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Peer Review File

Large-scale circulation drives atmospheric river landfall in the western United States

Corresponding Author: Dr Chanil Park

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Park,

Your manuscript titled "Beyond extratropical cyclones: Large-scale control on atmospheric river landfall in the western United States" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Min-Hui Lo, PhD
Editorial Board Member
Communications Earth & Environment

Alice Drinkwater, PhD

Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Title: Beyond extratropical cyclones: Large-scale control on atmospheric river landfall in the western United States
Authors: C. Park and Y. Ming
Manuscript #: COMMSENV-25-3730-T

Summary: In this study the authors investigate the role of large-scale, low-frequency dynamics in western US atmospheric

river landfalls using 45 years of reanalysis data. They find that western US landfalling ARs are most frequently tied to a combination of high- and low-frequency drivers characterized by extratropical cyclones and the novel CNP teleconnection pattern, respectively. These “intermediate” ARs are shown to predominantly contribute to total AR related precipitation and high impact landfalling events due to their tendency to last longer (because of their quasi-stationary nature) and be geometrically larger than their HF counterparts. The results and conclusions presented here are of interest to many parties that seek to advance subseasonal predictability of ARs as well as understand how AR activity will evolve in a future climate. Overall, the paper is very well written and organized, the methodology is thorough and sound, and the author’s assertions are strongly supported by the figures and text. In the opinion of this reviewer, this study adds to the existing body of scientific work in a tangible way and is worthy of publication with the minor revisions/comments outlined below.

Minor Comments:

1. Line 62: Do you mean to say, “first to show?”
2. Line 123: Should be “southwestern US.”
3. Fig 3/S6: Please distinguish what the contours and shading correspond to (i.e. IVTA and precipitation) in the correlation panel description. Also, in Fig. 3d it appears that the 99% confidence interval line is missing.
4. Line 367: “landfalling” not “landfilling”
5. Line 368-369: May want to remove “the” at the beginning of the sentence.
6. Line 411: What does “identical AR” mean? Is it a long-lasting AR with multiple landfall timesteps? Please clarify.
7. Line 422: Rewrite as “PC time series normalized by their standard deviation” for clarity.

Reviewer #2 (Remarks to the Author):

The authors ran an EOF analysis to find a new teleconnection pattern (the Circum-North Pacific Pattern), which has a strong direct influence on landfalling atmospheric activity on the US West Coast. They also quantify differences between intermediate and high-frequency ARs and find that intermediate ARs are more hazardous and persist for longer. I think the manuscript is well-written and can be valuable for the field. However, I do have comments and concerns that I think should be addressed. Please consider the feedback that is provided below.

Lines 12-13: The first sentence of the abstract could benefit from improved wording. You could try “Atmospheric rivers (ARs) play a crucial role in driving extreme precipitation and providing water resources to the western United States”
Lines 14-15: I think this sentence oversimplifies the processes that cause ARs to form. While ARs are often associated with extratropical cyclones, there are also warm conveyor belts, frontal boundaries, and tropical moisture exports are also at play.
Lines 15-16: “Here, we find that landfalling ARs in this region are not merely synoptic-scale phenomena but are also driven by large-scale circulation” I am confused by this sentence. Large-scale circulation is a synoptic-scale phenomenon. This will likely confuse readers.
Line 28: I suggest taking out the word “The”. The sentence should read “ARs exert major influences on regional water resource management …”
Lines 38-40 (and the following paragraph): I am not convinced that there is a “prevailing perception that ARs owe their existence exclusively to extratropical cyclones”. Is there a source for this? I would argue that ARTMIP (Shields et al. 2018) demonstrates that ARs are broadly defined and extratropical cyclones are rarely if ever required to define ARs when detection is applied
Lines 151-152: Does the negative phase of the CNP symmetrically suppress AR induced moisture transport?
Figure 2 and 3: I suggest mentioning the type of significance test in the figure captions
Lines 166-167: I suggest describing this implication for S2S predictability rather than just saying that the lack of connection to deep tropical convection has an implication
I also suggest creating a hypothesis for what is driving this mode. The authors seem to rule out tropical drivers, but do not offer a possible physical driver for the CNP
Line 183-184: I’m not following the logic here. If the differences in max IVT between intermediate and HF-dominated ARs are relatively minor, wouldn’t that be inconsistent with previous studies showing that hydrological extremes in the US are strongly linked to duration of AR conditions?
Line 211-214: I suggest rewording this sentence. It is already known that AR landfall can be directly influenced by large-scale circulation. I would change this to “… AR landfalls in the western US can be driven by the CNP teleconnection pattern …”
Lines 225-226: I’m not sure it is fair to say that the CNP pattern and its representation in climate models are “key to improving the predictability of AR landfalls” when there was no analysis of the predictability of the CNP in the S2S range and beyond. If someone was to run that analysis and find that the CNP does not have predictability in the S2S range, there is no reason to believe that it could contribute to landfalling AR predictability.
Figure 4e,f: If this is the percentage of (for example intermediate ARs) that fall into categories 3-5, I would simply describe that as a frequency rather than a probability
What percentage of landfalling ARs, AR IVT, and AR precip occur during the positive phase of the CNP?

