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Interactive Comment

Interactive comment on "Generation of free convection due to changes of the local circulation system" *by* R. Eigenmann et al.

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This is the cumulative response to the received reviews.

We are very grateful for the useful comments and suggestions, provided by two anonymous referees (Ref#1 and Ref#2), which helped us to improve the manuscript. In the following, we will quote the questions of the referees and answer each of them individually.

First general criticisms of Ref#1: There appears to be some misconception regarding the free convection regime in the boundary layer. The impression throughout is given that free convection is a regime that only appears in the boundary layer when the surface wind speed drops to near zero and that suddenly large convective elements develop with a potential to generate cumulonimbus etc when this point is reached. In fact, on flat terrain, at all moderate wind speeds, provided there is a positive heat flux, there is always free convection going on somewhere in the boundary layer. The point about zero wind speed is that only then does the free convection regime reach down to the surface. In this case, individual convective circulations may be more closely related to individual ground sources. In surface layer theory for flat terrain, free convection occurs for any wind speed at all heights above the Monin –Obukhov level L. The criterion used for free convection, i.e. that z/L exceeds unity simply means that the sonic (at height z) is positioned above the level L and so is within the free convection layer which, however, will rise above the sonic at higher wind speeds (or lower heat fluxes). The fact that free convection and coherent convection elements are ubiquitous in the boundary layer is shown by its use for soaring flight. Glider pilots use coherent convective elements (thermals) in the boundary layer for long soaring flights in wind speeds greater than those measured here. The main point is that as the wind speed increases, the thermals become ‘broken up’ at lower levels (i.e. the free convection level rises) and the pilot has to be launched to (and maintain) a height greater than this free convection level to enable a soaring flight to be made. Even if the wind speed is so great (or the heat flux so small) that convection is mechanically driven throughout most of the boundary layer, clouds will still develop at the top of the layer with their own coherent circulations. Now, circumstances may be very different over complex terrain (in which case concepts like u*, L etc might be irrelevant) but if this is so, the case needs to be argued.

Answer 1: We fully agree with Ref#1, that free convection is ubiquitous in the atmospheric boundary layer (ABL) and that the applied z/L criterion (z/L<-1) for free convection means that the free convective regime drops below the measurement height z. But that is exactly the point we want to address. It is a special feature of our study that we find at our low sonic measurement heights z (about 2-3 m) values of L smaller than z, what means that buoyancy already dominates over shear near the ground. According to the equation of zeta=z/L, this is only achieved in situations where high buoyancy fluxes and simultaneously low friction velocities are measured (see numerical example in Answer 18). The low friction velocities are the consequence of the reversal of the valley wind inducing a low wind speed period. Over homogeneous flat terrain, we do not find a drop of the friction velocity, which usually increases in the course of the day. Consequently, we do not find z/L<-1 in all our datasets over homogeneous flat terrain (e.g. Mauder et al., 2006; Oncley et al., 2007) as high buoyancy fluxes alone are not sufficient to get z/L<-1 (see numerical example in Answer 18). During the period of low wind speed and simultaneously high buoyancy fluxes, the convective elements are closely related to ground sources and can more effectively transport quantities of moisture, heat and trace gases enhanced in near-ground regions into the ABL. Evidence for this was provided by Mayer et al. (2008), who found that free convection, which started from the valley bottom, led to a strong and sudden ozone decrease at a mountain summit (Hohenpeißenberg) during a zero wind speed situation. The zero wind speed period was initiated by the onset of Alpine Pumping (Lugauer and Winkler, 2005) in the morning hours. Based on this study, we wanted to identify situations in the Kinzig valley, where the frequently occurring valley wind circulation system was able to trigger free convection by inducing a low wind speed period during its period of reversal of valley wind direction. We used the z/L<-1 criterion to relate free convection with the ground sources of our targeted land use type, i.e. a corn field, which is typical in this region and had the largest dimension in comparison to other fields. The corn field (length: 260 m, width: 140 m) is located within the patchy land use structure of the Kinzig valley and is assumed to exert a major influence on ABL thermodynamics and turbulence structure because of its large scale surface inhomogeneity according to Shen and Leclerc (1995). Evidence for the effective transport of near-ground air masses into the ABL is given in our study with the enhanced surface fluxes on “event days” (Fig. 12), the enhanced vertical wind speeds measured by the Sodar (Fig. 6d-f) and the spectral analysis methods (Fig. 8 and 9), which revealed the occurrence of large convective structures starting near the ground. It was not our intention that the impression arises that each of these free convection events will lead to the formation of deep convection, e.g. cumulonimbus, above the ABL. Rather, we often pointed out that the free convection events alter the temperature and moisture profiles in the ABL by the effective upward motion of near-ground heat and moisture and thus may possibly contribute to a subsequent cloud formation, e.g. by reducing CIN (Chaboureau et al., 2004) or serving as a lifting mechanism, which releases potential instability in the ABL due to high surface sensible heat fluxes (Segal et al., 1995). A contribution to the cumulonimbus at IOP8b (Fig. 10) was only speculated upon in our study and we are aware that no explicit evidence was given for this. The speculative Fig. 10 with its related text will not appear in the revised manuscript version (see also Answers 13 and 24). The development of deep convection at IOP8b is already discussed in detail within Kottmeier et al. (2008).

