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Comment on wcd-2022-2

Anonymous Referee #2

Referee comment on "Downstream development associated with two types of ridging South Atlantic Ocean anticyclones over South Africa" by Thando Ndarana et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-2-RC2, 2022

Reviews

Submitted to wcd-2022-2

Downstream development associated with two types of ridging South Atlantic Ocean anticyclones over South Africa

Authors: T. Ndarana et al.

The authors investigate two types of ridging high pressure systems in South Africa and surroundings from an eddy kinetic energy perspective using the ERA5 reanalysis. Results show that ridging highs located in more poleward latitudes are stronger than those in lower latitudes. The study also presents a comparison between the ridging high and cutoff low development. Nevertheless, after reading the paper, several fundamental questions come to mind, and my feeling is that if the authors can address some of these questions the scientific content of the paper will be improved making this a more valuable contribution.

- The authors compare two types of ridging highs and while I do agree that they are dynamically distinct it seems to me that the categorization is based on subjective interpretation rather than to information gained by a physical basis. A related question, can these ridging building be classified by cyclonic or anticyclonically breaking waves? If so, perhaps a more objective approach could be done by applying the anticyclonic and cyclonic types (LC1 vs. LC2 paradigm). Maybe this is not the goal of the study, but then the motivation for examining the differences between the two types of ridging

highs should be mentioned explicitly in the Introduction. Even the authors refer to their previous paper, the main differences between the two types of highs need to be clearly mentioned and why this classification is important.

- Another question is whether the ridging process may be induced by the South African topography. Presumably these types of ridging highs might be affected by topographic Rossby waves, and to me this could be mentioned in the manuscript.
- The paper does not discuss the possible contribution of diabatic processes to the ridging process. As mentioned by the authors, the vertical motion is significant in the type-S highs, in particular the downward motion, thus inducing eddy kinetic energy creation or destruction via baroclinic conversion. Diabatic processes are part of the eddy available potential energy (EAPE) budget, and EAPE is converted into eddy kinetic energy by baroclinic conversion. Thus, eddy kinetic energy can be indirectly enhanced by diabatic processes if these provide sufficient EAPE. Diabatic processes such as latent heating and radiative cooling are known to play an important for the evolution of mid- to upper-level atmospheric systems and these may contribute the development of ridging highs. Even though the accuracy of diabatic profiles is susceptible to errors in reanalysis, being more easily dealt in numerical models, I think this point should be discussed in the paper.
- Could quantify the relative contribution of each energetic term for the ridging phenomenon. More quantitative evidence of each mechanism (ageostrophic geopotential flux, baroclinic and barotropic conversions, etc) could provide a more comprehensive view of the contribution of each energetic term to the ridging highs through their life cycle. Also, it would be worth including upper-level winds or geopotential height fields to the composites for allowing a clear view of ridges and troughs.

Minor comments

Line 24: It should mention what level these quasi-stationary anticyclonic circulation occurs.

Line 60: Can explicitly state which “these waves” are.

Line 116: Add (Figure 1) after “During the first stage”

Line 167: This is not entirely true, other studies have already focused on the vertical profile of eddy kinetic energy, such as Rivière et al. (2015) and Pinheiro et al. (2021).

Rivière, G., Arbogast, P., & Joly, A. (2015). Eddy kinetic energy redistribution within windstorms Klaus and Friedhelm. *Quarterly Journal of the Royal Meteorological Society*, *141*(688), 925-938.

Pinheiro, H. R., Hodges, K. I., Gan, M. A., Ferreira, S. H., & Andrade, K. M. (2021). Contributions of downstream baroclinic development to strong Southern Hemisphere cutâ□□off lows. *Quarterly Journal of the Royal Meteorological Society*.

Line 176: How is t = 0 defined, would it be when the condition is firstly satisfied in the domain? This needs to be described in the methods and/or in the beginning of the discussion. Also, it's a bit confusing to present positive and negative values together, are these observed at different times?

Line 181: Does “The latter” refer to meridional perturbation velocity? Please mention the referred variable.

Line 186: baroclinic conversion reaches its maximum at lower levels, thus it does not explain the maximum tendency of kinetic energy near 250 hPa.

Lines 190-191: Figures 2h shows that the negative baroclinic conversion (ascending) maximizes at a higher level than the positive counterparts (descending), is there a physical explanation for this?

Line 191-192: The sentence that starts with “The ageostrophic geopotential flux has been shown…”seems to be disconnected from the rest of the paragraph or perhaps something is missing.

Line 223: Are these eddy kinetic energy centers computed vertically or at 1000 hPa, why don’t these centers match the maximum 1000-hPa eddy kinetic energy shown in shaded in Fig. 3? The authors do not mention if the Ke centers (namely I, II and III) are vertically integrated quantities as sometimes the energetics are referred to a single pressure level. After reading the paper, I assume these are integrated average fields, but nowhere is mentioned which levels are used for.

Line 232: replace “The latter” with “The centre II”

Line 297: In Fig. 6 it is not clear to me if the time lags coincide for all kinetic energy centers, I mean, does T=0 for center occur at the same time as that for center II? This is important for the correct interpretation.

Line 376: Please check if all fields are at 250 hPa as the caption of Fig. 9 says there are fields at the surface.

Line 444: Replace “the latter” with “type-S ridging”

Line 649: In Figure 3, can the quality of the lines that indicate positive/negative 1000-hPa geopotential height perturbations be improved as they are not clearly distinctable.

Line 766: In Figure 10, the acronyms UJ and DJ are not described. In that figure, what does mean the thick solid black oval contours?

Typos

Line 157: Replace “shifte” with “shift”

Line 223: replace “centre” with “centres” and “Fig. 3b and f” with “Fig. 3b and 3f”