EGUsphere, community comment CC2
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Reply on RC1

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Community comment on "Midlatitude Static Stability Alteration with Southern Hemisphere Blocking Onsets" by Li Dong et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1038-CC2, 2022

This work studies the static stability evolutions during blocking onset.

Reanalysis data is used to study 20 selected Southern Hemispheric blocking events during 1986-2008.

The work finds 30% of the anticyclonic PV anomaly attributed to weak static stability. This weak static stability is primarily attributed to horizontal advection and vertical stretching.

The perspective from static stability is new and interesting. However, I have more than major reservations as follows. I suggest giving authors plenty of time (preferably no deadline) to undergo more-than-major revision.

Major comments

- Figure 14: The static stability tendency budget is not closed. The difference is large and systematic. This cannot be explained by finite differencing (line 301). This cannot be compared with the difference seen in Teubler and Riemer (2016, their Fig. 6), which is much smaller and not systematic. This unclosed budget largely damages my confidence in the results.

Thanks for bringing this up. Our Fig. 14 is for the composite result of 20 blocking cases at daily interval whereas the Fig. 6 of Teubler and Riemer (2016) is based on one single case at 6 hourly interval. In order to make the comparison more consistent, we plotted the analyzed and computed static stability tendency comparison figure for each blocking case at 3 hourly interval. We attached these figures here as the PDF file. In these figures, the analyzed static stability tendency is computed with central difference such that the tendency is the change of static stability over 6 hours. For each day, there are 8 time steps (00,03,06,09,12,15,18,21) as shown in the tick of the time axis. The attached figures for each blocking case demonstrate that the agreement between the analyzed and computed static stability tendency is fairly good, in general. In fact, we want to thank the reviewer for raising this important question as we realize that we need to update our Fig.

14 by using the selected single case figure instead of the composite figure since those averaging processes (case compositing as well as daily averaging) in the composite figure tend to degrade the agreement of analyzed and computed static stability tendency time series. We will make this revision accordingly in our revised manuscript.

Advection by rotational wind is suspicious because 32S 135W in Fig 12b shows positive tendency, but northerly should bring low static stability at that latitude (no matter JJA or DJF, Fig 7).

It is correct that in the climatological sense the northerly wind should bring low static stability at that latitude based on the climatological static stability pattern as shown in Fig.7. In addition, the climatological static stability does not change much along the longitude direction while it greatly changes along latitude. Nevertheless, in Fig. 12b, a zonal gradient of static stability exists at location of (135W, 32S) such that the rotational wind over that region, which is primarily westerly and northwesterly winds, tends to advect relatively high values of static stability easterward. Hence it leads to a positive advection by rotational wind there.

- Different roles of static stability (low PV, high Eady growth rate) are confusingly presented (or not clearly distinguished). This makes the main finding unclear. Below is my understanding:

[Low PV] A detection criteria of blocking is low PV at the center of anticyclone. Low static stability at the center of anticyclone will help a system detected as blocking. This point is supported by Figure 11 and others.

[High Eady growth rate] Low static stability upstream can give high Eady growth rate and favors baroclinic eddies that maintain the blocking (line 358). This point is not supported by any figures. In fact, Figure 9 goes against this conjecture by showing high static stability upstream. I suggest largely cutting mentions of this conjecture (Lines 24-39, 321-322, 331, 351-365) and clearly saying that Figure 9 goes against this. Please also revise the title (avoid the word "preconditioning"), and rephrase line 14 and 348 (at least remove the word "upstream"), in order not to confuse with the unsupported/rejected conjecture.

Thanks to the reviewer for raising this important point. We realize that we should have been more careful with emphasizing the specific stage of blocking when we summarized our findings and surmised our conjecture. In fact, for the finding that reads "static stability reached its local minimum over the block-onset region on the block-onset day", this is primarily focused on the pre-blocking period, i.e. five days prior to block onset. This implies that during the pre-blocking stage the low-static-stability anomaly immediate upstream of block-onset region serves as a sort of wave generator which triggers block onset a few days later. Here "upstream" means the low-static-stability anomaly is upstream of the block-onset region before block onset takes place. Hence for our conjecture that reads "over upstream block-onset region, the static stability field should be relatively low such that the wave maker generates baroclinic eddies more efficiently to maintain the blocking structure", it is supposed to refer to the pre-blocking stage instead of the blocking maintenance stage. Meanwhile we do realize that we have used "to maintain the blocking structure" in our conjecture, which is not correct indeed. So thanks to the reviewer for pointing this out. We should have made our conjecture consistent with the findings of our study. Therefore we plan to clarify our conjecture in the revised manuscript and rephrase it as "Prior to blocking onset, over upstream block-onset region the static stability field should be relatively low such that the wave maker generates more baroclinic eddies seeding to enter the block-onset region hereby initiating block onset."

