

Review of “Preconditioning of block onset in the Southern Hemisphere: a perspective from static stability” by Dong et. al.

Suggested outcome: Major Revisions

Scientific significance: Fair (3)

Scientific quality: Fair (3)

Presentation quality: Fair (3)

This work evaluates the static stability budget during atmospheric blocking events over the Southern Hemisphere.

The approach is interesting however there are concerns with some of the results and explanations in the manuscript. I therefore propose that the manuscript undergoes major revisions before it can be considered for publication.

Major comments:

- 1) A major feature of this work is the treatment of vorticity and static stability as separate and mutually exclusive quantities. However, in reality this may not be the case. For example, the authors argue that the static stability can be created or destroyed by vertical stretching advection etc, however this is also true for vorticity, indicating that both static stability and vorticity will change simultaneously. For example, in a discussion around the evolution of static stability (Fig 8 and 9), the authors make the statement that the static stability evolves similarly to the IPV. Is this because the IPV and static stability cannot be decoupled? The authors should do more to convince the reader that of being able to treat static stability as a distinct quantity and that “the investigation of the local change of static stability would shed light on understanding the dynamic evolution of blocking events”.
- 2) A concern of this work is the lack of closure of the static stability budget (e.g. Figure 10, 11 and 14). The authors need to do some more work to convince the reader that this does not affect their work or their results.
- 3) There is generally a lack of dynamical reasoning or explanation in this work. The authors generally describe the figures and offer little dynamical explanation of the processes. For example:
Analysis of the vertical structure of static stability (Fig. 13) is done without analysis of the vertical differences in static stability and what it means for blocking.
- 4) This work is structured by analysing a composite and a case study similarly. The work could be streamlined such that arguments are not made twice where there is generally consistency between the case study and climatological composites. This can get quite confusing for the reader. I would suggest that the structure of the main body of the work be comprised of the climatological composite and a short section at the end of the analysis with the case study (in less detail) be used to corroborate the climatology (or visa versa).

- 5) Use of language is very colloquial at times. Phrases such as “upper-left corner”, “a few days” and an anomaly that “shows up” are frequently used. The authors should read through the work carefully and correct their use of language and general grammar throughout the manuscript. Please check plurals and add “a” or “the” in places where required (eg. Line 18 “outbreak” -> outbreaks, Line 47 “... formation of low-PV anomaly ...” -> ... formation of a low-PV anomaly, Line 203: “... intersects stratosphere is characterized with strong meridional gradient of static stability.” -> ... intersects the stratosphere is characterized by a strong meridional gradient of static stability

Specific comments:

- Line 17-18: Some citations of blocking in cold air outbreaks, heatwaves and drought required.
- Line 131-136: “20 typical cases” – why are these 20 cases chosen? What makes them typical? Why not choose all cases detected by the algorithms employed?
- Line 159-167: The descriptions of the various features being pointed out can be hard to follow. Consider improving this synoptic discussion and adding labels onto the relevant figures of the various important features.
- Figure 3 and relevant discussion: It is unclear what features the authors are trying to show with the vertical cross-section that cannot be readily seen in the 2D plots (Figure 2). Would a longitudinal cross-section not make more sense so that the north-south dipole can be seen?
- Figure 4: I recommend that the axes of these plots be changed such that they are “relative latitude and longitude” with the centre (0,0)
- Line 185: “... originating from the subtropics, gradually penetrating poleward ...” – I do not see the process that is described unfolding in Figure 5. In Figure 5, I see the low-PV “trough” extending equatorward from the poles and growing in amplitude over Days -5 to 0. Please explain this contradiction.
- Figure 6: See comment on Figure 4.
- Line 199-200: “it is evident that the subtropics are dominated by tropospheric air associated with relatively low static stability while the high latitudes are dominated by stratospheric air characterized by large static stability” – this certainly looks true for DJF but looks more complicated for JJA. For example in JJA over the Australian region there seems to be a local maxima in static stability close to the subtropics and a minimum near the poles. Please clarify and explain this contradiction.
- Line 209: “long-term mean” – annual, seasonal, daily mean? Please clarify.
- Figure 8+9: I wonder if all the panels and the very detailed description of each panel (Line 210-225) is absolutely necessary? Consider a generalised discussion and skipping some panels.
- Figure 12 and Lines 249-257: The authors make the case for the changes in local static stability in terms of Equation 6. Changes in vorticity (when analysing a vorticity budget equation) are governed by similar processes such as horizontal and vertical advection, stretching, tilting etc. Do the changes in vorticity by these processes correspond to the changes in static stability? Are they coupled. Such an analysis may help motivate the authors arguments for analysing changes in static stability.
- Line 259-261: day-5 “features the most representative patterns” – why are they representative and what do they represent? Do you mean they are closest to the

results in Tables 2 and 3? How much do the other days results differ as they there seems to be a large difference in the results as we move from day-5 to day 0 in Table 2 and 3).

- Figure 13: Difficult to see the relative contributions easily since the x-axes difference between panels. I suggest merging all panels onto a single graph with same x-axes limits.
- Line 296: “The findings are once again confirmed”. What findings? There seems to be very little analysis of Figure 6 and the differences in the various terms in the vertical. Please discuss.
- Figure 14: Labelling (1,2,...6) on plot should match the time lags (-5,-4,...0).
- Figure 15 and Lines 305-318: In previous sections and figures, diabatic heating is shown to have a small role in the local changes in static stability. It remains unclear as to what this analysis is trying to elucidate. Please clarify why this analysis is important and the points you are trying to make here.