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Interactive comment on "Assumptions on mixing heights influence the quantification of emission sources: A case study for Cyprus" *by* Imke Hüser et al.

Anonymous Referee #1

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The paper "Assumptions on mixing heights influence the quantification of emission sources: A case study for Cyprus" by Hueser et al. describes the impact of two different assumptions on the height of the mixing layer in a lagrangian particle dispersion model which influence the emission sensitivity and hence the contribution of individual emission source locations to a local concentration enhancement.

The paper itself is clearly structured, however in some cases it is difficult to follow the conclusions as some important classification of individual cases are not explicitly explained in the manuscript.

A key issue for my criticism is a missing or only roughly touched differentiation between boundary layer height and mixing height, as these two quantities are only describing

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Discussion paper

the same effect for well mixed convective boundary layers. The manuscript makes no distinct differentiation for boundary layer stability conditions. Stable, e.g. nocturnal or boreal winter inversion boundary layers are characterised by much lower vertical mixing or even downward mixing and the surface emission sources are not even mixed throughout the much more shallow boundary layer. For neutrally stratified boundary layers (e.g. nocturnal residual layers) also no vertical mixing takes places. Hence mixing layers as described in the paper are only representative for a fraction of the occurrence in boundary layer types. A better characterisation for the types of boundary layer, as well as a PDF of boundary layer heights along the major emission sensitivity paths (e.g. emission sensitivity larger than 0.5 or even larger than 0.1 smˆ2kgˆ-1) would help to understand the conditions for the scenarios.

Another key criticism the choice of using fire emissions for the case study together with the concept of the mixing layer. As fires represent local heat sources, the conditions of vertical motion and mixing of fire emissions are very different from other emission sources. This is one of the reasons why for fires usually effective emission heights are assumed in modelling, as fires trigger dry convection or often even pyroconvection which results in a much more enhanced vertical mixing and hence uplifting of air masses with enhanced biomass burning tracers. Therefore, the mixing layer height is a not well chosen concept for this emission type. However, for typical anthropogenic emissions from industry or road traffic, the concept is more suitable.

These two aspects should be properly discussed in a revised manuscript version. The statistical significance of the findings should be analysed for at least the climatological values.

Specific comments: Page 1, Line 19: "Local air composition is determined by transport processes....." This statement might be correct for locations which are far from emission sources. However, as a general statement this sentence is not correct, as local emissions and chemical production or destruction can be equally dominating the local air composition.

Page 2, Line 5: "...of the dispersion of an air mass by turbulence and convection in the lower troposphere." As these processes cannot directly resolved by lagrangian models on scales larger than a few kilometres, a good representation of these processes cannot be achieved. Statistical fluctuations are used as a tool to capture the main effects of these processes, only.

Chapter 2: The derivation of the equations is straight forward, such that they could be moved to an appendix, with a much more shortened explanation of the terms which are displayed in the figures (e.g. EQ2 and 4 are so similar except for the summation boundary, EQ3 and 5 except for the index in the denominator. Only EQ10 and EQ11 are of importance for the further analysis and discussion.

Page 7, line 30: What is the variability in PBL height? Here the PDF mentioned above would be a good way to illustrate this variability. Mean values usually do not help since they represent both daily and nocturnal conditions. However, as FLEXPART already provides the PBLH as output on the points of trajectory location, this information should be straight-forward to provide.

Page 8, line 5: This alternating pattern does not show a behaviour as expected from the change in diurnal and nocturnal boundary layer, which would result in larger areas with equal sign. Is this a consequence of the time integration? Are the differences statistically significant compared to the temporal internal variability?

Page 8: Line 15 to 20: Why is in the last 24 hours the difference only in negative direction and does not exhibit a gain in emission sensitivity any more? Comparing to Fig.6a the last 24 hours correspond to an increase in CO gain over Greece, however, this is not visible in Fig.2. Is this a consequence of different air masses and transport from a multitude of directions. If this is the case, then potentially a filtering of the data with respect to different wind directions or a cluster analysis of the origin of air masses is required a obtain a consistent picture.

Tab.1: Here a distinction in PBLH > MLH and PBLH < MLH height would be helpful.

Fig.3 and page 9 line 19-25: If the black line depicts the reference, it should be on the 0 line,as the difference of the reference to the reference is supposed to be zero. I would expect the black line to be the cumulative effect of all four processes, instead.

Technical points:

Page 8, line 20: time profile -> time series Page 9, line 32: Sentence structure appears wrong (verb is missing?). Page 11, line 24: ...both effects counteract with each other -> counteract each other Acknowledgements: This looks like a leftover from a preliminary manuscript version.

Section on data availability is missing.

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