Best regards,
Tim Higgins

Reviewer #3 (Remarks to the Author):

This manuscript was a delight to read and provides a thorough investigation of large scale and synoptic scale dynamic drivers of U.S. west coast ARs. The methodology is creative and appropriate for the problem, and includes redundancy that increases confidence in the results. This manuscript successfully argues for the importance of larger-scale dynamic drivers of ARs, especially for the most impactful events. I have a few major comments regarding the role of AR/cyclone lifecycle in differentiating ARs and the classification of Rossby Wave trains. After these comments are addressed and a few clarifications are made, I recommend this manuscript for publication.

Comments
1) Lines 45–53: The authors may find the research done on global teleconnections and/or blocking highs and Antarctic ARs1,2 relevant.
2) Figure 1a: I recommend changing this to a non-diverging color bar since there is no distinct separation at .65.
3) Somewhere in the results or figures please include the total number of ARs studied (total, low frequency, intermediate) so the reader has some idea of the number of events considered.
4) Figure S3: I really appreciate that the authors include multiple AR detection algorithms in their analysis. This meaningfully strengthens their results.
5) Lines 86–88: Figure 1c, Figure S2c, and Figure S3 show strong evidence of the decreased IVTA_HF associated with landfalling ARs compared to upstream ARs. For the GW15 AR detection algorithm, there is not strong evidence of an increase in IVTA_LF contributing to this difference. I think lines 86–87 are a bit overstated. Have the authors explicitly isolated the role of cyclones? Or more generally the faster-moving change in IVT? Some other factors contributing may include: lifecycle of cyclone and or/AR, topographical influence, and background moisture of the environment.
6) Generally: double check the order of supplementary figures, I think they may be out of order from how they appear in the text.
7) Figure 2: Related to comment 5, I am curious if the lifecycle of the AR/attendant cyclone plays a role in the difference between HF-dominant and Intermediate ARs. Does the lifecycle at the time of landfall correlate to the large-scale flow pattern and the association with precipitation/duration?
8) Lines 122–127: Thank you to the authors for including the supplementary movie – this was very helpful. Figure 2 and Supplementary Movie 1 show a clear large-scale evolution associated with Intermediate ARs including an anticyclonic upper-level anomaly over East Asia that is absent in the HF-dominant ARs. Based on the movie, I am not sure that this can be classified as persistent flow from a Rossby wave train as the anticyclonic flow over the U.S. develops and moves northeast over the 72 hours prior to AR landfall. I suggest the inclusion of a Hovmöller diagram or something similar to strengthen the classification of a Rossby Wave Train.
9) Figure 3 is really nicely put together and shows a clear result. Consider adjusting the written labels in 3d (rotating vertically?) as it was confusing to me that the labels referred to each vertical line.
10) Lines 152–153: Figure S6 shows strong correlation between the 1st EOF and precipitation north of the U.S., so it is confusing to say “do not exhibit comparable influence on AR-like moisture transport and coherent precipitation”. Maybe include more specifics here, do you mean magnitude of correlation? Or penetration inland?
11) Lines 165–166: This seems like a significant result! What does that mean about the origins of the Rossbly wave? What does that mean for the potential for increased moisture uptake associated with intermediate ARs?
12) The inclusion of the extreme landfall example provided a really nice illustration of your results!
13) Lines 230–231: Please clarify what you mean by “dominate the interannual variability of AR landfalls”. Is this based on a sigma of 6 versus 2.8? So higher year to year variability? Could include an r2 between interannual variability of intermediate ARs and all ARs to make this explicit.

1. Wille, J. D. et al. Atmospheric rivers in Antarctica. Nat. Rev. Earth Environ. 6, 178–192 (2025).
2. Baiman, R. et al. Synoptic and planetary-scale dynamics modulate Antarctic atmospheric river precipitation intensity. Commun. Earth Environ. 5, 127 (2024).

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Version 1:

Decision Letter:

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Dear Dr Park,

Your manuscript titled "Beyond extratropical cyclones: Large-scale control on atmospheric river landfall in the western United States" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have addressed all of my concerns. I recommend acceptance for publication.

Reviewer #2 (Remarks to the Author):

Dear Chanil Park,

Congratulations on writing a high-level manuscript and thank you for addressing my comments. I am now satisfied with the revised version and recommend that it gets accepted.

Best regards,

Tim Higgins

Reviewer #3 (Remarks to the Author):

Thank you for addressing all of my comments thoroughly. I recommend this paper for publication.

Reviewer #3

This manuscript was a delight to read and provides a thorough investigation of large scale and synoptic scale dynamic drivers of U.S. west coast ARs. The methodology is creative and appropriate for the problem, and includes redundancy that increases confidence in the results. This manuscript successfully argues for the importance of larger-scale dynamic drivers of ARs, especially for the most impactful events. I have a few major comments regarding the role of AR/cyclone lifecycle in differentiating ARs and the classification of Rossby Wave trains. After these comments are addressed and a few clarifications are made, I recommend this manuscript for publication.

→ We appreciate the reviewer's effort to provide constructive comments. We faithfully addressed the comments and improved the manuscript. The line numbers in this reply are based on the revised manuscript.

Comments:

1. Lines 45–53: The authors may find the research done on global teleconnections and/or blocking highs and Antarctic ARs[1],[2] relevant.

1. Wille, J. D. et al. Atmospheric rivers in Antarctica. Nat. Rev. Earth Environ. 6, 178–192 (2025).
2. Baiman, R. et al. Synoptic and planetary-scale dynamics modulate Antarctic atmospheric river precipitation intensity. Commun. Earth Environ. 5, 127 (2024).

