Interactive comment on "Impact of model improvements on 80-m wind speeds during the second Wind Forecast Improvement Project (WFIP2)" by Laura Bianco et al.

Jeffrey Freedman (Referee) jfreedman@albany.edu
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This paper describes the results of model improvements to the High Resolution Rapid Refresh (HRRR) model developed using observations and improved parameterization schemes developed during the second Wind Forecast Improvement Project (WFIP2).Overall, the paper is very well organized with results presented in a clear and concise manner. The breakdown of model performance (e.g., improvement) by regime is especially noteworthy. This was an enjoyable paper to review and will be of great value to the observational and modeling communities.
We thank Dr. Freedman for offering his opinion on our manuscript. We appreciate his thoughtful comments. We hope we have addressed all of the Referee's concerns and we think that our manuscript did benefit from the constructive comments made by both Referees.

General comments:

The manuscript refers to papers that are not yet available (e.g., Olsen et al. 2019a; McCaffrey et al. 2019). That made it problematic in reviewing the specifics regarding the differences between the HRRR CTL and EXP configurations (although the narrative does include parenthetical examples of the parameterizations/schemes that were modified).
Based on the comments from both Referees we decided to expand section 2.2 "NWP Models" to include a list with brief summaries of the complete set of model physical parameterizations and relevant numerical methods targeted for development in WFIP2. We still refer to Olson et al. (2019a; 2019b), which in the meantime have been accepted for publication and are available (Olson et al. 2019a as early online releases), for accurate details on the improved model configurations, but we hope this addition will give the reader all the needed tools for understanding the basic settings of the models' runs.

Although the other WFIP2 papers include a map of the instrument deployment/HRRR nests, if space were not an issue that would be helpful (readers, at times, are sometimes limited to printed versions).
We thank the Referee for the suggestion. According to the Referee's comment a topographic map, with the location of the sites, has been inserted as a new panel in Fig. 4. We hope that this and other additions we incorporated into the manuscript (see answer to the comment above) will make the paper more self-consistent.

There are several examples of text in the narrative that are figure captions.

We modified the text in the revised version of the manuscript when this was pointed out by the Referee.

Some more speculation as to why (from a meteorological perspective) model performance categorized by regime differed by season (e.g. spring versus fall for gap flows and HRRR physics) would be of interest and value.
Gap flow events are of different nature over different seasons. From our analysis it seems that in summer, thermally forced gap flow are problematic and difficult to forecast, but in winter, synoptically forced gap flows show an improvement in the model forecast. Some text about this has been added in the revised version of the manuscript.

Specific comments:

Page 1 (Abstract), line 25: use of the word "versus" perhaps should be consistent by just using "and." Page 1, line 34: ". . . also looking for the causes of model weaknesses" is a sentence fragment.
The word "*versus*" in the Abstract was changed to "*and*". Also, the sentence "*...also looking for the causes of model weaknesses*" was changed to "*Causes of model weaknesses are identified*"

Page 2, line 6: "hub-height" needs to be defined here (80 m given the other references).
Done.

Page 4, line 11: more specificity on the spin-up problems with the HRRRNEST?
The 3-km HRRR is directly initialized off of the 13-km RAP grid, so there is a spin-up period associated with the model atmosphere adjusting to the higher resolution terrain, which typically has much higher mountain peaks and lower valleys in the HRRR relative to the RAP. This spin-up problem would be even more exaggerated if the HRRRNEST was directly initialized from the RAP model atmosphere, so to minimize this problem, we chose to allow the HRRR model atmosphere to spin-up for 3 hrs before we initialized the HRRRNEST from the HRRR 3-hr forecast. New text has been added to the revised manuscript to clarify this issue.

Page 4, line 24: how "close" was the model layer to 80 m?
The text has been modified to include this info as: "*For our analysis, in order to compare to the observations, the 80-m wind field is obtained from model output horizontally bi-linearly interpolating to the 22 site locations using the 4 closest grid points, and linearly vertically interpolating the two closest heights (approximately 36 and 83 m).*"

