

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 #2

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Overview

This is an interesting paper in which measurements used to derive parameterizations of the optical properties of warm clouds are re-evaluated and extended, providing useful information for modelers. The key results include updating the microphysical measurements from past studies in which the available measuring instruments tended to overestimate the numbers of small droplets, thereby distorting the spectral shape. In addition, the authors suggest that in clouds which are not vertically uniform, the representative droplet size used to calculate the cloud radiative properties should be calculated from vertical averages of the individual moments of the droplet spectra, rather

C3542

than averages of their combination (such as in the original definition of effective radius). This follows similar arguments as found in Brenguier et al. (2000b). Finally, the authors propose a dilution factor for the CDNC which could be applied either with different values for cumulus and stratocumulus clouds or with a single compromise value for both cloud types.

The authors also attempt to define a representative droplet effective radius parameterization for a wide range of warm cloud types. Past studies have focused on particular cloud conditions (uniform or broken stratocumulus, small cumuli, cumulus interacting with stratocumulus, drizzle conditions) on the basis of the diverse microphysical characteristics of such clouds. By combining data from a wide range of conditions, and finding no systematic variation between them, the current authors propose a common compromise parameterization to fit all warm cloud conditions.

The conclusions of this paper are worthy of publication and the results are clearly presented. However, they are set in a rather negative context where the results of previous studies are dismissed rather than building on and advancing the work done in this area. In some instances, past work is misrepresented and misquoted. Further, while past findings relating to changes in droplet spectral shape between similar clouds in different air mass conditions are dismissed as instrumental artifact, they are replaced by a non-systematic accumulation of results from a variety of cloud types, levels of dilution, mixing processes and air mass type which, when taken together, result in a compromise measure of spectral shape as indicated by the cloud system k^* value, and the spread of k^* values is as large as the spread indicated by past results. While I can see the value in the cloud system approach for vertically stratified clouds, it is not clear whether a more systematic study of different cloud types in different air mass conditions would have yielded the same mixture of k values - it may well do so, but it is not proven by this study. It would be a shame if the measurement of the k factor was seen to be a "solved" problem on the basis of this work.

This paper is worthy of publication with revisions to address the context in which the

C3543

new results are presented.

Specific points

Abstract, last line: See above: I would suggest that while the updated correction factor may be an improved estimate for use in GCMs, it may not be the "best estimate".

page 5178 line 1 to 3: This is a misquotation of the Martin et al. (1994) result, who did not conclude that there was a "correlation" between the k factor and the CDNC. What they found was that there appeared to be a distinct difference in spectral shape between the marine stratocumulus clouds in continental and maritime airmasses, and they attributed this to the different types of aerosol present. Subsequent studies (e.g. McFarquhar and Heymsfield (2001; J. Geophys. Res., D106, 28675-28698) showed similarly distinct values between clouds in different airmass types. The idea of a correlation between k and N, and its potential consequences for the Twomey effect, was first postulated by Liu and Daum (2002).

Furthermore, the Martin et al. parameterization was explicitly stated as being relevant only for marine stratocumulus clouds which are relatively unaffected by entrainment, drizzle or penetrating cumulus clouds. It was meant as an illustration that consideration of the spectral shape is important and that ideally one would wish to parameterize k for a wide range of cloud types and conditions. Subsequent studies have attempted to do this, as mentioned in this paper, although many global models still apply the simple division expressed in the original work to all cloud types, conditions and locations, largely because such detailed information is not available in the models. Most significantly, there have been very few studies made in low clouds over land, despite the division between continental and maritime airmasses in models being made generally between land and sea only.

Section 3.1: There is a good selection of different cloud and airmass types represented here, including some measurements made over land. It would have been nice to see these partitioned on the plots, although the samples of some of the categories (e.g.

C3544

stratocumulus in polluted conditions) would be quite small.

page 5184 line 4: "this instrumental artifact can generate a fictitious relationship between the k factor and the CDNC" is a rather strong statement. This paper illustrates that the FSSP-100 can result in underestimation of the k factor, particularly when the mean droplet size is smaller. However, this only implies a spurious relationship if the basic shape of the droplet spectrum remains constant (so that it is only the instrument bias which affects the shape), which is not proved in this paper, and other studies have shown is not the case. Indeed, Martin et al. (1994) showed constant k values throughout vertical profiles in stratocumulus from spectra whose mean droplet sizes ranged from very small droplets near cloud base to much larger droplets near cloud top. If the change in spectral shape between continental and maritime airmasses was only due to spurious counting by the FSSP-100 in the smallest size ranges, then surely the measured k would decrease towards cloud base in the maritime airmasses.

