Second response to reviewer's and co-editor's comments

Dynamics of Gap Winds in the Great Rift Valley, Ethiopia: Emphasis on Strong Winds at Lake Abaya

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1. INTRODUCTION

We thank the reviewer and the co-editor for their second feedback and the suggestions to improve the manuscript. In the following we provide a point-by-point answer to their comments with the original comments in black and our responses in blue. In addition to the revised manuscript, we provide a version in which all changes have been highlighted in blue (i.e. parts added) and in red (i.e. parts removed)

2. RESPONSE TO REFEREE 2

2.1. Synopsis

Thank you for revising the manuscript. The authors have answered my questions adequately. However, the authors have not revised the manuscript based on some responses. I hope the authors not only answer the questions but also revise their manuscript. If they do not revise, I hope they will also write the reasons for not doing so. For the above reasons, I can recommend its publication in Weather and Climate Dynamics only after some minor revisions.

We are sorry that our previous revision was not complete enough and hope that the new changes have now sufficiently addressed the remaining concerns.

2.2. Comments

-Response to RC on Major comment (2) of reviewer 2.
I agree with the authors' explanation. However, if the authors argue that ΔZ is an important factor in predicting gap winds in GRV, I think you should provide quantitative values for when the gap winds occur, even if it is just one example. So, I think that it is necessary to write somewhere in the manuscript the information that the gap wind occur when the ΔZ exceeded 15 m

We agree and changed that. In the manuscript (L190) it reads now: '... Fig. 3b shows a 24-hour running mean of ΔZ in which the diurnal cycle has been filtered in order to highlight the synoptic forcing. For $\Delta Z_{800\,\mathrm{hPa}}$, this filtered signal exceeded 15 m (grey dash-dotted line in Fig. 3b) during the strongest winds in the GRV. The peak in ΔZ at 800 hPa in Fig. 3a on 14 January was therefore composed ...'
Concerning the southwest case, the manuscript (L253) reads now: '... which is similar to the northeast gap flow. The synoptic forcing below pass height, represented by the 24-hour running mean of $\Delta Z_{800\,\mathrm{hPa}}$ in Fig. 7b, showed a similar behavior as for the northeast case and exceeded 15 m. However, even before the investigated case the 24-hour average of $\Delta Z_{800\,\mathrm{hPa}}$ was close to 15 m and, hence, caused southwesterly winds in the GRV (Fig. 7d). Similar to the northeast case, the magnitude of the 24-hour running mean of ΔZ was much larger at 800 hPa than at 700 hPa (Fig. 7b), indicating that the large-scale forcing was

strongest below crest height, i.e. within the GRV.'
We added a dash-dotted line in Fig. 3 and 7 for ΔZ = 15¸m to emphasis the possible threshold for the synoptic forcing.

- Response to RC on 3.3. (7) of reviewer 2.
I agree with the authors' explanation. However, this information does not seem to be present in the manuscript. I think this information needs to be included in the manuscript as well for the benefit of readers who are not familiar with this area.

We changed that. The manuscript (L151) reads now: '...the Turkana Jet (Nicholson, 2016; Munday et al., 2022) brought potentially colder air into the Turkana Channel and the region around Lake Abaya (Fig. 2b). This air originated from the southern Indian Ocean and was deflected by the massive of Mt. Kenya towards the northeast, resulting in the southwesterly winds on Fig. 2b. Below 800 hPa, a branch of this southwesterly air stream was deflected by the Turkana Jet into the Turkana Channel (not shown). The different air masses...'

-Response to RC on 3.3. (16) of reviewer 2.
I understand. However, while the specific numbers of the distances are of course important, it is necessary to write "between where" in the manuscript so that "the readers can understand".

Thanks, now we got the meaning of the initial comment - it was not about the specific distance but rather about geographic landmarks. We changed it and the sentence (L334) reads now: 'The difference in geopotential height at 800 hPa (i.e., near pass height) over a horizontal distance of more than 700 km between the Afar Triangle and the Turkana Channel exceeded in both cases 30 m for the daily maximum and 15 m for the 24-hour average.'

Response to RC on 3.3. (19)-(2) of reviewer 2 (Just a comment, no need to reflect the manuscript).
I am sorry that I could not accurately inform you of my intentions. In the evening (14 UTC, Fig. 9a), gap winds develop near AG (between x=550 and x = 650 km). On the other hand, at night (19UTC, Fig. 9c), the gap winds near AG are weak and gap winds develop between x=100 and x=500 km. In other words, in the evening (14UTC), the location of the gap winds is limited to the vicinity of AG due to the thermally localized circulation (pressure gradient that drives the plateau and basin winds), and at night (19UTC), the gap winds occur over the entire valley because the thermally localized circulation decays. Is it correct? The authors think that this is probably future studies. I am looking forward to it.

Yes, it is conceivable that counteracting basin winds from the northeast into the GRV north of the pass could have prevented an earlier breakthrough or at least weakened the southwesterly gap winds in the northern part of the valley during the day. However, we did not find strong evidence for basin winds. Concerning the weak gap winds around AG in the night: this is most likely due to the flow blocking below pass height indicated by the isentropes intersecting the slope between PA and AG in Fig 9c.

3. Responds to co-editor's comments

Comments to the author: Dear C. Weiss et al.,

Thank you for submitting your revised manuscript and detailed response letter. You may see that both reviewers are positive, however, the second referee suggests further minor revisions for better clarity for the readers.

Please note that both reviewers raised concern regarding deltaZ at 800 hPa as a diagnostic for the gap winds, especially given that the diagnostic peaks also prior to the onset of the gap winds, e.g., during 12 January. The addition of the smoothed values as panel 3b in the revised manuscript partly addresses this issue, and indeed shows that a clearer peak emerges on 14 January. However, accompanying text is needed in the manuscript as well, as other readers may have the same concern. I agree with the second referee that a more elaborate discussion of the atmospheric structure during the days prior to onset is necessary to complete this description.

Furthermore, it is the aim of the study to reveal whether the flow is dynamically- or thermally-driven. However, using the simulation set it is not possible to disentangle the components, as noted by the second referee as well. I therefore suggest that you rephrase lines 80-82 along the lines of the conclusions of this study.

Looking forward to receiving your revised manuscript and response, Shira

Thank you for your feedback. We hope that our changes sufficiently address your remarks.
Regarding ΔZ: We elaborated the discussion on ΔZ and its 24-hour running mean for both cases. For the changes see Sec. 2.2 - 'Response to RC on Major comment (2) of reviewer 2' in this document.
Regarding the goals of the study: We rephrased the conclusive paragraph in the introduction. The manuscript (L77) reads now: '...the more specific goals of this study are to investigate the physical mechanisms of the two cases and their daytime and seasonal dependency. The paper is organized...'
Additionally, we added following paragraph at the end of the discussion (L400):
'Finally, it is noteworthy that the underestimation of the gap flow in the GRV in ERA5 agrees with the underestimation of the Turkana Jet recently found by Munday et al. (2022) based on radiosonde observations. Similar to the gap flow in the GRV, the Turkana Jet does not only exhibit a strong diurnal cycle due to the thermal forcing (Munday et al., 2022) but is also affected by synoptic-scale pressure gradients associated with low-level ridging along the East African coast (Vizy and Cook, 2019).'
This does not only show that another mesoscale wind phenomenon in the same region is strongly dependent on the time of the day but that ERA5 underestimates that phenomenon as well.

References

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Nicholson, S.: The Turkana low-level jet: Mean climatology and association with regional aridity, International Journal of Climatology, 36, 2598–2614, https://doi.org/10.1002/joc.4515, 2016.

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