

Interactive comment on “Mixing layer height and the implications for air pollution over Beijing, China” by G. Tang et al.

Anonymous Referee #2

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The article presented very interesting mixing height study in the Beijing area. The study built on very good meteorological measurement data and obviously also, at least partly on measured PM concentrations, although the observed concentration values of PM_{2.5} and PM₁₀ were unfortunately completely missing from the paper. / related: visibility was used as a proxy for pollution, why not directly PM concentrations ?

The paper is certainly worth of publishing as the study itself is extremely interesting, however some improvements/corrections are suggested.

Generally: the paper is occasionally very hard/heavy to read : page 28264 is a perfect example of the general problem : whole page is filled with lists of numbers and extremely long sentences without any clear structuring, so at the end the reader is definitely losing the whole idea of the text, and it is very hard to see what all those

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numbers try to prove. In many cases these long paragraphs could and should I be replaced by one simple table/figure, which would make it easy to see what is the real point. This same problem is occurring repeatedly from the very beginning of the paper to the very end, so some rethinking and reorganizing of the content would be required – even just dividing the long chapters to paragraphs & adding some explanatory tables would already help a lot.

Detailed comments: 1:25254: “station ZBAA is 39.48° N and 116.28° E ..”

While with most other numbers (like mixing height with 10 cm accuracy?) there are obviously too many decimals given in the paper - with this for some strange reason far to few ; ~1km accuracy is not normally very acceptable with a station location ?

2: 28255: “ we defined all of the meteorological sounding profiles as a convection state when they exhibited negative lapse rates for the virtual potential temperature within 200 m and bulk Richardson number within 100 m, and the other profiles were defined as a stable state.”

Would it not be useful to consider/define also neutral cases? (See also comment below)

3: 28258: “Therefore, the near-neutral atmospheric stratification that occurs when a cold air mass passes through is the main cause for the serious underestimation in the observation results by the ceilometer.”

This seems to be a logical conclusion: however, it remains still unclear, was it really verified that these situations were truly neutral ? The paper only states that these situations were corresponding to “conditions with low relative humidity and large wind speed, with winds mostly from the north” – but no direct proof on the “neutrality” is given?

4: 28259 “After determining the reasons for the underestimations and overestimations in the ceilometer data, the results with large errors according to certain principles must

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be eliminated.”

Yes, that is an easy option – much better one would be to correct the mixing height algorithm so, that it would give better estimates in these conditions- was this approach doomed to be impossible?

5: 28259 “The elimination results are good, and this method replaces the time-consuming method of filtering the data manually, which is of great practical value for future measurements of MLH with ceilometers. . . . For overestimations, we used the date of dust occurrence based on the sand–dust weather almanac to eliminate the time periods of dust crossing when the ratio of PM_{2.5} and PM₁₀ suddenly decreases.

Now, it would be extremely important to document what was exactly done here- was the PM-ratio used at all, or just some undocumented “sand-dust weather almanac”?? This method itself seems to be one of the very useful/new things developed, but unfortunately it remains very unclear how this could be utilized in e.g. other locations?

6: 28259 “First, the effectiveness of the data must be verified after performing the elimination by the aforementioned method. The results of the evaluation indicate that the effectiveness of the data in different seasons is significantly negatively correlated with wind speed and significantly positively correlated with relative humidity” – “Effectiveness” probably not the best possible label for availability of useful MLH data. The Figure 7 indicates that the above statements may be true, but the text “explaining” the reasons for over– and underestimation fails to give a clear explanation why this is to be expected: the text should be restructured so that the reasons behind Fig.7 would come more clear.

7:28261: “To avoid the influence of data elimination on the study, we analysed the relationship between daily changes of the mixing layer and the sensible heat flux and found that the average MLH from 12:00 to 17:00 LT and the sensible heat flux were well correlated (Fig. 8) and had a correlation coefficient of 0.65, which characterizes the dominant role of radiation in the variations” ? Not really clear what the beginning of

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the sentence says : “avoid” ? ! radiation is NOT a synonym for sensible heat flux - this should be clear in the text +This would be a perfect place for an equation /reference to some well-known formulas connecting sensible heat flux & MH ? any references to well-known MH formulas!

8: 28262: “Two components are closely related to turbulent energy: the heat flux caused by ra- diation and the momentum flux generated by wind shear” Why not simply state/show how (equation?) how these are EXACTLY related ?

9: 28263: “In summary, the mountainous wind in summer causes the mixing layer to gradually decline at night, which suppresses the development of the mixing layer before noon, and the prevalence of plain winds after noon causes the mixing layer to increase rapidly. Therefore, this regional circulation leads to the concave-down variation in the fast development stage of the mixing layer in summer compared to the spring. <- Ref Figure 9. Figure 9 does NOT really show significant differences between MLH spring vs summer ? So the long discussion & strong statements seem to be not justified ? Or is there a real reason why even those small differences are so significant/important ?

10: Figure 10. fraction velocity ->friction velocity

11: 28265: Equation 1/the only equation in the paper..

If you present an equation you should explain all the terms,not just virtual(?) potential temperature Describing those all would probably soon also reveal that differentiating gravitational constant is not generally a good idea (<-error in at least in the buoyancy production and depletion term + dividing rho should be rho0..in the 3rd term ?)

12: 28265 “The turbulent transportation term does not generate or destroy the TKE, and it just moves the TKE from one position to another position or redistributes the TKE. This term remains constant at zero in the entire mixing layer” Well, the first sentence reasonably correct, but don’t quite get the meaning of the second one? Sounds like you claim the second term to be zero everywhere?

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+13:More general on the conclusion “The presented results on the atmospheric mixing layer and its thermal dynamic structure under different degrees of pollution provide a scientific basis for improving the meteorological and atmospheric chemistry models and the forecasting and warning of atmospheric pollution.”

My first impression is that “scientific bases” is not the correct term here-maybe more like “useful empirical information “ or something similar: the paper does not really present any new parameterizations or models , not probably even directly supporting that

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