Section 4.2: The analysis of drizzle using the combined spectra is similar to that shown in Martin et al. and confirms that, for only light drizzle, the impact on spectral shape and k is minimal. It is not clear what range of drizzle conditions was sampled, and it would help if more information was given by e.g. showing some combined spectra for the most drizzling case. As Martin et al. showed, in stratocumulus clouds the largest drizzle drops tend to be near cloud base and it is only in that region that k is significantly affected. Thus, they concluded that the impact on the cloud optical depth, for lightly drizzling clouds, will be minimal. Heavily drizzling clouds were eliminated from their parameterization. Thus, the statement in page 5185 line 11 regarding the contradiction of the Martin et al. results is incorrect: Martin et al. showed clearly that drizzle will tend to reduce k, consistent with the present study. This will be the case for both polluted and clean clouds, although drizzle will be more prevalent in clean clouds.

Section 4.4: The term "dilution factor" is slightly unfortunate as one would tend to assume that a higher dilution factor would imply more dilution, whereas it is actually defined as (q_c/q_{cad}) . I wonder whether either a different term could be used, or

C3545

could the dilution factor be redefined as $(1.0 - q_c/q_{cad})$?

page 5187: The discussion of the effects of different entrainment-mixing processes on the spectral shape in cumulus and stratocumulus clouds is important, but I would take issue with the statement in lines 20-21 that inhomogeneous mixing "results in a decreases of the light extinction at constant LWC, hence to a decrease of the k factor". This cannot be true if inhomogeneous mixing does not change the droplet size (and hence the spectral shape remains the same) but purely reduces the droplet concentration. k must remain constant in that case. Homogeneous mixing, where all droplets are reduced in size, is more likely to change the spectral shape and hence change k. Although it is not clear which direction k will go, the data in Fig. 3 suggest that increased mixing in cumulus clouds reduces k. The statement on lines 9-10 (and line 24) of the same page, that the stratocumulus layers "exhibit lower values of k, and even the opposite trend with decreasing k values when the dilution factor increases", is not really backed up by Fig. 4. There is a wide range of k values and no clear trend with dilution factor, which is consistent with the inhomogeneous mixing process.

page 5188 lines 24-28: I'm not sure I understand the argument about the sampling differences between cumulus and stratocumulus. Clearly, stratocumulus clouds are more horizontally extensive and uniform so the sampling is more straightforward, while sampling a cumulus field is more difficult, but as long as the sampling does include some samples from the wide range of dilutions present in a field of cumuli, this will demonstrate the range of spectral shapes (and hence k values) that can result from varying levels and methods of dilution. I am not convinced that Figure 5a shows that varying sampling frequency is affecting the variability of LWC dilution ratio and k factor: surely it is likely that there really is this amount of variability at a particular level in a cumulus field, where cloud cores and edges are all present.

Section 4.5: I don't understand Figure 6 - the Martin et al standard deviations are ± 0.07 for both k values, whereas what is plotted appears to be ± 0.035 .

C3546

page 5189, lines 26-27: You would not necessarily expect the values for SCMS (small cumuli) to agree with Martin et al.'s value for stratocumulus clouds, pristine or polluted. And the polluted EUCAARI cases are cumulus clouds also, so again would not be expected to agree.

Section 4.6: Reference should be made here to Brenguier et al (2000b) where the issue of the effect of vertically-stratified clouds on the optical depth parametrisation (including re) that was originally based on vertically-uniform clouds was discussed in detail.

page 5193, line 10: In fairness, Martin et al. did not attempt to provide a "cloud system representative value" in the way in which the present study does. Their analysis was restricted to particular conditions of uniform, non-drizzling stratocumulus clouds and they pointed out that different parameterizations may be required for other cloud conditions. However, climate models applied the simple division expressed in the original work to all cloud types, conditions and locations, largely because such detailed information was not available in the models.

page 5194, lines 12-17: It is not clear what range of drizzle conditions was sampled or whether the larger drops could be ignored in a heavy drizzle case.

page 5195 lines 5-8: While there are clearly many data analyzed here, the lack of a systematic study of similar clouds in different airmass conditions, or of different clouds in similar airmass conditions, combined with the wide spread of measured k^* values, suggests that, while using a comprise value of k^* may be more justifiable at present than applying Martin et al.'s values for uniform non-drizzling marine stratocumulus everywhere in a model, more systematic studies should be done using the newer instruments and methodology suggested, in order to determine whether there really is a dependence of droplet spectral shape on airmass source in different cloud types.

page 5195 line 17-18 and 5196 line 27: As above; a correlation between k and N was not suggested by Martin et al. (1994): rather, a distinct difference in spectral shape

C3547

(for stratocumulus) was found between maritime and continental airmasses. This has been applied in different ways in different models, although commonly the distinction is simply made between land and sea (with no smooth transition in k values).

page 5196 line 2: What "morphological differences" are referred to? It is unfair to dismiss the original findings as "artifacts" without providing a systematic analysis as suggested above with which to disprove those findings.

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

C3548