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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of a boreal convective boundary layer and its impact on atmospheric chemistry during HUMPPA-COPEC-2010" *by* H. G. Ouwersloot et al.

Anonymous Referee #1

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This paper is proposed as a contribution to the special issue entitled "Summertime boreal forest atmospheric chemistry and physics (HUMPPA-COPEC 2010)". HUMPPA-COPEC is a field campaign, conducted at the Boreal forest research station SMEAR II in Hyytiälä, Finland, during summer 2010. The paper examined here contains a description of the various types of atmospheric boundary layer (ABL) encountered during the campaign, and presents a focus on a particular day during which the dynamics of the ABL development and its impact on some chemical species are studied in more detail, through the observations (by radiosondes and at the SMEAR II tower) and numerical tools (large eddy simulation (LES), mixed layer model (MXL model) and chemistry scheme). The paper is relevant for the assessment of the boreal forest behaviour,

and more particularly regarding the impact of dynamics on chemistry. The role of large scale forcings (horizontal and vertical (subsidence) advection is underlined, and an estimation of these terms is done from the observations. It is also demonstrated that a mixed-layer model is able to reproduce the observed boundary layer, provided that the vertical stratification is appropriately taken into account. The diurnal evolution of some key species of photo-oxidant pollution is thus accurately modelled. The paper is well written and well organised, and the illustrations are pertinent and in adequate number. It is therefore pleasant to read. My recommendation is that it warrants publication in ACP, once the authors will have responded to the comments below. These comments do not question the scientific value of the paper, nor its main conclusions, but needs to be addressed in order to clarify some points in the manuscript.

Specific comments: 1. ABL types (Fig. 1 and related comments in the manuscript). 1a. The a) and b) types in Fig. 1 are presented by the authors as different ABL types, with a mixed layer extending to about 2100 m in the a) type, while in type b) it is limited to about 1000 m and surmounted by a stable (though conditionally unstable) layer. This presumed difference is amplified by the temperature scale, which differs between the two diagrams by a factor of two. When looking at the radiosonde profiles exemplifying the types, the a) profile could also be regarded as a mixed layer developed up to 700 m and surmounted by a "conditionally unstable" layer between 700 m and 2100 m. So, if the authors want to distinguish between a) and b) types, they should define a threshold temperature gradient below which the profiles could be regarded as mixed, and above which they should be regarded as conditionally unstable.

1b. There is at least a fourth profile type: the one represented by the black line on Fig. 2 (a mixed layer below a warmer residual layer). Given the importance of this kind of profile in the study subsequently conducted by the authors on the 6th August case, it is really surprising to not consider it among the ABL types.

1c. The b) profiles do not show an inversion layer at the top of the mixed layer. This reflects the absence of entrainment at the top of the mixed layer. This point should be discussed. Another possible interpretation of the b) case would be that the mixed layer in fact extends up to 2500 m and is capped by an inversion. In this scenario, the intermediate layer (between 1100 and 2500 m) belongs to the mixed layer, and behaves as a counter-gradient layer with a positive buoyancy flux, as often observed in the upper half of the boundary layer (such a layer can be represented with mass-flux numerical schemes, or with the K-gradient schemes with the adjunction of a counter-gradient term). In fact, are the a) and b) types really different?

2. Entrainment. The paper suffers from a lack of discussion about entrainment. The entrainment ratio is prescribed at a value of 0.2, without considering the possible range of values and the consequences on the mixed layer average values. Furthermore, it has been shown that entrainment is better represented by first-order model than by zero-order model, i.e. by taking into account the non-zero thickness of the entrainment layer (e.g. Pino et al., 2006; Canut et al., 2012).

3. Dry deposition. Though mentioned as one of the key processes on the scheme of Fig. 9, dry deposition (of O3, NO2, ...) values are set to zero. It should be demonstrated that these processes are low enough to not significantly alter the average mixed-layer concentrations.

4. MXL model. MXL model gives good results. However, it is not able to "continuously" describe the ABL evolution, because it needs re-starting after the residual layer has been incorporated in the mixed layer. This limitation should be underlined in the conclusion of the paper.

