Review of "Revealing the dynamics of a local Alpine windstorm using large-eddy simulations" by Krieger et al.

In the article the authors show large-eddy simulations for a local high-impact wind storm in a narrow valley in Switzerland using a new numerical model. The simulations are performed in a semi-idealized framework investigating the influence of the ambient wind field and air mass stratification as well as topographic influences. The manuscript is very well-written and structured, has high relevance and fits the scope of WCD. I only have a few comments and questions on the manuscript listed below.

Personally I am very excited about this new model and especially the capacity of dealing with steep slope angles.

Specific comments

- Sec. 1: The introduction is very interesting and extensive, but could benefit from some streamlining. I suggest that especially the state-of-knowledge part between L39 and L96 could be more compact and streamlined towards the knowledge gap that is addressed in the study.
- Sec. 2.1: I assume there will be more information once Kühnlein et al. is published, in the meantime, however, more general information on the model is appropriate. Is the model mainly designed for LES applications or can it also be used in meso-scale applications? Can the model be run in a real-case setting as well? In this study moist processes and radiation are neglected, but does PMAP in general include microphysics and radiation schemes? Can you elaborate a bit more, why PMAP is able to handle such steep slope angles with terrain-following coordinates compared to other models? Is there an upper limit to the slope angles in PMAP? Apart from that, I want to encourage the authors to provide the model publicly available in line with the policies for data and software by WCD.

- L 166: You use a uniform profile of stratification and wind as initial conditions. However, in the case of Kyrill the upstream air mass shows a clear multi-layer structure with a higher-stability layer above 700 hPa (see Sprenger et al. 2018, Fig 4) as well as vertical shear in wind speed and direction (see Fig 1). How representative are the simulations presented here for the Kyrill-case given the strong influence of vertical variations in Scorer-parameter on gravity wave propagation?

- L285-290: I liked the spectral analysis and think it would deserve a more extensive evaluation, especially as it is stated, that the pulsating gusts are a key finding that was not known before. In the spectral analysis some of my questions remained open: for the layers below ∼100 m in Fig. 7 the spectral density between $\sim 4 \cdot 10^{-3}$ and $\sim 10^{-2}$ s^{-1} shows a local minimum which is not present for the higher levels. Do you have an explanation for this? Does that correspond to the thickness of the Laseyer-gust layer? As you show later that the gusts likely are linked to a enhanced downward motion and higher wind speeds in the valley atmosphere at about crest height (Fig. 9), do you see there a similar maximum in spectral density for a pulsation period of ∼450 s as well? I would like to see more on the mechanism behind the pulsating gusts especially as they have a period similar to reports in earlier studies (e.g. Peltier and Scinocca 1990; Belušić et al. 2007; Tollinger et al. 2019), but seem not to be caused by Kelvin-Helmholtz instabilities or gravity wave breaking as proposed in these studies.

- L358-365: I think in the conclusion of the governing mechanisms you should also include the role of the enhanced horizontal and vertical winds in the valley atmosphere at crest height mentioned in L346.

- Sec. 4.1: It might make sense to shift this section after Sec. 4.2 as you are referring to different ambient flow directions which have not been discussed yet.

- L448: unspecific location description with ”... roughly in the middle ...” , please specify more and check throughout the manuscript for these unclear location names. Consider also adding markers for important locations like Wasserauen station in the plots.

- L450-453: How much can we trust the results shown here, given the northerly mean wind for non-smoothed terrain at 285° ambient wind shown in Sec. 4.1, Fig. 12? How does the wind rose or the surface wind field for the non-smoothed case look like? Apart from that: could you elaborate more on the penetration mechanism in the 285° case? Is the recirculation/rotor still present in this case or is the air penetrating the valley somewhere further up-valley?

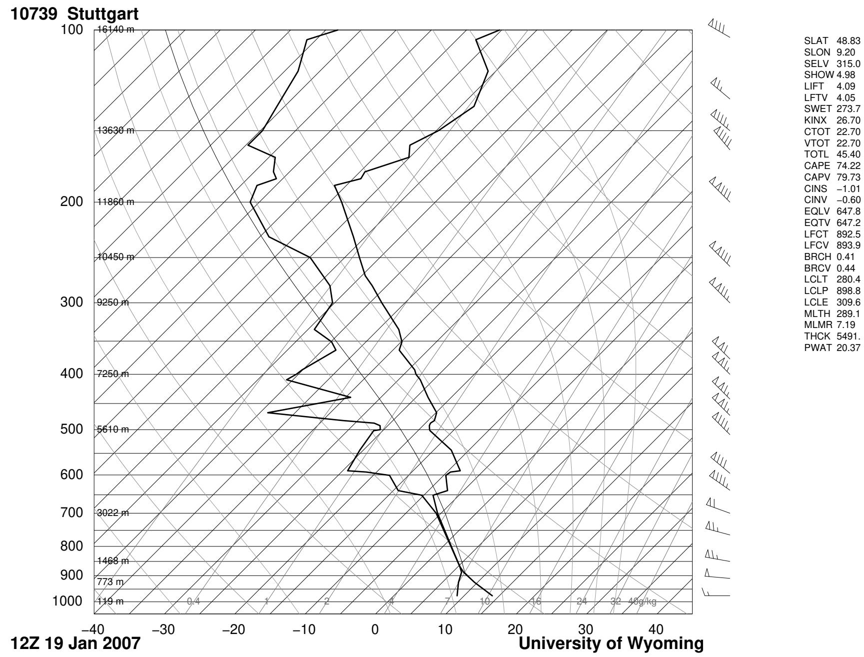

Figure 1: Radiosounding from Stuttgart on January 19, 2007 12 UTC. Taken from University of Wyoming (https://weather.uwyo.edu/upperair/sounding.html)

- L478-479: I do not understand this sentence: How can the flow in the valley resemble more the flow regime in north-westerly ambient cases, when the flow in the valley shows more northerly components but no south-easterly?
- L505: can you show the surface wind fields for the experiments in the supplement?
- L580: as mentioned earlier, the uniform profiles in wind and stratification should be added as a potential limitation here.

References

Belušić, D., M. Žagar, and B. Grisogono, 2007: Numerical simulation of pulsations in the bora wind. *Quarterly Journal of the Royal Meteorological Society*, **133 (627)**, 1371–1388, doi:10.1002/qj.129, URL `https://rmets.onlinelibrary.wiley.com/doi/10.1002/qj.129`.

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10.1175/1520-0469(1990)047⟨2853:TOOSDW⟩2.0.CO;2, URL http://journals.ametsoc.org/doi/10.1175/1520-0469(1990)047<2853:TOOSDW>2.0.CO;2.

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