Response to Reviewers

Minor Revision (egusphere-2026-332)

Atmospheric river trajectories organise along a global transport network

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Reviewer comments are shown in bold; author responses are given in italics. Line numbers refer to the document with highlighted changes.

Response to Reviewer 1

Reviewer: From a methodological standpoint, the framework is well formulated and constitutes a major strength. The use of multiple AR catalogs, Lagrangian trajectory-based construction, and null models provides a solid foundation. At the same time, the approach relies on several modeling choices—such as centroid-based representation, edge definition, thresholding, and consensus construction—that may influence the resulting topology.

We thank the reviewer for their helpful and detailed feedback. We really appreciate their constructive suggestions and think they will help us to make the manuscript ready for publication. We respond to all comments, including the raised matter of modelling choices, in a point-by-point manner below.

Reviewer: At the same time, several interpretations would benefit from more cautious framing. In particular, some findings (e.g., hubs, IVT changes) largely reflect known physical processes expressed in the network representation. Interpretations based on PageRank or shortest-path structures appear to reflect topological properties of the constructed network rather than independently demonstrated dynamical mechanisms. Similarly, claims regarding predictability are based on moderate correlations and structural arguments, and could be stated more conservatively.

We agree that the proposed framework does not discover independent dynamical mechanisms and that predictability claims should be stated more cautiously, as further described below. We have reworded accordingly.

Point 1

Reviewer: The results depend on the underlying AR catalogs and trajectory definitions. It would be useful to more explicitly quantify how robust the main structures are across detection methods, and how differences between catalogs and the chosen consensus construction propagate into the network.

We thank the reviewer for this comment. We agree that the choice of AR catalog and AR locator constitute the main sources of uncertainty in the proposed approach.

In the previous version of the manuscript, we investigated the catalog-sensitivity of key network properties only in one of the main figures (Fig. 4a) but more extensively in nine supplementary figures (Fig. S5, S6, S8, S12, S13, S14, S15, S18, S23). In the revised version of the manuscript, we have extended the catalog-sensitivity analysis by constructing AR networks from two additional extratropical AR catalogs compiled from algorithms with tracking capabilities: the ARCONNECT and IPART-1 algorithms. Figure S16 now provides an overview of edge agreement and edge weight differences between these four catalogs. We briefly discuss these as uncertainties stemming from different AR detection and tracking approaches and hint at the potential of future studies (e.g., by the ARTMIP project) to investigate them in more detail considering a larger ensemble of tracking-enabled AR catalogs (l. 326-329, l. 332-337).

We do consider that the choice of the AR locator is investigated comprehensively in the current manuscript, as we present how results differ with respect to distinct trajectory definitions (we consider the AR centroid, core and head) in five figures in the supplement (Fig. S4, S5, S6, S12, S14).

We had not considered alternative consensus construction approaches. Thus, we have now extended the analysis to briefly test the sensitivity of two alternative consensus definitions in the revised manuscript. To not extend the already long manuscript, we also present these new results in the supplement (Fig. S10) and discuss them concisely in the main manuscript (l. 172-175). The choice of the consensus formation technique does not seem to alter the large-scale network topology but mostly alters consensus weights in low activity regions.

Point 2

Reviewer: The connection between network structures and atmospheric dynamics could be clarified. Some interpretations (e.g., "highways" or accumulation regions) appear to reflect known circulation features or topological properties of the network, rather than independent mechanisms.

We agree with the reviewer that the observed topological properties do not reveal entirely novel atmospheric mechanisms. In the revised manuscript, we explicitly point out that the identified network features map to known circulation features but could in this sense help to improve our physical understanding of AR transport (l. 223-225, l. 655-658).

Point 3

Reviewer: The analysis depends on several methodological choices (e.g., AR representation, thresholding, spatial resolution, and consensus construction). A brief discussion of the sensitivity of the results to these choices would strengthen confidence in the conclusions.