→ Thank you for bringing these papers to our attention. We have briefly mentioned the role of planetary-scale waves and blocking in driving extreme AR landfall over Antarctica in the revised manuscript (*L. 52-53*).

2. Figure 1a: I recommend changing this to a non-diverging color bar since there is no distinct separation at .65.

→ We tested a non-diverging color bar; however, it did not clearly capture the upstream–downstream contrast in AR behavior. Instead, we refined the color-bar resolution so that the mid-point value (0.66) is more clearly discernible.

3. Somewhere in the results or figures please include the total number of ARs studied (total, low frequency, intermediate) so the reader has some idea of the number of events considered.

→ The fraction of HF-dominant ARs and intermediate ARs is provided in *L. 106*. In addition, the number of their 6-hourly snapshots is provided in *L. 418-420*. We would emphasize the fraction, rather than the number, because the latter varies substantially depending on the AR detection algorithm used, whereas the MI distribution is highly robust (Fig. S3).

4. Figure S3: I really appreciate that the authors include multiple AR detection algorithms in their analysis. This meaningfully strengthens their results.

→ Thanks for your appreciation!

5. Lines 86–88: Figure 1c, Figure S2c, and Figure S3 show strong evidence of the decreased IVTA_HF associated with landfalling ARs compared to upstream ARs. For the GW15 AR detection algorithm, there is not strong evidence of an increase in IVTA_LF contributing to this difference. I think lines 86–87 are a bit overstated. Have the authors explicitly isolated the role of cyclones? Or more generally the faster-moving change in IVT? Some other factors contributing may include: lifecycle of cyclone and or/AR, topographical influence, and background moisture of the environment.

→ Thank you for the comment. Please note that the contours for landfalling ARs represent larger probabilities than those for upstream ARs in the area of large $IVTA_{LF}^{AR}$. Climatologically, landfalling ARs exhibit smaller $IVTA_{HF}^{AR}$ by approximately 160 kg m^{-1} s^{-1} and larger $IVTA_{LF}^{AR}$ by approximately 16 kg m^{-1} s^{-1} compared to upstream ARs, justifying our statement.

We have not yet examined differences in lifecycle evolution between HF-dominant ARs and intermediate ARs; this will be addressed in our future work. Please see our reply to Comment #7 for our speculation regarding AR lifecycle dynamics.

6. Generally: double check the order of supplementary figures, I think they may be out of order from how they appear in the text.

→ Thanks for the comment. The supplement figures are displayed in the order of citation in the main text.

7. Figure 2: Related to comment 5, I am curious if the lifecycle of the AR/attendant cyclone plays a role in the difference between HF-dominant and Intermediate ARs. Does the lifecycle at the time of landfall correlate to the large-scale flow pattern and the association with precipitation/duration?

→ Thank you for the comment. A systematic investigation of the lifecycle evolution of HF-dominant and intermediate landfalling ARs is beyond the scope of the present study and will be pursued in our future work. Here, we briefly discuss our speculation that the primary factor differentiating their lifecycle evolution would be the large-scale circulation, not the attendant extratropical cyclones.

Zhou et al. (2018) identified two major AR genesis hotspots during boreal winter over the North Pacific: one upstream of the storm track and the other downstream, northeast of Hawaii (Fig. R1). The upstream hotspot coincides with a baroclinically unstable region where transient baroclinic waves are generated and grow most rapidly (Hoskins and Valdes 1990; see also enhanced local baroclinicity in Supplementary Fig. S1). We therefore hypothesize that HF-dominant landfalling ARs predominantly originate from this upstream hotspot and retain their HF character throughout their lifecycle until landfall (schematically illustrated as the hypothetical lifecycle path “A” in Fig. R2).

As explained in the manuscript, intermediate ARs arise from the constructive interference between HF moisture transport associated with extratropical cyclones and LF moisture transport induced by the large-scale circulation (i.e., the CNP mode). From this perspective, their lifecycle evolution may follow two plausible pathways:

1) initiation as HF-dominant ARs in the upstream region, followed by superposition with CNP-induced LF moisture transport as propagating downstream (hypothetical lifecycle path "B" in Fig. R2)
2) initiation as LF-dominant ARs in the downstream region, followed by superposition with approaching HF moisture transport associated with extratropical cyclones (hypothetical lifecycle path "C" in Fig. R2).

These conceptual lifecycle scenarios are also qualitatively consistent with Supplementary Movie S1. In the absence of large-scale circulation driving LF moisture transport, landfalling ARs would be expected to primarily follow a single lifecycle pathway analogous to path "A" in Fig. R2.

Our argument is based on the additive contributions of extratropical cyclones and large-scale circulation to AR evolution. However, we do not rule out the possibility that their nonlinear dynamical interaction may play a role, which will also be addressed in our future study.

Intermediate ARs exhibit longer landfall duration (Supplementary Fig. S8) due to the quasi-stationary nature of the CNP pattern. We plan to analyze a more detailed three-dimensional characteristics (e.g., fronts, jet streaks, moisture flux profile) of two AR types and their relation to precipitation at the landfall location in a separate study.