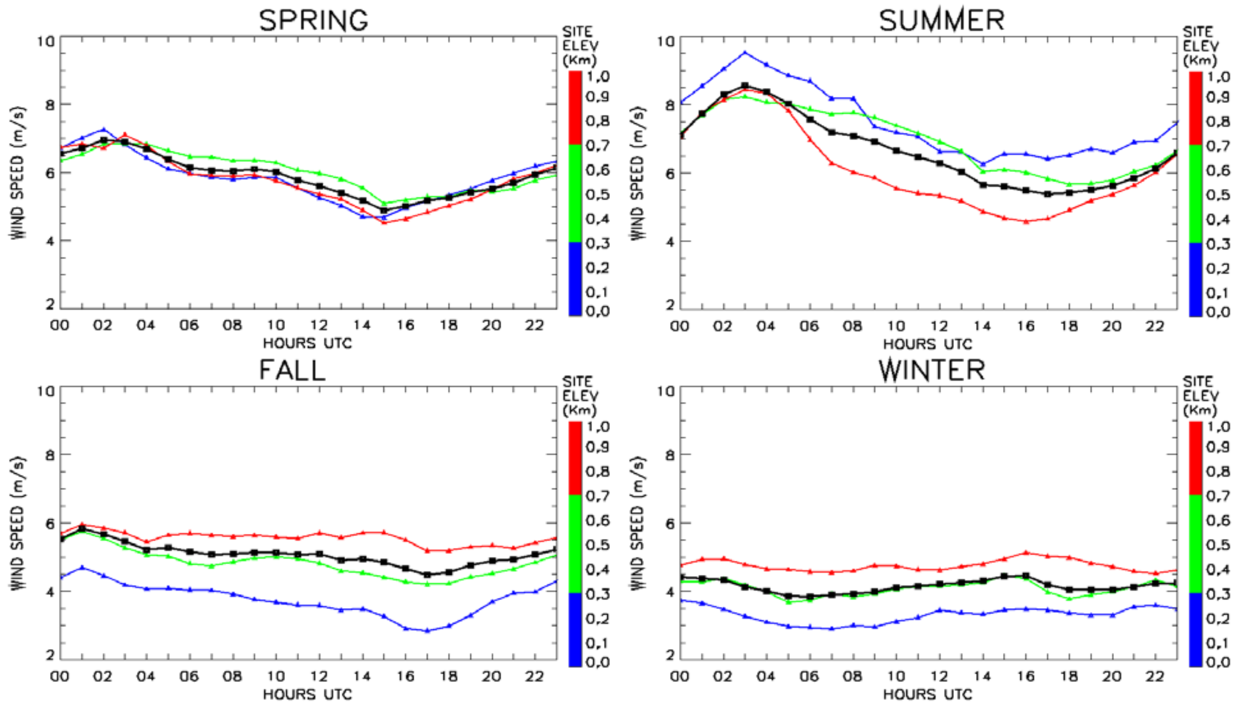
Page5, lines 6 - 7: "Initialization times. . . .theZ00 and Z12 values." This is a figure caption.
The text has been changed in the revised version of the manuscript to "*Initialization times are represented with the O's (Z00 runs) and with the X's (Z12 runs), while the averages between these values are in solid, bold lines.*"

Page 6, lines 9 - 10: "Figure 3 displays. . . ." Figure caption.

The text has been changed in the revised version of the manuscript to hopefully read less as a figure caption ("*MAEs of the 80-m wind speed, presented in the left panel of Fig. 3, show that the HRRR EXP (in blue) does better than the HRRR CNT (in red) in fall and in winter, but not in spring nor summer. MAEs of the HRRRNEST CNT (in yellow) are better than those of the HRRR CNT (in red), and the HRRRNEST EXP (in black) is now almost always better than the other models. Biases, presented on the right panel of Fig. 3, show values in the HRRR EXP (in blue) becoming way too negative (caused by the additional orographic drag employed in the HRRR EXP) compared to the HRRR CNT (in red) in the spring, summer and fall.*").

Page 6, Figure 3: any difference (in relative magnitude) if %MAE was used? That is, larger errors during nocturnal period may have been due to higher wind speeds?
We thank the Referee for making this good point. We think including the observed averaged diurnal 80-m wind speed cycle is important, but since adding a new figure was not an option due to the already large number of figures in the manuscript, we decided to include in the revised version of the manuscript an insert to panel *a* of Fig. 1, with the diurnal cycle of the averaged observed 80-m wind speeds for the four reforecast periods for reference. This new insertion shows how wind speeds are larger at nighttime, particularly in summer and to a lesser extent in spring, but less so in fall and winter. We also included some text regarding this in the revised version of the manuscript when discussing the magnitudes of the errors for the different periods (Sec 3.1: "*For reference, the insert of panel a of Fig. 1 presents the diurnal cycle of the averaged observed 80-m wind speeds for the four reforecast periods, showing that 80-m wind speeds are higher at nighttime, particularly in summer and to a lesser extent in spring (contributing to MAE to be larger at nightime compared to daytime), but less so in fall and winter.*").
To address the Referee's comments (both the current and the next comment) we made a plot (shown below but not included in the manuscript), for the four reforecast periods separately, with the averaged observed 80-m wind speeds at all sites (black line) and over an average at three elevation ranges:

- 0-300 m: AON3, AON7, BOR, RFS, ARL (in blue),
- 300-700 m: AON2, AON4, AON5, GDL, WCO, WWL, YKM, VCR (in green),
- > 700 m: AON1, AON6, AON8, AON9, CDN, DCR, PVE, RTK, GDR (in red).