As described in our response to Point 1, we have deepened the sensitivity analysis with respect to AR catalog choice and consensus formation. The effect of thresholding in the previous version was only showcased through AR network visualization (Fig. S8). The impact of different spatial resolutions had not been analyzed. For the revised manuscript, we have added two supplementary figures: Fig. S9 shows how several network properties change *when the threshold is varied continuously, clarifying the role of this important parameter, and Fig. S7 demonstrates how changing the spatial resolution alters node strength, edge betweenness centrality and AR basin structures. These sensitivities are explained in the supplement and briefly pointed out in the main manuscript (l. 125, l. 412-413).*

Point 4

Reviewer: The predictability perspective is interesting but currently limited. A more cautious framing or clearer statement of limitations would improve the discussion.

We agree with the reviewer that this should be phrased more carefully, given the limited evidence at this stage. We have phrased our promising but limited findings more carefully (l. 412-413, l. 650-651, l. 666-667, l. 691-692).

Reviewer: Overall, the manuscript presents a clear and useful framework with relevant insights for the community. The study is well executed and suitable for publication after minor clarifications and a more careful framing of interpretation and predictability claims.

We thank the reviewer again for their thoughtful review.

Response to Reviewer 2

Reviewer: The authors put forward a framework that identifies the major Atmospheric River (AR) basins on a global scale, speculate on the respective AR drivers, and explore how they change in response to seasonality and the El Niño Southern Oscillation (ENSO) climate mode. The paper is well written and insightful, with the figures conveying the results concisely. However, a justification of some of the choices made is required and a stronger link to meteorology is also needed. I believe that after a major revision the manuscript will be in a suitable form to be published in this journal.

We thank the reviewer for their detailed revision of our manuscript. We agree that their main concern—justifying some parameter choices and linking the identified topological features to meteorological features more closely—will further improve the manuscript. We also greatly appreciate their constructive and creative suggestions, pointing towards the potential of our framework to answer many additional research questions (e.g., long-term AR transport changes, local vs. remote moisture sources for specific AR basins, interactions between seasonality and ENSO in modulating AR highways). We address their concerns point-by-point below. With our response, we hope to convince the editor and reviewers that, given the current length of the manuscript, investigating all of the suggested additional research questions is unfortunately beyond the scope of our manuscript. In this regard, we are looking forward to future applications and further developments for which we are very happy to collaborate as well.

Point 1

Reviewer: The ARs used to construct the global atmospheric river transport network (ARTN) are extracted from the Potsdam Institute for Climate Impact Research (PIK) Atmospheric River Trajectories version 1 (PIKART-1) and the Tracking Rivers Globally as Elongated Targets Version 4 (tARget-4) catalogues. Why are these catalogues selected? How sensitive are the results to the choice of the AR catalogues and hence to the way ARs are diagnosed? The authors should also provide in the text further information on how the ARs are identified in these two products, this is not clear from lines 76–79.

We thank the reviewer for this important remark. We agree that the choice of the AR catalog is indeed one of the main sources of uncertainty in the proposed approach. In the current version of the manuscript, we investigate the catalog-sensitivity of key network properties only in one of the main figures (Fig. 4a) but more extensively in nine supplementary figures (Fig. S5, S6, S8, S12, S13, S14, S15, S18, S23). As of now, not all AR catalogs include tracking. An exhaustive analysis that could integrate all the available AR identification algorithms that have tracking capabilities (e.g., as they are collected in the ARTMIP project) is unfortunately beyond the scope of this manuscript. However, in the revised version of the manuscript we have extended the analysis of catalog-sensitivity by constructing AR networks from two additional extratropical AR catalogs compiled from algorithms with tracking capabilities: the AR-CONNECT and IPART-1 algorithms (Fig. S16). We briefly discuss it in the main manuscript as well (l. 326-329, l. 332-337). Here, we also comment on how some central choices in the type of AR detector may affect AR network topology. While the precise detection and tracking algorithms used to compile these AR catalogs is not the topic of this manuscript, we revised and slightly expanded the description of their identification approaches in the methods (l. 81-92).

