground truth. Given that the main focus of this paper is to understand the origin of discrepancies between moisture tracking techniques and rectify them (rather than identifying moisture sources), we believe that this contextual and methodological framing should be in the introduction.

L97 'a series of analyses': consider elaborating

We will reformulate this sentence for clarity. Specifically, to "Section 3 includes the main results of our study, where we first focus on the comparison of the results produced by the Lagrangian methodologies with those provided by the WRF-WVTs model, and then assess how the Lagrangian and Eulerian approaches can be brought into closer agreement."

L312-324: Please consider explaining what the term 'non-physical humidity fluctuations' refers to, especially as the term is not used by Sodemann et al. 2008. Sodemann et al. (2008) discuss: "other physical or numerical processes caused the moisture increase in the traced air parcel, such as convection, evaporation of precipitating hydrometeors, subgrid-scale turbulent fluxes, numerical diffusion, numerical errors associated with the trajectory calculation, or physical inconsistencies between two ECMWF analysis time steps". If the authors refer to the same processes as Sodemann et al. (2008), which seems to be the case, the term 'non-physical' would be confusing.

We agree with the reviewer that the term "non-physical" may be misleading. As in Sodemann et al. (2008), we refer here to processes other than surface evaporation or precipitation that may change the humidity of the parcel computed by FLEXPART. We consider that such processes should be excluded from the calculation of moisture sources for precipitation. In the revised version of the manuscript, we will replace the term "non-physical humidity fluctuations" to "humidity fluctuations" and explicitly clarify that we are referring to changes in the parcel moisture not caused by surface evaporation or precipitation.

L51-54 "This is reflected in the definition of AR given in the Glossary of Meteorology, where it is indicated that the sources of moisture can be tropical and/or extratropical (Ralph et al., 2018)." Please consider reformulating and/or elaborating. It is unclear how this definition constitutes a reflection of tracking model uncertainties. Also, please check the citations (the reference Ralph et al. 2018 does not lead to the Glossary of Meteorology, and the Glossary of Meteorology does not cite Ralph et al., 2018).

In this paragraph we are trying to explain why the problem of the moisture sources in ARs remains unresolved, despite the existence of several studies addressing individual cases or computing climatologies for selected regions. In our opinion, the statement "the sources of moisture can be tropical and/or extratropical" reflects this lack of information, as it encompasses nearly all possible combinations of tropical and extratropical contributions. Furthermore, by putting together all the information extracted from these studies, it can also be deduced that the issue is not completely closed from a global and climatological perspective.

Regarding the citations, Ralph et al. (2018) does not lead to the Glossary of Meteorology, but to the article where they explain how the AR definition was created. We will include a citation to the Glossary of Meteorology also in the revised version of the manuscript.

L262: typo?

We agree that the last sentence is not correctly written right now. We will change it to "Finally, in Sect. 3.3 we test the proposed modifications when the trajectories are generated by FLEXPART-ERA5, with input data from the ERA5 reanalysis. In this case, the additional fields required by the diagnostic tools (e.g., evaporation and precipitable water in the case of DB99) are also taken from ERA5, rather than from WRF simulations.”

L463: Consider reformulating the heading and/or first sentence to make it clear at a glance that the section addresses the validation of the adjustments.

We thank the reviewer for this suggestion. We will change the heading to “Extension of the proposed modifications to ERA5 forced simulations” in the revised version of the manuscript.

L578 ”in global or climatological applications”: Please check the formulation. The analyses focused on specific regions and events, rather than global or climatological moisture tracking.

Although our analysis focused on specific regions and events, it was not limited to a single geographical area or time period. In addition, the results consistently pointed to the same conclusions, which suggests that our findings are broadly applicable to precipitation events (at least those associated with ARs) anywhere on the globe. In this scenario, the modifications we propose may be applied in global or climatological studies. We will incorporate this clarification into the manuscript for greater clarity.

References

Gimeno, L., Nieto, R., Vázquez, M., and Lavers, D. A.: Atmospheric rivers: a mini-review, Front. Earth Sci., 2, https://doi.org/10.3389/feart.2014.00002, 2014.

Lavers, D. A. and Villarini, G. (2013). The nexus between atmospheric rivers and extreme precipitation across Europe. Geophysical Research Letters, 40(12), 3259-3264.

Ralph, F. M., Neiman, P. J., Wick, G. A., Gutman, S. I., Dettinger, M. D., Cayan, D. R., and White, A. B.: Flooding on California’s Russian River: Role of atmospheric rivers, Geophys. Res. Lett., 33, https://doi.org/10.1029/2006GL026689, 2006.

Ralph, F. M., Dettinger, M. D., Schick, L. J., and Anderson, M. L. (2020). Introduction to atmospheric rivers (pp. 1-13). Springer International Publishing.

Zhu, Y. and Newell, R. E.: A Proposed Algorithm for Moisture Fluxes from Atmospheric Rivers, Mon. Weather Rev., 126, 725–735, https://doi.org/10.1175/1520-0493(1998)126<0725:APAFMF>2.0.CO;2, 1998.