Second general criticisms of Ref#1: The second criticism of the paper as it stands is that it is not concisely written – there is a large amount of material that is not very relevant to the main study, particularly in the initial sections and this becomes tedious to the reader, detracting from an appreciation of the results.

Answer 2: The revised manuscript will be extensively abbreviated according to the suggestions of Ref#1 and Ref#2. Especially the sections which deal with data quality are shortened. We thank the reviewers that they have confidence in our data quality control routines, which seem to be no longer necessary in such detail. However, former discussion with scientists always concentrated on data quality control in order to exclude bad data quality as the reason for the measured low values of z/L. This may explain our extensive discussion of data quality. Following the suggestions for shortening of Ref#1 and Ref#2, we feel that the paper now appears to be concisely written. All changes to individual sections of the revised manuscript are explained in detail within the related comments of Ref#1 and Ref#2 below.

Ref#1: The abstract almost gives the impression that the point of the paper is to show that free convection occurs in zero wind speeds – it should be made clearer that the point is to measure the variation in turbulence quantities during the transition as the

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free convection level drops below that of the anemometer – and also to give empirical data relating to the valley wind reversals. The term 'wind speed collapse' rather over-dramatises the fact that the wind speed drops to zero.

Answer 3: Please refer to our Answer 1 to the first general criticisms of Ref#1 above. We will slightly change the abstract in the revised version of the manuscript in order to meet the suggestions of Ref#1. Especially, the following sentence will be reworded: "These situations are detected by the stability parameter (ratio of the measurement height to the Obukhov length) calculated from directly measured turbulent fluxes." In the revised manuscript it will be changed to: "These situations are detected by the stability parameter (ratio of the measurement height to the Obukhov length) when the level of free convection, which starts above the Obukhov length, drops below that of the sonic anemometer." The term "wind speed collapse" will be replaced by the term "drop of the wind speed" within the entire revised manuscript.

Ref#1: Section 1: The introduction is excessively long – it should be reduced to a very few relevant sentences.

Answer 4: The introduction will be shortened in the revised manuscript. Especially, the section with the very general definition of free and forced convection will not appear.

Ref#1: Section 2.1: A brief mention of the COPS campaign is all that is necessary.

Answer 5: This section really appears to be redundant and will be deleted completely. The COPS campaign is already mentioned in the introduction and the turbulence network will be introduced in one new sentence in the experiment set-up section within the revised manuscript version.

Ref#1: Section 2.2: Again there is too much detail of the site set-up. Only the sonic anemometer description is really necessary. Is figure 1 really necessary?

Answer 6: The set-up description is reduced to the eddy-covariance complex and the Sodar/RASS system. In our opinion, the Sodar/RASS should still be mentioned as it

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is used to describe the local valley wind circulation system and also results are shown in Fig. 6. We feel that Fig. 1 is necessary as it provides information about the land use structure in the surroundings of the measurement station and about the distance between the eddy-covariance tower and the Sodar/RASS system and the extension of the targeted land use type (corn). We are of the opinion that this is more informative as a map showing the measurement site in relation to the entire Black Forest as suggested by Ref#2 (see related comment of Answer 17), because we investigate a local phenomenon (initiation of free convection near the ground) and overview maps are already shown by Wulfmeyer et al. (2007). With the help of the given coordinates, the interested reader can easily get a topographical overview on a bigger scale and e.g. relate the measurement site to the Black Forest. Figure 1 should still appear in the revised manuscript.

Ref#1: Section 2.3: It is good to know that a serious effort was put into quality control but again this section is far too detailed and long. Is Figure 2 really necessary?

Answer 7: This section will be reduced to a few relevant sentences including the citations for the applied turbulence data processing routine, the footprint analysis and the check for internal boundary layers (IBL). The data flow figure (Fig. 2) and the related text will not be included in the revised manuscript. The header of the section reduces to “Quality control effort”.

