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Interactive comment on "Sensitivity analysis of PBL schemes by comparing WRF model and experimental data" *by* A. Balzarini et al.

Anonymous Referee #2

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Sensitivity analysis of PBL schemes by comparing WRF model and experimental data
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Summary This paper evaluates the WRF model performance using different PBL schemes versus observations, both routine weather observations as well as observations from tethered balloons and remote sensing during a field campaign held in Milan. The paper has several deficiencies in terms of English language, methodology, overlooking the required integrated approach of the soil-vegetation-land-surface-atmosphere continuum, and lacks a significant contribution in bringing knowledge a step further. Some conclusions are not justified based on the presented findings, and are only suggestive. On the other hand, the dataset at hand is very useful for an in-depth model evaluation, and to develop model improvents

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Interactive Discussion

Discussion Paper

Recommendation: Reject

Major comments 1. The paper uses an old version of WRF which is known to have a bug in the YSU code for the stable boundary layer. This bug causes the surface layer stability not to be accounted in the calculations, resulting in insufficient stratification at night. This bug has been fixed from version 3.4.1. Therefore I think it is not useful to proceed with the current runs. The bug has been documented in (at least) these two papers:

Sterk, H. A. M., G. J. Steeneveld, and A. A. M. Holtslag (2013), The role of snow-surface coupling, radiation, and turbulent mixing in modeling a stable boundary layer over Arctic sea ice, J. Geophys. Res. Atmos., 118, 1199–1217.

Xiao-Ming Hu, Petra M. Klein, Ming Xue, Evaluation of the updated YSU planetary boundary layer scheme within WRF for wind resource and air quality assessments, Journal of Geophysical Research: Atmospheres, 2013, 118, 18 2. Methodology: The paper evaluates the PBL schemes of the WRF model but also applied nudging of the model fields to observations. In such a way it is not a fair confrontation between model results and observations. 3. Methodology: The analysis misses the opportunity isolate cloudy and cloud free conditions. Therefore the model evaluation is a rather statistical exercise, but not many new insights in the representation of the underlying physics are provided. Some days are preselected, but it remains unclear why these days were chosen. 4. The paper misses an in-depth analysis of the causes behind the model biases and behavior, although the paper could be valuable if that would have been included. The part of the atmosphere that is studied is in direct connection to the land-surface, and thus the paper should address this. Unfortunately the surface radiation and energy balance have only been marginally mentioned. As such the paper provides an incomplete picture of the full story behind the modelled soil-vegetation-land-surface-atmosphere continuum and thus insufficient new insights. In addition, many findings are explained by only superficially referring to the model formulation (local vs non-local; explicit vs non-explicit entrainment etc), but none of the findings are explained in

a physical way, with a closed argumentation and reasoning. 5. Language: the paper lacks hyphenation where necessary, and has many paragraphs of a single sentence, which is awkward. Moreover, often the phrase "over prediction" is used, while "overestimation" is more appropriate. 6. Model results in the current study should be better connected with findings in other studies. Do the current results agree with earlier results and if not do you understand the physical reasons behind it?

Minor comments P6134, ln 1: representation instead of reconstruction P6134, ln 17+18: add the measurement level, i.e. 2-m temperature and 10-m wind speed P6134, ln 18: significance: these MB values cannot be given in more than 1 significant number since the measurement uncertainty is of the order 0.1 m/s or K. Same holds for the whole results section. P6134, ln 22: Temperature cannot be important here: if the same sensible heat is provided to a boundary layer with the same initial conditions, it does not matter whether that is done in the tropics or in the artic. It is the difference in surface fluxes (or initial profile) that drive the PBL differences. P6135, ln 12-13: not necessary to introduce 2 abbreviations for the boundary layer P6135, ln 17: citation to Yerramelli should be removed since the made an wrong interpretation about the link between PBL results and air quality. See the comment associated to that paper. P6135, ln 27-28: This sentence contradicts to the previous sentence. P6137, ln 23: here and elsewhere in the manuscript: check hyphenation. Eddy-diffusivity assumption. P6137, ln 25: K is not introduced. P6138, ln 27: Rewrite "Because...." Awkward sentence P6138, ln 29: Rewrite ".. to the MM5 surface-layer scheme" P6139, ln 9: List the number of levels in the lowest 1000 m and the lowest 200 m. Justify whether these are sufficient levels to answer the research question. P6139, ln 15: List the USGS horizontal resolution used. Is the USGS landuse up-to date for Milan? USGS is old and from 1992. Please justify. P6139, ln 16: .... operational analysis.... P6139, ln 18: Justify whether 5 days of spinup was sufficient. P6139, ln 21: Cite in chronological order! P6140, ln 8: Based on... P6140, ln 28: ... at 0 and 12 UTC. P6141, ln 4: Cite in chronological order! P6141, ln 5: Use the abbreviation PBL. (also P6142, ln 10) P6141, ln 15-20: Does this aerosol production also work well in the stable boundary

layer when turbulence intensity is low? Need to be justified. Earlier studies using the same instrument found that the LD40 fails to measure the stable boundary layer depth in the first half of the night. P6141, ln 25: measurement uncertainties of the records by this instrument should be quantified. P6142, ln 2: ....inspection: what were the criteria for the decision making? P6143, ln 26: Usually MYJ is too cold and too moist in the daytime PBL. This is not mentioned, neither further analysed. P6144, ln 15-20: quantify in terms of Bowen ratio or in W/m2 P6147: Reasoning is difficult to follow. It is more a summation of findings without connection. P6147: Explanation of entrainment fluxes is handwaving since the variables have not been plotted. Idem for bottom of P6149. Surface fluxes are not studied, but entrainment fluxes are studied here. Incomplete picture! P6151: Lacking anthropogenic fluxes are brought to the table. First, this could have easily been included in WRF. Secondly the model already produces a too warm PBL, so adding anthropogenic heat will enhance the bias. P6151, ln 16-23: conclusion about surface layer scheme is not justified, only suggestive. Conditions of stability, wind speed and thus the exchange coefficients may differ since the profiles provided by the PBL scheme are not the same in all cases. P6152, ln 3: compared to what as a reference? P6152, 14-17: conclusion about entrainment fluxes is not justified since they have even not been plotted.

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