Answer to Harald Sodemann (R2) in the Interactive comment on “Simple physics-based adjustments reconcile the results of Eulerian and Lagrangian techniques for moisture tracking in atmospheric rivers” by Alfredo Crespo-Otero, Damián Insua-Costa, Emilio Hernández-García, Cristóbal López and Gonzalo Míguez-Macho

This is my second review of this manuscript. The authors addressed most of my comments to the previous submission in a very constructive way and adjusted the manuscript accordingly. Still, I was very surprised that the authors state they find no sensitivity to the specific humidity threshold Δq. The authors claim throughout the text that they use the method "WaterSip", which in my understanding refers to a specific computer code implementation of the Sodemann et al. (2008) method. In an attempt to understand where these differences come from, I have made a comparison of two cases with the code that was used in previous studies for cases investigated here. It turns out that the results I get from running WaterSip V3.2, based on a global run with 5 million particles using ERA-Interim (Läderach and Sodemann, 2016) look entirely different for the Greenland case (Fig. 1), and only somewhat similar for the South Africa case (Fig. 2). The differences are so large that I do not think this could be due to the WRF simulation setup, and I rather suspect that the way the Sodemann et al. (2008) algorithm has been implemented differs in some way from the original. In fact, the results in Fig. 1b suggest that only a small contribution comes from the North Atlantic domain as defined by the authors, and a larger part from the North American continent. As the findings from the Greenland case are a cornerstone in the argumentation throughout the manuscript, this is a critical situation that needs to be resolved before publication.

Please note that WaterSip refers to a specific computer code that implements the Sodemann et al. (2008) method for FLEXPART and LAGRANTO with additional diagnostics. This software is now in public discussion (Sodemann 2025). In order to avoid confusing readers with regard to what software has been used, I would like the authors to not claim that they use WaterSip in their analysis, but rather their own implementation of an algorithm that is maybe inspired or resembles the Sodemann et al. (2008) method. The major differences that I see for the Greenland case imply that something else is done in the authors' code that leads to moisture uptakes identified close to Greenland in that case, and it would be misleading to assign these differences to either WaterSip or the Sodemann et al. (2008) method in itself. The lacking sensitivity to Δq is another hint at substantial differences in the author's code to what is done in Sodemann et al. (2008).

Thank you very much for continuing the review of our article. As you mention, the results presented were obtained with our own implementation of the Sodemann et al. (2008) method. For clarity and rigor, as you suggest, we will explicitly state this in the revised version of the manuscript, claiming that we cannot assure that the results are identical to those obtained with WaterSip. We will also change everywhere “WaterSip” to “SOD08”. In addition, as requested by the editor, we will make our code available in an open-access repository, hoping this helps to clarify some of the doubts you have regarding our results.

In an attempt to explain the differences you observed between your results and ours, we generated a similar figure to the one included in your review by changing the scale accordingly (Fig. 1 in this reply). As you mention, the discrepancies for the Greenland case in the No ABL configuration are noticeable, as we obtain a very important contribution from a large area just to the South of Greenland, and this may suggest that we made a mistake when implementing the Sodemann et al. (2008) method. However, the discrepancies can better be explained by the differences between the WRF simulation and the reanalysis, as the pattern obtained in the case of FLEXPART-ERA5 trajectories is much more similar to yours (see Fig. 2).

To better illustrate that the results shown here correspond to those in our manuscript, at the end of this document you can find Fig. 4, which is exactly Fig. S8 in the supplement, and Fig. 5 (same figure with the new scale). Figure 6 and Fig. 7 present the same results for FLEXPART-ERA5 trajectories. We remark that the precipitation sources in this last case were not included in the manuscript or the supplement so far. As you may observe, for the other cases analyzed the differences between the results obtained with FLEXPART-WRF and FLEXPART-ERA5 are much less important than in the Greenland case.

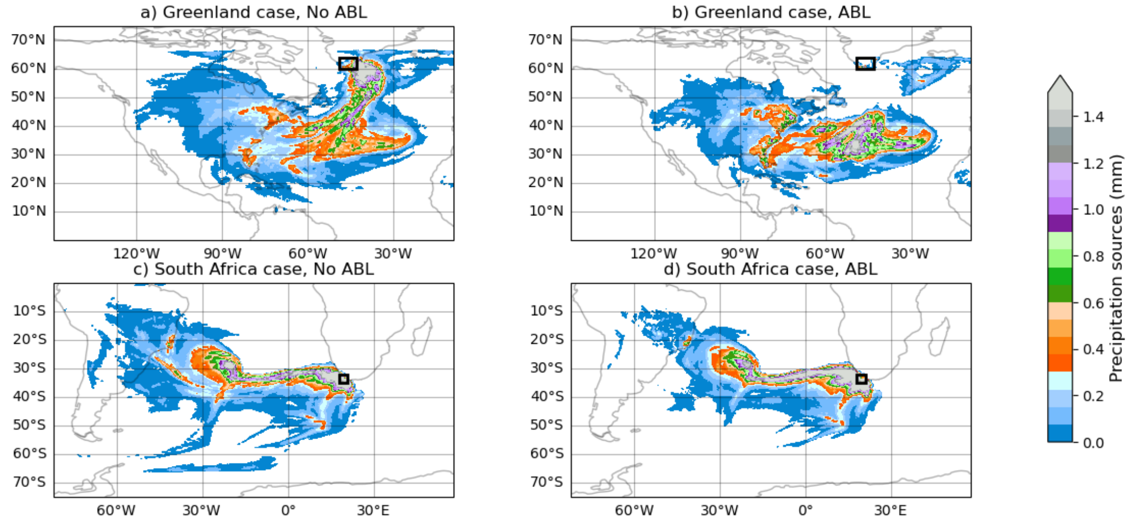


Figure 1: Precipitation sources for the Greenland and South Africa events, computed with our implementation of the Sodemann et al. (2008) method, for trajectories generated with FLEXPART-WRF. A threshold of 0.2 g kg-1 in a 6 h interval is used in all cases. In panels a) and c) all moisture increments larger than the previous threshold are considered, while in panels b) and d) increments above the ABL are also neglected.