As for the reviewer's suggestion upon avoid using "preconditioning" in the title, we agree on that as it appears somehow inaccurate. Now we tentatively modify the title as "Static Stability Variability and its Relation to Southern Hemisphere Blocking Onsets".

- My challenge to the low PV idea concerns the relevance of extreme weather conditions. It seems to me that blocking leads to extreme weather conditions (line 17) because of its wind anomaly, not static stability anomaly. In this sense, static stability will be relevant only if there is conversion to/from wind anomaly (or absolute vorticity). Is there such conversion (stretching term)? Or static stability and absolute vorticity are both doing their own thing without interaction?

This is an interesting question and thanks for bringing it up. Blocking leads to extreme weather conditions such as heat waves (due to extremely high temperature), cold-air outbreak (due to extremely low temperature), flooding (due to extreme precipitation) and so on. Even though these extreme conditions are not explicitly represented by the static stability anomaly, this static stability anomaly is tightly integrated into the blocking onset procedure through both thermodynamic and dynamic processes. For instance, heat waves are commonly associated with extremely high temperature as well as low wind (partly due to adiabatic warming accompanied with strong sinking motion). The static stability anomaly could contribute to these temperature and wind anomalies through static stability advection or stretching processes, as the reviewer suggested. In addition, the static stability and absolute vorticity do interact with each other, even though the former is a thermodynamical indicator and the latter a dynamical indicator. For instance, the stretching term in the static stability tendency equation links the static stability and absolute vorticity perfectly in that both thermal stratification and convergence field (which is closely linked to absolute vorticity) work together to give rise to conditions favorable to block onset.

- Overall, description of results is not so balanced, not so scientific, and not so insightful. Some examples below:

Line 316-317: "Fig. 12(f)… positive values poleward side." It might be unfair to highlight these positive values, which are much much weaker than the negative values equatorward.

Yes, we totally agree that these positive values are much weaker than the negative values on the equatorward side. But since we intended to focus on the block-onset region, we felt obliged to fully describe the 2-D distribution of static stability tendency attributable to the direct effect of diabatic heating over the block-onset region. In addition, the point that we attempted to deliver from Fig. 12(f) is that these positive values are much smaller than the negative values over the block-onset region in Fig. 12 (c), i.e. the indirect effect of the diabatic heating outweighs the direct effect significantly.

Line 210: "lower left"->"southwest"? Also line 268 and 270.

Thanks for pointing this out. We agree that we should have used more scientific terms in above circumstances. We have made the modifications following the reviewer's suggestions.

Line 161: "became a cut-off low IPV anomaly on the following day." The cut-off low anomaly might be referring to 50S 155W on 23 July (Fig 2d). Not sure if aforementioned understanding is right but this cut-off low measures less than 10 degrees in diameter and lasts only one day. Also, "cut-off" in anomaly field is not quite noteworthy (compared to cut-off in absolute field). Pointing to these fine details might not provide much insights.

Thanks for raising these concerns. We agree that the cut-off low PV anomaly is not significant in terms of both its size and magnitude. We have removed descriptions related to "cut-off" anomaly in the revised manuscript.

- Please focus on the role of static stability in giving low PV, by removing off-focus discussions. Examples below:

Equations 1-3: I don't see the need to introduce sigma. You can directly introduce \partial \theta/\partial p, and use that in place of sigma.

The reason that we induced sigma, as shown in Eqs.1-3, is that we want to particularly refer to the static stability parameter in the QG height tendency equation as described in the textbook by Bluestein (1992). This static stability parameter has dual effects in the QG height tendency equation. Plus, Smith and Tsou (1988) used the generalized height tendency with the exact form of this static stability parameter to discuss the variations of static stability associated with cyclogenesis. As our work is closely related to the work of Smith and Tsou (1988), we intended to keep the form of the static stability parameter intact for easier comparison.

Many figures: Rather than outlining the block-onset region as the wind reversal region, please try to highlight the low-PV region (e.g., where you detect PV anomaly).

This is a great suggestion. In fact we had also noticed that there are several blocking cases in which we detected the block-onset region based on the geopotential height blocking index but found out that the defined block-onset region did not exactly overlap with the low-PV center. In the revised manuscript, we will update the figures by outlining block-onset region with the low-PV standard.

- A few previous papers are misinterpreted.

Line 123: Pelly and Hoskins (2003) were based on reversal of absolute field, not anomaly-based. I suggest removing the citation here.

