

Vapor Kinetic Energy for the Detection and Understanding of Atmospheric Rivers

Corresponding Author: Professor Da Yang

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

This manuscript discusses the role of integrated vapor kinetic energy (IVKE) on atmospheric river lifecycle, which is novel in the sense that IVKE is not typically used. There is a bit of an identity crisis as a new technique for AR detection is introduced but not thoroughly evaluated or used for the analysis in the paper. The methodology would benefit from additional clarification and is atypical for the field. References to the vast body of literature published in the JGR special collection over the past couple years (many of which are on the topic of AR detection and lifecycle in reanalysis products) are absent ([https://agupubs.onlinelibrary.wiley.com/doi/toc/10.1002/\(ISSN\)2169-8996.ARTMIP](https://agupubs.onlinelibrary.wiley.com/doi/toc/10.1002/(ISSN)2169-8996.ARTMIP)). Finally, the figures are not publication quality and should be improved.

Abstract: There typically are not references given in an abstract. Given that IVKE is a main component of the paper, it seems odd that it isn't mentioned in the abstract.

Lines 71-78: This section should be expanded. There are subtle differences between the detection with IVT and IVKE. Are these good or bad? Are the results with IVKE closer to the ARTMIP ensemble mean, median, or consensus? Are aspects of an AR getting picked up with one method but not the other? Why should one use IVKE for detection or should detection be completed with a different method and investigated using KE?

Figure 1: Please add titles to panels a and b, the variable name and units to the color bar, a legend to panel c, and axis labels to panel c. It appears as though there may be artifacts in panels a and b with vertical gray bars and splotches of white adjacent to blue. Double check this.

Line 85/Supplementary Material: The derivation in the supplement is not easy to follow. Additional detail should be given. Note this is not my expertise so I will have to trust that the equations and derivation are correct.

-Line 90: Either go through the equation with what each term represents with the variables in the equation or add in another version of the equation in terms of the abbreviations given in the text for VKE, etc. Does "other" include the analysis increments? If so, this needs to be stated in the text.

-Line 102: A sentence should be added here stating what the composite AR is. Why go through the process of detecting ARs with IVKE if it isn't used for the analysis? I am also on the fence about using this composite approach as opposed to a more traditional method for AR detection. How would this methodology impact the results?

Figures 2 and 3: These figures would greatly benefit from panel titles and a label on the color bar for IVKE. The captions should also mention that these are the fields regressed up area averaged IVKE in the selected box (assuming I am understanding what is being shown here).

Figure 4: This figure should account for the analysis increment.

Methods

-Line 230: How do the values computed using the tendency terms derived from the 3-hourly collections compare to the KE tendency terms available in `tavg1_2d_int_Nx`?

-Lines 241-246: For reproducibility, it is best to include more detail here and give the IVT and IVKE thresholds and spatial requirements. Were the tropics excluded? Was the Laplacian of IVKE tuned so that it matches the results for IVT? Was only the Laplacian threshold replaced for IVKE? Would IVKE work for a different style ARDT like Mundhenk or GuanWaliser?

-Line 253: What is "USA Wind" from? Is that a value given with IBTrACS?

-Line 255-256: It wouldn't hurt to show a plot of the time series in the supplemental. Is this an hourly timeseries?

-Line 258: Everything(!) is statistically significant? There is nothing masked out in the figures.

-Line 262: What is this equation? Why is it not explained in more detail?

Reviewer #2

(Remarks to the Author)

Review of "One Stone, Two Birds: Using Vapor Kinetic Energy to Detect and Understand Atmospheric Rivers" by Ong and Yang

The authors proposed a vapor kinetic energy (VKE) framework to further understand the physical processes that govern the maintenance and propagation of ARs. For example, they found that the conversion term from potential energy to kinetic energy explains the AR's maintenance the most, while horizontal VKE advection dominates the AR propagation. Overall, the article is well-written and interesting. The VKE framework is novel and can quantitatively resolve a fuller picture of the physical processes. It has the potential to diagnose large-scale weather phenomena like ARs. However, there are several major concerns over the analysis, such as the lack of a vigorous examination of the closure issue of the VKE framework to investigate the robustness of the framework in time and space. Also, the way that the authors computed the horizontal advection terms were not sufficiently justified. Also, there are rooms to improve the clarity of the analysis methods.

Major comments:

I believe the derivation of the integrated vapor kinetic energy (IVKE) prognostic equation (Eqs. S1-S7) is correct. The author also demonstrated that the terms related to S_T , S_C , F_m , and F_G are negligible (Table S1, Eq. 5). My major concern is about the turbulence-related momentum F_T term and the moisture source/sink term S_M . These two terms, to my knowledge, are rarely available from most reanalysis datasets, probably because they can only be derived from numerical simulations but not from observations. If this is correct, then the budget analyzed here may be subject to the potential biases of MERRA-2 in representing these processes. Hence, it would be useful if the authors could include each term's uncertainties in Table S1. Also, I suggest the authors apply their IVKE framework to other reanalysis datasets (if any) to validate the importance of the IVKE budget terms. Otherwise, I suggest the authors discuss more on the potential biases in MERRA-2 that may affect the results.

In the SI and Methods, the authors stressed that they "calculate HAKE by subtracting VAKE and PEKE from the IVKE tendency due to dynamics on KE." And they computed HAV in a similar manner. But, in my humble opinion, this is not a common practice. HAKE and HAV should be easily computed right away. The authors' implicit calculation of HAKE and HAV would unavoidably include potential residues due to non-closure problems (if any) into the budget term, which is not ideal. I suggest the authors compute these two horizontal advection terms directly and compare the difference in the results.

Since this paper proposes a new IVKE framework to understand ARs, naturally, the generality of the framework should be vigorously tested beforehand. In this respect, for example, I suggest the authors closely examine the closure of the IVKE budget equation (i.e., the residual term) in different seasons/months and locations, searching for conditions when the framework may suffer from a non-negligible residue.

Figure 2: Although the authors indicated that the other terms in Eq. 3 are negligible, it would be helpful also to add a panel showing the sum of the remaining budget terms due to other physical processes to check if there is any spatial pattern.

Figure 3: I am a bit surprised that, given that ARs are mostly east-west elongated in the region (as shown in Figure 1), the cross-section shows a perfectly symmetric, upright, and localized maximum in the VKE profile at 167.5E, 37.5N. Can you explain why?

Figure 3 and other composites: I suggest the authors do a bit more diagnosis on environmental fields (e.g., 3-D winds, geopotential height, specific humidity, etc.) to facilitate the interpretation the VKE budgets here. This also helps cross-check the main findings indicated by the VKE budget.

L160-170: I can understand the idea of performing regressions here. But it is hard to follow what the authors meant by "the IVKE map" and "the map of total IVKE tendency." These terms are not defined in their Methods section. This impedes an accurate understanding of what the contributions in Figure 4 really mean. Please define the terminology clearly in the Methods section.

Minor comments:

Figure 1: Suggest adding your selected box region to this figure.

L102-L104: Why only analyze the IVKE budget in 2019 instead of your research period (1980-2020). As shown in S1, there are also other AR hotspots in the North Pacific, e.g., the Asia region and the eastern North Pacific region. Could you add more regions to conduct the composite to reflect the region-to-region difference in the AR evolution?

L104-L106: "We calculate the IVKE time series of this region" — do you mean you simply compute the regional IVKE? Or do you actually mean the IVKE of the ARs in the region? Later in L128, you mentioned "the AR's IVKE anomaly" — how did you compute such an "anomaly"? Please clarify.

L128-L130: There are also negative values in the Figure 2c. Does it mean that the PE conversion to KE term only intensify and maintain the central AR region and weaken the surrounding AR regions?

L138-L139: Similar with the horizontal dimension of the composite AR, there are also negative values in the Figure 3c. Besides, are the results here sensitive to 37.5°N you choose? Maybe a 3D composite figure of the terms in the IVKE budget could make the results more intuitive and comprehensive to understand.

L160-L170: This paragraph just repeats the conclusion mentioned earlier and doesn't give new insights or conclusions. It's better merge this paragraph into the previous parts.

L207-L215: Apart from the questions you raise here, I hope to know about the main roles of different terms in ARs' lifecycles, e.g., the generation, intensification and weakening and decay.

L241-248: In the TempestExtremes algorithm, both the northward vapor transport and the eastward vapor transport are taken to investigate AR and non-AR poleward moisture transport. Since the VKE combine them together, how could you detect the ARs with the IVT equation, or do you neglect this procedure in this manuscript? Besides, you manually select the IVKE threshold according to the result. A more objective method should consider the corresponding quantiles of the threshold in IVT and IVKE.

L252: Please clarify the extent of the area you masked around each TC track point.

Reviewer #3

(Remarks to the Author)

Review of NCOMMS-22-50129-T

Overview

I appreciate the authors' efforts to develop a novel framework for studying the dynamics of atmospheric rivers (ARs). The "vapor kinetic energy" (VKE) framework enables diagnosis of processes associated with the co-evolution of kinetic and water vapor fields, and thus provides insight into the governing dynamics of ARs. The paper reads fairly well; the text is concise and clear, and figures provide clear illustrations of the main findings of the paper. Praises aside, I have some major concerns regarding the novelty and justification for this study, as well as the composite methods used to analyze the VKE budget for ARs. I therefore recommend that the manuscript undergo major revisions.

Comments

1. In my opinion, the authors need to provide some additional discussion regarding the novelty of this VKE framework and its advantages over the traditional kinetic energy and moisture budget framework. What new insights does the VKE budget provide that cannot be gleaned from considering the kinetic energy and moisture budgets? A main finding is that the VKE growth is due mainly to the conversion of potential energy to kinetic energy and, to a lesser extent, vertical advection of water vapor [which is of course related to moisture convergence; see Norris et al. (2020, section 5)]. These results are not surprising; those two processes are known to be sources of kinetic energy generation in cyclones and increases in moisture in ARs, respectively.

2. In my opinion, this regression-based composite approach based on the time series of IVKE a geographically fixed box is not appropriate for the IVKE budget analysis of ARs. The regression analysis indicates the relationship between IVKE for one particular 1° x 1° box and the different terms in the IVKE budget equation. Because the regression is performed for the entire time series of IVKE, it, of course, includes a number of times in which no AR or only part of an AR is actually present over this region. So, I find it misleading to refer to the structure appearing in the regression-based maps as the "composite AR." Additionally, I am concerned that the fixed box (25–50°N; 147.5°E–172.5°W) used for computing the budget terms in Fig. 4 is quite large, encompassing a region much larger than the spatial scale of an AR. Thus, the results of the budget calculations in Fig. 4 may not accurately represent the contributions of the various terms to ARs.