Ref#1: Section 2.4: As above. However Figure 3 actually shows some results

Answer 8: Figure 3 relates to Section 3.1 and will be included in the revised manuscript.

Ref#1: Section 3.1: Again, is so much detail really necessary to understanding the results? Is Figure 4 really necessary? It is good to know that you have checked the energy balance, but I would have thought a simple sentence saying this would suffice – Figure 5 is not really necessary.

Answer 9: This section will also be shortened in the revised manuscript. Figure 4 and its related text will be deleted. Results of the footprint analysis and IBL check will then reduce to Table 1. The paragraph of the discussion of the IBL check will be deleted. The non-closure of the energy balance will only be mentioned with the relevant references and Fig. 5 will not appear in the revised manuscript.

Ref#1: Section 3.2: This is the first really relevant section. My general comment at the start applies here – it may well be that the thermally driven valley wind circulations produce a burst of cloud when they initiate, but is this somehow related to the level terrain concept of free convection reaching down to the surface? Might it perhaps rather be the result of excess moisture trapped in the stable valley air overnight being released?

Answer 10: The point of the paper is to detect situations in which free convection is already initiated near the ground during a period of low wind speed and simultaneously high buoyancy fluxes, so that heat and moisture, e.g. trapped near the ground (as mentioned by Ref#1), can be effectively transported upwards. Mayer et al. (2008) show in their study that the criterion of $z/L<-1$ was successfully applied to explain ozone drop events on a mountain summit due to free convection starting near the valley bottom. Referring to our study, we did not intend to address the fact that valley winds themselves may produce a "burst of cloud", but that instead the reversal of the valley winds serve as the trigger mechanism for free convection starting from the ground. This near-ground generated free convection will then have a significant impact on the thermodynamic structure of the ABL. We did not intend to relate the free convection events to cloud formation (see Answer 13). We are aware that valley winds themselves may trigger clouds, but that's not the point we wanted to address in our study. We wanted to have a close look at free convection, which starts closely above the targeted land use type (corn), initiated by the valley wind reversal.

Ref#1: Some of the main results are in Figure 7, but the plots are far too small for study – if the unnecessary Figures 1, 2, 4 and 5 are removed, there will be more room to expand the plots of Figure 7 (and Figure 12 – see below).

Answer 11: Figures 7 and 12 will be expanded in the revised manuscript. Figures 2, 4 and 5 will be deleted.

Ref#1: Figure 8 is relevant but shows that there are large scale circulations throughout the period – they are not particularly limited to the period when the wind speed changes.

Answer 12: Yes, that is true. But the highest spectral density values are clearly visible within the period of free convection for the most relevant scales of about 1 to 7 min. We also think that the pattern of the shift of significant areas of enhanced spectral power from high-frequency turbulence scales towards scales of lower frequency within the wavelet power spectrum of the vertical wind speed (Fig. 8a) is remarkable. The enhanced spectral density areas within scales of about 15 to 30 min in the wavelet power spectrum of the sonic temperature (Fig. 8b) can be attributed to a slight temperature trend still present even after our trend removal routine. Fitting a higher order polynomial would reduce spectral density within these non-relevant scales.

Ref#1: My general remarks at the start apply with regard to the lack of any evidence from satellite photography that there was any cloud formation observed at the time of valley wind direction reversal. The radar evidence shown in figure 10 is far from convincing.

Answer 13: It was not our intention to relate any individual event of free convection initiated near the ground to a certain cloud formation event. Unfortunately, the measurement set-up does not provide the required data for this purpose. Fig. 10 was only a speculation that free convection events in the Kinzig valley may have contributed to the pre-convective environment of the cell which developed at IOP8b. To avoid any further misleading, Fig. 10 will not appear in the revised manuscript. We also follow Ref#2, who suggested deleting Fig. 10 in the case of a lack of more evidence for this assumption (see related comment of Answer 24), which we actually can not provide. The development of deep convection at IOP8b is already discussed in detail within

Kottmeier et al. (2008).

Ref#1: Section 3.3: Another important section. In fact Figure 11 is one of the really significant results of the experiment. It shows not only the frequency of valley wind thermal circulations throughout the observing period (results over several years would be very valuable) but also the periods of light winds during reversal and the times relative to sunrise and sunset. It is striking that the reversals occur at around the times that morning and evening transition occur over flat terrain (i.e. around two hours after sunrise for morning transition and two hours before sunset for evening transition). Perhaps the authors would like to discuss this coincidence – or even relate it to numerical modelling studies.

