Atmos. Chem. Phys. Discuss., 15, C12692–C12698, 2016
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Atmospheric Chemistry and Physics Discussions

ACPD

15, C12692–C12698, 2016

Interactive Comment

Interactive comment on "Airborne observation of mixing across the entrainment zone during PARADE 2011" *by* F. Berkes et al.

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Received and published: 24 February 2016

We would like to thank both anonymous referees for their valuable comments on the manuscript which led to a significant improvement of the manuscript. According to the reviewers suggestions we included an additional Figure (now Figure 6). For the specific replies below we refer to the figure numbering of the original version. Referee 1 comments are given in bold, the answers in standard font. Changes to the text are in italics.

General comments: 1) I miss through the paper a clear separation between the role of synoptic and mesoscale in the presentation of the results (section 3). At section 3.1 they described the synoptic situation, but little is mentioned on the

potential arrival of anabatic winds characterize with different meteorological and atmospheric composition. I realize that it is difficult to establish a clear difference between the synoptic and mesoscale contribution, but I believe it is necessary to comment the role of mesoscale for a measurement site that is higher than 800 meters.

Indeed the mountainous terrain can influence the regional circulation around the measurement site, resulting in local flow conditions which may have an impact on the diurnal patterns of the tracer measurements. Figure 12 and Figure 13 show the diurnal cycles of CO_2 and O_3 at the surface site. The diurnal cycles are not shifted nor they vanish entirely, which one would expect from advected local "polluted" air from the valleys, if anabatic winds would play a strong role on the studied day. Additionally, during the entire day the wind direction is quite constant which is now shown in the new Figure 6.

2) Key variables like the potential temperature and wind (speed and direction) could be shown and discuss more in depth. The values of the potential temperature inversion are very large (3 and 10 K) for typical boundary layers formed over land. I believe a figure and a more elaborated discussion is needed here.

We included a new Figure 6 with profiles of the virtual potential temperature, wind speed and wind direction from the radio soundings just before and after each flight. We changed the manuscript accordingly: *Figure 6 shows radio sounding profiles of the virtual potential temperature, wind speed and wind direction from the surface up to an altitude of 3200 m before and after the flights on 6 September 2015. [...]. The wind direction within the PBL and FT was mostly constant during that day, whereas the wind speed increased within the FT and decreased within the PBL during the day.* Further discussions are given to reviewer 2.

3) At section 3.2 it is mentioned the existence of the aerosol layer. Is it impacting the transfer of radiation and the subsequent development of the boundary layer?

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How does it evolve? Can it be characterized (for instance with the aerosol optical depth)? It will be interesting to discuss the role of these observed aerosols on the boundary layer and the entrainment zone (see for instance Yu et al. J. Geophys. Res. 107, D124142 (2002) or Barbaro et al. J. Geophys. Res. 119, doi:10.1002/2013JD021237 (2014).

The height of the mentioned aerosol layer referred to the height of a temperature inversion which marks the top of the residual layer at night and at early morning. At this time we observed only low particle number concentration within the residual layer and much lower in the free troposphere. Figure 5 shows the global radiation observed on Mount Kleiner Feldberg. The global radiation increased after sunrise similar to the previous days but decreased substantially after clouds formation occurs around 9 to 10 UTC above the measurement site. We don't think that the small amount of particles have significantly impact on the radiation budget in the morning and the growth of boundary layer. We agree that there are additional interesting areas (ex. impact of cloud shadows) that require further research, but we think that this is beyond the scope of this study.