The composite analysis should really be done in a quasi-Lagrangian or AR-following composite framework using the ARs that were detected using the Tempest Extremes algorithm. This approach would, in my view, be far more suitable for

examining the dynamics associated with the evolution of ARs. It could be particularly enlightening to perform the analysis at different stages of the AR life cycle (e.g., formation stage, time of maximum intensity, and lysis stage).

3. Lines 34–37: This introductory discussion fails to mention that ARs are, in essence, components of baroclinic waves/extratropical cyclones. I recommend including a couple of sentences regarding how ARs form in the context of the life cycles of baroclinic waves/cyclones. This is critical contextual information for the analysis that appears later in the paper.

4. Lines 40–42: This statement is inaccurate and misleading. There is a large body of research on the structure and dynamics of extratropical cyclones and the embedded low-level jets; this relevant research should be mentioned in the text. Also, my understanding is that the eastward movement of ARs is largely the result of the eastward movement of the parent baroclinic wave system and its embedded low-level jet. Because an AR is not a wave, it is not accurate to say that it propagates.

5. Line 93: Note that the PE to KE conversion term is the only term in the equation that can actually generate KE following the flow. This term can be further decomposed into a geopotential flux divergence term, which corresponds to the redistribution of kinetic energy, and a baroclinic conversion of potential energy to kinetic energy (see, e.g., Orlanski and Sheldon 1995; Papritz and Schemm 2013). It would be really interesting to see how these different terms contribute to the PE–KE conversion. I recommend that the authors include this decomposition to provide further insight into the processes associated with the evolution of VKE.

6. Line 250: Please explain the reasoning for only using data for 2019. Inclusion of more years of data would, of course, yield more robust results.

References

Norris, J. R., and Coauthors, 2020: The observed water vapor budget in an atmospheric river over the Northeast Pacific. *J. Hydrometeor.*, 21, 2655–2673, doi:10.1175/JHM-D-20-0048.1.

Orlanski, I., and J. P. Sheldon, 1995: Stages in the energetics of baroclinic systems. *Tellus A*, 47, 605–628, doi:10.1034/j.1600-0870.1995.00108.x.

Papritz, L., and S. Schemm, 2013: Development of an idealised downstream cyclone: Eulerian and Lagrangian perspective on the kinetic energy. *Tellus A*, 65, 19539, doi:10.3402/tellusa.v65i0.19539.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors did a great job with responding to the reviewer comments. I have a couple other comments however they are mostly wording related.

Line 31: Can “running” be modified?

Line 33: transports more what? Add “water” here.

Lines 42-43: What eastward drift? This topic hasn’t been introduced and is not cited.

Figure 1: Are panels a and d the ARTMIP generated data? If so, the doi should be cited.

Line 100: Typo (should be “changes”)

-Line 118: Why limit the composite to begin in 2010?

-Line 181: Consider replacing “bottom” with “surface” or “1000 hPa”

-Line 299: 2020 or 2019?

-Line 301: The staggering is intentional. The (3 hourly averaged) tendency is the time rate of change between two instantaneous time steps.

-Line 343: The figure caption says 1980 to 2019.

Reviewer #2

(Remarks to the Author)

After carefully reviewing the manuscript titled "Using Vapor Kinetic Energy to Detect and Understand Atmospheric Rivers" by Hing Ong and Da Yang, I cannot recommend this manuscript for publication in Nature Communications. Below, I outline several critical reasons for this decision:

1. Lack of Novelty

While the concept of using Vapor Kinetic Energy (VKE) to study atmospheric rivers (ARs) is intriguing, the manuscript does

not convincingly demonstrate that this approach offers significant new insights or improvements over existing methods. The IVKE-based detection results are shown to be almost identical to those obtained using the traditional Integrated Vapor Transport (IVT) framework. This redundancy raises questions about the added value of the proposed methodology.
Ref: <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022GL102091>

2. Insufficient Justification for Methodology

The authors introduce VKE as a novel metric for AR detection and analysis, yet the manuscript lacks a comprehensive justification for why this metric is necessary. The claim that IVKE captures the physics of ARs' evolution more effectively than IVT is not substantiated with compelling evidence or detailed comparison across a sufficiently varied set of case studies. Although the authors compare their findings to the median of ARTMIP, but the uncertainties uncovered by the variety of ARTMIP participating algorithms is another important merit that this manuscript missed. I appreciate that the authors also compared to another independent algorithm in ARTMIP: Mundhenk et al. (2016), however, as mentioned above, the uncertainties uncovered by the various algorithms is the merit of ARTMIP, there are other algorithms. A similar global map of AR between this method and Mundhenk et al. (2016), is not convincing. A recent study on global future ARs using another ARTMIP is just published in JGRAtmos (Zhang et al. 2024), their algorithm also claim to be able to investigate the physical process (Pan and Lu, 2019).

Ref: Zhang et al. <https://doi.org/10.1029/2023JD039359>

Original algorithm of Zhang et al. also in ARTMIP: <https://doi.org/10.1029/2018WR024407>

3. Limited Theoretical Development

The theoretical development of the IVKE framework and the associated budget equation is not sufficiently detailed. The manuscript references supplementary materials for key equations (S1-S7), but these should be integrated into the main text to ensure the reader can fully understand and evaluate the proposed framework without needing to refer to additional documents.

4. Overstated Claims

The manuscript makes several broad claims about the advantages of the VKE framework, such as its ability to explain the evolution of ARs better than existing methods. However, these claims are not supported by robust statistical analyses or a diverse range of empirical validations. The examples provided are limited, and the comparison between IVT and IVKE detection appears superficial.

5. Incomplete Analysis and Presentation

The manuscript presents some results from the TempestExtremes and Mundhenk et al. algorithms but fails to provide a thorough analysis of the differences between these algorithms and the implications for AR detection. Additionally, the visualizations (Figure 1) are not sufficiently clear or informative to support the authors' conclusions.

6. Ambiguous Results and Conclusions

The results section contains ambiguities regarding the practical implications of adopting the IVKE framework. The manuscript does not convincingly demonstrate that using IVKE leads to significant improvements in AR detection or understanding of their physical processes. The conclusions drawn are therefore speculative and not well-supported by the presented data.

7. Lack of Broader Context

The manuscript could benefit from a more comprehensive discussion of how the proposed VKE framework fits into the broader context of atmospheric science. It lacks a critical comparison with other recent advancements in the field and does not adequately address the potential limitations or challenges associated with the new approach.

Conclusion

In summary, while the manuscript introduces an interesting concept, it falls short in terms of novelty, methodological justification, theoretical development, and empirical validation. The claims made are not sufficiently supported by the data and analyses presented. For these reasons, I recommend that this manuscript not be accepted for publication in Nature Communications.

Reviewer #3

(Remarks to the Author)

The authors have made numerous revisions that have improved the manuscript. I have a number of comments on the revised manuscript for the authors to consider. Once these comments are satisfactorily addressed, I believe the manuscript may be acceptable for publication in Nature Communications.

Comments:

There is large amount of supplementary material appended to the manuscript. The main discussion refers to supplementary text and figures frequently. I am okay with this; however, I propose that some of the material might be suitable for inclusion in the manuscript. For instance, it could be worthwhile to include Figs. S10 and S11 for the Jan 2023 case study in the manuscript, perhaps as a single 4-panel figure. Also, it might be appropriate to include the derivation of the IVKE budget in the Methods section.

Line 60–61: "their impact is largely attributed to intense winds" I am confused by this statement. How have you come to the conclusion that the impact of ARs is largely attributed to intense winds? By "impact" do you mean precipitation amounts? It

is my understanding that the impact of ARs in terms of precipitation is attributed to winds, moisture transport, and, of course, vertical motion.

Line 117: In my view, it is not entirely accurate to refer to the regression maps a "composite AR" given that you are performing a regression analysis on the IVKE time series and are not actually generating composites for a sample of detected ARs.

Line 118–119: The fact that similar methods have been used previously for studying other phenomena is not in itself sufficient justification for use of this regression method here. In my opinion, some discussion is needed to explain the advantages for using this regression method versus generating composites in AR-relative coordinates for samples of ARs that were detected and tracked using Tempest Extremes. It would also, in my view, be helpful to discuss caveats when using the regression method to examine processes governing ARs. One could generate a more generalized AR-relative composite based on a large sample of ARs detected/tracked over the North Pacific. This approach would also enable investigation of different stages of AR lifecycles (as you mention in line 286).

Line 122: There is seasonal variability in the dynamics and frequency of ARs over the North Pacific. As far as I can tell, the regression analysis focuses on the entire time series of VKE. How would the results change if the the budget analysis were done for individual seasons? Would the relative strength of the terms differ between winter and summer? Consider discussing this issue in the text (perhaps in the part where you discuss what to do next).

Line 132: Here and elsewhere, "move"/"movement" would be better than "drift".

Line 151–153: The supplementary text regarding PE-to-KE conversion and Fig. S15 should be cited here.

Line 163: Here and elsewhere, it might be more accurate to say that the processes producing negative VKE tendencies over the AR core result in reductions of VKE. It seems misleading to say that these negative tendencies are resulting in the decay of the AR. The regression analysis does not enable diagnosis of the decay of ARs.

Line 185: I suggest describing the vertical distribution of specific humidity and KE in a different, more precise manner. The terms bottom-heavy and top-heavy are ambiguous. Perhaps say "specific humidity is maximized at lower levels, while KE is maximized at upper-levels" or something to that effect.

Line 262: The association between ARs and low-level jets is already well-known, having been documented in a number of prior observational studies. Consider changing "suggest" to "confirm".

Minor text corrections:

Line 31: "pushed along" → "transported"

Line 33: "transports more water than"

Line 55: "primary source of water vapor in ARs"

Line 100: "changes"

Line 155: "slides down" → "is directed across"

Line 161: "rich" → "large"

Line 164–165: "show no signal in plots using the color scheme" → "with no values above the minimum contour level"

Line 179: "Both processes show bottom-heavy structures." → "The tendencies for both processes are concentrated between 1000 and 800 hPa."