5. Advection. The horizontal gradients are assumed height-independent. This is a strong assumption which should be more clearly stated (I recognize it is evoked in one sentence on p. 13634, lines 26-27, but it is presented as an argument to explain the reduced impact of the advection, while it should have been presented in the description of the model). A consequence of this assumption is that the mixed-layer thickness is supposed horizontally constant.

6. Radiosonde profiles. 6a. Moisture. The accuracy of 1% on the relative humidity of the Graw system seems too optimistic to me. A value somewhere between 2 and 4% would probably be more realistic (see http://www.wmo.int/pages/prog/www/IMOP/publications/IOM-107_Yangjiang.pdf).

6b. Wind. The wind being computed from the GPS balloon positions, the accuracy depends on the time interval between the positions used for the computation. An accuracy of 0.1 m/s, as indicated by the authors, could be reached but at the expense of the vertical resolution. The corresponding altitude range should be indicated. This is an important point, because 0.1 m/s accuracy would allow a direct estimate of the horizontal wind divergence (and thus subsidence) from simultaneous profiles, like this is suggested for temperature and moisture horizontal gradients estimates (p. 13641, lines 6-8).

7. Buoyancy. The relevant parameter for ABL stratification is the virtual potential temperature, and not the potential temperature. The differences between the two can be significant in the inversion layer, or in case of small vertical gradients. The ABL types should be determined accordingly.

8. Measurement height. In the comparison between measured and modelled chemical species concentrations (e.g. Fig. 8), it is assumed that the observations taken at a single height are representative of the mixed-layer average concentrations. From the Williams et al. (2011) HUMPPA-COPEC overview paper, we know that the chemistry measurement height is 24 m, i.e. something like 10 m above the top of the canopy. We know that concentration profiles could be complex in the atmospheric surface layer, due do dynamics and chemistry (e.g. Kristensen et al., 2010). The authors should discuss whether these measurements are representative of the bulk of the mixed layer or not.

Minor and technical comments:

1. p. 13638, line 13: Why is the entrainment contribution to mixed layer NOx positive

? This would mean that the NOx mixing ratio is higher in the free troposphere than in the mixed layer. Is this compatible with “initial mixing ratios (. . .) equal in the boundary layer and the free troposphere” (p. 13635), and a surface emission of NOx?

2. p. 13638, lines 27-28: Does’nt chemistry have any role in the O3 mixing ratio evolution?

3. p. 13641, lines 19-22: I do not understand why Taylor’s frozen turbulence hypothesis is invoked, since the purpose is only to prevent from instantaneous deviations by time averaging.

4. p. 13641, lines 16-17: UHF wind profilers are also powerful tools for boundary-layer thickness monitoring (e.g. Bianco et al., 2011).

5. p. 13643, line 25: Not the free tropospheric value, but rather the value at the bottom of the free troposphere.

6. p. 13644, line 21: Knowing that subsidence is a descending motion of air in the atmosphere occurring over a rather broad area (as mentioned in e.g. the NOAA glossary), the “subsidence velocity” should, strictly speaking, be positive for descending air, which is opposite to the usual vertical velocity sign. A less ambiguous term, like “large scale vertical velocity”, should therefore be more appropriate (and would also not exclude upward large scale velocity conditions).

7. p. 13645, equations (A6) to (A8): in these formulae, q must be expressed in kg/kg. This should be indicated, because elsewhere in the text the used unit is g/kg.

8. p. 13645, equation (A7): the two members of the equation are approximately (not strictly) equal.

9. Table 3: Cp, Lv and air density are not defined.

10. Fig. 7: “Boreal” and “Amazonian” should be indicated on the diagrams.

11. Fig. 9: What represent “A” and “B”? “Entrainment” should be mentioned close to

the curved arrows.

References:

Bianco, L et al., 2011, BOUNDARY-LAYER METEOROLOGY Volume: 140 Issue: 3 Pages: 491-511.

Canut, G et al., 2012, BOUNDARY-LAYER METEOROLOGY Volume: 142 Issue: 1 Pages: 79-101.

Kristensen, L et al., 2010, BOUNDARY-LAYER METEOROLOGY Volume: 135 Issue: 1 Pages: 181-183.

Pino, D et al., 2006, JOURNAL OF APPLIED METEOROLOGY AND CLIMATOLOGY Volume: 45 Issue: 9 Pages: 1224-1243.

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