Thanks for pointing this out. We will remove Pelly and Hoskins (2003) on Line 123 accordingly.

Line 63-66: "The injection of diabatically processed anticyclonic PV is usually interpreted as the direct effect of latent heat release…" I suspect this is a misinterpretation of previous studies, at least of Teubler and Riemer (2016).

Thanks for pointing this out. We will remove Teubler and Riemer (2016) on Line 63-66.

- Figure 12: After closing the budget (comment 1), if panels b,d,e continue to be highly (anti-)correlated, please do a bit more discussion. I think horizontal convergence (panel e) correlates with sinking (panel d) because 300 mb is slightly below tropopause, so air is squeezed downward when it converges. Sinking correlates with equatorward motion (panel b) because air tend to move along isentropic surface, which is tilted in such way.

This is great suggestion. We will provide more discussion regarding the connection among variables in Fig. 12. The reviewer is right about the sinking motion due to the convergence nearby tropopause, as shown in omega being greater than 4 Pa/s (in contour, positive means sinking motion) in Fig. 12(d). And this sinking motion is closely related to the *southwesterly wind which moves toward equatorward since when air moves toward equatorward it would descent along the isentropic surface (the isentropic surface is tilted from Equator to Pole). By adding this discussion, it would make the whole picture more clear and complete. So we thank the reviewer for this great suggestion.*

Having the dominate terms counteracting with each other does complicate the picture. Would it be better to use isentropic coordinate? - Also because it is low static stability on isentropic coordinate that helps low isentropic PV.

This is another great suggestion too. We checked one blocking case by converting the static stability budget figure (similar to Fig. 12) from 300mb to 320K and found that the general pattern is quite similar. We are processing composite Fig. 12 now on the 320K isentropic surface and will post the updated figure soon.

- Fig 8f and 10c: Why zero lines differ in the two figures? Because of pressure coordinate vs. isentropic coordinate? Perhaps both should use isentropic coordinate (320K). Also Fig 9f vs 11c.

Yes, the zero lines from Fig. 8f and Fig. 10c slightly differ in terms of their locations. The reviewer is right that this discrepancy is resulted from the different coordinates used in the figures as Fig. 8f is on 300mb isobaric surface while Fig. 10c is on 320K isentropic surface. At this moment we are updating Fig. 8 and Fig. 9 by converting these static stability anomaly from 300mb to 320K isentropic surface. We will post the updated figures soon.

- Figure 10abd: Why as low as -120%? Does the sign change? If the sign changes, it is perhaps infinitely more important than stability changes.

When the absolute vorticity is small, a moderate change of absolute vorticity can lead to a relative change over 100%. Hence the relative change of -120% occurs because the initial value of absolute vorticity is small and also this quantity changes sign after a moderate change. Regarding the change of sign, it means the relative vorticity changes from a small positive number (i.e. climatological value) to a negative number (anticyclonic vorticity). Overall, the focus of Fig. 10 is to demonstrate what Eq.(10) suggests, i.e. the sum of the relative change of static stability and absolute vorticity equals to the relative change of IPV. Thus Fig. 10 serves the main purpose.

- Figure 10abc: At 30S 160W, both panel b and c shows >40%, why panel a is <80%, not >96% (1.4*1.4=1.96)? Is mean(IPV) not equal to mean(vort)*mean(sta)? If numbers are confirmed to be correct, please add an explanation in caption.

The mismatch between 80% and 96% is primarily due to two reasons. One is that Eq. (10) is expressed in partial differential format which requires the time step to be as small as possible when using finite difference to approximate this partial difference. Here we used the 3-hourly reanalysis data to compute Eq. (10) and presented the daily averaged result in Fig. 10. We believe that the discrepancy between the two fields would decrease if we could further reduce the time interval when calculating the relative changes. Another reason is that the reanalysis data does contain observation errors which may contribute to this mismatch as well. Overall, based on Fig. 10(a) and Fig. 10(d), we feel that these two fields generally have a reasonable agreement. We will add some necessary explanation in the caption of this figure in the revised manuscript.

- Line 240: What does “long-term mean” mean? Since there is a great seasonal cycle (e.g., in static stability, Fig 7), would be good to use one season.

“Long-term mean” here refers to the climatological value of a particular month. For

instance, for the July 1999 blocking case (block onset on July 25), the long-term mean static stability refers to the climatological static stability value for the month of July. Hence, the seasonal cycle has been removed indeed.

- Table 1: Most case is in JJA or May or September, except case 16 is in March. I suggest removing case 16.

Yes, we agree that case 16 (03/17/2003 blocking case) can be removed such that the remaining blocking cases took place around Southern Hemisphere winter season.