Line 189: Insert "findings of" before "previous"

Line 221: "cutting" → "doing so cuts off"

Line 223: Insert "off" after "cuts"

Line 281: Insert "weather" after "extreme"

Line 281: "landfalls" → "landfall"

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

I appreciate the authors' rebuttal response. But I do not see substantial changes/improvement made to the manuscript rather than reinforcing their existing points. Also, nature communications emphasises multidisciplinary work. While this work is an interesting work to atmospheric science community providing an additional point of view on atmospheric rivers, it doesn't appear to me as a good fit for NC. I recognize the innovation of IVKE framework, but I think the authors overstate the importance of this IVKE framework, because the key results that were learnt from this framework is not new to the AR community. However, the framework itself provides a new technique.

The authors state that many previous studies "implicitly assumed that evolution of winds is of secondary importance to the physics of ARs, a compromise of lacking an analysis method that accounts for both winds and humidity" (line 57-59). I respectively disagree. The AR community (for example led by ARTMIP) have been emphasizing on the importance of understanding the physical evolution of AR, especially the AR lifetime, including its growth, propagation and dissipation. I would like to invite the authors to explore more recent literatures in the AR community.

As commented above, the manuscript mainly highlights the following three results and argue that these were not done by the IVT framework - I disagree:

- ARs grow mainly because of potential energy conversion to kinetic energy
- ARs decay largely owing to condensation and turbulence,
- ARs' eastward movement is primarily due to horizontal advection of VKE.

IVT/IWV based AR identification captures the existence, growth, development and dissipation of AR in their lifetime. Owing to ARs' synoptic scale, the growth is attributable significantly to large scale circulations, i.e., the steering. AR is a system reflecting large scale circulations, therefore studies have analysed the energy profile (associated with advection and vertical growth/changes), especially the earlier work comparing warm conveyor belt, extratropical cyclones, and ARs.

When it comes ARs' decay, it is nice to provide the IVKE framework, which I recognize as an innovation. However, it is well known in the AR community that ARs' decay is because of condensation (leading to precipitation, or heavy rainfall) and turbulence (destroy the coherence of AR). The case study of the AR event on 4 January 2023 shows a good example of the IVKE framework as a good diagnostic framework of its lifetime evolution, which I think it is quite useful in the AR community. - I do recognise its utility here.

Regarding ARs' movement/propagation, the authors find it is due to horizontal advection of VKE. In line 292 - 296, the authors state "Additionally, the advection of both vapor and KE contributes to ARs' eastward movement, indicating that both humidity and winds are important to ARs' evolution. This analysis framework complements earlier studies that focused on either ARs' humidity evolution or the role of extratropical cyclones in ARs' development, which puts AR research in a firmer footing in atmospheric dynamics." It is quite often I see the authors incorrectly summarize the previous findings in order to support their findings as novel and as not-have-been-done-before. The AR community, from the very beginning after Newell et al. (1992)'s pioneer work, recognizes the importance of both humidity and circulation (e.g. EC as mentioned by the authors). This is also the reason for selecting IVT as a better proxy than IWV to emphasize the steering, but at the same time keeping the humidity q . The literature cited by the authors to support their claim that the AR community argues either for humidity or circulation is misleading. These studies contribute to our overall understanding of ARs, with some providing deep investigations into large-scale circulations and others focusing on the presence and variability of humidity, often linking these factors to energy as well. In fact, there are quite some discussion of wind-dominant or humidity dominant ARs, so already AR community has evolved to the next stage rather than arguing which is more important.

Lastly, while I do not agree with many of the overstated claims of novelty, I do recognize the IVKE framework and its potential utility in future AR studies. I suggest submitting the manuscript to a traditional journal in the atmospheric science field, emphasizing the novelty of the technique and framework. The key findings should be used to support its utility. Although the current findings are not new to the AR community, I believe the IVKE framework has the potential to lead to new insights once it is widely adopted.

Reviewer #4

(Remarks to the Author)

I believe that the authors have made an honest review of the manuscript and responded to the reviewers frankly and objectively.

There is still much to be analyzed in this IVKE-based approach to atmospheric rivers, but I consider this article a good starting point a valuable contribution to better understand the physical phenomena involved in the formation and maintenance of ARs.

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Reviewers' comments:

Reviewer #1 (Remarks to the Author):

This manuscript discusses to the role of integrated vapor kinetic energy (IVKE) on atmospheric river lifecycle, which is novel in the sense that IVKE is not typically used. There is a bit of an identity crisis as a new technique for AR detection is introduced but not thoroughly evaluated or used for the analysis in the paper. The methodology would benefit from additional clarification and is atypical for the field. References to the vast body of literature published in the JGR special collection over the past couple years (many of which are on the topic of AR detection and lifecycle in reanalysis products) are absent ([https://agupubs.onlinelibrary.wiley.com/doi/toc/10.1002/\(ISSN\)2169-8996.ARTMIP](https://agupubs.onlinelibrary.wiley.com/doi/toc/10.1002/(ISSN)2169-8996.ARTMIP)). Finally, the figures are not publication quality and should be improved.

[We have carefully revised the paper according to the reviewer's suggestions. Please see our detailed responses below.](#)

Abstract: There typically are not references given in an abstract. Given that IVKE is a main component of the paper, it seems odd that it isn't mentioned in the abstract.

[We have revised the abstract and mentioned IVKE in Line 23.](#)

Lines 71-78: This section should be expanded. There are subtle differences between the detection with IVT and IVKE. Are these good or bad? Are the results with IVKE closer to the ARTMIP ensemble mean, median, or consensus? Are aspects of an AR getting picked up with one method but not the other? Why should one use IVKE for detection or should detection be completed with a different method and investigated using KE?

[This paper aims at developing a unified framework that can be used to detect ARs and understand their physics. For this purpose, we developed the concept of VKE \(see lines 64-68\). To address the reviewer's questions, we have carefully compared our detection results with ARTMIP ensemble median results. Additionally, we have compared the AR statistics from two detection algorithms, TempestExtremes and Mundhenk et al., using detection criteria based on both IVT and IVKE. IVKE and IVT results seem remarkably similar. We updated the paragraph with detailed comparison of the IVT and IVKE results \(lines 73-91\):](#)

["We first show the IVT- and IVKE-based detection results using the TempestExtremes algorithm²⁴ and the MERRA-2 data. The detection results are almost identical in terms of AR presence \(Video S1\) and frequency \(Figure 1a, b\). The frequency difference between IVT and IVKE is on the order of 1 day per year \(Figure 1c\). Both Video S1 and Figures 1a, b seamlessly juxtapose the panels of results using IVT and IVKE. The results match in most details, and the transition between the results is almost continuous; the detected ARs on the right of the left panel \(using IVT\) appear on the left of the right panel \(using IVKE\) as if the right panel was an extension of the left panel. Also,](#)

the AR frequency in Figure 1 is similar to the ensemble median of ARTMIP in terms of the spatial patterns and the maximum values¹⁷. We then use an independent detection algorithm developed by Mundhenk et al.²⁵ to further compare the IVT- and IVKE-based AR detection. The overall results remain similar: using IVKE can effectively capture AR events (Figure 1d-f). These results suggest that ARs are streams of air with enhanced IVKE, and the evolution of IVKE may explain the evolution of ARs.

Figure 1c, f provides an opportunity to compare AR detection results based on different algorithms. The frequency difference due to switching from TempestExtremes to the Mundhenk et al. algorithm is on the order of 10 days per years, which is similar to the ensemble interquartile range of ARTMIP¹⁷. Clearly, switching from IVT to IVKE introduces less AR frequency difference than switching between detection algorithms.”

Additionally, we added lines 268 to 273 for explanations on why IVKE budget would be more insightful than the traditional KE budget:

“One may think that analyzing the VKE budget is equivalent to analyzing the budgets of specific humidity and KE separately. However, that is not true. Due to stronger winds aloft, KE budget would emphasize much on the energetics in the upper troposphere, whereas lower-troposphere winds are most relevant to water vapor transport and thereby AR dynamics. VKE is the KE weighted by q^2 . It shows a bottom-heavy structure and allows us to focus on the dynamics associated with low-level jets that effectively transport water vapor.”

Figure 1: Please add titles to panels a and b, the variable name and units to the color bar, a legend to panel c, and axis labels to panel c. It appears as though there may be artifacts in panels a and b with vertical gray bars and splotches of white adjacent to blue. Double check this.

We updated Figure 1 following the suggestions.

Line 85/Supplementary Material: The derivation in the supplement is not easy to follow. Additional detail should be given. Note this is not my expertise so I will have to trust that the equations and derivation are correct.

We added thorough explanation of every single step including detailed explanation about applying the rules of Calculus (Supplementary Information pages 2 to 4).

-Line 90: Either go through the equation with what each term represents with the variables in the equation or add in another version of the equation in terms of the abbreviations given in the text for VKE, etc. Does “other” include the analysis increments? If so, this needs to be stated in the text.

We have added explanations of each term in the equation. Please see lines 104-115 and the Supplemental Materials.

For example, We clarified in lines 105 to 108 that “horizontal advection of VKE ($-\mathbf{u} \cdot \nabla_p(q^2K)$), vertical advection of VKE ($-\omega \frac{\partial q^2K}{\partial p}$), potential energy (PE) conversion to KE ($-q^2\mathbf{u} \cdot \nabla_p\Phi$), turbulent dissipation of KE ($q^2\mathbf{u} \cdot \mathbf{F}_T$, including surface flux of momentum), and condensation of vapor ($2KqS_M$; MERRA-2 combines subgrid-scale moist convection, condensation, and evaporation of condensate into one variable, S_M ²⁶).”

We also clarified in 113 to 115 that “The other terms are negligible in our analysis (See Equations S5-S10 and Table S1, where the analysis tendency introduced by the reanalysis²⁹ is included, and turbulence tendency of vapor includes surface flux of vapor²⁶).”

-Line 102: A sentence should be added here stating what the composite AR is. Why go through the process of detecting ARs with IVKE if it isn't used for the analysis? I am also on the fence about using this composite approach as opposed to a more traditional method for AR detection. How would this methodology impact the results?