Point 2

Reviewer: The AR basins displayed in Fig. 5a may not be always active during the full study period. For example, ARs in the northern Arabian Peninsula are more prominent in El Niño winters and following spring season (Dasari et al., 2017; Esfandiari and Rezaei, 2022) when the mid-latitude storm track is also shifted equatorwards (as seen in Fig. 4h). In summer following El Niño winters, ARs are less frequent in the subtropics over southeast Asia (Liang and Yong, 2021). Can the authors identify the meteorological conditions under which each AR basin is more prominent? This would allow for predictability: if a similar environmental set up is forecasted, we have an idea of the regions more likely to be impacted by ARs.

The reviewer raises an important and interesting point: it is true that the identified AR basins are long-term aggregates and may exhibit varying activity at seasonal and interannual time scales. We believe that this nonstationarity and its meteorological drivers are worthwhile to be investigated in depth in a future study (see Point 3). We agree with the reviewer that this could also promise enhanced predictability. Allowing for deformations and spatial shifts in the structure of AR basins, however, introduces several complexities: we would have to determine when a basin is still the same basin or when it has to be *considered a distinct one. The plethora of community detection methods from network theory allows to define such margins and account for transient communities (see e.g., Aslak et al. (2018)). This community detection task in an evolving network is, however, highly non-trivial. In this regard, considering additional climate data to elucidate which climate states (e.g., circulation regimes) preferentially give rise to which community appears very interesting, albeit non-trivial.*

As we would prefer not to add to the already long manuscript with additional method development and extensive climate data, we now provide a first look into how AR basins are altered seasonally (Fig. S28) and briefly discuss those (l. 570-575). With this, we hope to directly respond to one of the reviewer's interesting questions, i.e., if certain communities are more active during a given season.

Point 3

Reviewer: Have the authors explored trends in the AR features and basins? For example, are some AR basins becoming less relevant in detriment of others? What about the AR IVT changes? Can these trends be linked to changes in the background state as given by ERA-5 that is used to extract the ARs? The long study period allows for a statistically robust trend analysis to be performed.

We thank the reviewer for this question as it reassures us that this is one of the most natural and promising next steps for the introduced framework. As briefly described in Point 2, tracking AR basins across time is a challenging task. Quantifying trends in the other network properties is more straightforward. A bachelor's thesis on this topic is currently being supervised, so we hope to expand on this intriguing question in a follow-up manuscript soon. We consider the question on changing baseline IVT also as very interesting. Especially the PIKART algorithm is well-suited for appropriately detecting and tracking ARs on a drifting IVT background as it extracts ARs from endogenous anomalies in IVT variability. We explored some trends in our previous study (Vallejo-Bernal & Braun et al. (2025)). We will try to study the direct impacts of drifting IVT on basin structures in the above-mentioned follow-up study.

Point 4

Reviewer: From a meteorological perspective, are all the AR communities in Fig. 5a independent from each other? For example, take the one over western Greenland. The ARs that occur here develop locally or originate from those that occur in the communities around it? In lines 465–467 it is stated "Physically, AR communities can be understood as enclosed geographical regions whose boundaries are determined by persistent steering flows, topography, coastal moisture gradients and thermodynamic limits on AR life cycles (depending on the rates of evaporation and precipitation over an AR's life cycle)." Have the authors considered adding a table with the major features of each AR basin? Can the authors quantify the relative contribution of local vs. remote moisture sources for the ARs in each basin? This may help justify some of the considered AR communities.

We are glad about the reviewer's overall interest in the AR basin approach. AR basins are not fully independent. For any pair of neighbouring basins, one can still expect a certain degree of inter-community traffic. The intra- versus inter-community transport frequencies are visually indicated by the edges in Fig. 6e–h. Here, we can see that communities are coupled weakly at the highest hierarchical level (Fig. 6a/e) and become more tightly linked the further we "zoom in". Meteorologically, the AR basins represent emergent transport regions shaped by atmospheric dynamics, but they are not strictly closed systems. Their physical meaning lies in preferred pathways and residence regions, not strict source–sink separation.

