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Interactive comment on "On the computation of planetary boundary layer height using the bulk Richardson number method" *by* Y. Zhang et al.

Anonymous Referee #1

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The paper assesses the critical Richardson number approach for the modeling of the planetary boundary layer height (PBLH). The model defines the PBLH as the height at which some bulk Richardson number increases to a critical value (Ri_cr), and the paper focuses on the values to use for this Ri_cr. The PBLH is first determined from 4 experimental datasets using various observational methods, the model skill is then assessed by comparing the modeled PBLHs to these observed PBLHs. The novelty of the paper is linked to the extensive and careful analysis that allows the authors to propose a Ri_cr that depends on the bulk structure of the PBL: one for the very stable, one for the mildly stable, and one for the unstable ABL.

Major comments: Page 4051, lines 7-9: This part is unclear: if the radiosondes are at 15 minutes past the hour then the sounding during the next hour can always be used.

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So not sure what the authors mean by these lines.

Equation 2, second line: I see that the condition on the second gradient is taken from a reference but the authors should elaborate on the physical rationale of such condition since it seems rather ad-hoc.

Page 4051, lines 16-18: The selection of the value of δ seems rather arbitrary and seems due more to measurement accuracy. Also according to this picture there is an abrupt transition when H goes from -1 to 1 from a stable to an unstable PBL, but physically it is unclear if that actually happens. In a modeling framework, that would suddenly alter the height of the PBL by potentially hundreds of meters as the Ri_cr is switched from the SBL to UBL in the proposal model at H $\approx$ 0.

Page 4052: line 4: what is the magnitude of the drop, particularly that the drop in the figure seem to be of different magnitudes? Is it automated?

Page 4054, line 2: defining the lowest level as the PBLH seems ad-hoc and maybe these periods should instead not be used.

Page 4059, last line: The absolute bias the authors use should in fact be able to reflect the dispersion since negative and positive errors would not cancel out as with the regular bias (by the way this should be called absolute bias rather than bias). So the first part of the line should be removed.

It seems the model performance is in general sensitive to zs, so why not optimize for the value of zs also?

Minor comments: Page 4048, lines 1-2: The statement is very generic and I do not recall seeing it in Stull stated in that way. For example, when the turbulence diminished to what? It should be revised.

Page 4048, lines 13: in the SBL the buoyancy force can be positive or negative, depending on whether the parcel is displaced upwards or downwards from its equilibrium position, so please remove the word “negative” (it is the buoyancy TKE term that is on

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average negative in the SBL).

Page 4049, line 26: delete “there is even” or fix the next line to be grammatically correct.

Page 4051, line 8: add “a” before “time”

Page 4052, line 19: replace “noises” by “variability”

Page 4054, line 20: replace “classified” by “reclassified”

Page 4056, line 18: replace “replaced by” by “estimated as the”

Figure 10,11 and related figures: It would be good if the authors can homogenize the y-scales and make them similar for a given metric

The legend of Figure 11 seems wrong. For example it is unclear which part of the figures or lines correspond to the SBLs and UBLs mentioned in the caption.

Interactive comment on Geosci. Model Dev. Discuss., 7, 4045, 2014.