We create composites to study the “averaged” or “statistical” behaviors of ARs, not just one single event. The composite method is based on AR detections, which can be done via the IVKE method or the IVT method. We have used the IVKE information for creating the composite AR. Additionally, composite results would be similar if we use IVT given that both IVKE and IVT provide similar detection results (Figure 1 and lines 73 to 91). To provide further clarification, we updated the paragraph with detailed explanation for the composite AR (lines 117-127):

“We make a composite AR and analyze its IVKE budget using MERRA-2 reanalysis data²⁹ from 2010 to 2019. Our composite AR is based on a widely used composite method to study extratropical cyclones³⁰, baroclinic waves^{31,32}, and convectively coupled circulations^{33,34}. This analysis focuses on an active AR region in the North Pacific, where the IVKE temporal variance maximizes among regions that do not have tropical cyclones (Figure S1). We first calculate the time series of areal mean IVKE in the $1^\circ \times 1^\circ$ box centered at $155^\circ\text{E } 35^\circ\text{N}$ and calculate its anomaly from the temporal mean. We then perform a regression analysis at each grid point: we regress anomalous fields of VKE, IVKE, and their tendency terms on the time series of IVKE anomaly that we just obtained. We multiply the regression coefficient of a given field at each grid point by one sample standard deviation of the IVKE time series to reconstruct the corresponding field of the composite AR. See the Methods Section C for more details about the composite.”

Also, in the Methods section (lines 358 to 368), we clarified that “We test the robustness of our composite results by switching between MERRA-2 and ERA5 data. Additionally, we excluded the time steps when AR is not detected in the $1^\circ \times 1^\circ$ box centered at $155^\circ\text{E } 35^\circ\text{N}$ using the TempestExtremes as described in Part B of Methods, and the results remain similar (Comparing Figures 2 to S13). All shown regressed fields are significant at 95% confidence level at each grid point with the two-tailed Student's t -test, where the degrees of freedom account for lag autocorrelation^{39,46}. The statistically insignificant data points are small valued, so we color them in white in the figures. We analyze IVKE budget of the composite AR by projection as described

in Part A of Methods. We test the robustness of our projection results by varying the boundary of our composite domain, and the results remain similar as long as the boundary does not cut off any composite AR signal (Comparing Figures 4ab to S14)."

Figures 2 and 3: These figures would greatly benefit from panel titles and a label on the color bar for IVKE. The captions should also mention that these are the fields regressed up area averaged IVKE in the selected box (assuming I am understanding what is being shown here).

We updated Figures 2 and 3 following the suggestions.

Figure 4: This figure should account for the analysis increment.

Please see Table S1. The analysis effect is accounted for, but its magnitude is too small to show any discernable signal in Figure 4.

Methods

-Line 230: How do the values computed using the tendency terms derived from the 3-hourly collections compare to the KE tendency terms available in `tavg1_2d_int_Nx`?

The KE tendency terms in `tavg1_2d_int_Nx` are dry KE tendencies, which is not useful to this study because of the following inequality:

$$\int_{p_B}^{p_T} q^2 K dp \neq q^2(p) \int_{p_B}^{p_T} K dp$$

What we need is on the left-hand side, and what `tavg1_2d_int_Nx` provides is the integral on the right-hand side.

-Lines 241-246: For reproducibility, it is best to include more detail here and give the IVT and IVKE thresholds and spatial requirements. Were the tropics excluded? Was the Laplacian of IVKE tuned so that it matches the results for IVT? Was only the Laplacian threshold replaced for IVKE? Would IVKE work for a different style ARDT like Mundhenk or GuanWaliser?

Thank you for the suggestions. We have performed new analyses using Mundhenk's algorithm, and the results remain robust. We have revised the text with more details. Please see lines 335 to 343.

-Line 253: What is "USA Wind" from? Is that a value given with IBTrACS?

Yes, it is. We revised the method as "We confirm that there is high AR frequency at the selected points (Figure 1) and no tropical cyclone track within 2° around the selected points from 2010 to 2019⁴⁵." (Lines 351 to 353)

-Line 255-256: It wouldn't hurt to show a plot of the time series in the supplemental. Is this an hourly timeseries?

We added an IVKE time series as Figure S12 (Three-hourly time series of IVKE in the $1^\circ \times 1^\circ$ box centered at 155°E 35°N in January 2019 from MERRA-2 data.) and cited it in Line 354.

-Line 258: Everything(!) is statistically significant? There is nothing masked out in the figures.

In Lines 363 to 364, we clarified that “The statistically insignificant data points are small valued, so we color them in white in the figures.”

-Line 262: What is this equation? Why is it not explained in more detail?

In Line 316 to 321, we clarified that “We denote any given term on the right-hand side of Equation (3) as X . Then following Anderson and Kuang³³’s method and notation: we estimate X ’s contribution to AR’s growth and drift by projecting X onto IVKE and total IVKE tendency ($\partial_t \text{IVKE}$):

$$\text{contribution to growth or decay} = \|X(\text{IVKE})\|/\|(\text{IVKE})^2\|, \text{ and} \quad (4a)$$

$$\text{contribution to drift} = \|X\partial_t \text{IVKE}\|/\|(\partial_t \text{IVKE})^2\|. \quad (4b)''$$

Reviewer #2 (Remarks to the Author):

Review of “One Stone, Two Birds: Using Vapor Kinetic Energy to Detect and Understand Atmospheric Rivers” by Ong and Yang

The authors proposed a vapor kinetic energy (VKE) framework to further understand the physical processes that govern the maintenance and propagation of ARs. For example, they found that the conversion term from potential energy to kinetic energy explains the AR’s maintenance the most, while horizontal VKE advection dominates the AR propagation. Overall, the article is well-written and interesting. The VKE framework is novel and can quantitatively resolve a fuller picture of the physical processes. It has the potential to diagnose large-scale weather phenomena like ARs. However, there are several major concerns over the analysis, such as the lack of a vigorous examination of the closure issue of the VKE framework to investigate the robustness of the framework in time and space. Also, the way that the authors computed the horizontal advection terms were not sufficiently justified. Also, there are rooms to improve the clarity of the analysis methods.

Major comments:

I believe the derivation of the integrated vapor kinetic energy (IVKE) prognostic equation (Eqs. S1-S7) is correct. The author also demonstrated that the terms related to S_T , S_C , F_m , and F_G are negligible (Table S1, Eq. 5). My major concern is about the turbulence-related momentum F_T term and the moisture source/sink term S_M . These two terms, to my knowledge, are rarely available from most reanalysis datasets, probably because they can only be derived from numerical simulations but not from observations. If this is correct, then the budget analyzed here may be subject to the potential biases of MERRA-2 in representing these processes. Hence, it would be useful if the authors could include each term's uncertainties in Table S1. Also, I suggest the authors apply their IVKE framework to other reanalysis datasets (if any) to validate the importance of the IVKE budget terms. Otherwise, I suggest the authors discuss more on the potential biases in MERRA-2 that may affect the results.

[We follow these suggestions and substantially revised our analyses. We estimate the uncertainty of MERRA-2 data \(e.g., lines 211 to 212 and Equations S8 to S10\). We further show that ERA5 reanalysis data yield similar results to MERRA2 \(e.g., lines 227 to 233 and Figure 4\).](#)

In the SI and Methods, the authors stressed that they “calculate HAKE by subtracting VAKE and PEKE from the IVKE tendency due to dynamics on KE.” And they computed HAV in a similar manner. But, in my humble opinion, this is not a common practice. HAKE and HAV should be easily computed right away. The authors’ implicit calculation of HAKE and HAV would unavoidably include potential residues due to non-closure problems (if any) into the budget term, which is not ideal. I suggest the authors compute these two horizontal advection terms directly and compare the difference in the results.

Thank you for the suggestions. We refined the analysis by explicitly calculating the horizontal advection terms. See Lines 304 to 306 and Figures 2, 3, 4. Additionally, the results seem robust in both MERRA-2 and ERA5 datasets.

Since this paper proposes a new IVKE framework to understand ARs, naturally, the generality of the framework should be vigorously tested beforehand. In this respect, for example, I suggest the authors closely examine the closure of the IVKE budget equation (i.e., the residual term) in different seasons/months and locations, searching for conditions when the framework may suffer from a non-negligible residue.

We further tested the framework over the North and South Atlantic, and “To test the robustness of our results, we make composite ARs in the AR active regions in the North Atlantic (Figure S5) and South Atlantic (Figure S6) and analyze their IVKE budget. The overall results remain similar. See Methods Section C for more details about robustness testing.” (Lines 165-168).

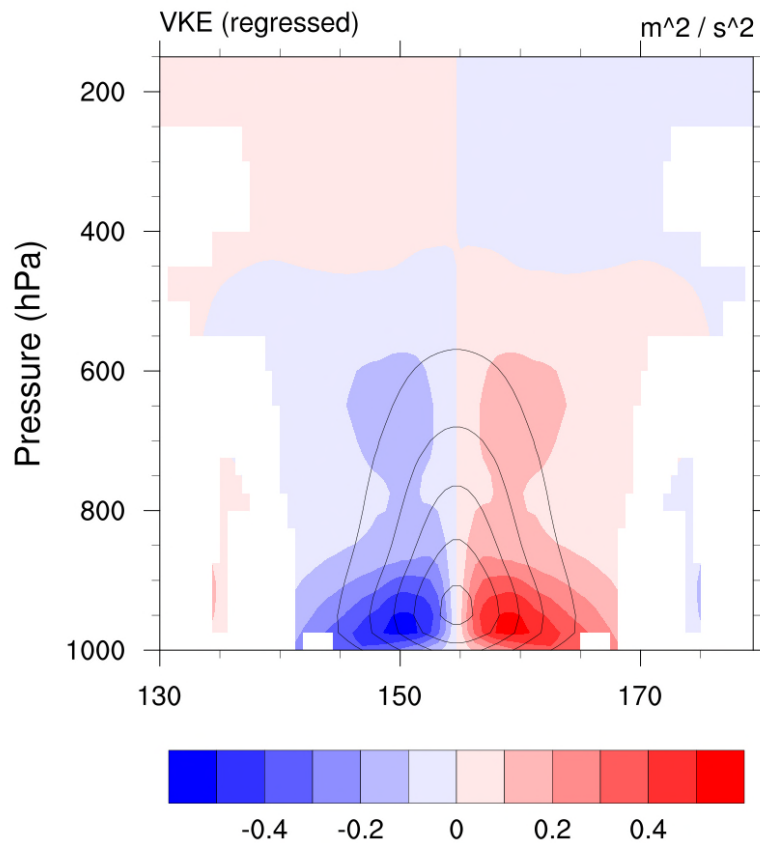
Additionally, we have used ERA5 data to perform the IVKE budget analyses, and the results are similar (Lines 227 to 233 and Figure 4).