Answer 14: Assuming that we understand the comment correctly, we can tell that we do not find z/L<-1 in all our experiments performed over homogeneous flat terrain (e.g. Mauder et al., 2006; Oncley et al., 2007). The thermally-induced valley wind circulation is a precondition for z/L<-1, as the valley wind reversal induces the necessary low wind speed period. Therefore, the about two hour difference to sunrise and sunset is caused by the local circulation. But the generation of the local circulation and its numerical modelling is beyond the scope of this study.

Ref#1: Figure 12 is also a very relevant result and it would be worth enlarging the individual plots as with Figure 7.

Answer 15: Please see Answer 11.

Ref#1: Section 4: The conclusions. These are very relevant but my general comments at the start regarding free convection apply here. You really must explain why you think there is something special about the free convection regime momentarily reaching down to the level of your sonic and why you do not expect large convection elements possibly with related clouds at other times (and which are clearly visible in figure 8 at other times). As mentioned above, there may be other possible reasons related to trapped humidity for clouds to form at the time of morning valley wind reversal. You

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should also explain why the satellite photography (mentioned in section 3.3) does not show any strong cloud formation at the time of valley wind reversal.

Answer 16: Please refer to our Answer 1 to the first general criticism of Ref#1 and to Answer 12 (Fig. 8) and Answer 13 (Fig. 10). All comments mentioned here are explained by the answers to the corresponding comments above.

Ref#2: Figures: in my PDF copy some of the images are so small that they are almost impossible to read the scales and axes. Could the figures be made bigger / more legible. It would be helpful to have a map showing the location of the measurement site in relation to the Black Forest. Figure 1, although informative, does not help to orientate the reader.

Answer 17: The figures will appear bigger and more legible in the revised manuscript. Especially the important Figs. 7 and 12 will be enlarged. We feel that Fig. 1 is more informative as a map showing the location of the measurement site in relation to the Black Forest. Reasons for this are given in the related comment of Ref#1 above (Answer 6). Figure 1 should not be deleted in the revised manuscript.

Ref#2: The authors state that "...free convection to be induced in situations where high buoyancy fluxes and a simultaneously occurring wind speed collapse were present". Are these conditions *necessary and sufficient* for free convection to occur? Are there any instances of FC where these conditions are not met? conversely, are there instances of low zeta (or instability) when FC did *not* occur?

Answer 18: The conditions of high buoyancy fluxes and a simultaneously occurring drop of the wind speed are necessary (z/L<-1) for the occurrence of free convection initiated near the ground (z=2.29 m). A small numerical example shall underline this referring to the equation of zeta=z/L. Assuming that we have a buoyancy flux of about 250 $\mathrm{Wm^{-2}}$ (the highest value we measured during our 3 month measurement period), we will only get values of zeta of -0.4 if we assume an average friction velocity of 0.25 $\mathrm{ms^{-1}}$ during daytime. In turn, small values of the friction velocity (e.g. 0.1 $\mathrm{ms^{-1}}$) alone

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would not suffice to get values of zeta below -1 if only small buoyancy flux values (e.g. 30 Wm^{-2} for zeta=-0.78) are measured. As the cube of the friction velocity appears in the formula of zeta (Eq. 2), it is also clear that lowered values of the friction velocity have more impact on the value of zeta compared to increasing buoyancy fluxes. To sum up, a certain threshold of the buoyancy flux must be exceeded simultaneously with a drop of the friction velocity in order to measure free convection events near the ground. Free convection in general, however, is a ubiquitous phenomenon in the free convection layer of the ABL as also mentioned by Ref#1 (see related comment of Answer 1). Here, free convection, i.e. thermals or convective elements, may also occur during higher wind speed situations. However, our intention was to ensure with the criterion of z/L<-1, to detect free convection situations near the ground, which can effectively transport air masses of near-ground regions, enriched in moisture and heat, into the ABL. Instances of low zeta which are not related with free convection events could be frequently observed in the evening hours during periods of low data quality and buoyancy fluxes around zero. When the friction velocity is also very low, we get values of zeta which indicate either very stable or very unstable conditions depending on the sign of the buoyancy flux around zero. These values of zeta are not meaningful and are flagged by our data quality flagging routine due to bad integral turbulence characteristic values within the not fully developed turbulence regime at these times. This information will be added to the revised manuscript in Sect. 3.2.

