

Author's response for egusphere-2025-1731

The interaction of warm conveyor belt outflows with the upper-level waveguide: a four-type climatological classification

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The following section presents the reviewers' comments in **blue** and our responses in **black**.

Reviewer 1

First of all, I thank the authors for their appreciative style in replying to my comments. Furthermore, I appreciate that the authors have responded in a constructive and thoughtful manner, which clarifies the authors' perspectives (for me, as a reviewer, but more importantly also in the manuscript for future readers).

To me personally, it is still conceptually more appealing to think of the overall problem in terms of a disturbance of the "waveguide" by the WCB's divergent outflow and then, in a second step, of how this disturbance subsequently evolves, rather than combining the "divergent forcing" and the subsequent (nonlinear) evolution of associated PV anomalies into the metric under consideration. Of course, the authors are free to take their "combined" perspective. The authors have clarified their perspective in the revised version, and I am happy to recommend the manuscript for publication after consideration of the few minor comments below.

Kind regards

We sincerely thank the reviewer for the positive and encouraging feedback on our revised manuscript and response letter. We truly appreciate your engagement and invaluable suggestions that improved our work.

We acknowledge the reviewer's point that a two-step framing, divergent forcing followed by nonlinear PV evolution, may provide a more intuitive representation of the problem. While in our study, we deliberately adopted a combined perspective to capture both the direct forcing and the subsequent evolution within a unified framework, we agree that the reviewer's framing is equally valid and insightful. We are grateful that the revised manuscript now meets the reviewer's expectations and appreciate the recommendation for publication. Below, we address the remaining minor comments in detail.

Minor comments:

"interaction intensity & interaction types": In the revised version, these terms are now more explicitly defined and better motivated. I have no issues with the authors' definition. To me, however, the terms "interaction intensity" and "interaction types" reflect their definitions poorly at best; in the worst case the terminology is misleading and confusing to future readers. I thus recommend that the authors re-consider this terminology to further improve their manuscript.

We thank the reviewer for this thoughtful comment. We acknowledge that terminology in this context can be challenging, as any choice may have limitations in fully capturing the underlying concepts and being clear and intuitive for the future readers. However, after careful consideration (we discussed terminology a lot during our study!), we prefer to retain the terms “interaction intensity” and “interaction types”, as they provide a consistent framework throughout the manuscript. We have taken care to define and explain these terms in detail where they first appear in the paper to avoid ambiguity, and we believe that the additional explanations we added in the revised version will enable readers to understand the intended meaning.

In response to my previous comment on the concept of age of outflow, the authors have revised their manuscript (L369-L372). In this revision, the language used implies causality, i.e., how the WBC influence the larger-scale flow. Please revise this revision according to your reply and revisions made in response to my first general comment of the previous review.

Thank you for pointing this out. We have revised the sentence at Line 371 from “...provides insight into the evolving influence of WCB outflows on the large-scale flow” to “...provides insight into the evolution of WCB outflows within the large-scale flow”, to avoid implying causality and to ensure consistency with our clarified framing of interaction.

This last comment is rather some minor food for thought for the authors and does not imply an action item. The comment refers to the authors’ response to my second last (specific) comment of the previous review (which referred to what is now the first paragraph of 6.2). There, the authors write that “the upper-level flow” is “largely governed by dry dynamics”. A statement that seems to contradict the authors’ response to my specific comment on their L32, and the review paper by Wernli and Gray. Is the upper-level, midlatitude flow largely governed by dry dynamics or do moist process play an important role? It seems to me that both statements cannot be true at the same time. (The authors’ statement in the response is inconsequential for the manuscript and I am happy with the authors’ modification made to the manuscript.)

We thank the reviewer for raising this important point. We agree that our phrasing in the response could have been misleading. What we intended to express is that most aspects of the upper-level midlatitude flow can be interpreted and understood to first order within the framework of dry dynamics, while moist processes add an important additional layer of complexity. This does not imply that moist dynamics are unimportant; on the contrary, as highlighted by Wernli and Gray (2025), they play a crucial role in shaping and amplifying the flow.