To provide further details of individual AR events, we performed a case study. See Lines 235 to 255, Figures S10 and S11, and Video S3.

Figure 2: Although the authors indicated that the other terms in Eq. 3 are negligible, it would be helpful also to add a panel showing the sum of the remaining budget terms due to other physical processes to check if there is any spatial pattern.

We plotted it, and it is a blank page using our current color schemes. We clarified that “Other physical processes make negligible contributions to the IVKE budget and show no signal in plots using the color scheme in Figure 2.” (Lines 164 to 165)

Figure 3: I am a bit surprised that, given that ARs are mostly east-west elongated in the region (as shown in Figure 1), the cross-section shows a perfectly symmetric, upright, and localized maximum in the VKE profile at 167.5E, 37.5N. Can you explain why?



The above figure shows the symmetric part (contours, interval: $2 \times 10^{-3} \text{ m}^2 \text{ s}^{-2}$) and the asymmetric part (shades, units: $10^{-3} \text{ m}^2 \text{ s}^{-2}$) of the AR composite VKE anomalies (contours in Figure 3). The asymmetric part is on the order of 10% of the symmetric part. In other words, the cross-section is not perfectly symmetric.

Perhaps a more interesting question is whether our composite AR shows any asymmetry found in previous studies. The short answer is yes. We added a north-south vertical cross section and discussion in Lines 186 to 190 “Figure S8 shows a south-north vertical cross section through the center of the AR along 155°E . The AR’s anomalous air motion is poleward and upward (Figure S8). The VKE contours above 950 hPa resemble a hill where the poleward slope is steeper than the equatorward slope (Figure S8). Such motion and asymmetry are consistent with previous studies and are associated with the structure of the cold front^{1,4,19}.”

Figure 3 and other composites: I suggest the authors do a bit more diagnosis on environmental

fields (e.g., 3-D winds, geopotential height, specific humidity, etc.) to facilitate the interpretation the VKE budgets here. This also helps cross-check the main findings indicated by the VKE budget.

We have added new analyses, 3 new figures and a new video to address this comment. See lines 151 to 158 and 186 to 190, Figures S3, S4, and S8, and Video S2.

“We emphasize that the PE-to-KE conversion mainly arises from the anomalous winds that can have a significant ageostrophic component. For example, Figure S3a shows specific humidity, geopotential, and wind anomaly of the composite AR at 950 hPa, where VKE maximizes. There is a significant anomalous wind component that slides down the geopotential gradient, converting PE to KE and accelerating the low-level jet (Figure S3a). On the other hand, the mean winds mainly follow the geopotential contours and contribute less to the PE-to-KE conversion (Figure S3b). We see similar patterns throughout pressure levels from 975 to 850 hPa (Figure S4 and Video S2).” (Lines 151 to 158)

It would be interesting to further decompose the tendency terms into mean and anomalous parts. For example, PE-to-KE conversion without coupling to moisture can be decomposed as:

$$-\mathbf{u} \cdot \nabla_p \Phi = -(\bar{\mathbf{u}} + \tilde{\mathbf{u}} + \mathbf{u}^{\prime}) \cdot \nabla_p (\bar{\Phi} + \tilde{\Phi} + \Phi^{\prime}),$$

where the notations are defined as follows: overbar represents temporal mean; tilde represents AR-filtered signals; prime represents the rest (e.g., $\mathbf{u}^{\prime} = \mathbf{u} - \bar{\mathbf{u}} - \tilde{\mathbf{u}}$). We can then expand the equation:

$$\begin{aligned} -\mathbf{u} \cdot \nabla_p \Phi = & \quad -\bar{\mathbf{u}} \cdot \nabla_p \bar{\Phi} - \bar{\mathbf{u}} \cdot \nabla_p \tilde{\Phi} - \bar{\mathbf{u}} \cdot \nabla_p \Phi^{\prime} \\ & -\tilde{\mathbf{u}} \cdot \nabla_p \bar{\Phi} - \tilde{\mathbf{u}} \cdot \nabla_p \tilde{\Phi} - \tilde{\mathbf{u}} \cdot \nabla_p \Phi^{\prime} \\ & -\mathbf{u}^{\prime} \cdot \nabla_p \bar{\Phi} - \mathbf{u}^{\prime} \cdot \nabla_p \tilde{\Phi} - \mathbf{u}^{\prime} \cdot \nabla_p \Phi^{\prime}. \end{aligned}$$

Plugging each term into the definition of \tilde{A} , we find that the red terms become zero. However, we are left with six complicated terms, and we have not coupled the moisture yet. Such analysis would be worth another paper like Brenowitz et al. (2018, doi:10.1029/2018MS001335).

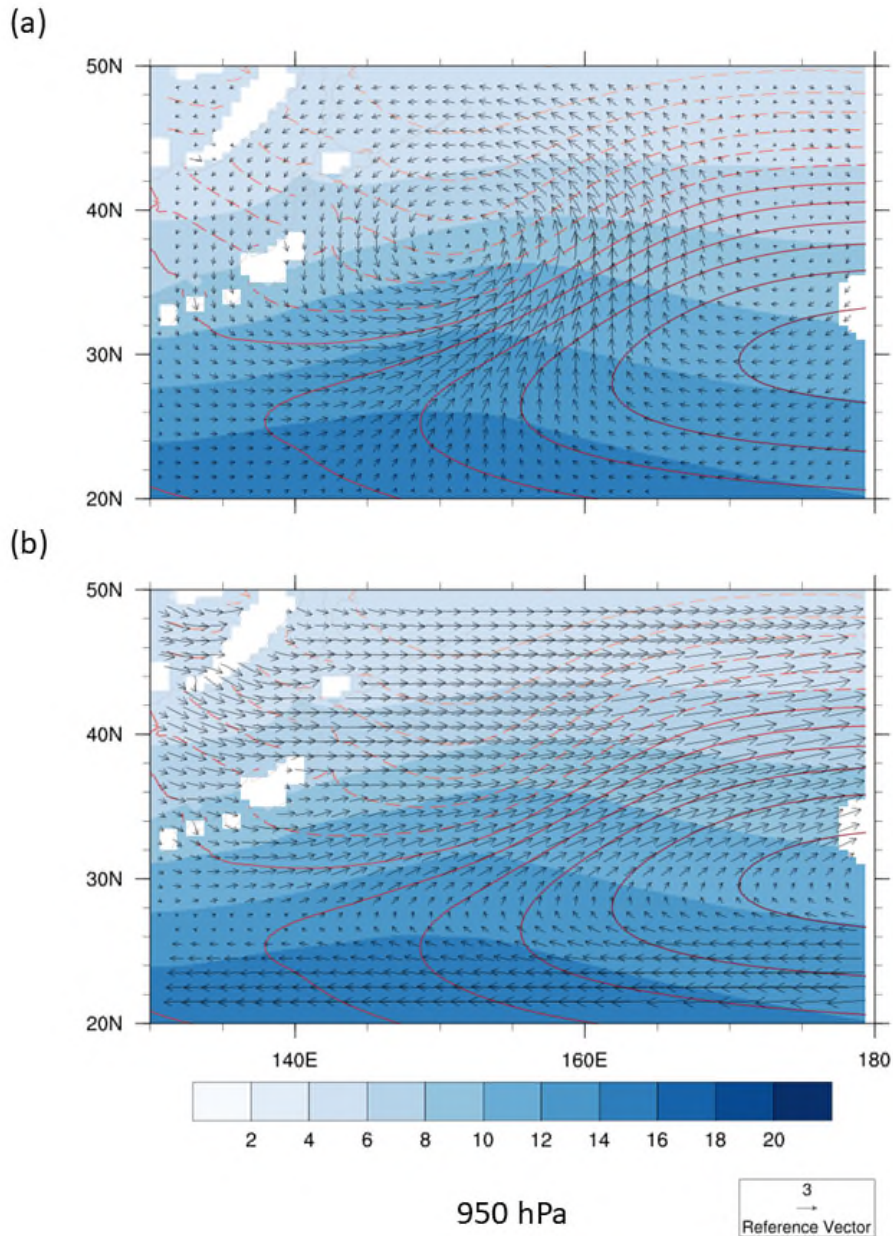


Figure S3. Total geopotential height (contours, interval: 10 m), total specific humidity (shading, units: g kg^{-1}), and (a) anomalous winds (arrows, units: m s^{-1}) (b) temporal mean winds at 950 hPa of the composite AR in Figure 2. Solid reddish contours are higher than the areal mean geopotential height in the plotting domain, and dashed yellowish contours are lower than that. The AR composite anomalies are calculated by regressing upon areal mean IVKE in the $1^\circ \times 1^\circ$ box centered at 155°E 35°N from MERRA-2 data from 2010 to 2019. The total geopotential height is calculated by adding the AR composite geopotential height anomaly and the temporal-mean geopotential height.

L160-170: I can understand the idea of performing regressions here. But it is hard to follow

what the authors meant by “the IVKE map” and “the map of total IVKE tendency.” These terms are not defined in their Methods section. This impedes an accurate understanding of what the contributions in Figure 4 really mean. Please define the terminology clearly in the Methods section.

We updated the paragraph with detailed explanation for the composite AR (lines 117-127):

“We make a composite AR and analyze its IVKE budget using MERRA-2 reanalysis data²⁹ from 2010 to 2019. Our composite AR is based on a widely used composite method to study extratropical cyclones³⁰, baroclinic waves^{31,32}, and convectively coupled circulations^{33,34}. This analysis focuses on an active AR region in the North Pacific, where the IVKE temporal variance maximizes among regions that do not have tropical cyclones (Figure S1). We first calculate the time series of areal mean IVKE in the $1^\circ \times 1^\circ$ box centered at 155°E 35°N and calculate its anomaly from the temporal mean. We then perform a regression analysis at each grid point: we regress anomalous fields of VKE, IVKE, and their tendency terms on the time series of IVKE anomaly that we just obtained. We multiply the regression coefficient of a given field at each grid point by one sample standard deviation of the IVKE time series to reconstruct the corresponding field of the composite AR. See the Methods Section C for more details about the composite.”