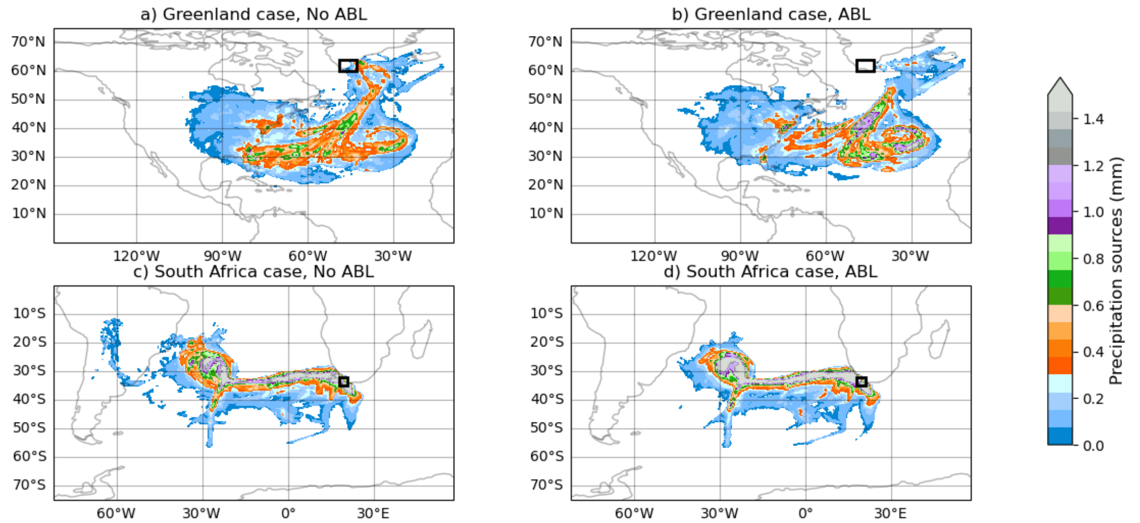


Figure 2: Precipitation sources for the Greenland and South Africa events, computed with our implementation of the Sodemann et al. (2008) method, for trajectories generated with FLEXPART-ERA5. A threshold of 0.2 g kg-1 in a 6 h interval is used in all cases. In panels a) and c) all moisture increments larger than the previous threshold are considered, while in panels b) and d) increments above the ABL are also neglected.

We believe that the similarities between the FLEXPART-ERA5 results and those in your review —especially in the No ABL setup— support the correctness of our implementation of the Sodemann et al. (2008 method. While some differences remain, there are several plausible explanations for them:

- First, and most importantly, the differences between the reanalysis driving the Lagrangian models. In our case we use ERA5, while the results you show correspond to simulations done with ERA-Interim. Apart from the improved resolution, ERA5 provides much more reliable precipitation fields. In Fig. 3 we compare the total precipitation in the Greenland event derived from ERA5 (left) and ERA-Interim (right), where we can check the differences between both patterns. While the Sodemann et al. (2008) method does not use directly the precipitation, it uses specific humidity, and there need to be also differences between ERA5 and ERA-Interim specific humidities to close the water balance. Thus, the different reanalysis used can affect the calculated moisture source field.
- Second, the configuration of the Lagrangian model. Apart from a different version of FLEXPART, as we use FLEXPART v10.4, Pisso et al. (2019), in Läderach and Sodemann (2016) it is stated that, on average, 70 air parcels reside at each column over a 1º x 1º grid cell and instant. Thus, an average of 6 x 4 x 70 = 1680 parcels are being used for the calculation of the moisture sources at each instant. In our case we are not using global simulations, but releasing 500000 parcels hourly over the black box shown in Fig. 3. This different configuration may also contribute to the discrepancies observed — particularly in the ABL setup, where the largest differences occur. In this case, aside from our use of a larger number of parcels, which could help identify more moisture uptakes within the ABL, it is also important to note that the boundary layer height computed by different versions of FLEXPART (each using a different reanalysis) may not align, potentially leading to inconsistencies.

