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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Parameterizations for convective transport in various cloud-topped boundary layers" *by* M. Sikma and H. G. Ouwersloot

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We thank W. Angevine for his comments that clarify the manuscript and increase the quality of the results. Below, we will respond on his comments point by point and include the changes that will be included in the revised manuscript. The reviewer's comments are shown in italic.

**General comments:**



*1. The abbreviation SCu is used for shallow cumulus. This is easy to confuse with stratocumulus. Maybe ShCu would be a better choice, or shallow cumulus could be spelled out each time.*

We agree and replaced SCu by ShCu throughout the manuscript.

*2. p.10719 line 22 and following paragraph: It is not clear to me what is being claimed here or its relevance to the rest of the paper. Are you claiming a universal relationship between cloud and core area? Should this not be affected by changes in lapse rate, surface flux partitioning, etc., at least in some extreme cases? Furthermore, what does this ratio have to do with the rest of the paper? If such a relationship holds, why do you use two different functional forms for the cloud and core parameterizations?*

With the rough relationship shown in Fig. 2b, we want to stress that not all clouds effectively transport air. Based on their characteristics, only approximately half of the clouds can be considered as active. Indeed, the exact ratio is case specific, but does not seem to deviate significantly between different cases. Therefore, this ratio (2.12) is presented to give a first impression about the relative impact. To only consider the clouds that enable vertical exchange, we further characterize the various types and present and quantify the area fraction of only the active clouds. In the revised manuscript we will update this paragraph to convey the message more clearly that $a_c$ and $a_{cc}$ are not similar and roughly differ by a factor of 2 during the phase with active convection. We will stress that the exact factor differs between conditions and that the independent parameterization of $a_{cc}$ will be derived in Sect. 3.2.

*3. There should be more attention to uncertainty and significance of the results. Some* *of the coefficients are compared to very slightly different values in the literature, but without information about, for example, the uncertainty of the fits used to derive them.*

The reviewer raises a good point. By revising and developing the parameterizations, we used a least square error fit to fit the data most accurately. This least square error was not presented in the original document, but will be introduced in the figures and their discussion in the revised manuscript.

**Specific comments:**

*1. The first sentence of the abstract suggests a more general study than what is presented. It might be better to say something like, "We investigate the representation of transport of atmospheric compounds by boundary-layer clouds..."*

We agree with the reviewer's suggestion to be more specific and changed the sentence to: *"We investigate the representation of convective transport of atmospheric compounds by boundary-layer clouds that can be applied in large-scale models."*

*2. p.10714 lines 2-4: It is not clear that forced clouds produce no transport. They can be quite deep in some cases, and may detrain. You should simply say that you neglect them here.*

Regarding the forced clouds we use the cloud classification scheme of Stull (1985). We will clarify this earlier in the paragraph. According to Stull, forced clouds do not reach the level of free convection, which normally makes them quite shallow. However, additionally we will clarify the paragraph and state that we neglect forced clouds in this



study.

*3. p.10719 line 13: Please clarify the sentence. The number of clouds decreases, the total area stays constant, so the area of each cloud must increase. Right?*

We thank the reviewer for pointing out this imprecise sentence. Instead of 'amount of forced clouds' we should have used 'area fraction of forced clouds'. Additionally, we should clarify that while $a_{cc}$ remains constant, $a_c$ decreases due to a decrease in the area of forced clouds, leading to a relative increase in the active cloud area fraction compared to the total cloud area fraction.
We changed the sentence to: *"As this transport of energy out of the sub-cloud layer affects the thermal structures, the area fraction of forced clouds decreases due to a decrease in the amount of thermals that reach the cloud layer. The area fraction of active clouds is not significantly affected by this process, while $a_c$ decreases, so that the $\frac{a_{cc}}{a_c}$ ratio increases."*

*4. p.10721 line 25: I don't understand what this has to do with an overestimate of cloud fraction. Cloud fraction must always be greater than or equal to core fraction, regardless of how well they are estimated. Please clarify.*

The reviewer is right that this sentence is out of place. Since it does not contribute to the message, we will remove it from the document.

*5. p.10724 line 9: It should be kept in mind (of the authors and readers) that the effects of segregation are usually quite small and depend on the reaction and mixing time scales. Are the effects significant here?*

We agree with the reviewer that the effects of chemical segregation are usually small. Shown by Ouwersloot et al. (2011) for clear sky conditions, deviations due to segregation are around the order of 12% for isoprene, which is within the uncertainty range of the measurements. However, in the case of cloud-topped boundary layers, the dynamical segregation can be substantial, as indicated in Fig. 5 of Ouwersloot et al. (2013). This is the background of why $\phi_{cc} \neq < \phi >$ and Eq. (12) is applied. Because the properties of escaping air differ from the mean, we have to take this effect into account. For chemistry, the effect is not quantified in this study, but will likely be relatively small due to compensating effects, as indicated by Ouwersloot et al. (2011).

References

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