Third review of "SNOWstorm (v1.0) - a deep-learning based model for near-surface winds and drifting snow in mountain environments", by Saigger et al.

General

This is my third review of this manuscript. Following the first and second revision, the authors have improved the manuscript considerably and have clarified most of my concerns. The manuscript is now in good shape, and I only have three remaining comments, which I hope will further strengthen the paper.

Specific concerns

1. *First manuscript Line 105-110: The motivation and reasoning for choosing topographies with power law spectral scaling are not made clear. Particularly, given that your approach is built on that from Helbig et al, 2017, who used Gaussian over power law topography models based on a better statistical agreement with real terrain slope characteristics, and given that power laws do not hold across spatial scales.* Regarding the first point: you are correct on our comment that SNOWstorm predictions are not transferable to other resolutions is referring to the concern that power law spectra do not hold across spatial scales. The use of our synthetic topographies follows the same idea as for Gaussian topographies: to reflect basic terrain characteristics and therefore simulate terrain-atmosphere interaction independent of geographic location. Our approach, however, concentrates on spectral terrain characteristics, and as SNOWstorm is not designed to be transferable to other spatial resolutions, the transfer problem is not a concern in our case. We changed the manuscript to emphasize this in Section 2.1 and 3.4.
   Unfortunately, I could not find a new change in Section 3.4 with regards to this. I suggest it could be made clear in Line 458-460: E.g. [..] due to the choice of power laws for training model topographies.

2. With regards to Fig. 3 showing "Distribution of the atmospheric input variables and surface conditions to drive the individual numerical simulations of the training data": Please indicate if you can really achieve numerically stable wind flow when initiating WRF with 30 m/s at 50 m grid resolution on local terrain slopes larger than 30°?

3. *Section 3.2 and Line 329-330 comments:*
   In my two previous comments, I asked the authors to justify the use of numerical simulations in LES mode for training in the context of previous wind downscaling models. In response, they presented examples of chaotic, unsteady flow features from the WRF LES training simulations under weak-wind conditions (Fig. R2), but they also state that these features are not captured by SNOWstorm, whereas SNOWstorm does capture the larger turbulent structures necessary for coupled predictions of wind and

drifting-snow variables. Could the authors show an example of such a larger turbulent structure that is simulated by WRF in LES mode and reproduced by SNOWstorm? Without such an example, it remains unclear to me how training on LES simulations improves the downscaled wind fields compared with previous approaches. Could the authors clarify this point in the manuscript by including an example similar to those shown in Figs. 6 and 7, illustrating a larger turbulent structure that is well reproduced by SNOWstorm?