Ref#2: Would it be possible to show a scatterplot of zeta (the stability parameter) against some independent measure of free convective activity, for the whole of COPS (maybe split into "event" and "non-event" days)?

Answer 19: The only ground-based remote sensing values we have available are the vertical wind speeds of the Sodar measurements. Enhanced values of vertical wind speed could be related to our measured periods of free convection near the ground (Fig. 6d-f). Unfortunately, no radiosondes were launched and no other ground-based remote sensing techniques were installed in close vicinity to our measurement site in order to calculate convective indices, e.g. CIN, CAPE or lifted index (LI). Thus, we do not have the possibility to relate an independent measure of free convective activity with our near-ground initiated free convection events.

Ref#2: Too much technical information regarding quality control: could these sections be abbreviated? Also is Fig. 2 strictly necessary?

Answer 20: The sections will be abbreviated. Figures 2, 4 and 5 will not appear in the revised manuscript. See related comments of Ref#1 above (Answers 7 and 9).

Ref#2: p11380 "...as buoyant forces (B)...then dominate over shear forces (S)..." This is not clear from the definition of zeta. The reader would also need to know that zeta = Richardson number for Ri<0.

Answer 21: This is a good point. The related sentence will be reworded in the revised manuscript version considering this comment.

Ref#2: p11383-11384 regarding differing turbulent regimes for temp. and for vertical wind. Does this interesting finding have any implications for numerical modelling, parametrisations, etc?

Answer 22: In our case, the scales (<0.5 min) in which the mentioned finding is observed are usually not parameterised in numerical models.

Ref#2: P11387 "The mean duration of...FCE[s]...is 1h and 24 min with a standard deviation of 57min" change 1h and 24 min –> 84 min. Also, this represents a quite high value for the coefficient of variation - would the authors like to comment on this?

Answer 23: 1h and 24 min will be changed to 84 min in the revised manuscript version. The duration of free convection generated near the ground depends on the duration of the reversal of valley wind direction and thus on the duration of the low wind speed period. The period of the reversal of the valley wind direction from down to up-valley winds in the morning hours was very different on individual days. On some days, the reversal took place within half an hour, but on other days it took e.g. 2 hours for the

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down-valley wind to cease and the up-valley wind to fully build up. These differences explain the high value of the standard deviation.

Ref#2: Sect 3.2, p11385, l25 This section is quite unconvincing and needs further expansion or deleting entirely. The authors state that "now its [FC's] contribution to possible cloud formation or even precipitation events over the cops area shall be discussed." Then, mention is made of satellite imagery which is not shown, and the remainder of this section is about 15 lines long - which does not justify the assertion quoted above. Also, in the Conclusions, it is stated that FCEs "may have a strong contribution to subsequent possible cloud formation and precipitation over the COPS region". This should either be removed or more evidence provided for its justification.

Answer 24: Please see the Answer 1 and Answer 13 to related comments of Ref#1. Figure 10 will not appear in the revised manuscript as well as the sentence in the conclusion quoted by Ref#2. The corresponding paragraph in the conclusions has been slightly modified in order to clarify our findings. However, the sentence in the conclusion quoted by Ref#2 must be seen in the context that we argued that the free convection events first alter the thermodynamic structure of the ABL e.g. by reducing CIN, and may only then possibly contribute to cloud formation.

Ref#2: Conclusions "Consequently, it is not possible to clarify the horizontal dimensions of the near-ground air masses destabilized in the Kinzig valley and thus the exact quantities of heat and moisture transported upwards into the ABL." Do the authors think this is a serious failing with this work, as no general conclusions or suggestions can be made regarding this aspect of the BL? I think this sentence should be re-worded, as it sells this paper short, and does not do the interesting findings the justice they deserve.

Answer 25: We do not think that this is a serious failing of our work as we detected days with free convection initiated near the ground and provided evidence that these events have an impact on ABL temperature and moisture profiles: Proof for this was shown by the enhanced surface fluxes of the “event days” (Fig. 12), the large-eddy scale character of the events (Fig. 8) and the enhanced vertical wind speeds found in the Sodar measurements (Fig. 6d-f). Our findings result in a good base for a further detailed investigation of some selected days showing free convection with the help of LES modelling techniques in combination with the experimental results of this study. But these investigations are still being worked on and need one more year before results can be published and such a paper will have another context. In order to underline and concentrate on the findings of our study and not to give such an extensive overview of things which will be investigated in the future, the relevant parts of the conclusion will be reworded and shortened within the revised manuscript.

Ref#2: Typographical comments etc

Answer 26: All typographical comments will be considered in the revised manuscript.

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