Zhou et al. (2018): Life Cycle of Atmospheric Rivers: Identification and Climatological Characteristics. *Journal of Geophysical Research: Atmospheres*
Hoskins and Valdes (1990): On the Existence of Storm-Tracks. *Journal of the Atmospheric Sciences*

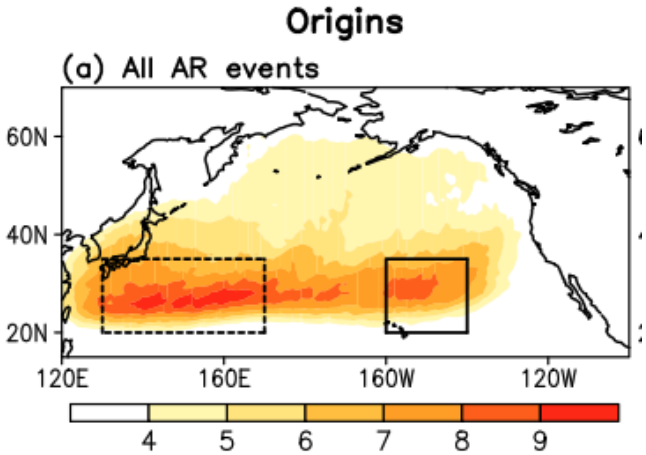

Figure R1. Wintertime AR genesis frequency (%) in the North Pacific. Taken from Zhou et al. (2018)

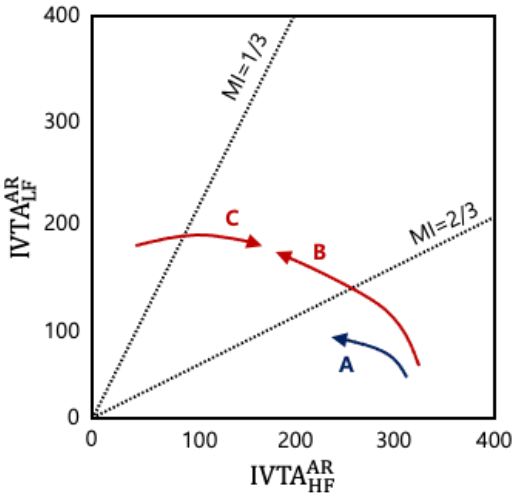

Figure R2. Hypothetical lifecycle paths of HF-dominant AR landfall ("A") and intermediate AR landfall ("B" and "C") are represented on the $IVTA_{HF}^{AR}$–$IVTA_{LF}^{AR}$ plane. The two thin black lines denote MI=2/3 and MI=1/3, respectively, separating HF-dominant ARs, intermediate ARs, and LF-dominant ARs.

8. Lines 122–127: Thank you to the authors for including the supplementary movie – this was very helpful. Figure 2 and Supplementary Movie 1 show a clear large-scale evolution associated with Intermediate ARs including an anticyclonic upper-level anomaly over East Asia that is absent in the HF-dominant ARs. Based on the movie, I am not sure that this can be classified as persistent flow from a Rossby wave train as the anticyclonic flow over the U.S. develops and moves northeast over the 72 hours prior to AR landfall. I suggest the inclusion of a Hovmöller diagram or something similar to strengthen the classification of a Rossby Wave Train.

→ Thank you for the comment. The anticyclonic anomaly around the southeastern U.S. at the time of intermediate AR landfall does not come from upstream but is generated **in situ** due to downstream Rossby wave energy dispersion. Please note that the wave train in Move 1 exhibits **LF-filtered** geopotential height anomalies, which by construction have negligible phase speed. This interpretation is well supported by the southeastward Rossby wave activity flux.

While a Hovmöller diagram can be useful for illustrating the group velocity of Rossby waves, it is intentionally not provided because the CNP pattern exhibits a Rossby wave ray along an arc of the North Pacific coast, making it difficult to represent in a traditional Hovmöller diagram.

9. Figure 3 is really nicely put together and shows a clear result. Consider adjusting the written labels in 3d (rotating vertically?) as it was confusing to me that the labels referred to each vertical line.

→ Fig. 3d has been revised following your suggestion. Thank you.

10. Lines 152–153: Figure S6 shows strong correlation between the 1st EOF and precipitation north of the U.S., so it is confusing to say "do not exhibit comparable influence on AR-like moisture transport and coherent precipitation". Maybe include more specifics here, do you mean magnitude of correlation? Or penetration inland?

→ Thank you for the comment. We revised the sentence for improved clarity as:

L. 156-158: *"In contrast, the PNA and NPO-like patterns do not exhibit comparable influence on* ***filamentary*** *moisture transport and the coherent precipitation* ***in the western U.S.*** *(Supplement Figures S6c,d)."*

11. Lines 165–166: This seems like a significant result! What does that mean about the origins of the Rossbly wave? What does that mean for the potential for increased moisture uptake associated with intermediate ARs?

→ Thanks for the comment. Because the CNP mode is believed to be an extratropical *internal* mode that does not require external forcing, its origin or source is difficult to specify. Instead, its maintenance and preferred preconditioning can be investigated. Although speculative, we suspect that the land-sea thermal contrast along the North Pacific coasts during boreal winter may provide a favorable environment for baroclinic energy conversion (CP; Tanaka et al. 2016):

$$\mathrm{CP} = -\frac{R}{pS_p}\left(u'T'\frac{\partial \bar{T}}{\partial x} + v'T'\frac{\partial \bar{T}}{\partial y}\right)$$

where S_p is the static stability parameter; R is the dry gas constant; overbar and prime denote background and anomaly, respectively (the other symbols follow the convention in atmospheric science). We briefly discuss this point in the revised manuscript (*L. 173-175*).

