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Comment on acp-2021-257

Anonymous Referee #2

Referee comment on "Investigation of near-global daytime boundary layer height using high-resolution radiosondes: first results and comparison with ERA5, MERRA-2, JRA-55, and NCEP-2 reanalyses" by Jianping Guo et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-257-RC2, 2021

"Investigation of near-global daytime boundary layer height using high-resolution radiosondes: First results and comparison with ERA-5, MERRA-2, JRA-55, and NCEP-2 reanalyses" provides validations of simulated boundary layer heights from four commonly-used reanalysis products on a near-global scale. The manuscript is nicely organized and comprehensive. Given the important role of reanalysis products in climatological analyses, in energy-focused resource assessments, and as inputs to higher-resolution models, validations such as the one presented here are essential for understanding reanalysis biases and limitations. Comments and suggestions for enhancement of this manuscript follow.

The discussion on the vertical resolution limitations of IGRA and the reanalysis products (Lines 96-99) would improve by including the numerical vertical resolutions (exact, on average, or a range) for each of these products, instead of simply stating that they are sparse. This information is provided in Section 2, but since the manuscript defines the resolution of GPS RO on Line 92 it would be helpful for comparison to have this information for IGRA and the reanalyses in this location as well.

The authors are disregarding the packaged BLH parameter from MERRA-2 and recalculating BLH in a more similar fashion to the ERA5 definition (Lines 202-207). For the benefit of reanalysis users, it is highly recommended that the MERRA-2 packaged BLH parameter is also validated along with the author-derived version. Can this comparative analysis be included?

Figure 1 is a helpful case study to assist the reader in the methodology. Could ERA5 be included in the graphic as well, instead of a brief mention in the figure caption?

Section 3.3 provides an interesting attempt to correlate BLH with near surface measurements, however it should be moved to a different location in the manuscript, as it does not involve the reanalysis products and therefore does not flow with the surrounding sections. Additionally, the enthusiasm over the correlation results in this section should be tempered. 0.39 is not a "relatively high positive correlation coefficient". Perhaps "moderate" might be a better choice.

I second Anonymous Reviewer #3 in suggesting that presenting results according to reanalysis minus radiosondes is much more easily understood that radiosondes minus reanalysis.

Specific comments:

Line 33: Suggest adding "analysis" after "air quality, weather and climate".

Line 85: Suggest rewording "And notable diurnal and seasonal cycles have been revealed" to Notable diurnal and seasonal cycles in BLH variation have been revealed".

Line 113: Elaborate numerically on "a rough consistency".

Line 184: Suggest rewording "As a result, ..." to "Using this definition, 190,013 profiles including soundings launched at both synoptic times and during IOP, spanning January 2012 to December 2019, are used to obtain the BLH in the daytime."

Line 190: Reword "undergo" to "has undergone".

Lines 205, 382, 384, 460: Change "MERR-2" to "MERRA-2".

Line 213: Give NCEP-2 its own paragraph beginning here, as was done for the other reanalysis products.

Line 363 and Figures 5-8: "Austria" should be "Australia"?

Line 377: Recommend numerically describing the seasonal differences in the bias.

Line 394: Reword "acceptable" to "more in line with the observations". Acceptable is too subjective.

Line 425, Figure 11: Change "EAR-5" to ERA5.