Minor comments

- Line 18: For blocking and extreme weather, it might help to cite Kautz et al. [doi:10.5194/wcd-3-305-2022], a review article at WCD.

Thanks for this suggestion. We checked this review article and found that it does have certain connection to our study. We will cite this work in our revised manuscript.

- Line 58: Hauser et al. [doi:10.5194/wcd-2022-44] also confirmed the importance of divergent outflow aloft. It might help to cite that as well. Please also comment (e.g., on line 343, 349) whether their study agree or disagree with yours.

Thanks for suggesting this article. We checked this paper and found it quite interesting. In fact it is tightly linked to our work here. This article confirmed that the moist process is important during blocking onset stage, which is consistent with our findings here. But it also revealed that the indirect effect of moist processes, i.e. the PV advection by the divergent flow, does not appear prominent from the Eulerian low-frequency perspective because the Eulerian approach only captures the local evolution of blocking onset whereas the upstream moist process is omitted from the Eulerian perspective. This is really an interesting finding to us. In our study, we did use the Eulerian perspective (but not the low-frequency perspective), and we found that the indirect effect of moist process is more significant than the direct effect, nevertheless the indirect effect is a secondary contributor to block onset compared to the dominant advection term. In this sense, our work agrees with Hauser et al (2022) 's results. In the revised manuscript, we will add discussion on how our findings are linked to Hauser et al (2022).

- Line 66-72: The mentions of WCB, PRE and ET do not tie well to the paper. They only connect to latent heat release, but not blocking. Perhaps simply remove them all.

We agree that the material related to WCB, PRE and ET kind of distracts the readers from the main topic on blocking. We will remove them in the revised manuscript.

- Line 130: "screened these blocking cases against PV-anomaly based blocking criteria." Please give more details how that is done.

Thanks for this suggestion. The PV-anomaly based blocking criteria refers to that the 320K low-IPV anomaly reaches at least 1 PVU and persists for at least 5 days. We used this criteria to double check blocking cases that are first detected with geopotential height reversal criteria. We will add these details in the revised manuscript.

- Table 1: If composite is done by overlapping the blocking centers (line 176), then table 1 should list the center, rather than the west boundary.

Yes, we agree that in Table 1 the block-onset regions can be represented by the center of the specific region instead of the west boundary. We will make the correction in the revised manuscript.

- Line 166: The definition of block-onset region should be moved earlier, because it is already used in Figure 1. You may also include the definition in figure caption. (Also see comment 5 for suggested modification on definition.)

Yes, this is a great suggestion. We will make the modifications in the revised manuscript as the reviewer suggested.

- Figure 2 caption: Is contour interval 0.5 PVU instead of 1.0? Probably you don't need to say it in caption. Though you do need to mention the unit of PVU.

Thanks for pointing this out. The contour interval should be 0.5 PVU and we are sorry for making this typo in the caption. We will correct it in the revised manuscript.

- Line 168: Instead of "blocking center", please say 60S.

Yes, we will make this correction in the revised manuscript.

- Figure 4: Please add tick labels for x-axis to show the scale.

Yes, we will make this correction in the revised manuscript.

- Line 185: "originated from subtropics" - How do you see this?

Thanks for pointing this out. As the reviewer suggested, this description is not accurate so we will remove this from the revised manuscript.

- Figure 6: Please modify the color scale so that white is 0.

Yes, we will make this correction in the revised manuscript.

- Figure 12b: Vectors at high latitude are not parallel to contour. Maybe this is a map projection issue (one degree longitude measures different length at different latitude). Please either fix it, or add an explanation in caption.

Thanks for bringing this up. In fact, it is primarily due to the compositing effect. We have checked the corresponding plot for single blocking case and confirmed that the rotational wind vectors are mostly parallel to the geopotential height contours even at high latitudes. Note that in Figure 12(b), the 300-mb rotational wind vector is overlaid with 500-mb geopotential height field instead of 300-mb geopotential height field.

- Figure 12d: Upward motion not shown? Please mention in caption.

Thanks for pointing this out. In Fig. 12(d), to highlight the sinking motion associated with convergence region, we only plotted the positive vertical velocity (omega) with magnitude greater than 2 Pa/s and omitted the rising motion. We will add these explanations in caption of the revised manuscript.

- Line 353,354: "Rossy"->"Rossby", "amply"->"amplify"? (Actually, I suggest removing the paragraph in comment 2.)

We are sorry for these typos. We will follow the reviewer's suggestion by removing that paragraph.

Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-1038/egusphere-2022-1038-CC2-supplement.pdf