Since the CNP mode exhibits southwest-northeast tilted wave packets in the downstream region, it can directly pump moisture from the subtropical ocean without the aid of extratropical cyclones. We suggest that this process may be closely related to the Pineapple Express. Further detailed investigation of the physical/dynamical processes underlying subtropical moisture uptake (e.g., Lagrangian parcel trajectory, wave intrusion toward the westerly duct) will be conducted in our future study.

Tanaka et al. (2016): Vertical structure and energetics of the Western Pacific teleconnection pattern. *Journal of Climate*

12. The inclusion of the extreme landfall example provided a really nice illustration of your results!

→ Thank you!

13. Lines 230–231: Please clarify what you mean by "dominate the interannual variability of AR landfalls". Is this based on a sigma of 6 versus 2.8? So higher year to year variability? Could include an r2 between interannual variability of intermediate ARs and all ARs to make this explicit.

→ Thank you for the comment. We have included the linear correlation coefficient against all

ARs in Supplementary Fig. S11. By providing both the standard deviation and the correlation coefficient, it is now more evident that intermediate ARs dominate the interannual variability of AR landfalls.

Reviewer #2 (Tim Higgins)

The authors ran an EOF analysis to find a new teleconnection pattern (the Circum-North Pacific Pattern), which has a strong direct influence on landfalling atmospheric activity on the US West Coast. They also quantify differences between intermediate and high-frequency ARs and find that intermediate ARs are more hazardous and persist for longer. I think the manuscript is well-written and can be valuable for the field. However, I do have comments and concerns that I think should be addressed. Please consider the feedback that is provided below.

→ We appreciate the reviewer's effort to provide constructive comments. We faithfully addressed the comments and improved the manuscript. The line numbers in this reply are based on the revised manuscript.

Comments:

1. Lines 12-13: The first sentence of the abstract could benefit from improved wording. You could try "Atmospheric rivers (ARs) play a crucial role in driving extreme precipitation and providing water resources to the western United States".

→ Revised. Thank you.

2. Lines 14-15: I think this sentence oversimplifies the processes that cause ARs to form. While ARs are often associated with extratropical cyclones, there are also warm conveyor belts, frontal boundaries, and tropical moisture exports are also at play.

→ Thank you for this comment. We agree that there are various driving mechanisms of ARs, as shown in this study. Owing to the word limit of the abstract, however, we aimed to convey the main message as concisely as possible without enumerating all contributing mechanisms.

More importantly, despite phenomenological analogies, conceptually distinct from warm conveyor belts and tropical moisture exports (Ralph et al. 2018). A warm conveyor belt refers to **a moist, ascending airstream along the moist isentrope** within extratropical cyclones (Dacre et al. 2019), whereas ARs are characterized by horizontal moisture transport. Tropical moisture export describes **poleward moisture uptake from tropical moisture reservoirs** (Knippertz et al. 2013), but ARs are not necessarily confined to tropical margins. These distinctions are schematically illustrated in Fig. R1.

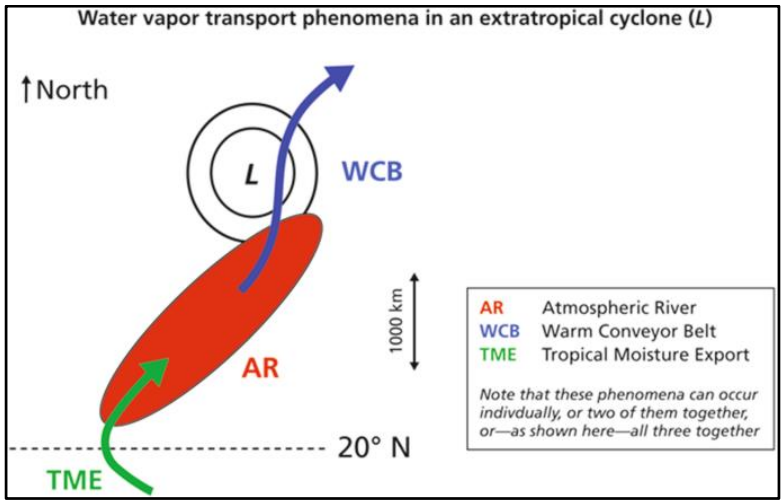

Figure R1. Phenomenological relation between ARs, warm conveyor belts, and tropical moisture transports within extratropical cyclones. Taken from Fig. 2.8 in Ralph et al. (2020).

Ralph et al. (2018): Defining "Atmospheric river": How the glossary of meteorology helped resolve a debate. *Bulletin of the American Meteorological Society*
Dacre et al. (2019): Linking Atmospheric Rivers and Warm Conveyor Belt Airflows. *Journal of Hydrometeorology*
Knippertz et al. (2013): A global climatology of tropical moisture exports. *Journal of Climate*
Ralph et al. (2020): Atmospheric Rivers. *Spinger*

3. Fig 3/S6: Please distinguish what the contours and shading correspond to (i.e. IVTA and precipitation) in the correlation panel description. Also, in Fig. 3d it appears that the 99% confidence interval line is missing.
→ Thank you for pointing these out. The figure caption has been revised.