4) I have two major comments in the discussion. The first one is related to the role of clouds. In the discussion the ventilation of atmospheric compounds from the PBL intro the free troposphere driven by the mass flux is not mentioned neither discussed. In my opinion, boundary layer height and the transport by mass flux is an important contribution to the budget of the atmospheric components in the sub-cloud layer. Mass flux influences boundary layer height according to (see Equation 4 at Ouwersloot et al., 2014, J. Geophysical Research 118, doi: 10.1002/2013D020431, 2013)): (dh/dt) = we + ws + wm, [1] where (dh/dt) is the boundary layer growth, we the entrainment velocity, ws the subsidence motion and wm is the mass flux velocity. This mass flux leads to a reduction of the boundary layer growth and dominates also the vertical transport of atmospheric compounds (see Equation 3 at Ouwersloot et al, 2014). For instance, it leads

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to drying of the sub-cloud layer. The authors mentioned the descending air between the clouds resulting from the presence of roll vortices, but I believe it is necessary to include in the discussion how the mass flux influence the transport of ozone and CO2 in the studied case. In other words, it is necessary to show that the descending air between the clouds (1st paragraph at page 29188) is as important as the mass flux vertical mixing contribution. Note that once the air is introduced in the cloud layer, the stability of the environment diminish the capacity of mixing and therefore the downward transport of atmospheric components (see figure 10a at Vrezijbergh et al., Atmospheric Chemistry and Physics 9, 1289-1301, 2009)

We agree that a clear identification of the mass flux and mixing air masses would be the ideal case to identify individual contributions to the observed mixing signature in our data. We think that the vertical upward mass flux is essential for the redistribution of the tracer signatures. Indeed air masses from the surface are lifted below the clouds and may keep their signature particularly under conditions of reduced mixing, which are evident in our data. We think we flew through such air masses in regions where the cloud or parts of the cloud already dissolved. As a result one would expect large variability of tracers since the mixing has not yet homogenized tracer gradients between the different air masses involved. This is indicated by the double peak structure with the CO2 profile during the mixing event, one peak in the subcloud-layer and one peak where the cloud dissolved (as also concluded by Vila et al 2005). However, we have no additional in flight data which allow to identify the different flux contributions, but we think, that the observed tracer signature is the result from the combination of different mass fluxes and the subsequent mixing.

We included into the section 4.2: *The descending air in between the clouds can be affected by air, which has been uplifted by convective motions. The mass flux dominates mostly the vertical transport of atmospheric compounds from the PBL into the FT (Vila et al 2005, Ouwersloot et al 2013).*

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Connected to this, this section needs to be more quantitative in the description of clouds. At the last paragraph at page 29187 it is mentioned that the clouds are driven by shallow convection (typical cloud covers 20%) whereas at the second paragraph at page 29190, they mentioned that there is a large cloud fraction. The authors needs to provide and discuss of the evolution of the cloud cover during the analysed day.

We added the evolution of the total cloud cover provided by Eumetsat to the supplement. North-west of the studied region one can recognize that high and mid-level clouds are advected towards the measurement area. We extracted the total cloud cover over the measurement site from the COSMO-7 model. The total cloud cover increases from the morning to the afternoon up to 100 % in the late afternoon. Figure 4 gives an idea of the different cloud layers at different layers at 10 UTC. It is true that the cloud cover in the lower atmosphere is driven by the observed cloud streets while the total cloud cover increases during the day. We replaced "large cloud fraction" with "cumulus clouds" and the sentence on page 29190 reads now: *Since the measurements site is covered by cumulus clouds (see Fig. 4), it is obvious that particles larger than 100 nm are activated to form cloud droplets.*

5) My second comment is related to the relation between ABL growth and entrainment (last paragraph section 4.2 at page 29188). As equation [1] indicates there are other processes that influence boundary layer growth. I disagree with the statement that subsidence (ws) limits entrainment. From Equation it can be seen that assuming no clouds (wm=0), (dh/dt) can become 0 (no growth ABL) in the case that the entrainment velocity is equal to the subsidence. I other words, entrainment is still a relevant process (since we>0) in spite there is not boundary layer growth. Similar to clouds, here large scale subsidence is mentioned, but it is not quantified. I believe this information is useful to complete the case description and it can be extracted from a meteorological model (COSMO, WRF or ECMWF).