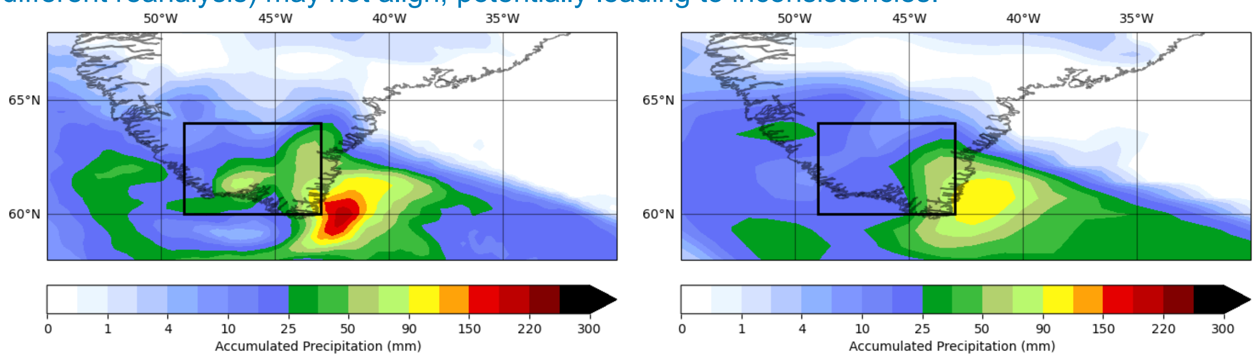


Figure 3: Precipitation during the Greenland event derived from the ERA5 (left panel) and ERA-Interim (right panel). The black box shows the region where precipitation is being tracked.

I still find the setup of the WRF moisture source tags overly coarse to make strong conclusions. In their reply, the authors state that they made a simulation with more finely resolved source boxes, but I do not see this discussed or shown in the manuscript, except for a figure in the supplementary material that is not mentioned anywhere (maybe I missed it). If the setup stays like this in the manuscript, it is in my opinion of critical importance to openly discuss the limitations of their approach in this study. For example, the boundary for the South Africa case at 30 deg N has the source footprint just south of that boundary (Fig. 2). This makes the verification statistics very sensitive so small meridional shifts in the source footprint, while really the AR case is one dominated by zonal advection. Thus, if the aim is to verify differences in the remoteness of sources, a setup with boxes over different longitudes would be needed to make the results more robust and help to support the conclusions, rather than a setup with different latitudes as is now shown in the supplementary material. All this may be still ok for showing the principle of how such a verification can be done, but such limitations need to be stated and discussed clearly to come to a well-justified overall assessment.

Regarding the finer configuration of sources that we used for a better comparison, the results are included in Fig. S12 of the supplement, as you point out. This is indeed commented in the last paragraph of Sect. 3.2.2 (L453 to L462), after the strongest differences between both approaches appear by comparing Fig. 6 and 8:

“Finally, an important difference can be observed by comparing the results for the Greenland case in Figures 6 and 8. In the case of WaterSip (even in the “RH” configuration) there is an important contribution from the northernmost part of the North Atlantic source (above 45º N), whereas this contribution is much less important in the case of the DB99 methodology. Our selection of source regions when comparing with WRF-WVTs overlooks this difference, and this could make our results not valid. However, by looking at the precipitation sources fields for all cases in Figures S8 and S9 in the Supplement we observe that only for the Greenland case there are important differences between the fields computed with the two different approaches. Moreover, we recomputed the RMSEs in Figures 5 and 7 with a finer (and more complex) selection of source regions, such that the ocean where the AR is located for each case is divided in four regions, instead of two. The results, shown in Figure S12 in the Supplement, demonstrate that the modifications we analyze and propose here provide also the best configuration with this new selection of source regions. “

In order to better reflect the limitations of our work we will move this paragraph to the conclusions. We will also mention that a moisture source setup with finer and different regions would be better to obtain more robust conclusions about the performance of the Lagangian methodologies. However, we understand that the changes we propose are validated by our experiments for the objective we posed: obtaining a Lagrangian methodology which can be an alternative to WRF-WVTs, where the tagged regions are typically large (oceanic basins or continents).

I also have a number of minor and technical comments below:

L. 14 and elsewhere: Instead of "WaterSip", which denotes a particular implementation of the Sodemann et al. (2008) method, refer to your own implementation of the Sodemann et al. (2008) algorithm here and elsewhere.

We will explicitly state in the introduction that we are assessing our implementation of both methodologies, by changing the first sentence of the paragraph starting at L86 to “In our case, the FLEXPART-WRF model is employed to generate back trajectories of air parcels contributing to precipitation in five AR events, and our own implementation of two widely used Lagrangian diagnostic tools for estimating moisture sources are assessed: the Sodemann et al. (2008), and the Dirmeyer and Brubaker, (1999) methodologies.”. Moreover, we will change WaterSip to Sodemann et al. (2008), abrreviated to SOD08, everywhere, and in Sect. 2.3, L192, we will explicitly claim that we cannot assure our implementation of this method is identical to WaterSip.

L. 37: This reference is missing from the reference section:

Sodemann, H., Wernli, H. and Schwierz, C., 2009: Sources of water vapour contributing to the Elbe flood in August 2002: A tagging study in a mesoscale model, Quart. J. Royal Meteorol. Soc., 135, 205-223, doi:10.1002/qj.374.

We will include this article in the reference section and correct the citation at L72.

L. 43 and elsewhere: There is occasionally an extra comma before the bracket with references.

Thank you for bringing this to our attention. We will review the entire manuscript and remove the extra commas before the brackets with references.

L. 60: remove extra '.'

We will remove the extra punctuation mark in the revised version of the manuscript.

L. 150: What are the units of QFX?

QFX is the surface moisture flux, so its units are kg m^{-2} s^{-1}.

L. 152: The term "assimilate" is used here in a non-standard way, and can be confused with data assimilation (which I don't have the impression is meant here). Please rephrase to e.g., "ingest directly from ERA5" or the like.

Thank you for this suggestion. We will change “assimilate” to “ingest” in the revised manuscript.