Also, in the Methods section (lines 345 to 358), we clarified that “We design an AR composite based on IVKE temporal variance independently from any AR detection method. Here we use data from 2010 to 2019. We sought the IVKE standard deviation map (Figure S1) for local maxima of IVKE variance without overlapping with tropical cyclone tracks⁴⁵. We select three spatial maxima of IVKE variance at 155°E 35°N (North Pacific), 60°W 40°N (North Atlantic), and 27.5°W 37.5°S (South Atlantic). The corresponding plotting and projecting regions are 130°E to 180° , 20°N to 50°N (North Pacific), 85°W to 35°W , 25°N to 55°N (North Atlantic), and 52.5°W to 2.5°W , 52.5°S to 22.5°S (South Atlantic). We confirm that there is high AR frequency at the selected points (Figure 1) and no tropical cyclone track within 2° around the selected points from 2010 to 2019⁴⁵. We take the time series of spatial averaged IVKE in the $1^\circ \times 1^\circ$ box centered at the selected points as an AR index (e.g., Figure S11). Also, we removed the temporal mean from the fields of the VKE, IVKE, and all the tendency terms as anomaly field. Then, we regress the anomaly at each grid point upon the AR index and multiply the regression coefficient at each grid point by one sample standard deviation of the AR index. This calculation yields the composite AR.”

Minor comments:

Figure 1: Suggest adding your selected box region to this figure.

We added the selected boxes in Figure S1.

L102-L104: Why only analyze the IVKE budget in 2019 instead of your research period (1980-2020). As shown in S1, there are also other AR hotspots in the North Pacific, e.g., the Asia

region and the eastern North Pacific region. Could you add more regions to conduct the composite to reflect the region-to-region difference in the AR evolution?

In the original version, Figure 1 utilized data spanning from 1980 to 2019, whereas the subsequent analyses were based solely on data from 2019. We have now extended the dataset for these analyses to cover the years 2010 to 2019. Additionally, we conducted an in-depth examination of the AR active regions in both the North and South Atlantic, yielding similar results, as detailed in lines 160 to 168 and illustrated in Figures S5 and S6.

To further test the robustness of the results, we also performed the analysis using an independent reanalysis dataset ERA5. See lines 227 to 233 and Figure 4.

L104-106: “We calculate the IVKE time series of this region” — do you mean you simply compute the regional IVKE? Or do you actually mean the IVKE of the ARs in the region? Later in L128, you mentioned “the AR’s IVKE anomaly” — how did you compute such an “anomaly”? Please clarify.

We have revised the text to clarify this comment. See lines 117 to 127 and 345 to 358. Also see our previous response to comment on L160-170.

Here we provide brief answers: 1) yes, we calculated the time series of the areal/regional mean within $1^\circ \times 1^\circ$ box centered at the selected points at $155^\circ\text{E } 35^\circ\text{N}$ (North Pacific), $60^\circ\text{W } 40^\circ\text{N}$ (North Atlantic), and $27.5^\circ\text{W } 37.5^\circ\text{S}$ (South Atlantic); 2) we calculated the anomaly of IVKE from the temporal mean of the IVKE time series that we just obtained; 3) we have performed composite analyses with various parameter values to test the robustness of the results, such as including vs. excluding non-AR days in the regression analysis.

L128-L130: There are also negative values in the Figure 2c. Does it mean that the PE conversion to KE term only intensify and maintain the central AR region and weaken the surrounding AR regions?

We clarified that “IVKE tendency that lies outside of the AR’s IVKE anomaly does not project onto the AR’s IVKE anomaly and, thus, cannot affect the AR’s intensity.” (Lines 148-150)

L138-L139: Similar with the horizontal dimension of the composite AR, there are also negative values in the Figure 3c. Besides, are the results here sensitive to 37.5°N you choose? Maybe a 3D composite figure of the terms in the IVKE budget could make the results more intuitive and comprehensive to understand.

Thanks for the reviewer’s comments. We have made new plots to show why there are negative PE-KE conversion (Figures S3 and S4 and Video S2), and we hypothesize that the negative PE-KE conversion might be related to the anti-cyclone ahead of the AR (Guo et al. 2020, #2 of our

reference). Additionally, we have tested the results at different geographic locations, and the results seem robust. See Figures S5 and S6, and lines 165 to 168.

L160-L170: This paragraph just repeats the conclusion mentioned earlier and doesn't give new insights or conclusions. It's better merge this paragraph into the previous parts.

We removed the repeated discussions and added comparison between MERRA-2 and ERA5 budget analyses to this paragraph (Lines 227 to 233):

L207-L215: Apart from the questions you raise here, I hope to know about the main roles of different terms in ARs' lifecycles, e.g., the generation, intensification and weakening and decay.

We have modified the discussion section to accommodate the interesting questions that the reviewer suggested. See lines 284 to 287. Additionally, we have added analysis of a case study. See lines 235 to 255 and Figures S10 and S11.

L241-248: In the TempestExtremes algorithm, both the northward vapor transport and the eastward vapor transport are taken to investigate AR and non-AR poleward moisture transport. Since the VKE combine them together, how could you detect the ARs with the IVT equation, or do you neglect this procedure in this manuscript? Besides, you manually select the IVKE threshold according to the result. A more objective method should consider the corresponding quantiles of the threshold in IVT and IVKE.

To the best of our knowledge, the TempestExtremes 2.2 algorithm does not consider the northward and eastward vapor transport separately in the process of detecting ARs. Please see Ullrich et al. (2021, Ref#27), McClenny et al. (2020, Ref#44), and Rhoades et al. (2020, Ref#45).

To the best of our knowledge, Ullrich et al. (2021, Ref#27), McClenny et al. (2020, Ref#44), and Rhoades et al. (2020, Ref#45) chose an arbitrary threshold of IVT. Following their approach, we use Laplacian of IVT $< -40,000 \text{ kg m}^{-1} \text{ s}^{-1} \text{ rad}^{-2}$ as the threshold.

L252: Please clarify the extent of the area you masked around each TC track point.

We are not doing masking of TC tracks any more. Instead, " We confirm that there is high AR frequency at the selected points (Figure 1) and no tropical cyclone track within 2° around the selected points from 2010 to 2019⁴⁵." (Lines 351 to 353)

Reviewer #3 (Remarks to the Author):

Review of NCOMMS-22-50129-T

Overview

I appreciate the authors' efforts to develop a novel framework for studying the dynamics of atmospheric rivers (ARs). The "vapor kinetic energy" (VKE) framework enables diagnosis of processes associated with the co-evolution of kinetic and water vapor fields, and thus provides insight into the governing dynamics of ARs. The paper reads fairly well; the text is concise and clear, and figures provide clear illustrations of the main findings of the paper. Praises aside, I have some major concerns regarding the novelty and justification for this study, as well as the composite methods used to analyze the VKE budget for ARs. I therefore recommend that the manuscript undergo major revisions.

Comments

1. In my opinion, the authors need to provide some additional discussion regarding the novelty of this VKE framework and its advantages over the traditional kinetic energy and moisture budget framework. What new insights does the VKE budget provide that cannot be gleaned from considering the kinetic energy and moisture budgets? A main finding is that the VKE growth is due mainly to the conversion of potential energy to kinetic energy and, to a lesser extent, vertical advection of water vapor [which is of course related to moisture convergence; see Norris et al. (2020, section 5)]. These results are not surprising; those two processes are known to be sources of kinetic energy generation in cyclones and increases in moisture in ARs, respectively.

Thank you very much for the helpful suggestions. We have added discussions to emphasize the novelty or advantages over the KE and moisture frameworks. Please see Lines 269 to 274: "One may think that analyzing the VKE budget is equivalent to analyzing the budgets of specific humidity and KE separately. However, that is not true. Due to stronger winds aloft, KE budget would emphasize much on the energetics in the upper troposphere, whereas lower-troposphere winds are most relevant to water vapor transport and thereby AR dynamics. VKE is the KE weighted by q^2 . It shows a bottom-heavy structure and allows us to focus on the dynamics associated with low-level jets that effectively transport water vapor."

Additionally, we have added text to emphasize the link between ARs and extratropical cyclones. Please see lines 32 to 33, 59 to 62, and 264 to 266. For example, "ARs often form with the development of extratropical cyclones and the associated cold front¹⁻³." (Lines 32 to 33)

We added in Lines 59 to 62 that “However, since the development of ARs is closely tied to the life cycle of baroclinic waves and extratropical cyclones¹⁻³, and their impact is largely attributed to intense winds, it is imperative to establish an analysis framework that organically incorporates both wind and humidity elements.”

We retain the citation to Norris et al. (2020) in the previous version. However, we add a caveat here on the relation between vertical vapor advection and horizontal vapor flux convergence. In our vertically integrated VKE framework, vertical VKE advection $[-\frac{1}{g} \int_{p_B}^{p_T} (-\omega \frac{\partial q^2 K}{\partial p}) dp]$ is equal to a horizontal convergence term $[-\frac{1}{g} \int_{p_B}^{p_T} (-q^2 K \nabla \cdot \mathbf{u}) dp]$. First, use the product rule:

$$-\frac{1}{g} \int_{p_B}^{p_T} (-\omega \frac{\partial q^2 K}{\partial p}) dp = -\frac{1}{g} \int_{p_B}^{p_T} (q^2 K \frac{\partial \omega}{\partial p}) dp - \frac{1}{g} \int_{p_B}^{p_T} (-\frac{\partial q^2 K \omega}{\partial p}) dp$$

Then, use the continuity equation:

$$-\frac{1}{g} \int_{p_B}^{p_T} (-\omega \frac{\partial q^2 K}{\partial p}) dp = -\frac{1}{g} \int_{p_B}^{p_T} (-q^2 K \nabla \cdot \mathbf{u}) dp - \frac{1}{g} \int_{p_B}^{p_T} (-\frac{\partial q^2 K \omega}{\partial p}) dp$$

Next, solve for the second integral on the right-hand side:

$$-\frac{1}{g} \int_{p_B}^{p_T} (-\omega \frac{\partial q^2 K}{\partial p}) dp = -\frac{1}{g} \int_{p_B}^{p_T} (-q^2 K \nabla \cdot \mathbf{u}) dp + \frac{1}{g} [q^2 K \omega]_{p_T} - \frac{1}{g} [q^2 K \omega]_{p_B}$$

Given no VKE flux through the top and bottom boundaries ($[q^2 K \omega]_{p_T} = [q^2 K \omega]_{p_B} = 0$), we get:

$$-\frac{1}{g} \int_{p_B}^{p_T} (-\omega \frac{\partial q^2 K}{\partial p}) dp = -\frac{1}{g} \int_{p_B}^{p_T} (-q^2 K \nabla \cdot \mathbf{u}) dp$$

In the vertically integrated vapor framework like Norris et al. (2020), similar relation applies; simply replace $q^2 K$ with q . However, in our VKE framework, vertical vapor advection is coupled with KE ($-2Kq\omega \frac{\partial q}{\partial p}$) and yields complicated integrals that do not reduce to flux through the boundaries like the above derivation.

