

Review of wcd-2022-60_response

“The role of boundary layer processes in summer-time Arctic cyclones”

by

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Recommendation: Minor revisions

General Comments:

The authors addressed several of the concerns raised, though some remain, see response to the authors’ response below

1. ... whilst we agree that a range of diagnostic tools are available to understand cyclone development, we feel that the PV framework provides a unique and valuable insight here...

As indicated, there have been other approaches to quantify the addressed effects, which confirms that the PV framework is not “unique” to this research question. Therefore, a comparison to these other findings using other methods was suggested and is still recommended, especially as some recent studies highly the importance of indirect effects of surface exchange, see comments further down.

3. ... The central arguments in this work relate to thermal wind balance between the vorticity and potential temperature gradients, and what happens to cyclone structure when non-conservative processes modify either wind (friction) or potential temperature (diabatic processes). The benefit of using a PV framework is that structural changes within the cyclone can be inferred from the results. Changes in circulation and the constraint of thermal wind balance are not transparent in energetic frameworks and this is one of their major limitations.

PV only allows to assess structural changes related to stratification and vorticity if one implies balance assumptions, which, as pointed out, is highly questionable when focusing on processes in the boundary layer. The authors should further clarify how the use of PV should be enlightening in such a context, also given that they themselves state that “it is difficult to say how the BL PV tendencies contribute to the tropospheric depth-integrated circulation evolution.” Furthermore, as pointed out further below, the neglect of how diabatic processes affect PV in the free troposphere, which is often argued to be rather significant for cyclone development, needs to be further substantiated.

... the volume integral of the PV equation tells us about the processes contributing to changes in circulation without needing to invoke a specific balance relation.

While “a” circulation can be inferred, it is not given that it is “the” circulation associated with the circulation in the cyclone, e.g., if the tilt in the isentropes is

significant and the circulation is mainly occurring in the vertical plane. If assumptions about the stratification are needed to invoke inferences, this and potential sensitivities of the results should be clarified. The latter is also related to the response below to 5.

... this is not a precise interpretation of C when isentropic surfaces are tilted and intersect the upper or lower boundaries of the volume, as the reviewer points out. In the revised manuscript, we now use the more general term of “depth-integrated PV budget”, rather than the “depth-integrated circulation budget”, to ensure that the reader is not misled.

This is fine, though the authors’ response above referred to circulation.

Whilst there may be latent heat release happening in the free troposphere, our study focuses on the effects of the surface on Arctic cyclones, including surface heat fluxes and frictional processes. Non-conservative processes in the free troposphere (including latent heating) are not examined and impact of the dry BL processes is isolated. ... latent heat release is not examined for simplicity, so the impact of the dry BL processes is isolated.

As indicated in the original review, surface exchange can have direct and indirect effects on cyclone development, which have recently been assessed in both a PV and energy framework using theory and idealised numerical simulations, respectively (references see original review). These studies showed that the direct effects of surface exchange are usually small compared to the indirect effects (i.e., changes in latent heat release in the free troposphere). Hence, excluding free tropospheric non-conservative effects for “simplicity” for an investigation of surface effects on cyclones appears questionable. If it turns out that the non-conservative effects in the free troposphere are dominant, the exclusive focus of this study on only the direct effects of surface exchange could be misleading. It is also not correct to state that “the impact of the dry BL processes is isolated”, as the indirect effects were neither controlled nor assessed.

If the inclusion of diabatic effects in the free troposphere is not feasible in the context of this study, the authors need to clearly state potential shortcomings of their study with respect to this neglect and how this might impact their main conclusions.