L. 177: This should be cited as Sodemann and Stohl (2009) as in the references.

The citation will be corrected in the revised version of the manuscript.

L. 183: Here it says hourly, in the sentence in L. 187 it says 3-hourly. Which one is correct?

Both are correct. In L183 we state that we are storing hourly trajectories for both FLEXPART-ERA5 and FLEXPART-WRF, while in L187 we refer to input data, which is 3-hourly in the case of FLEXPART-WRF and hourly in the case of FLEXPART-ERA5.

L. 188: Give a typical pressure height of the topmost retained level.

Thank you for this suggestion. In the revised version of the manuscript we will indicate that a typical pressure for the highest ERA5 level we use is 140 hPa.

L. 208: add "at arrival" after relative humidity.

We will add “at arrival” after relative humidity in the revised version of the manuscript.

L. 222: 0.05 g kg 6 h-1 is only ¼ of the typically used value for mid-latitudes of 0.2 g kg 6 h-1

This is a typo. The results included here correspond to the recommended configuration, 0.2 g kg^{-1} for a 6 h interval.

L. 262: "coming also": check sentence

We agree that the last sentence is not correctly written right now. We will change it to “Finally, in Sect. 3.3 we test the proposed modifications when the trajectories are generated by FLEXPART-ERA5, with input data from the ERA5 reanalysis. In this case, the additional fields required by the diagnostic tools (e.g., evaporation and precipitable water in the case of DB99) are also taken from ERA5, rather than from WRF simulations.”

L. 264: "basic results" -> "baseline"?

Thank you for the suggestion. We will rephrase the heading of Sect 3.1 from “Basic results for WRF-WVTs vs WaterSip and DB99 (Dirmeyer and Brubaker, 1999)” to “Raw comparison of WRF-WVTs vs SOD08 and DB99”.

Figure 3: I don't really see a great value in using a figure instead of a table, which I would think would be clearer/more easy to read with bigger font and better contrast. Values could by highlighted by making them bold or italtic.

We appreciate your suggestion, but we consider that a figure with a continuos color scale is more appropriate to highlight the different contributions of the moisture sources.

L. 285 and onward: The terms "good" and "bad" or "better" and "worse" could be more neutrally be rephased by using expressions such as "more consistend with reference" or "less consistent", or "larger deviation", "smaller deviation". There is not really a way of telling objectively here at what value a difference can be considered good or bad.

Thank you for your comment. We will replace these terms with others more precise in the revised version of the manuscript.

L. 320: Such fluctuations are always a remaining part of the uncertainties from offline trajectory calculations. This is why the Δq threshold has been introduced. Given that there is virtually no impact from the threshold on the results, as in Fig. 5 indicates that there could be an error in the way the method has been implemented.

There are differences when changing Δq, but the impact on the RMSE is small compared to that of the time step. For example, the precipitation fractions for the source “Tropical Atlantic” in the Iberian Peninsula case are 19.87, 22.80, 22.74, 22.68 and 22.59 for Δq equal to 0.01, 0.05, 0.1, 0.2 and 0.3 g kg-1 for a 6 h interval. The change is relevant when going from 0.01 to 0.05, and then there appears to be a plateau. If we continued to increase Δq we would probably obtained more different results, although the overall RMSE would probably decrease.

As mentioned before, we believe our implementation of the Sodemann et al. (2008) method to be correct, and we will make our code available in an open access repository, in accordance with ESD policies.

Figure 5: The low contrast in some of the tables makes the numbers hard to read.

Thank you for the suggestion. In the revised version of the manuscript we will change the color of the first rows in panels c) and d) to make them more readable.

L. 342: It was not easy to follow the narrative here, please rephrase.

Thank you for your comment. We will reformulate this sentence to “Figure 5a shows the RMSE of the precipitation fractions computed using the SOD08 diagnostic, using

WRF-WVTs results as the true values (Fig. 3), while Fig. 5b shows the average RMSE across the five cases.”.

L. 353: important for what?

In L355 we explain that in the South Africa, Iberian Peninsula and Greenland cases there is a large bias for the contribution of the most important source (South Atlantic in the first case, North Atlantic in the other cases), which is almost halved when introducing the proposed modification. Thus, it makes sense to consider the improvement an important one. In any case, in the revised version we will change “important” to “noticeable” for clarity.

