Author response: The role of boundary layer processes in summer-time Arctic cyclones

The editor's and reviewer's comments are copied below in black, with our responses in blue. Note that all line numbers and figures refer to the previously revised copy of the manuscript that you evaluated.

Editor:

Thank you for your detailed revision and reply to the referees' comments. Based on the second round of reviews and my own evaluation, the manuscript may be acceptable for publication after some minor revision. Please address all the remaining concerns raised by Referee #3. For the revision, I also have some suggestions for consideration.

1. I agree with Referee 3 that it would be better to compare your results with previous studies using other methods, which will promote the contribution of your study to the topic.

Please see response to Reviewer 3, comment 1 and 5.

2. I also fully agree with the referee that the authors should add more discussions on the possible caveats of the study. As the referee suggested, the neglect of non-conservative processes in the free troposphere (including latent heating) is the one deserving discussion. Furthermore, I suggest the authors also made some discussions on the assumptions they made when including the BL processes in their PV budget calculation. For example, the formulas of the BL PV in Eqs. 9, 11,13,14 are all the ones derived for midlatitude cyclones. Are the boundary layer characteristics in Arctic similar to that in midlatitude for cyclones? How much do we know about the structure of the fluid in the Arctic BL? Why do the authors use such set of formulas? I suggest the authors either provide the rationale for using the formulas or adding some discussions in the conclusion part. Also, it is not clear to me how the authors calculate \tau_s and H_s in Eq. 8 in the study.

Regarding the caveats of the study (including neglect of non-conservative processes in the free troposphere), please see response to Reviewer 3, comment 5.

The motivation for using the BL PV tendency equations is that they have been used previously to investigate BL processes in mid-latitude cyclones (line 93-145), allowing direct comparison with these studies. The assumptions made in the derivation of the BL depth-averaged PV tendency equation (Equation 11) are only that the horizontal variation of fluxes is much smaller than the vertical variation (line 227-228), that there is a linear flux gradient in the BL (line 230), and that there is a constant density in the BL (line 238). These are general assumptions about turbulent mixing in the BL that are not specific to mid-latitudes or polar regions. To clarify this, a sentence is now added to the revised manuscript at line 248: "Although these equations have been derived and used previously in the context of mid-latitude cyclones, the assumptions made regarding turbulent mixing are equally applicable in the Arctic.".

Note that Equation 9 is the partition of the terms in Equation 6 into vertical (V) and horizontal (H) components, with no approximations made. In the newly revised manuscript we drop the V and H notation, which may be confusing to the reader, and re-write Equation 9 as follows:

$$\frac{DP}{Dt} = \underbrace{\frac{1}{\rho_0}\left[(\nabla\times\boldsymbol{F})\cdot\hat{\mathbf{k}}\frac{\partial\theta}{\partial z}\right]}_{(F_{EK})} + \underbrace{\frac{1}{\rho_0}\left[(\nabla\times\boldsymbol{F})\cdot\nabla_H\theta\right]}_{(F_{BG})} + \underbrace{\frac{1}{\rho_0}\left[\boldsymbol{\zeta_a}\cdot\hat{\mathbf{k}}\frac{\partial}{\partial z}\left(\frac{D\theta}{Dt}\right)_{shf}\right]}_{(S_V)}$$

$$+ \underbrace{\frac{1}{\rho_0}\left[\boldsymbol{\zeta_a}\cdot\nabla_H\left(\frac{D\theta}{Dt}\right)_{shf}\right]}_{(S_H)} + \underbrace{\frac{1}{\rho_0}\left[\boldsymbol{\zeta_a}\cdot\nabla\left(\frac{D\theta}{Dt}\right)_{lhf}\right]}_{(L)}$$

where ∇_H is the horizontal gradient operator.

The motivation for using the depth-integrated PV budget equations is to link the BL PV tendencies with cyclone evolution above the BL (line 262-263). The only assumptions made in the derivation of Equations 13 and 14 in the previously revised manuscript is that the BL height is flat (i.e. $\hat{n} = \hat{k}$). This was applied to simplify the expression for the flux across the BL top. However, as we don't calculate this term (i.e. it is not a major discussion point in the paper), in the newly revised manuscript we write the full equation without this assumption. Furthermore, in Equation 14 we have added the term representing non-conservative processes in the free troposphere (and explain in the manuscript that we do not calculate this term explicitly, as it is not the focus of this study). Equations 13 and 14 in the newly revised manuscript are as follows:

$$\frac{d}{dt}\langle P\rangle_{BL} = \iint \rho h \widehat{\frac{DP}{Dt}}\, dA - \iint\limits_{z=h} \rho P(\boldsymbol{u}-\boldsymbol{u_b})\cdot\hat{\mathbf{n}}\, dA - \int_0^h \oint \rho P(\boldsymbol{u}-\boldsymbol{u_b})\cdot\hat{\mathbf{l}}\, dl dz$$

$$\frac{d}{dt}\langle P\rangle_{TROP} = \int_h^{z_{top}} \iint \rho\frac{DP}{Dt}\, dA dz + \iint\limits_{z=h} \rho P(\boldsymbol{u}-\boldsymbol{u_b})\cdot\hat{\mathbf{n}}\, dA - \int_h^{z_{top}} \oint \rho P(\boldsymbol{u}-\boldsymbol{u_b})\cdot\hat{\mathbf{l}}\, dl dz$$