4. Lines 15-16: "Here, we find that landfalling ARs in this region are not merely synoptic-scale phenomena but are also driven by large-scale circulation." I am confused by this sentence. Large-scale circulation is a synoptic-scale phenomenon. This will likely confuse readers.
→ Thanks for the comment. We agree that the original wording could be confusing, as the term "large-scale circulation" can be interpreted differently across subdisciplines of atmospheric sciences. To avoid this ambiguity, we have revised the sentence as:

L. 15: "Here, we find that landfalling ARs in this region are not merely synoptic-scale phenomena but are also driven by large-scale circulation ***unrelated to extratropical cyclones****."*

5. Line 28: I suggest taking out the word "The". The sentence should read "ARs exert major influences on regional water resource management …".
→ Done.

6. Lines 38-40 (and the following paragraph): I am not convinced that there is a "prevailing perception that ARs owe their existence exclusively to extratropical cyclones". Is there a source for this? I would argue that ARTMIP (Shields et al. 2018) demonstrates that ARs are broadly defined and extratropical cyclones are rarely if ever required to define ARs when detection is applied.

→ Thank you for the comment. We revised the sentence to adopt a more cautious tone:

L. 40-43: "Although the use of "typically" allows some flexibility, this definition has contributed to the prevailing perception that ARs owe their existence ***primarily*** *to extratropical cyclones,* ***with limited attention paid to other potential driving mechanisms****."*

We would also like to clarify that the statement that ARs are "broadly defined" in Shields et al. (2018) refers to the diversity of detection methodologies (e.g., thresholds and geometric criteria), rather than to a broad phenomenological interpretation of the physical mechanisms underlying ARs.

Following extensive community discussion on the AR definition, the AMS definition places particular emphasis on the association between ARs and extratropical cyclones (see the historical overview in Ralph et al. 2018). While this emphasis does not preclude the involvement of other driving mechanisms, it has arguably shaped a research focus in which alternative pathways have received comparatively less consideration.

Finally, we note that the absence of explicit extratropical cyclone information in many AR detection algorithms likely reflects practical and technical challenges rather than an intentional adoption of a broad phenomenological definition. This has led to the phenomenon-definition mismatch (as exemplified in *L. 47-53*) for which this study provides additional evidence in the context of landfalling ARs in the western US.

Ralph et al. (2018): Defining "Atmospheric river": How the glossary of meteorology helped resolve a debate. *Bulletin of the American Meteorological Society*

7. Lines 151-152: Does the negative phase of the CNP symmetrically suppress AR induced moisture transport?

→ Figure 3b shows the "linear" correlation coefficients. Thus, the mirrored suppressed AR-likely moisture transport is expected in the negative CNP mode.

8. Figure 2 and 3: I suggest mentioning the type of significance test in the figure captions.

→ Done.

9. Lines 166-167: I suggest describing this implication for S2S predictability rather than just saying that the lack of connection to deep tropical convection has an implication I also suggest creating a hypothesis for what is driving this mode. The authors seem to rule out tropical drivers, but do not offer a possible physical driver for the CNP.

→ Thank you for this constructive comment. We believe the deterministic S2S predictability of the CNP mode may be limited, given its red noise-like behavior and the lack of significant correlations with external climate states. This point has been clarified in the revised manuscript (*L. 171-173*).

Because the CNP mode is believed to be an extratropical *internal* mode that does not require external forcing, it is difficult to specify a unique origin or source. Instead, its maintenance and preferred preconditioning may be explored. Although speculative, we suspect that the land-sea thermal contrast along the North Pacific coasts during boreal winter may provide a favorable environment facilitating baroclinic energy conversion (CP) which can be expressed mathematically as (Tanaka et al. 2016):

$$\mathrm{CP} = -\frac{R}{pS_p}\left(u'T'\frac{\partial \bar{T}}{\partial x} + v'T'\frac{\partial \bar{T}}{\partial y}\right)$$

where S_p is the static stability parameter; R is the dry gas constant; overbar and prime denote background and anomaly, respectively (the other symbols follow the convention in atmospheric science). This idea is briefly discussed in the revised manuscript (*L. 173-175*).

Tanaka et al. (2016): Vertical structure and energetics of the Western Pacific teleconnection pattern. *Journal of Climate*

10. Line 183-184: I'm not following the logic here. If the differences in max IVT between intermediate and HF-dominated ARs are relatively minor, wouldn't that be inconsistent with previous studies showing that hydrological extremes in the US are strongly linked to duration of AR conditions?

→ Thank you for the comment. The sentence has been edited for improved clarity as:

L. 191-192: "This is consistent with previous studies showing that floodings in the western U.S. are strongly linked to the duration of AR conditions, ***rather than AR intensity****."*

11. Line 211-214: I suggest rewording this sentence. It is already known that AR landfall can be directly influenced by large-scale circulation. I would change this to "… AR landfalls in the western US can be driven by the CNP teleconnection pattern …"

→ Thank you for the comment. While the role of large-scale circulation in AR landfalls has been extensively studied, it has generally been viewed as acting indirectly (e.g., through storm-track modulation), which oversimplifies the multiscale hierarchy of AR activity. To our knowledge, no previous study has explicitly demonstrated a **direct** contribution of large-scale circulation to **individual** ARs in the western US. We discuss this distinction in the revised manuscript (*L. 226-230*, *L. 231-232*).