L. 358 and L. 362: significantly: rephrase if no statistical test has been performed

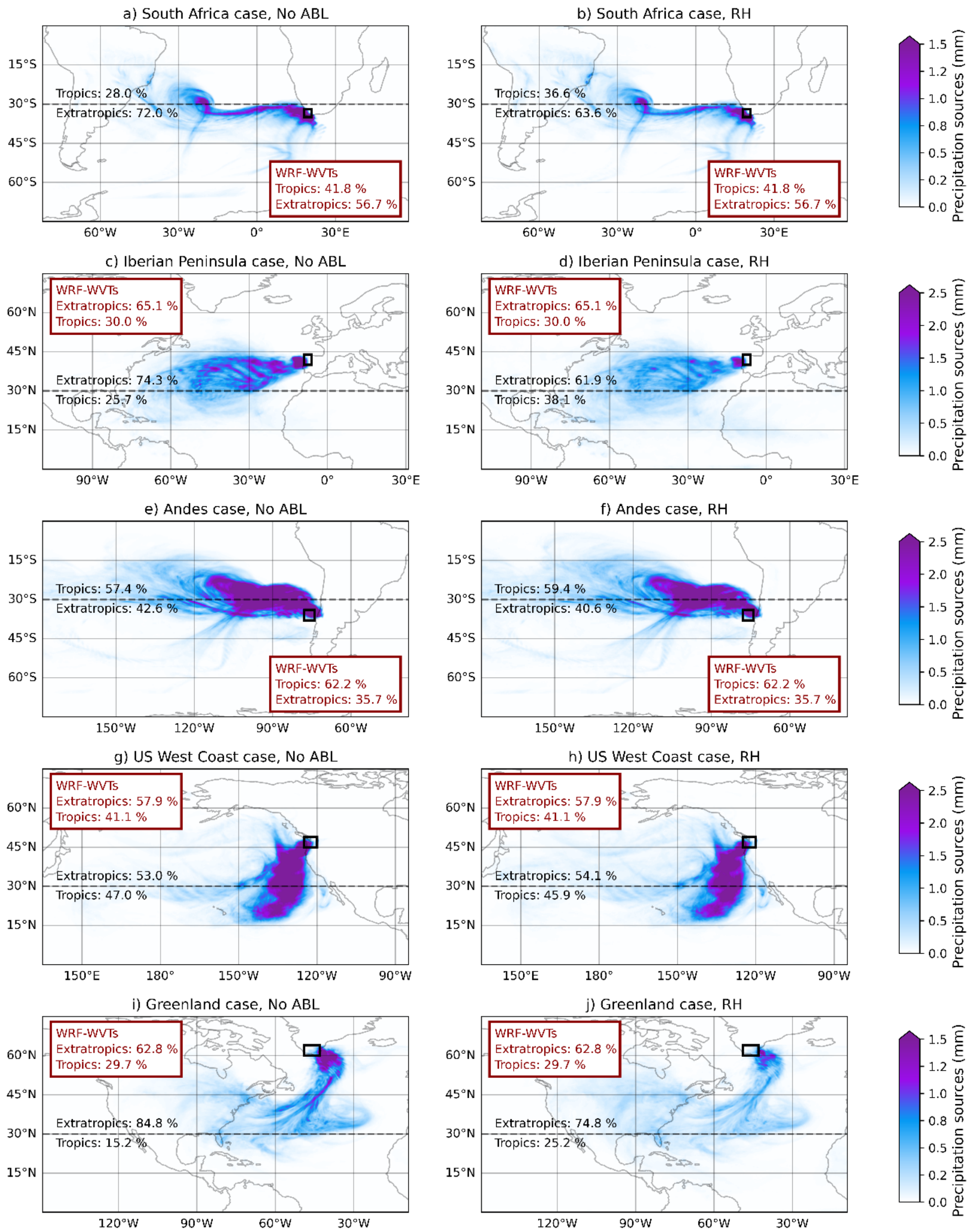
Thank you for this suggestion. In the revised version of the manuscript, we will change “significantly higher” to “noticeably higher” and “significant effect” to “considerable effect”.

L. 369: Move to figure caption

In the revised version of the manuscript, we will clarify that “the most basic configuration” refers to the “No ABL setup” in the caption of Fig. 6.

L. 374: What means "the moisture field is less intense"?

It is a typo, it should be “the moisture source field is less intense”. We are trying to explain that the values of the spatial distribution of precipitation sources are smaller. For clarity we will rephrase this sentence as “the moisture source field takes lower values over the North Atlantic”



445 Figure 4: Precipitation sources for the different AR-related rainfall events, computed with WaterSip, for trajectories generated with FLEXPART-WRF. Panels show the results for the most basic configuration, while panels on the right present the results of the “RH” configuration. The fraction of precipitation coming from the tropics and the extratropics is shown in black for each case, and the red box shows these same contributions from WRF-WVTs.