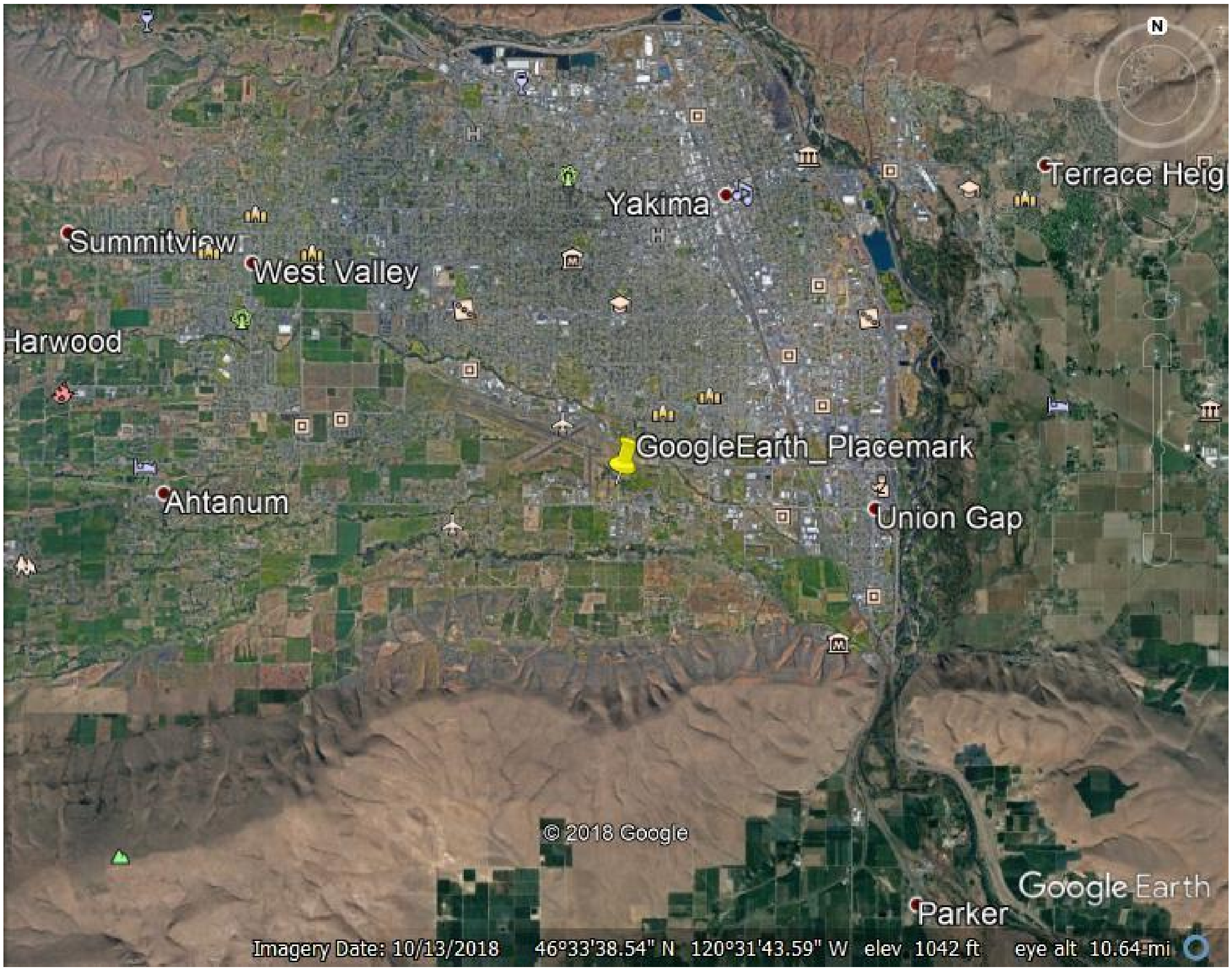


From this figure we see similar diurnal patterns for wind speed for all three elevation ranges, but more interesting is to notice that, while the sites with lower elevation (blue and green lines) experience stronger 80-m wind speeds compared to those at higher elevation (red line) in summer, for fall and winter the opposite is true. This might be due to gap flow events happening more often in summer, and cold pool events, with lower wind speeds closer to the surface and higher wind speeds above, happening more often in fall and winter. Spring does not show much difference in the diurnal behavior of 80-m wind speeds for sites at different elevations.