Reviewer 2

Vishnupriya et al. have successfully addressed the comments I've given out, and have implemented major revisions including statistical testing and sensitivity analysis for robustness of their warm conveyor belts (WCB) outflow analyses. Careful wording has been done to avoid causality suggesting interaction, and to rather highlight the two-way influence / co-occurrence between the upper-level troposphere and WCB outflows. Here are some minor changes needed to make the paper suitable for publication.

We sincerely thank the reviewer for the positive evaluation of our work and for acknowledging the major revisions we have implemented. We are grateful for the constructive feedback throughout the review process, which has helped us to substantially improve the manuscript. We are pleased that the reviewer considers the manuscript suitable for publication. Below, we address the remaining minor comments in detail.

Minor comments:

WCB outflow classification.

The authors have clearly clarified the possibility of each WCB outflow trajectory transitions, based on their 6-hourly updates of classifications, and the Figure R4 is very helpful in reading out the composition of WCB parcels in blocking by "age".

With this mutual exclusiveness with progression in time, what you are really referring to is that "blocking but whose part hasn't been anomalous enough to be a cut-off (high)" would have younger WCB parcels making up to 55.6%. The parcel could have still stayed within the block for longer, but just transitioned to be categorized as a cutoff by this progressive classification.

If we think from a meteorological common sense - a true blocking, taking your Figure 2b's Ural blocking as an example, would be separated into (1) block (minus cutoff) and (2) cutoff parts by your definition; the block (minus cutoff) part would have more young WCB parcels, but the cutoff (the majority area of this Ural blocking!) would have way less young WCB parcels. This actually answers my confusion of "[why am I not seeing] a developed block being slightly older than spawned ridges" - you are just referring to the small edges of the block as block (minus cutoff), and the older parcels might just sit in the cutoff classification in the blocking center. Is there a nicer way to avoid using the misleading "block" that the audience might mistake it for the entire region of the blocking high? We thank the reviewer for the detailed feedback. The transition from block to cutoff interactions occurs only in a subset of cases, as cutoff interactions are much less frequent than block interactions. The differing spatial hotspots of block and cutoff features (Fig. 1b,c) further confirm that these are largely distinct. The point-of-interaction regions of block interactions (Fig. 5c) exhibit similar hotspots to those of block features (Fig. 1b), indicating that the classification effectively captures the majority of block interactions.

The reviewer's concerns are already addressed in the manuscript: (i) we explain that interacting WCB parcels do not cover the entire weather feature, ridge, block, or cutoff since we focus only on the WCB parcels within the features (L286–290, 300–303), (ii) we elaborate on the classification hierarchy, which enforces mutual exclusivity, in Sections 2.3 and 2.4. We believe these clarifications will help future readers accurately interpret our classification methodology.

Preexisting ridge vs new ridge - co-occurrence and causality

The authors have clarified about their interaction to be co-occurrence of WCB outflows with the classified features, and have edited the manuscript to incorporate the situation of WCB outflows

injecting into preexisting ridges by L392-393: “the WCB trajectories contribute to the formation or maintainance”. There’s a spelling error: the noun form of “maintain” is “maintenance”, not “maintainance”.

We thank the reviewer for pointing out the typo. We have corrected “maintainance” to “maintenance” in the revised manuscript (L394).

Stratosphere WCB outflows.

The authors have correctly edited the classification ratio in the abstract to be conditioned on troposphere only, and have included Figure R3 as a preliminary examination of the stratospheric WCB outflow. Figure R3 could thus be placed in the supplementary materials and refer to it in manuscript L218, or in section 6.2 as a possible future study direction.

We thank the reviewer for the suggestion. We agree that the preliminary examination of stratospheric WCB outflows is interesting, and we have included Fig. R3 in the supplementary materials (Fig. S4) with reference to it in the manuscript (L218).

Figure 11 caption: “np-interaction” should be “no-interaction”.

We thank the reviewer for carefully checking our figure captions. The caption of Figure 11 has been corrected from “np-interaction” to “no-interaction.”