

Interactive comment on “Downscaling surface wind predictions from numerical weather prediction models in complex terrain with WindNinja” by N. S. Wagenbrenner et al.

Anonymous Referee #1

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Review

This is very well-written and well-constructed, scientifically sound paper that is of benefit to the scientific community. I don't have any major issues with the paper as-is. However, some discussion of the following issues would strengthen the paper in my opinion:

1. For Figures 1, and 6-8 it would be really helpful to have the Big Southern Butte area zoomed in more. I have a hard time seeing what is going on in that region and then the rest of the plots are mostly empty with a few observations scattered downstream. What I would recommend would be to have a zoomed-in plot of the Big Southern Butte area.

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2. On the same note, it would be helpful if sensor R2, R26 discussed in Fig. 4 were indicated on the map in Fig. 1.

3. Why is the only meteorological parameter discussed in this paper wind? Is there no parallel for downscaling for other critical fire parameter: relative humidity and temperature? For the general reader some discussion of this in the intro would be helpful for the more general audience of ACP.

4. I am under the impression that strongly-forced wind events are the most important factor for high fire spread, and therefore the strengths of this study play to that need. I think this point should be made somewhere, even if it is fairly obvious.

5. WRF-HRRR fields are now available to drive the 1.33 km model. Were any tests conducted to determine the improvement in WRF using HRRR analyses to drive the 1.33 km model instead of NARR? Could this be included as potential future work?

6. I would separate the discussion more clearly into 'externally forced flows' (large-scale winds) and locally-forced flows (upslope and downslope). The current text discusses weak external forcing in many places; my opinion is that it would be better to distinguish the two cases by referring to 'locally-forced' more when the external forcing is weak.

7. Why was a 'quasi-large-eddy simulation' (200-400 m horizontal grid spacing) not conducted with WRF and compared to the other runs? This simply requires turning off the PBL scheme and a few other parameters (WRF-LES settings). A lack of numerical resources is a good argument. While the argument is made that these simulations are not feasible in real-time, the 1.33 km runs are also not feasible in real-time. If the LES run was conducted, then a comparison could be made such as 'the downscaling is equivalently good as the quasi-LES run over the area where the terrain structure is explicitly simulated. There may be issues with CFL criteria with high resolution topography in the simulations, but many investigators are starting to use WRF as a LES model with the PBL turned off...would be curious how these results would compare

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to the downscaled. In any case, a sentence or two in future work discussing how this could be done in the future would be good.

8. The potential limitations of the slope flow parameterization and discussion of a companion paper is looking at this is discussed but in a rather scattered manner. If a summary of the strengths and weaknesses of WindNinja were included in the introduction this would help make the findings in the body of the paper flow better.

9. The weakness with windninja simulating lee-side flows is an important point, as many fires occur on the 'downslope' side of mountain ranges. Does this weakness also happen during strongly forced conditions? It was not clear to me.

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