Page 6, Figure 4: do higher elevations feature, on average, higher wind speeds? Perhaps a plot (or part of a plot) could show the diurnal average of the wind speeds for individual stations.
To address this Referee's question relative to Fig. 4, we added (as a dotted black line) the averaged 80-m wind speed at each site for the 4 reforecast periods (panels *a* to *d* of Fig. 4). To include these extra lines we incorporated right axes to each panel. These added lines can be used to answer the Referee's comment. Specifically there is some dependence of 80-m wind speed with site elevation in fall and winter, most likely caused by cold pool events with lower wind speeds confined to lower elevations. On the other side, and also according to the figure in the answer to the comment above, we see that sites at higher elevations do not show higher 80-m wind speeds compared to that of sites at lower elevations neither in summer nor in spring.

Page 6, Figure 4: one station (ykm at 330 m) seems to have an unusually high bias any explanation for this?
The Yakima site is the one to the farthest North in the study area, as visible form the new *e* panel of Fig. 4 (included in the revised version of the manuscript). Forecasts at

this site are particularly difficult due to the presence of the developed area, (on the North-East of the site), crops on its South-East, and a very steep ridge on its south. While the elevation of the site is at ~330m, the top of the ridge is at double this elevation. These features are visible in the map below.
This is a challenging location for models to get the details correct. Also, the ridge separates Yakima from the main study area, which could lead to different results.



Page 7, lines 7 - 8: "<u>In this analysis</u>. . . ." This is interesting a "decoupling" (assuming a well-mixed PBL over the region not sure of this) of some sites at different times?
We think the text the Referee is referring to our statement that "*Terrain complexity is not as powerful of a predictor of model bias as site elevation. A similar analysis to that presented in Fig. 4 was performed but sorting the sites by the complexity of the surrounding terrain (see Table 1). <u>In this analysis</u> (not shown) the trend of 80-m wind speed MAE and bias was not clearly defined.*" The point we are attempting to make is that using the complexity of the terrain surrounding the sites to sort the elements on the x axes we do not see a well-defined trend in neither MAE nor bias. But we do not know what kind of decoupling we can make responsible for this; therefore, we did not change the text in the revised version of the manuscript.

Page 7, lines 21 - 26, sentence beginning “The upper panels display. . . .” Figure caption.

Text in the revised version of the manuscript has been modified to read less as a figure caption.

Page 7, lines 26 - 29: this is the only text describing Figure5.

Figure 5 is described in the entire section 4.1.

Page 8, bottom lines, Figure 8: caption appears to be incomplete. It does not mention this is for the combined impact.

Thanks to the Referee for catching this oversight. The label of Fig 8 has been modified to: “*As in Fig. 6 but for HRRRNEST EXP (in black) vs HRRR CNT (in red) runs, showing the combined impact on 80-m wind speed MAE of the experimental physics and finer model horizontal grid spacing.*”

Page 10, line 9: “In truth, this figure does not tell the entire story.” Literary flourish?

We are trying to keep the reader’s interest up at this point!….

Page 11, line 7: “. . .different atmospheric characteristics.” In what way? On what scale. (At the bottom of this paragraph [lines 14 - 16] there is a mention of stability and wind profiles. Is this what is meant?)

Yes, we did use the time-height cross sections of microwave radiometer temperature, winds from the radar wind profiler, and radio acoustic sounding system virtual temperature to find that the cold pool at the beginning of January is brought in by sustained easterly winds and has weaker stable stratification compared to the cold pool event in the second half of January, which is characterized by very low wind speeds close to the surface and more strongly stable stratification. According to the suggestion of Referee #1 Fig. 14 (presenting these time-height cross sections) was removed from the revised version of the manuscript, some discussion on the behavior of the models due to the different atmospheric characteristics of the cold pool events highlighted in Fig. 13 is nonetheless still discussed in the text.

Note on Page 3, lines 14 - 17 contain a repetitive clause: ",...includes 3 449-MHz, 8915-MHz radar wind profilers with radio acoustic sounding system temperature profiles,19 sodars, 5 scanning lidars, 5 profiling lidars, 4 microwave radiometers, 10 microbaro-graphs, a network of sonic anemometers, and many surface meteorological stations."

We thank the Referee for catching the repetition, which has now been removed.