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We agree and rechecked charts from different models. The measurement site was not located within the center of the high pressure system (Fig. 4) and therefore the influence of large scale subsidence is weak. We agree with the reviewer that the subsidence might be insignificant for the boundary layer growth and entrainment in this study. We changed the section accordingly which reads now as follows:

Therefore, the growth of the PBL is an indication of entrainment from the FT into the PBL. On the other hand the increase of the temperature inversion (cause from large-scale subsidence) provides strengthening of the inversion layer and limited the further boundary layer growth, but entrainment still occurs (Ouwersloot et al, 2013).

References:
Ouwersloot, H. G., de Arellano, J. V.-G., H. van Stratum, B. J., Krol, M. C., Lelieveld, J. (2013). Quantifying the transport of subcloud layer reactants by shallow cumulus clouds over the Amazon. Journal of Geophysical Research: Atmospheres, 118(23), 13,041–13,059. http://doi.org/10.1002/2013JD020431

Vilà-Guerau de Arellano, J., Kim, S.-W., Barth, M. C., Patton, E. G. (2005). Transport and chemical transformations influenced by shallow cumulus over land. Atmospheric Chemistry and Physics, 5(12), 3219–3231. http://doi.org/10.5194/acp-5-3219-2005

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C12692/2016/acpd-15-C12692-2016-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29171, 2015.

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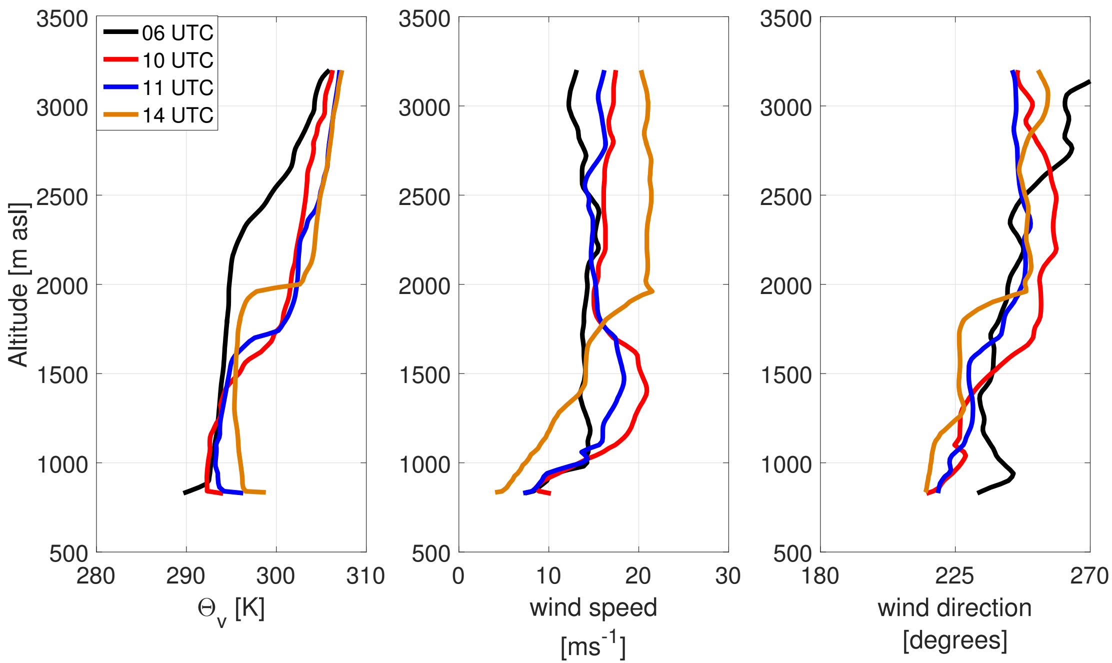

Fig. 1. Profiles of a) virtual potential temperature, b) wind speed, and c) wind direction derived from radiosondes during the day (black : 6 UTC, red: 10 UTC, blue: 11 UTC, orange: 14 UTC) on 6 September 201

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