12. Lines 225-226: I'm not sure it is fair to say that the CNP pattern and its representation in climate models are "key to improving the predictability of AR landfalls" when there was no analysis of the predictability of the CNP in the S2S range and beyond. If someone was to run that analysis and find that the CNP does not have predictability in the S2S range, there is no

reason to believe that it could contribute to landfalling AR predictability.

→ We totally agree with your comment. The sentence has been revised as:

L. 233-235: "For example, the atmospheric dynamics underlying the CNP pattern and its representation in climate models warrant evaluation, as a necessary step toward exploring whether the CNP pattern could be leveraged for subseasonal-to-seasonal prediction of impactful AR landfalls."

13. Figure 4e,f: If this is the percentage of (for example intermediate ARs) that fall into categories 3-5, I would simply describe that as a frequency rather than a probability. What percentage of landfalling ARs, AR IVT, and AR precip occur during the positive phase of the CNP?

→ The term "frequency" may be misleading here, as it is often interpreted as the number of days with events divided by the total number of days (e.g., "AR frequency" in this study). Figures 4e,f quantify the conditional likelihood that a given AR type is accompanied by high-rank events. To better convey this interpretation, "probability" has been replaced with "conditional probability".

Approximately 67.6% and 75.9% of HF-dominant ARs and intermediate ARs, respectively, make landfall during the positive phase of the CNP (i.e., PC3 > 0). Intermediate ARs exhibit a noticeably higher percentage of AR IVT and precipitation during the positive compared to the negative CNP phase, whereas this enhancement is less pronounced for HF-dominant ARs (Figs. R2 and R3). We do not include these figures in the revised manuscript, as they do not provide additional information.

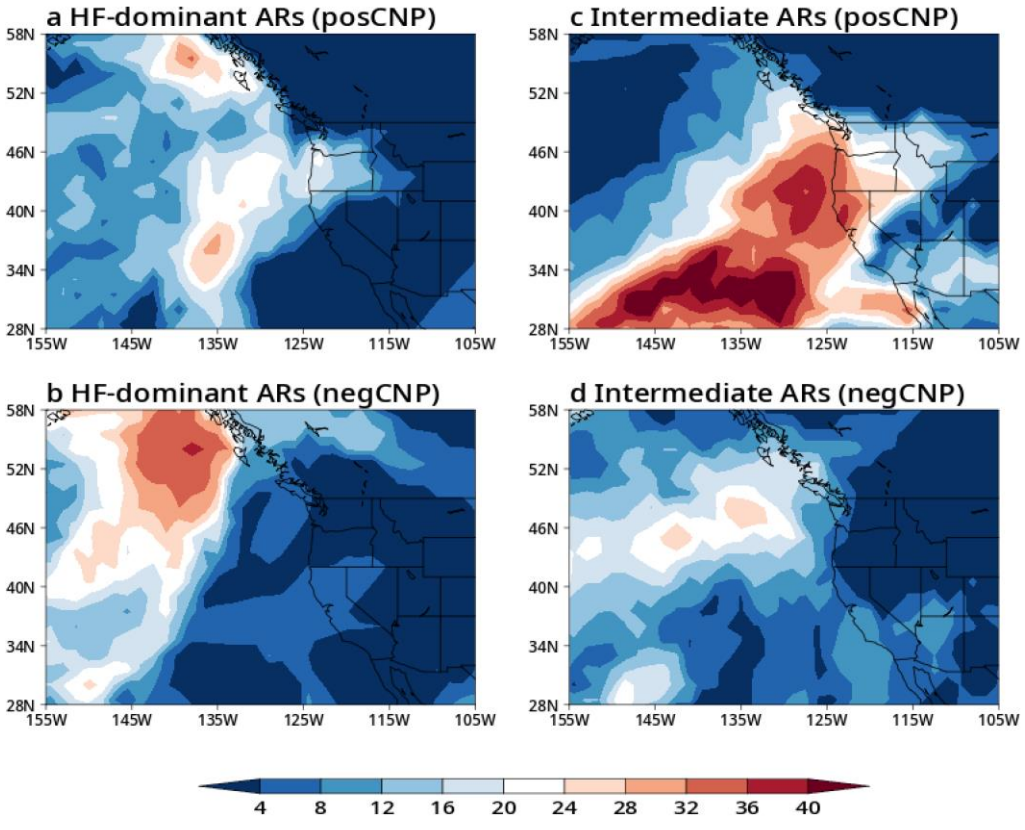

Figure R2. Percentages of IVT associated with (left) HF-dominant ARs and (right) intermediate ARs among total IVT during boreal winter, in (top) positive and (bottom) negative phases of CNP mode.