Given the above complication, we use the term “vertical VKE advection” or “vertical advection of VKE” in our paper and avoid relating it to horizontal convergence.

2. In my opinion, this regression-based composite approach based on the time series of IVKE a geographically fixed box is not appropriate for the IVKE budget analysis of ARs. The regression analysis indicates the relationship between IVKE for one particular $1^\circ \times 1^\circ$ box and the different terms in the IVKE budget equation. Because the regression is performed for the entire time series of IVKE, it, of course, includes a number of times in which no AR or only part of an AR is actually present over this region. So, I find it misleading to refer to the structure appearing in the regression-based maps as the “composite AR.”

Thank you for the suggestions. Our composite analysis is based on a widely used method. We have revised the text and added analyses to address the comments.

We clarified in lines 118 to 119 that “Our composite AR is based on a widely used composite method to study extratropical cyclones³⁰, baroclinic waves^{31,32}, and convectively coupled circulations^{33,34}.” Lim and Wallace (Ref. # 32) gave detailed explanations on why the results from this method can be viewed as composites.

We added that “Additionally, we excluded the time steps when AR is not detected in the $1^{\circ} \times 1^{\circ}$ box centered at 155°E 35°N using the TempestExtremes as described in Part B of Methods, and the results remain similar (Comparing Figures 2 to S13).” (Lines 359 to 361)

Additionally, I am concerned that the fixed box ($25\text{--}50^{\circ}\text{N}$; $147.5^{\circ}\text{E}\text{--}172.5^{\circ}\text{W}$) used for computing the budget terms in Fig. 4 is quite large, encompassing a region much larger than the spatial scale of an AR. Thus, the results of the budget calculations in Fig. 4 may not accurately represent the contributions of the various terms to ARs.

We have performed sensitivity test to see if the domain size affects our analysis results. We clarified that “We test the robustness of our projection results by varying the boundary of our composite domain, and the results remain similar as long as the boundary does not cut off any composite AR signal (Comparing Figures 4ab to S14).” (Line 365 to 368). We updated the paragraph with detailed explanation for the composite AR (lines 117-127 and 345 to 358).

The composite analysis should really be done in a quasi-Lagrangian or AR-following composite framework using the ARs that were detected using the Tempest Extremes algorithm. This approach would, in my view, be far more suitable for examining the dynamics associated with the evolution of ARs. It could be particularly enlightening to perform the analysis at different stages of the AR life cycle (e.g., formation stage, time of maximum intensity, and lysis stage).

Thank you for your suggestions. We have supplemented a case study and performed the IVKE analysis at different stages of the AR lifecycle. See Lines 235 to 255, Figures S10 and S11, and Video S3.

Additionally, we have clarified in lines 118 to 119 that “Our composite AR is based on a widely used composite method to study extratropical cyclones³⁰, baroclinic waves^{31,32}, and convectively coupled circulations^{33,34}.” These studies have shown insightful results based on the regression-based composite methods.

3. Lines 34–37: This introductory discussion fails to mention that ARs are, in essence, components of baroclinic waves/extratropical cyclones. I recommend including a couple of sentences regarding how ARs form in the context of the life cycles of baroclinic waves/cyclones. This is critical contextual information for the analysis that appears later in the paper.

We have revised the introduction to include the suggested information (Lines 32 to 33 and 59 to 62).

4. Lines 40–42: This statement is inaccurate and misleading. There is a large body of research on the structure and dynamics of extratropical cyclones and the embedded low-level jets; this relevant research should be mentioned in the text. Also, my understanding is that the eastward movement of ARs is largely the result of the eastward movement of the parent baroclinic wave system and its embedded low-level jet. Because an AR is not a wave, it is not accurate to say that it propagates.

We have included a discussion on the dynamics of baroclinic waves and their connection to atmospheric rivers (ARs), as well as extratropical cyclones, which is detailed in lines 32 to 33 and 59 to 62. Additionally, we have incorporated several references to support this discussion: Guo et al. (2020), Zhang & Zheng (2019), Orlanski & Sheldon (1995), and Papritz & Schemm (2013). Furthermore, we have substituted the term "propagate" with "drift" consistently throughout the text.

5. Line 93: Note that the PE to KE conversion term is the only term in the equation that can actually generate KE following the flow. This term can be further decomposed into a geopotential flux divergence term, which corresponds to the redistribution of kinetic energy, and a baroclinic conversion of potential energy to kinetic energy (see, e.g., Orlanski and Sheldon 1995; Papritz and Schemm 2013). It would be really interesting to see how these different terms contribute to the PE–KE conversion. I recommend that the authors include this decomposition to provide further insight into the processes associated with the evolution of VKE.

Thank you for the suggestion. Please see Supplementary Text (Page 5) and Figure S15.

6. Line 250: Please explain the reasoning for only using data for 2019. Inclusion of more years of data would, of course, yield more robust results.

We expanded the time range to 2010–2019 to yield more robust results.

References

Norris, J. R., and Coauthors, 2020: The observed water vapor budget in an atmospheric river over the Northeast Pacific. *J. Hydrometeor.*, 21, 2655–2673, doi:10.1175/JHM-D-20-0048.1.

Orlanski, I., and J. P. Sheldon, 1995: Stages in the energetics of baroclinic systems. *Tellus A*, 47, 605–628, doi:10.1034/j.1600-0870.1995.00108.x.

Papritz, L., and S. Schemm, 2013: Development of an idealised downstream cyclone: Eulerian and Lagrangian perspective on the kinetic energy. *Tellus A*, 65, 19539, doi:10.3402/tellusa.v65i0.19539.

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The authors did a great job with responding to the reviewer comments. I have a couple other comments however they are mostly wording related.

Line 31: Can “running” be modified?

We have deleted the word.

Line 33: transports more what? Add “water” here.

Done.

Lines 42-43: What eastward drift? This topic hasn't been introduced and is not cited.

We have revised the text and now mention it earlier. See lines 19 and 35.

Figure 1: Are panels a and d the ARTMIP generated data? If so, the doi should be cited.

We generated them using MERRA2 data.

Line 100: Typo (should be “changes”)

Corrected.

Line 118: Why limit the composite to begin in 2010?

We have added clarification: “We make a composite AR and analyze its IVKE budget using MERRA-2 reanalysis data from 2010 to 2019, a 10-year period sufficient to produce statistically significant results.” (lines 141)

Line 181: Consider replacing “bottom” with “surface” or “1000 hPa”

Done.

Line 299: 2020 or 2019?

Should be 2019, We have corrected the text.

Line 301: The staggering is intentional. The (3 hourly averaged) tendency is the time rate of change between two instantaneous time steps.

Clarified. See lines 480 – 482.

Line 343: The figure caption says 1980 to 2019.

This is correct. The analysis detection data is from 1980 to 2019. The composite and budget analyses require 3D data for multiple variables. For the limited storage we have, we only conducted the composite and budget analyses from 2010 to 2019, a 10-year period sufficient to produce statistically significant results. The case study was performed for a historic case in January 2023.

Reviewer #2 (Remarks to the Author):

After carefully reviewing the manuscript titled "Using Vapor Kinetic Energy to Detect and Understand Atmospheric Rivers" by Hing Ong and Da Yang, I cannot recommend this manuscript for publication in Nature Communications. Below, I outline several critical reasons for this decision.

We appreciate the time you have taken to review the paper. While we respectfully disagree with some of the points you raised, we have carefully considered all your comments and have made significant revisions to address your concerns. Below, we outline our responses and the changes made to the manuscript in light of your feedback.

1. Lack of Novelty

While the concept of using Vapor Kinetic Energy (VKE) to study atmospheric rivers (ARs) is intriguing, the manuscript does not convincingly demonstrate that this approach offers significant new insights or improvements over existing methods. The IVKE-based detection results are shown to be almost identical to those obtained using the traditional Integrated Vapor Transport (IVT) framework. This redundancy raises questions about the added value of the proposed methodology.

Ref: <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022GL102091>

We have added a new table (Table 1) to highlight our novel contributions. This paper introduces a unified framework for tracking and *understanding* ARs based on vapor kinetic energy (VKE). As Reviewer 2 noted, IVKE can effectively track AR events. Additionally, we have developed a governing equation for IVKE that enhances our understanding of AR physics. Table 1 facilitates the comparison between the widely used IVT framework and our IVKE framework. The IVKE framework addresses fundamental questions about the development and movement of ARs.

Table 1. Comparison between the IVT and IVKE frameworks. Both frameworks can effectively track ARs. The IVKE framework introduces a governing equation based on the conservation of energy and moisture, allowing quantitative diagnoses of the development and movement of ARs.

| | | IVT | IVKE |
|-------------------------------------|--|-----|---|
| Tracking ARs using winds & moisture | | Yes | Yes |
| Understanding | Governing equation | NA | Equation (3): $\partial_t VKE = -VKE \text{ advection} + PE \text{ conversion to KE} - KE \text{ dissipation} - \text{condensation.}$ |
| | What processes lead to the development of ARs? | NA | Equation (10a) & Figs. 2, 3, 4, 5: Growth: PE conversion to KE and vertical advection of water vapor; Decay: Condensation of vapor and turbulent dissipation of KE. |
| | What processes lead to the movement of ARs? | NA | Equation (10b) & Figs. 2, 3, 4: Horizontal advection of KE and water vapor leads to the eastward movement of ARs. |

2. Insufficient Justification for Methodology

The authors introduce VKE as a novel metric for AR detection and analysis, yet the manuscript lacks a comprehensive justification for why this metric is necessary. The claim that IVKE captures the physics of ARs' evolution more effectively than IVT is not substantiated with compelling evidence or detailed comparison across a sufficiently varied set of case studies.

