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Earth System Science Data Discussions

Interactive comment on "Realtime WRF LES Simulations to Support UAS Flight Planning and Operations During 2018 LAPSE-RATE" *by* James O. Pinto et al.

Anonymous Referee #2

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The submitted manuscript was, for the most part well written and informative. The key aspect of the manuscript is the timeliness of the study as the community looks to expand observations and UAS provide lower cost alternatives to more expensive doppler and lidar profilers.

1) In Figure 1 it might be worthwhile to physically label the locations of the Saguage Canyon, the Rio Grande Canyon, and the San Juan Mountains. Furthermore, the black terrain contour is a little difficult to visualize.

2)The last sentence of the first paragraph (58-60) is a key motivation for the study and the authors might like a sentence or two of additional detail.

3)Line 69: Replace depict with deptiction.

4)Line 75: Replace value with added value as the authors are interested in how observations from the field experiment and LES domains in M2M modeling compares relative to operational forecasts.

5) Line 175: How well behaved was the integration/solution using a non-standard nesting ratio (9 as as opposed to 3 or 5) over complex terrain?

6)The reviewer is a little confused by the operational setup and using both GFS and HRRR forecasts as forcing. While Rai et al. (2019) found little issue with using NARR reanalysis as forcing for their M2M simulations, why use both? Computational expense? Avoidance of lateral boundary conditions with GFS?

7) Line 200: Please define weak in the statement "...boundary layer winds were rather weak and localized during LAPSE-RATE".

8) Lines 247-248: While wind speed and shear are obvious main drivers of the study, in comparing the simulations to observations in a Land-Atmosphere Interaction based study, the relative evolutions of temperature and dew point would be a significant component. Even in the absence of eddy-covariance stations, a sense of the relative magnitude of the surface fluxes can be ascertained from the diurnal evolution of temperature and dew point at the AWOS site with the LES data.

9) Are the date formats consistent with journal specifications (more of a question than a comment)?

10) With respect to the outflow boundary discussion on 17 July 2018 beginning on line 276, very weak winds appeared on the evening before the passage of the strong winds in the afternoon (Fig. 3). Did the strong nocturnal inversion provide enough stability to allow CAPE to be built up over SAG so that storms that formed during the day from upslope flow could propagate downwind to SAG without weakening or perhaps strengthening?

11)There are other times when the observed and modeled winds were not in agreement, particularly in the pre-dawn and early morning hours. Is this just a manifestation of the difficulties in modeling very stable boundary layers and the role of diffusion in PBL parameterizations?

12) Fig. 5 is quite difficult to follow. In the text it notes wind speed and direction but the legend in the wind plots suggest they are the cartesian velocities. What sigma level is used in the contour fill subplots of potential temperature in the same figure?

13) Along the same line, Figure 6 is also a little bit difficult to visualize. While it is difficult to handle the different sampling times of the model and the soundings, it might be advantageous to show some difference fields at the sounding times.

14) Line 338: It is noted that the model is drier that the observations. As this study is Land-Atmosphere Interaction based, a significant amount of moistening or drying comes from latent heat flux or lack thereof. It is vital in such studies that the land surface state is initialized as acuratly as possible. Along those lines, an offline LSM may have been conducted to not only to provide more accurate ICs, but to provide suitable time for adjustment, just as was done for the atmosphere.

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