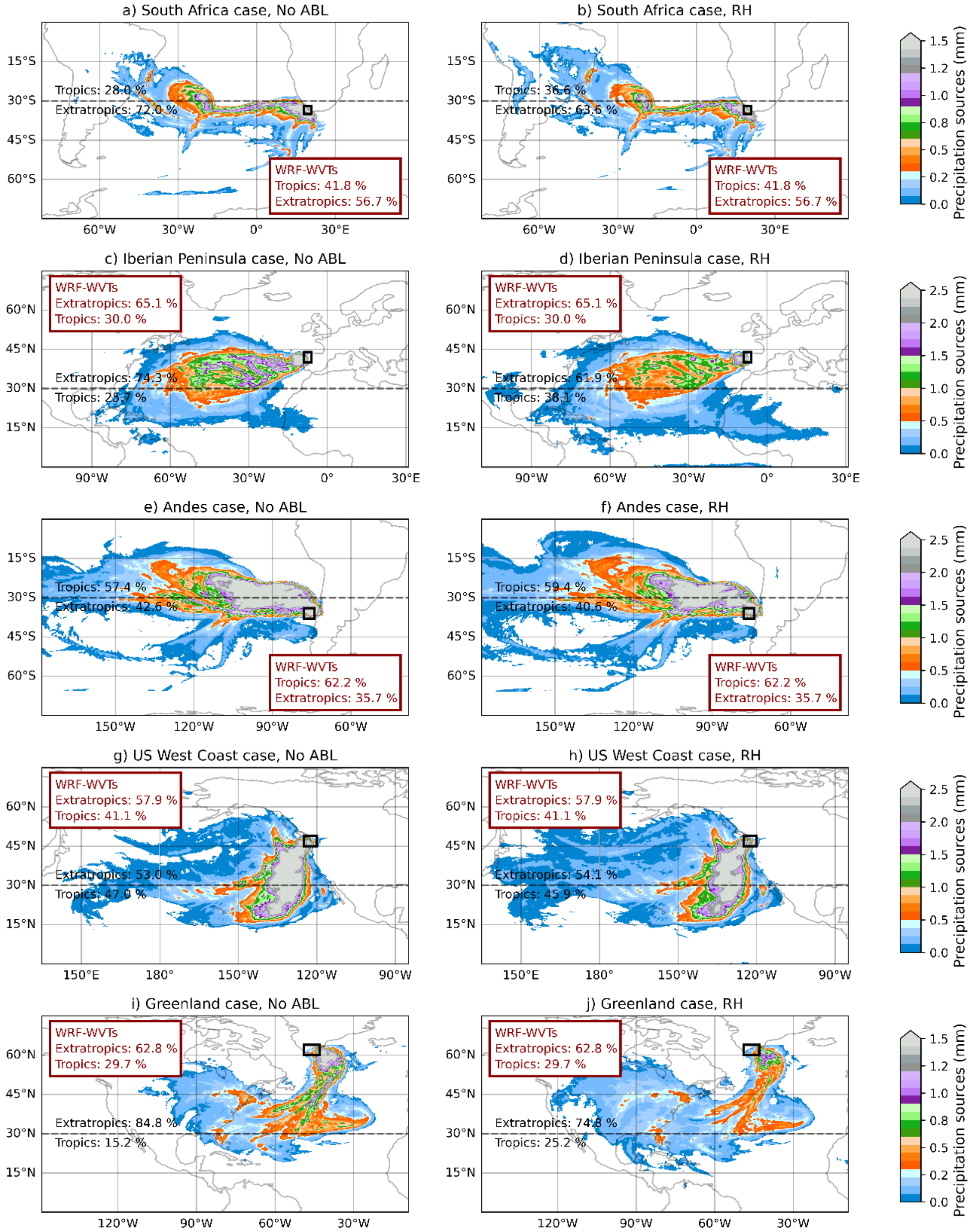


Figure 5: Same as Fig. 4, different scale.

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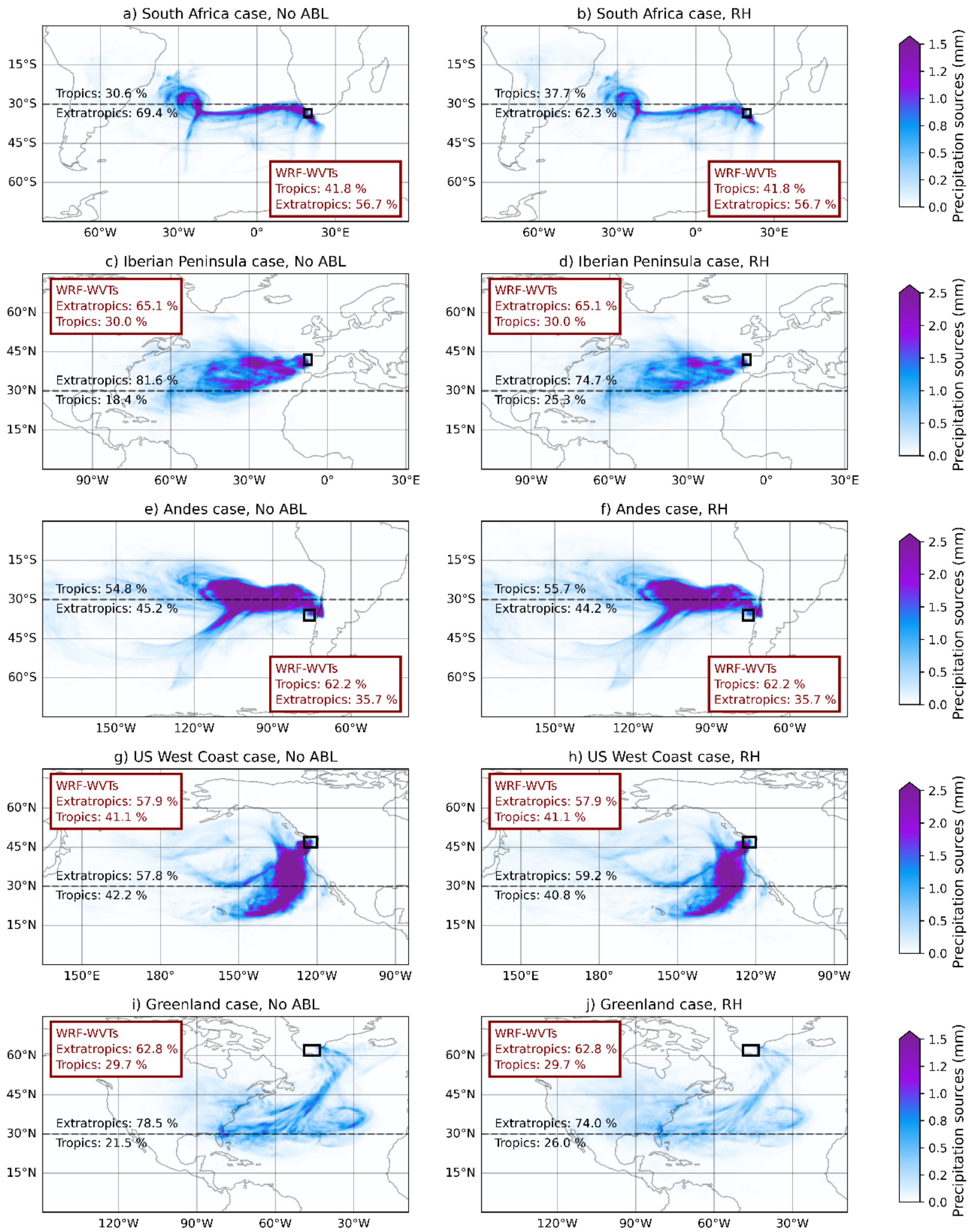

