Second Review of WCD-2020-52:

"Potential-Vorticity Dynamics of Troughs and Ridges within Rossby Wave Packets during a 40-year reanalysis period"
by
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General Comments:

The authors have significantly reworked the manuscript based on the feedback they received. There are still some remarks that should be clarified.

Regarding my comment on the partitioning of PV anomalies, which the authors regard as "out-of-the-box" thinking, their response requires some clarification. In general, one uses interior PV and boundary conditions for potential temperature on the upper and lower boundaries as well as azimuthal wind conditions in the lateral direction to perform a complete PV inversion, assuming a geostrophic and hydrostatic state. These conditions can either be set to trivial values or be prescribed making certain assumptions. In my original comment, I wondered about the implications of the assumption that the PV anomaly in the respective domain would not be associated with a potential temperature anomaly on both vertical boundaries, which it appears is what the authors are assuming. While it might be common practice, it has also been shown that this can bear significant caveats (e.g., Egger and Spengler, 2018, DOI: 10.1175/JAS-D-17-0039.1).
In their response, the authors then blend arguments of regular PV inversion, including interior PV and boundary conditions as outlined above, with PV thinking of delta sheets introduced by Bretherton (1966), where all information is condensed into delta perturbations of potential temperature, i.e., static stability anomalies, at the boundaries, which can be extended to a given amount of these discrete delta layers of these type of PV anomalies (e.g., Heifetz et al., 2004, de Vries et al., 2009). Based on my reading of the manuscript, however, the PV inversion outlined by the authors deals with interior PV in given volumes and potential temperature anomalies on the vertical boundaries, which is also what my comment and question was based on. As indicated previously, the exact inversion procedure including the assumptions should be clearly outlined and a justification of the assumptions, especially regarding the boundary conditions, would be highly appreciated.
My comment was not about the respective PV anomaly projecting on static stability anomalies, but on the used potential temperature anomalies in the inversion, as it is these anomalies, based on my understanding of the authors' method, that are used for the inversion. Regarding my comment, it would be correct that these potential temperature anomalies are then part of the respective PV anomaly. In their response, the authors then use an argument that would refute their own assumption, where they argue that vertical velocity is negligible at the upper and lower boundary, thereby suppressing an influence of the distant PV anomaly on this surface. However, the same argument would then also apply to the PV in proximity of the boundary if it is seen as more or less rigid. Therefore, no potential temperature anomalies along the boundaries would be attributable to any PV anomalies, though one appears to happily make certain a priori choices.
A thought experiment; if one wants to infer the anomalous flow field in the lower domain based on the PV in the upper domain, actually no information of the PV in the upper domain is needed if one knows the potential temperature at the interface between the

upper and lower domain as well as the potential temperature at the surface, given that one inverts zero PV in the lower domain. Thus, the main question would be how the PV of the upper domain would project on the potential temperature at these interfaces. If a priori prescribing half of that information, what is one really assessing? To further substantiate the claims of the authors, sensitivity experiments could be performed by moving the vertical boundaries of the chosen pressure levels as well as varying the choices of the boundary conditions. If the interior PV really is the most significant detail, then the respective results should not depend significantly on these choices if the volumes are chosen as such that the PV that is regarded as significant is always contained in the chosen volumes. In addition, a completeness test could be performed to further substantiate the assumptions of the authors with respect to their choices, where one would check if the sum of the anomalies yields the total field, or how closely it would resemble it. If the total field cannot be satisfactorily reconstructed, this would also point to deficiencies of the method and/or choices for the inversion.

There was a conflict in the response of the authors, where they in response to my comment 7. clearly state that they are arguing in an isentropic framework, though in response to my comment on line 560 they refer to a calculation and argument in pressure coordinates. The implications of this shift of framework in argumentation should be clarified. As indicated, adiabatic displacements would be invisible in an isentropic framework, except for implied divergence between isentropic surfaces. As the authors now include a more detailed discussion of the divergence, this appears to be more directly linked, but the authors are encouraged to double check for potential inconsistencies.

Regarding the authors' response to my point 7., they claim that my comment contained an incorrect statement. This is not true and in a way the reviewers more or less merely paraphrased my comment. To further clarify, with "ensuing", an effect, which can be referred to as indirect, of the heating was implied in my comment, i.e., the heating yields a secondary circulation redistributing mass and vorticity and thereby PV. It appears the authors are aware of that. Furthermore, the more detailed discussion of the role of the divergence relates to my previous comment and thus addresses some of my concerns, in particular the relation of the divergence to the diabatic heating.