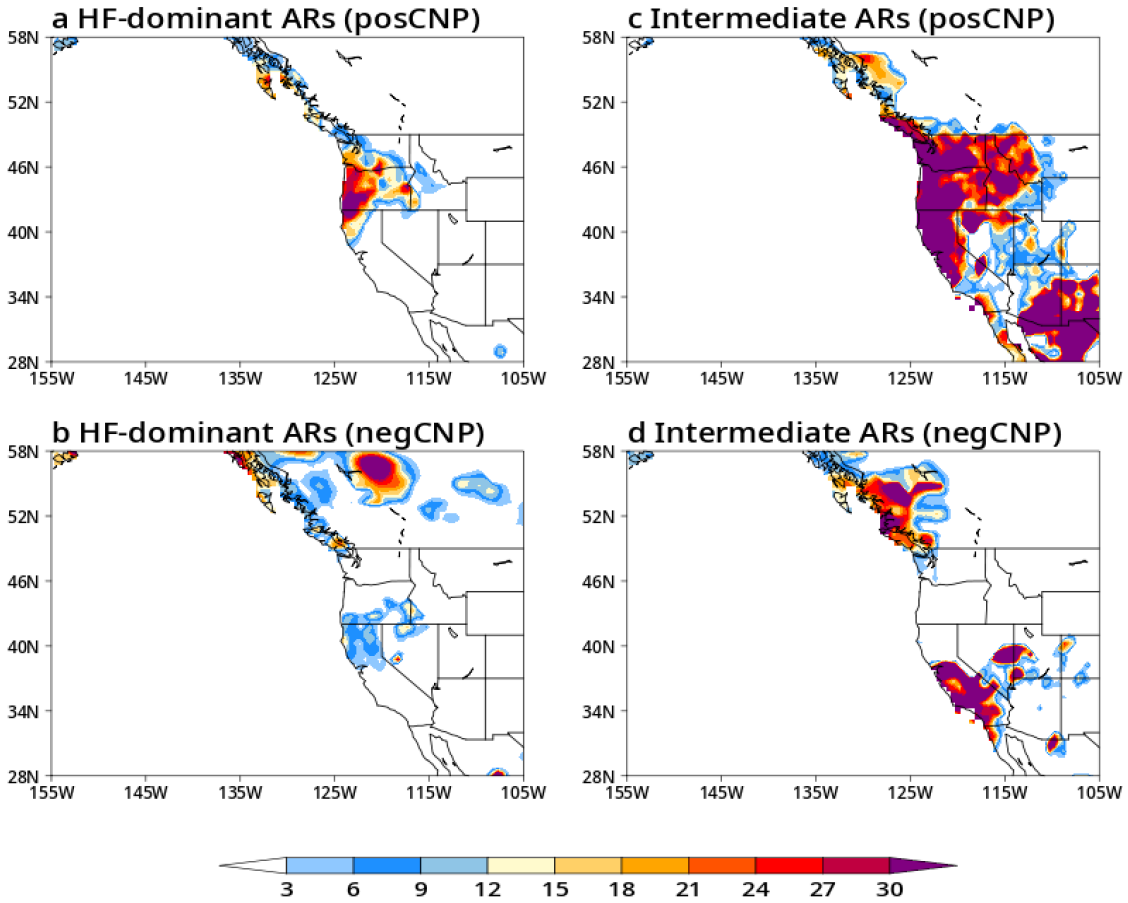

Figure R3. Percentages of precipitation associated with (left) HF-dominant ARs and (right) intermediate ARs among total precipitation during boreal winter, in (top) positive and (bottom) negative phases of CNP mode.

Reviewer #1

In this study the authors investigate the role of large-scale, low-frequency dynamics in western US atmospheric river landfalls using 45 years of reanalysis data. They find that western US landfalling ARs are most frequently tied to a combination of high- and low-frequency drivers characterized by extratropical cyclones and the novel CNP teleconnection pattern, respectively. These "intermediate" ARs are shown to predominantly contribute to total AR related precipitation and high impact landfalling events due to their tendency to last longer (because of their quasi-stationary nature) and be geometrically larger than their HF counterparts. The results and conclusions presented here are of interest to many parties that seek to advance subseasonal predictability of ARs as well as understand how AR activity will evolve in a future climate. Overall, the paper is very well written and organized, the methodology is thorough and sound, and the author's assertions are strongly supported by the figures and text. In the opinion of this reviewer, this study adds to the existing body of scientific work in a tangible way and is worthy of publication with the minor revisions/comments outlined below.

→ We appreciate the reviewer's positive evaluation of our study. We addressed all the minor comments raised by the reviewer.

Minor comments:

1. Line 62: Do you mean to say, "first to show?"

→ Yes, we do. The sentence is revised for improved clarity.

2. Line 123: Should be "southwestern US."

→ Revised. Thanks for the correction.

3. Fig 3/S6: Please distinguish what the contours and shading correspond to (i.e. IVTA and precipitation) in the correlation panel description. Also, in Fig. 3d it appears that the 99% confidence interval line is missing.

→ Thanks for pointing these out. The figure caption has been revised.

4. Line 367: "landfalling" not "landfilling"

Corrected.

5. Line 368-369: May want to remove "the" at the beginning of the sentence.

→ Edited.

6. Line 411: What does "identical AR" mean? Is it a long-lasting AR with multiple landfall timesteps? Please clarify.

→ We meant the long-lasting landfall by a single AR object. To avoid confusion, we have revised the sentence as:

L. 418: "When the AR landfall is successively observed over multiple time steps,"

7. Line 422: Rewrite as "PC time series normalized by their standard deviation" for clarity.

→ Revised.

Reviewer #1
The authors have addressed all of my concerns. I recommend acceptance for publication.
→ We appreciate your time and effort to review our manuscript.

Reviewer #2
Dear Chanil Park,

Congratulations on writing a high-level manuscript and thank you for addressing my comments. I am now satisfied with the revised version and recommend that it gets accepted.

Best regards,
Tim Higgins
→ We appreciate your time and effort to review our manuscript.

Reviewer #2
Thank you for addressing all of my comments thoroughly. I recommend this paper for publication.
→ We appreciate your time and effort to review our manuscript.