

## ***Interactive comment on “Cloud optical thickness and liquid water path. Does the $k$ coefficient vary with droplet concentration?” by J.-L. Brenguier et al.***

### **Anonymous Referee #1**

Received and published: 4 April 2011

Review of the paper "Cloud optical thickness and liquid water path. Does the  $k$  coefficient vary with droplet concentration?" by Brenguier, Burnet, and Geoffroy.

Recommendation: accept after revisions

This paper discusses a relatively wide spectrum of airborne cloud observations aiming at constraining representation of cloud radiative effects in large-scale models. This problem is important, especially for the climate model evaluation of the so-called first indirect effect as pointed out in the introduction. The first author is the leader in airborne instrumentation. His group collected an enormous amount of in-cloud data over the last couple decades. The analysis presented in the paper is perhaps the best possible to

C1479

address the link between cloud microphysics and radiative transfer, and it should be published and eventually applied in large-scale models. I have a few comments that the authors should consider while revising the presentation.

Specific comments.

1. This comment applies to the general problem of the representation of cloud-radiation interactions in models of various complexity. I do not expect the authors to address my point in the revisions, but perhaps they can think about this problem and offer a new approach in the future. I would argue that the amount of data the lead author has may allow a new strategy for the representation. As shown in Eq. 1 in the paper, scattering of shortwave radiation in a cloud depends on the vertical integral of the second moment of the droplet spectrum. For reasons that are not clear to me, this integral is typically converted to the integral of the LWC divided by the effective radius ( $r_e$ ), and such a formulation is then used in models. This is ironic because rather than predicting one variable (the 2nd moment), one faces necessity of predicting independently two quantities, the LWC and  $r_e$ . Perhaps this is because one can measure relatively easily LWC and the assumption that  $r_e$  is simply close to the mean volume radius is relatively accurate. I wonder if working directly with the extinction, rather than with LWC and  $r_e$ , would be simpler. Again, I do not expect the authors to respond to this point, but I hope that their past interactions with radiative transfer community may lead to new methodologies to represent the impact of clouds on the solar radiation.

2. I feel the explanation of the difference between previous analyses and the current one, namely the instrumental artifacts, should be made stronger, with specific references to papers and figures that suggested relationships current analysis questions. Did the other papers use the probes shown to offset the results because more continental clouds have droplet spectra shifted towards smaller sizes? For instance, specific values of the  $k$  coefficient might be included in the abstract of the paper and discussed in the introduction. Such changes will better expose the significance of the results.

C1480

3. The paper attempts to contrast results obtained in the analysis of cumulus (Cu) and stratocumulus (Sc) clouds. My suggestion is that this part of the analysis, namely the difference between the two cloud systems, should be significantly strengthened. This is because these types of clouds play the key role in climate and climate change, and they are often represented in climate models using different techniques. So my suggestion is to group results for Sc and Cu separately, for instance, by replacing single-panel figures with double-panel figures, one with results for Cu and the second one for Sc. Although the figures already include such separation, various symbols used to divide the data in figures 4, 6, and 7 are too small to clearly distinguish the cloud types (and one has to remember which cloud type is associated with each acronym). Perhaps Fig. 8 can stay as it because of a clear separation of the two regimes, Sc versus Cu. Such a change should be quite simple for the authors.

4. The separation between  $k$  and  $k^*$  values, that is,  $k$  valid at a given level, and  $k^*$  derived for the entire cloud depth, should perhaps be better motivated. From the modeling point of view, the  $k$  coefficient is more relevant because models consider vertical structure of a cloud field and radiative transfer is applied in a multi-level configuration. That is, values of  $k$  parameter are needed for each level, not for the entire depth of the cloud. So what is the reason for introducing  $k^*$ ? Obviously, one can (and perhaps should) include variability of LWC and  $re$  within each of the levels and then the  $k$  coefficient should be somewhere between  $k$  and  $k^*$  (arguably, close to  $k$  for many shallow cloud layers and close to  $k^*$  for clouds spanning only a few model levels). An extreme approach would be to remove the section related to  $k^*$  altogether from the paper.

5. The statement from the Martin et al. paper cited at the bottom of p. 5187 and documenting that diluted volumes were excluded in their analysis should be brought up in the introduction. This is a very important point, with implications to the generality of  $k$  values reported in Martin et al. and often used in models. The fact that current analysis includes diluted volumes and attempts to stratify the results by the LWC and droplet concentration dilution is important.

C1481

6. An interesting aspect of Fig. 8, perhaps worth mentioning in the text, is the difference between Cu and Sc clouds beyond the separation coming from different levels of dilution. For Sc, small dilution of LWC is accompanied by a small dilution of  $N$ , with data points grouped around 1:1 line. This I think attests to the inhomogeneity of mixing during entrainment, as previously argued by the first two authors. For the Cu case, on the other hand, the data points seem to cluster above 1:1, which in my view documents more homogeneous mixing and/or the role of secondary activation of cloud droplets due to entrainment/mixing. This might be worth pointing out when discussing the figure.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5173, 2011.

C1482