Detailed analyses of AR lifecycles within their main transport regions have been the focus of previous studies (e.g., Guan & Waliser (2019)). A detailed investigation of how these should be revisited given the basins identified here is beyond the scope of our study. However, in the revised version of the manuscript, we now added the clarification that AR basins are not fully independent (l. 531-532) and have added a supplementary table and figure to list central lifecycle attributes for each AR basin (Fig. S27 and Tab. S1, l. 559-569). An analysis of local versus remote moisture sources, e.g. using FLEXPART, is a great direction for future work but currently beyond the scope of this study.

Point 5

Reviewer: For the ENSO results displayed in Fig. 4, and based on what is shown in Fig. S18, I believe the authors took all El Niño and La Niña months in the period 1950–2023 irrespective of the season. Given the seasonal contrast in AR highways that is more prominent in the Northern Hemisphere (Figs. 4e–f), it would be better to generate seasonal maps for El Niño and La Niña as well. There are also different flavours of ENSO (Newman et al., 2011), which can also be considered.

We agree with the reviewer that seasonal contrast between the identified AR highways is more pronounced in the northern hemisphere. Considering ENSO flavours and combinations between ENSO regimes and seasons will likely lead to interesting insights in how ARs are modulated by multi-scale climate oscillations but would lead away from the main objective of our study, that is, introducing the general methodological framework and demonstrating how it offers a unified representation of global AR transport. We have added some more explicit phrases to emphasize this objective more clearly in the revised version (l. 63-64, 651-652).

Minor Comments

Reviewer: Line 17: "narrow and long channels of anomalously high water vapor transport"

We have amended the definition accordingly (l. 17).

Reviewer: Lines 341–343: This is in line with the findings of Ramos et al. (2019), which also stresses the role of moisture from South America in driving cold-season ARs over the western CAPE.

We thank the reviewer for providing this reference and have added it (l. 387-388).

Reviewer: Lines 438–440: This should go into the Methods section.

It is not entirely clear to us why the respective sentence would be better placed in the Methods section as it does not touch on an aspect of the AR network method. To us, its content is descriptive/interpretative rather than methodological. We paste it here for potential further discussion: "The Western Hemisphere Warm Pool represents one of the main NH evaporative regions and has been previously identified as a global hub of AR moisture uptake (Algarra et al., 2020), but Fig. 2a additionally reveals that the LLJs can be distinguished from a background of overall high moisture uptake."

Reviewer: Line 445: "Washington state" or "northwestern North America"

We have changed it to "northwestern North America" (l. 498).

Reviewer: Lines 571–573: What is meant by "dangerous impacts" here? Aren't ARs also impactful outside of the polar regions?

We rephrased (l. 664).

Reviewer: Lines 604–606: The methodology used here can also be applied to aerosol atmospheric rivers (AARs; Lapere et al., 2024), in which aerosols such as dust, black carbon, organic carbon, and sea salt are transported polewards within a dry or moist air mass.

We have added this very interesting application to the outlook and thank the reviewer for sharing this good idea (l. 699).

Additional revisions

We corrected several typos and modified the phrasing/wording of several sentences. We substantiated the qualitative description of how similar ARTNs from pre-1979/post-1979 ERA5 and MERRA2 data appear by quantitative estimates of their edge overlap and edge weight correlations (l. 77-79). We adapted the order of supplementary figures to still match the flow of the revised manuscript.

References

Aslak, U., Rosvall, M., & Lehmann, S. (2018). Constrained information flows in temporal networks reveal intermittent communities. *Physical Review E*, 97(6), 062312.

Guan, B., & Waliser, D. E. (2019). Tracking atmospheric rivers globally: Spatial distributions and temporal evolution of life cycle characteristics. *Journal of Geophysical Research: Atmospheres*, 124(23), 12523–12552.

Vallejo-Bernal, S. M., Braun, T., Marwan, N., & Kurths, J. (2025). PIKART: A comprehensive global catalog of atmospheric rivers. *Journal of Geophysical Research: Atmospheres*, 130(15), e2024JD041869.