Geoscientific
Model Development
Discussions

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

Anonymous Referee #3

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General Comments: In this paper, the authors performed WRF simulations using five PBL schemes and compared the simulation results with observational data. The authors then analyzed the PBL sensitivity to the simulation results which could provide references for atmospheric community. However, I have detected several critical issues that need to be addressed. The first and foremost aspect is the novelty of the paper. Actually, in Hu et al., 2010 JAMC, a systematic investigation has been conducted regarding the PBL scheme sensitivity on both temporal and vertical variations of temperature, mixing ratio, and wind speed. The current work did not show significant improvements over Hu et al. Although two additional PBL schemes (UW, MRF) were considered in the current work, it is not clear why these two PBL schemes deserve serious considerations (e.g., are they becoming more popular in recent years? do they show potential advantages in the simulation of PBL structures?). Secondly, there is a

serious deficiency in the methodology that I believe is not acceptable. Specifically, in the WRF simulations, the authors used only 27 grid points in the vertical direction. This vertical grid resolution is far too coarse, especially considering that the authors tried to capture detailed flow structures in boundary layer. For example, as can be visually seen in Fig. 4, the simulated profiles have less than 5 grid points in the boundary layer. Given this issue, I believe all the simulation results are not reliable in the current paper, the authors should re-run the simulations with a higher vertical resolution. Lastly, this paper only discussed the sensitivity of PBL schemes on the WRF simulations and no further improvement suggestions were given. In this sense, I am not sure how relevant this work is (for the journal) and how significant its impact is (for the improvement of PBL parameterizations of WRF). In summary, I do NOT recommend this paper for publication given the above critiques. Some specific comments can be seen as follows. Hopefully these suggestions can help to improve the quality of the paper.

Specific Comments: P6134. The abstract is too long. The authors should cut it to one paragraph. P6134, line 1. "model biases in reconstructing the Planetary Boundary Layer (PBL) height", well, you also analyzed temporal and vertical variations of T, Q, U, etc. P6134, line 14. "Results show that all five parameterizations produce similar performances in terms of temperature, mixing ratio and wind speed in the city of Milan, implying some systematic errors in all simulations." This sentence is rather confusing. Why "5 PBL schemes produce similar performance" could mean "they have systematic errors"? May be the authors wanted to say 5 PBL schemes produce similar overestimation/underestimation? P6135. What did the authors try to say in this paragraph? In line 24, the authors said several studies have explored the PBL scheme sensitivity. However, in line 27, the authors said all the aforementioned studies did not discuss PBL scheme sensitivity. It is so contradictory. P6136, line 8. To my surprise, here the authors did not elaborate on a most relevant work by Hu et al., 2010. What were the main findings of their work? What aspects were not covered in their work? What is new in the current study? The authors should elaborate on these points and justify the novelty and significance of the current work. P6139, line 9. 27 vertical layers are far

too coarse, see the general comments. P6139, line 15. The authors used only one initial and boundary condition dataset (ECMWF), did the authors check other dataset like GFS? P6139, line 18. Why did the authors use a so long spin up time (5 days)? Usually, 24 hours are enough for spin up. P6139, line 20. The authors should add a map of simulation domain with location markers. Now it is not clear where are Po Valley and Milan in Figs. 1 and 2. P6142, line 1. It is not appropriate to visually determine PBL height, a more robust approach is needed. P6143, line 24. The authors observed systematic errors for T. It may attribute to the too-long spin up time which drives the modeling results away from observation. It may also due to biases in the initial and boundary condition dataset. Check them. P6144, line 7. "the most pronounced differences of HFX " why did the authors say this? It is rather confusing since this paper didn't show observational HFX. The difference is between what and what? P6147, line 4. change "some days" to "two days" P6147, line 6. I don't know what did the authors try to say in this paragraph. I can't find early morning and night profiles in the current paper.

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