

Interactive comment on “Remote sensing of the tropical rain forest boundary layer using pulsed Doppler lidar” by G. Pearson et al.

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The paper presents a study of the tropical boundary layer over the Borneo forest as an important contribution to the OP3 project. The importance is that the findings on the boundary layer dynamics and structure pose an important constraint on the previously presented analysis on the chemical exchange presented by Pugh et al. (ACP, 2009). In that paper a mixed layer depth small as 800m was used for the simulations of chemical exchange which seemed to be very shallow boundary layer depth compared to observations collected over the Amazon rainforest. Overall the paper is well written, shows some very useful analysis of the advantages as well some limitations on application of lidar technology to study the dynamics and structure of the Mixed Layer (ML). The readability of the paper could really profit from some more explanation of the specific

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terminology to facilitate the interpretation for readers with not a strong background on lidar technology. Actually this paper is really interesting to the climate and atmospheric chemistry community including those that rely on use of numerical models. One of the main findings of this paper is that inversion of the ML height (MHL) based on aerosol vertical distribution might give a too small overall MHL also probably due to some interactions between aerosols and moisture that seem to result in a much shallower domain of the main presence of the aerosols. Not so much an explanation for this observation is being provided and also not addressed in this context is how the exchange of other compounds, gaseous compounds with different lifetimes would exhibit a very different domain of well-mixed conditions. In order to come back to the applied MLH in the model analysis by Pugh et al. it would also be useful to at the end give some indication about what typical mixed layer depth one could really apply for the Borneo tropical rainforest. On the other hand, seeing the rather large range of estimates on the different applied approaches one could come to the conclusion that it is still a major challenge to really define one specific MLH.

Minor comments;

Pp 5022; Suggestion to modify sentence 15-18; These dynamics have as driving force the incoming solar radiation, the surface energy partitioning, vertical gradients of wind speed, potential temperature and moisture and the geostrophic wind.

Pp 5022; line 21; ...reactions of these species in the boundary layer and inside the canopy are strongly.

Pp 5023, line 14; instead of using here 6 million hectares it would to also express this in %, or add the percentage between brackets behind this number

Talking about the importance of forests, and in particular on tropical forests you could also refer not only to its significance in the carbon cycle but also, because this is a paper on BL dynamics and chemistry, on the importance of the tropics in chemical sources and sinks (VOC emissions, NO_x emissions and dry deposition, e.g. of ozone).

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Pp 5024; line 30, In the statement about the importance of the characterization of the MLH to estimate surface fluxes it would be useful to add that this holds for analysis that apply the convective boundary budget method to estimate surface emission fluxes from the observed concentrations whenever direct flux measurements are not available.

Pp 5025; line 3; ...a too shallow boundary layer. ...

Pp 5025; in the discussion on the results of Ganzeveld et al. (2009) included in Figure 1 it is interesting to see that this diurnal cycle inferred from the HCHO data suggests a decrease in the MLH whereas all the other curves simply suggest that at some point the MLH doesn't increase anymore. It is also stated in line 14 that there is a large collapse rate from the daytime MLH to nocturnal inversion layer depth. I would modify this by not showing the points whenever the MLH does not increase anymore to avoid misinterpretation of the mechanism of decaying boundary layer turbulence; the ML changes into a residual layer that does not grow anymore and where an new layer (inversion) builds up from below. The points simply reflect the simulated diagnostic MLH as a function of the simulated forcing. Consequently I would cut off the sentence to: ...was approximately 225 m h⁻¹.

Pp 5027; not being a specialist on the available technology to infer the MLH from LIDAR as many of the potential readers I was wondering what actually makes that the pulsed Doppler measurements are not that strongly affected by the aerosol distributions in the ML. In other words, it would be useful to shortly explain the main principle that distinguishes this particular method from the other available instruments being sensitive to the assumptions on aerosol distribution.

Pp 5028; reading over the section on the outline of the various locations of the different instruments that have been deployed in OP3 it is clear that surface heterogeneity with valleys deep as 200m and as highest point a mountain of 1093 is large and makes you wonder about the development of internal boundary layers dependent on mixing conditions and the aspect ratio of these landscape features.

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Pp 5029; line 25-26; since this campaign was focused on atmospheric chemistry processes and because it is obvious that this feature of ML dynamics and structure is relevant to the interpretation of chemical exchange I would suggest to change the sentence to: ...for use in correctly parameterising the tropical boundary layer in climate and atmospheric chemistry models. (and also change this in the introduction and conclusions). Also the chemistry community needs to be reminded on the importance of this topic for atmospheric chemistry research.

Pp 5033; lines 25-28. I think it is important to stress in this context, also as indicated in the caption of Figure that 10 that the previously published data by Ganzeveld et al. (2008) are reflecting the diagnostically calculated MLH from a single-column chemistry-climate model. These simulations have however been set-up in a way that the model reproduced the actually observed maximum height of the first inversion during the Gabriel campaign (long statement, when using this probably rephrase)

Pp 5034; lines 8-13; The statement about the location of the lidar at the bottom of the valley comes back to my point on the issue of landscape heterogeneity and how this complicates interpretation of the various measurements collected during OP3; So, reading this statement it becomes obvious that the nocturnal observations are simply not applicable to interpretation of the nocturnal observations at the GAW tower. Actually this issue of the nocturnal mixing volume and the separation between the inversion layer and overlaying residual layer is of potential large relevance for interpretation of the chemical exchange as we discussed in model analysis on the Gabriel campaign (Ganzeveld et al., 2008). It would be useful to provide some suggestions here how such features could be better organized in future campaigns such that this technology can actually help us to better assess the role of nocturnal exchange.

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