Figure 6: Precipitation sources for the different AR-related rainfall events, computed with WaterSip, for trajectories generated with FLEXPART-ECMWF. Panels show the results for the most basic configuration, while panels on the right present the results of the "RH" configuration. The fraction of precipitation coming from the tropics and the extratropics is shown in black for each case, and the red box shows these same contributions from WRF-WVTs.

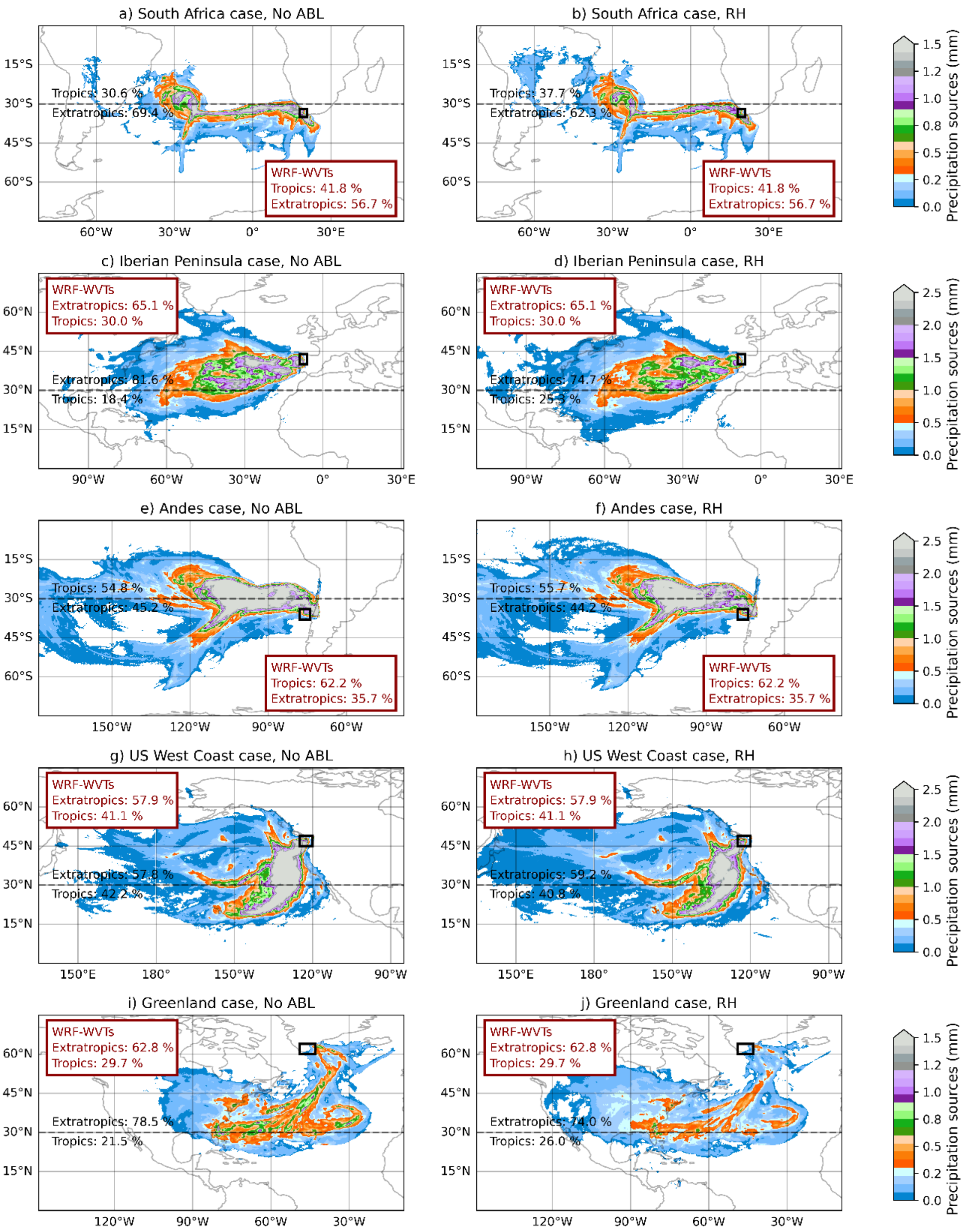

Figure 7: Same as Fig. 6, different scale.