The surface momentum flux (τ_S) and surface heat flux (H_S) are directly output from the ECMWF IFS model. This is now clarified to the reader in Section 2.2 by adding the following sentence at line 173: "The surface momentum flux and surface sensible heat flux are also used directly from the IFS model, and are computed using bulk formulae with exchange coefficients (ECMWF, 2020)."

3. Results in Figures 9 and 10 are interesting. I suggest the authors think of replotting some panels with different colors to make them easier to read. For example, it is very hard to distinguish curves for S_V and S_H under the yellow and purple background. In addition, in Figure 10a, why is L not at the center of the cyclone? How is the cyclone center defined?

We agree that Figures 9 and 10 could be improved to make them easier to read. In the revised manuscript, the background shading (denoting surface type) is now only displayed in a layer at the bottom of the subplots (rather than the full y-range). This reduces overlap with the curves and clashing of colours, so that the curves are now easier to distinguish from each other. We have also increased the thickness of the curves. We think that these revised plots are now easier for the reader to interpret.

In Figures 10 and 11 the vertical purple dashed line denotes the cyclone centre as determined by the TRACK algorithm, which is based on 850 hPa relative vorticity (see Section 2.3). The red L on these plots refers to the minimum sea level pressure across the section. The cyclone centre (based on 850 hPa relative vorticity) and minimum sea level pressure are not necessarily aligned, especially during the baroclinic growth phase (i.e. Cyclone B in Figure 10a). To remind the reader of how the cyclone centre is defined, a reference to Section 2.3 is now added in the caption for Figure 9 in the revised manuscript.

Reviewer 3:

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.

We chose the PV framework to build on work by Cooper et al. (1992), Stoelinga (1996), Adamson et al. (2006) and others, but we acknowledge that other approaches could be used. Accordingly, we now refer to the results of Haualand and Spengler (2020) and Bui and Spengler (2021) in the introduction of the newly revised manuscript. However, as we haven't fully quantified the impact of friction and sensible heat fluxes on the cyclones in this study (i.e. quantified the response of the 3-D winds within the cyclones), we cannot put our results in context of the suggested studies here. Please see Reviewer 3, comment 5 for an extended discussion of this.

2. "… 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.

In the derivation of the BL depth-averaged PV tendency equation, no assumptions about PV conservation or invertibility have been made, as stated at line 247-248 in the previously revised manuscript. The impact of friction and sensible heat fluxes on cyclones in model simulations has been demonstrated by other modelling studies using this PV framework (e.g.

Adamson et al., 2006; Boutle et al., 2007; Plant and Belcher, 2007). In this study no balance assumptions are needed to calculate or interpret the PV tendencies and their integrals (i.e. they are just diagnostic of the BL processes identified in previous modelling studies).

It is true that in order to relate the evolution of the cyclone outside the BL to the action of non-conservative processes within the BL, it is necessary to make approximations and to use a framework describing how the large-scale flow within a cyclone must respond to such changes. For example, in the simplest case of a barotropic cyclonic vortex, the action of friction within the BL on the vortex above the BL is typically explained using the Ekman pumping mechanism. The response of the secondary circulation in the r-z plane is central, but the mechanism can be understood qualitatively without detailed specification of a balance approximation, i.e. lines 104-111 in the previously revised manuscript, based on Hoskins and James (2014). This is the approach taken in this paper. The volume-integrated PV changes in the BL are partitioned into an Ekman term and other terms related to surface fluxes in a baroclinic environment. These terms are quantified explicitly. However, the response of the 3-D wind field to these terms is anticipated through a dynamical argument. In further work moving beyond this article, we are attempting to quantify the response of the winds (in a balanced-dynamics, semi-geotriptic, framework) to the non-conservative processes, but this involves a new tool and is beyond the scope of this article (see Reviewer 3, comment 5).

Here, we have qualitatively inferred the impacts of these processes by examining the 3-D structures of the cyclone. The dynamical arguments invoke thermal wind balance above the BL. In particular, we examine the F_{BG} term and it's impact on the cyclone (summarised in Figure 11). We make the argument that the reduction of near-surface winds due to friction changes the wind shear across the BL. For winds just above the BL to remain in thermal wind balance, the horizontal temperature gradient (above the BL) must change accordingly (i.e. we are assuming thermal wind balance above the BL, not within the BL). To clarify this to the reader, we ensure that we refer to the thermal wind vector and temperature gradient "just above the BL" in the newly revised manuscript. It is apparent from the cross-sections in Figures 9b and 10b that thermal wind balance applies on the scale of the cyclone, with cyclone A having a warm-core and cyclonic flow maximum near the BL top, while cyclone B has a cold-core and the magnitude of the cyclonic flow increases with height.