We have added Table 1 to compare the IVT and IVKE frameworks. It is evident that both frameworks can track AR events, but only the IVKE framework includes a governing equation that can diagnose the processes leading to the development and movement of ARs. This conclusion is supported by comprehensive composite and budget analyses using two different datasets (ERA and MERRA), across different geographic locations, and a case study of a historic AR event. These results convincingly demonstrate that the IVKE framework significantly enhances our understanding of the development and propagation of ARs.

Although the authors compare their findings to the median of ARTMIP, but the uncertainties uncovered by the variety of ARTMIP participating algorithms is another important merit that this manuscript missed. I appreciate that the authors also compared to another independent algorithm in ARTMIP: Mundhenk et al. (2016), however, as mentioned above, the uncertainties uncovered by the various algorithms is the merit of ARTMIP, there are other algorithms. A similar global map of AR between this method and Mundhenk et al. (2016), is not convincing. A recent study on global future ARs using another ARTMIP is just published in JGRAtmos (Zhang et al. 2024), their algorithm also claim to be able to investigate the physical process (Pan and Lu, 2019).

Ref: Zhang et al. <https://doi.org/10.1029/2023JD039359>

Original algorithm of Zhang et al. also in ARTMIP: <https://doi.org/10.1029/2018WR024407>

We have carefully reviewed the papers mentioned by the reviewer. To the best of our knowledge, these studies are based on the IVT framework. Their equations focus on defining IVT (Equations 1-3 in Zhang et al., Equations 1-4 in Pan and Lu). Consequently, they do not provide a governing equation capable of quantitatively diagnosing the development and movement of ARs, which is not the primary focus of these papers.

Additionally, we have included new text comparing IVKE-based and IVT-based detection results (lines 96-103). However, a detailed intercomparison between different tracking algorithms is beyond the scope of the current paper and should be conducted within ARTMIP, a multi-institution project.

3. Limited Theoretical Development

The theoretical development of the IVKE framework and the associated budget equation is not sufficiently detailed. The manuscript references supplementary materials for key equations (S1-S7), but these should be integrated into the main text to ensure the reader can fully understand and evaluate the proposed framework without needing to refer to additional documents.

Although the heading of this comment suggests a "limited development," the detailed feedback recommends relocating Equations S1–S7 to the main text. We have implemented this suggestion and moved the equations accordingly. Please see Section A in Methods. I trust this modification fully addresses your Comment #3.

4. Overstated Claims

The manuscript makes several broad claims about the advantages of the VKE framework, such as its ability to explain the evolution of ARs better than existing methods. However, these claims are not supported by robust statistical analyses or a diverse range of empirical validations. The examples provided are limited, and the comparison between IVT and IVKE detection appears superficial.

To address this comment, we have added Table 1 to the main text. This table highlights the novelty of our approach and demonstrates that the IVKE framework helps answer fundamental questions about the development and movement of ARs.

Additionally, we have validated our results through a suite of comprehensive analyses using two different datasets (ERA and MERRA) (e.g., Figs. 1, 2, 3, 4), across various geographic locations (e.g., Figs. S5, S6, S12), and with a case study of a historic AR event (Fig. 5; Movie 3). The conclusions are robust across all these examples: PE conversion to KE and vertical advection of water vapor are primarily responsible for AR development, while horizontal advection of KE and water vapor drives the eastward movement of ARs.

5. Incomplete Analysis and Presentation

The manuscript presents some results from the TempestExtremes and Mundhenk et al. algorithms but fails to provide a thorough analysis of the differences between these algorithms and the implications for AR detection. Additionally, the visualizations (Figure 1) are not sufficiently clear or informative to support the authors' conclusions.

We have included new text comparing the two IVT-based detection results (lines 96-103). Additionally, Figure 1 includes both a map view and time-mean, zonal mean statistics, which are sufficient for our purposes. However,

further intercomparison of AR detection algorithms should be conducted via the ARTMIP project and is beyond the scope of the current paper, which focuses on improving the process-level understanding of ARs.

6. Ambiguous Results and Conclusions

The results section contains ambiguities regarding the practical implications of adopting the IVKE framework. The manuscript does not convincingly demonstrate that using IVKE leads to significant improvements in AR detection or understanding of their physical processes. The conclusions drawn are therefore speculative and not well-supported by the presented data.

We have included a new table (Table 1) that clearly compares the IVT and IVKE frameworks, highlighting our novel contributions.

The conclusions are supported by thorough composite and budget analyses using two different datasets (ERA and MERRA) (e.g., Figs. 1, 2, 3, 4), covering different geographic locations (e.g., Figs. S5, S6, S12), and a case study of a historic AR event (Fig. 5; Movie 3).

The results convincingly demonstrate that the IVKE framework significantly improves our understanding of the development and propagation of ARs.

7. Lack of Broader Context

The manuscript could benefit from a more comprehensive discussion of how the proposed VKE framework fits into the broader context of atmospheric science. It lacks a critical comparison with other recent advancements in the field and does not adequately address the potential limitations or challenges associated with the new approach.

We have added Table 1, which critically compares the IVKE framework with the IVT framework. In the current version, we discuss our research in the context of other AR studies, highlighting the knowledge gaps (e.g., lines 41-45, 47-67). We also address the potential limitations of the current methods and outline future research directions, some of which are inspired by the referenced studies (e.g., lines 156-163, 405-425).

Reviewer #3 (Remarks to the Author):

The authors have made numerous revisions that have improved the manuscript. I have a number of comments on the revised manuscript for the authors to consider. Once these comments are satisfactorily addressed, I believe the manuscript may be acceptable for publication in Nature Communications.

Comments:

There is a large amount of supplementary material appended to the manuscript. The main discussion refers to supplementary text and figures frequently. I am okay with this; however, I propose that some of the material might be suitable for inclusion in the manuscript. For instance, it could be worthwhile to include Figs. S10 and S11 for the Jan 2023 case study in the manuscript, perhaps as a single 4-panel figure. Also, it might be appropriate to include the derivation of the IVKE budget in the Methods section.

We have revised the paper according to the suggestion. See Fig. 5 for the combined Figs. S10 and S11, and Equations 3 – 9 in Section A in Methods.

Line 60–61: "their impact is largely attributed to intense winds" I am confused by this statement. How have you come to the conclusion that the impact of ARs is largely attributed to intense winds? By "impact" do you mean precipitation amounts? It is my understanding that the impact of ARs in terms of precipitation is attributed to winds, moisture transport, and, of course, vertical motion.

We have revised the sentence and added a reference (lines 65-66). Here we meant to say that AR events can also cause wind extremes, in addition to precipitation extremes, such as described in Waliser and Guan (2017).

Line 117: In my view, it is not entirely accurate to refer to the regression maps as a "composite AR" given that you are performing a regression analysis on the IVKE time series and are not actually generating composites for a sample of detected ARs.

Thank you for the comment. We have clearly acknowledged the difference between our regression-based analysis and AR-following composite. See lines 140, 151, 156 – 163.

Line 118–119: The fact that similar methods have been used previously for studying other phenomena is not in itself sufficient justification for use of this regression method here. In my opinion, some discussion is needed to explain the advantages for using this regression method versus generating composites in AR-relative coordinates for samples of ARs that were detected and tracked using Tempest Extremes. It would also, in my view, be helpful to discuss caveats when using the regression method to examine processes governing ARs. One could generate a more generalized AR-relative composite based on a large sample of ARs detected/tracked over the North Pacific. This approach would also enable investigation of different stages of AR lifecycles (as you mention in line 286).

Thank you for the suggestion. We have added a paragraph discussing the two composite methods. This suggestion also highlights an interesting future research direction. Please see lines 156-163 and lines 422-425 for the updated discussion.

Line 122: There is seasonal variability in the dynamics and frequency of ARs over the North Pacific. As far as I can tell, the regression analysis focuses on the entire time series of VKE. How would the results change if the budget analysis were done for individual seasons? Would the relative strength of the terms differ between winter and summer? Consider discussing this issue in the text (perhaps in the part where you discuss what to do next).

We have added discussions on the seasonality of ARs in the context of future work. Please see lines 405 – 408.

Line 132: Here and elsewhere, "move"/"movement" would be better than "drift".

Done.

Line 151–153: The supplementary text regarding PE-to-KE conversion and Fig. S15 should be cited here.

Done. Please see [lines 209 – 210](#).

Line 163: Here and elsewhere, it might be more accurate to say that the processes producing negative VKE tendencies over the AR core result in reductions of VKE. It seems misleading to say that these negative tendencies are resulting in the decay of the AR. The regression analysis does not enable diagnosis of the decay of ARs.

Thank you for the suggestion. We have reworded the text accordingly. Please see [lines 215 – 216, 237 - 238](#).

Line 185: I suggest describing the vertical distribution of specific humidity and KE in a different, more precise manner. The terms bottom-heavy and top-heavy are ambiguous. Perhaps say "specific humidity is maximized at lower levels, while KE is maximized at upper-levels" or something to that effect.

We have revised the text using the suggested description. Please see [lines 244 – 245](#).

Line 262: The association between ARs and low-level jets is already well-known, having been documented in a number of prior observational studies. Consider changing "suggest" to "confirm".

Thank you for the suggestion. We have revised the text accordingly. Please see [lines 359 – 361](#).

Minor text corrections:

Line 31: "pushed along" → "transported"

Done.

Line 33: "transports more water than"

Done.

Line 55: "primary source of water vapor in ARs"

Done.

Line 100: "changes"

Done.

Line 155: "slides down" → "is directed across"

Done.

Line 161: "rich" → "large"

Done.

Line 164–165: "show no signal in plots using the color scheme" → "with no values above the minimum contour level"

Done.

Line 179: "Both processes show bottom-heavy structures." → "The tendencies for both processes are concentrated between 1000 and 800 hPa."

Done.

Line 189: Insert "findings of" before "previous"

Done.

Line 221: "cutting" → "doing so cuts off"

Done.

Line 223: Insert "off" after "cuts"

Done.

Line 281: Insert "weather" after "extreme"

Done.

Line 281: "landfalls" → "landfall"

Done.