Regarding diabatic processes in the free troposphere, please see response to Reviewer 3 comment 5.

3. "… 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.

We agree that the previous wording could have been misleading, and this has now been clarified to the reader in the previously revised manuscript at lines 265-269 (or lines 266-269 in the newly revised manuscript).

4. “… 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.

All uses of the term “depth-integrated circulation” have been changed to “depth-integrated PV budget” in the revised manuscript.

5. “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.

We re-write lines 134-138 to include reference to previous work on the impact of sensible heat fluxes on cyclones as follows: “Haualand and Spengler (2020) and Bui and Spengler (2021) demonstrated that the direct effect of surface sensible heat fluxes is to weaken mid-latitude cyclone development by reducing low-level baroclinicity, using PV and energy frameworks respectively in idealised modelling setups. However, both studies found that the impact of sensible heat fluxes was relatively small compared to that of latent heating. Sensible heat fluxes also modify the action of friction by altering BL stability and by weakening frontal gradients (Plant and Belcher, 2007).”

In this study we focus on characterising and understanding the effects of friction and sensible heat fluxes in the BL on two Arctic cyclone cases, and identifying the dependence of these frictional effects on large-scale cyclone structure. We have not attributed quantitatively the response of 3-D winds within the cyclones to these BL processes. This is a limitation and we suggest in the conclusions that this should be the subject of future work. Indeed, this is the subject of current work by the co-authors, including the role of latent heating (coupled with vertical motion). For this reason, we are not yet in the position to be able to put our results in the context of the indirect effects discussed by Haualand and Spengler (2020) and Bui and Spengler (2021). We also acknowledge that latent heating in the free troposphere may be a significant factor in the evolution of Arctic cyclones, but this is not the subject of this particular study. To clarify this to the reader, we have made the following changes to the manuscript:

- Equation 14 has been re-written to include a term representing non-conservative processes in the free troposphere. Rather than this term being “neglected”, we explain in Section 2.6 that this term is not calculated explicitly in this work (along with the vertical and horizontal fluxes of PV on the RHS of the equation), as the action of non-conservative processes in the free troposphere are not the subject of this study. However, the changes in $\langle P \rangle$ diagnosed in the IFS model include the effects of all processes including latent heat release above the BL.
- In particular, lines 279-281 have been re-written as “Non-conservative processes in the BL and free troposphere are included in the formulation (although the latter are not calculated explicitly). Note that whilst non-conservative processes in the free troposphere may occur at mid-levels within Arctic cyclones, in particular latent heating, these are not the subject of this study. However, the changes in $\langle P \rangle$ diagnosed in the IFS model include the effects of all processes, including latent heat release above the BL.”.
- The brackets on line 453 are re-written as “(which is not explicitly calculated here)”.
- We extend the future work section at the end of the conclusions (lines 654-658): “Moist processes and diabatic effects in the free troposphere (in particular latent heat release coupled with the vertical motion) have not been considered here. Although we may expect latent heating to be less important in the Arctic than lower latitudes due to reduced absolute humidity, Terpstra et al. (2015) demonstrated that low-level disturbances are able to amplify in a high-latitude moist baroclinic environment in the absence of other processes (upper-level perturbations, surface fluxes or radiation) using an idealised baroclinic channel model. This suggests that latent heating can be significant for the development of polar cyclones. The work here is focused on characterising the effects of friction and sensible heat fluxes at the lower boundary on Arctic cyclones in two cases with contrasting structure. The authors are conducting further study to quantify the response of the 3-D wind field within the cyclones to the BL processes explored here, and the amplification of ascent by latent heat release, using the diagnostic tool of Cullen (2018) assuming semi-geostrophic balance dynamics. Quantifying the relative importance of non-conservative processes in the BL and free troposphere in the evolution of Arctic cyclones and understanding the sensitivity of cyclone evolution to surface properties, are also areas for future research.”.

References added to manuscript:

Bui, H., and Spengler, T.: On the Influence of Sea Surface Temperature Distributions on the Development of Extratropical Cyclones. *J. Atmos. Sci.*, **78**, 1173-1188, 2021.

Cullen, M.: The use of Semigeostrophic Theory to Diagnose the Behaviour of an Atmospheric GCM, *Fluids*, **3**, 72, 2018.

Terpstra, A., Spengler, T., and Moore, R. W.: Idealised simulations of polar low development in an Arctic moist-baroclinic environment. *Quart. J. Roy. Meteor. Soc.*, **141**, 1987-1996, 2015.