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Atmospheric Chemistry and Physics Discussions

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

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General comment:

We agree with the reviewer that this study does not cover all cloud types in all air masses and geographical areas. Indeed continental Sc are missing. However, with 33 case studies of Sc and shallow Cu from Florida, NE Atlantic, NE Pacific, the Caribbeans, the Baltic sea and the Netherlands, resulting in a total of 3000 km of cloud samples, using 3 different types of cloud droplet spectrometers, it is among the most comprehensive ones. Obviously the k factor issue is not "solved", which is not our objective, but the dependence of the k factor as a function of CDNC, as proposed by

Martin et al. and as implemented in a few GCMs is seriously questioned. Our ambition is limited to that specific objective because using such a parameterization in climate models significantly mitigates the predicted Twomey effect. Consequently, we have not tried to make a review of the numerous papers published after Martin et al. 1994, considering that the Liu et al. 2008 paper already provides such a review. Our intention is certainly not to dismissed previous works but rather to show how results from studies based on FSSP-100 data are biased by instrumental limitations and samples selection issues.

Specific comments:

Abstract, last line: We agree the issue is not solved hence have replaced "best estimate" by "today best estimate"

p 5178, l 1 to 3:

Martin et al quotation: We don't understand that comment since Equ 14 to 16 in Martin et al. explicitly establishes a relationship between k and the droplet concentration NTOT. We have replaced "correlation" by "relationship" in Sec. 2 and change the sentence in the conclusion.

First reference to the Twomey effect: We don't understand either the remark that Liu and Daum were the first to mention the potential consequences for the Twomey effect in 2002, since in 1994, Martin et al. already made that explicit reference to the Twomey effect : "By changing the droplet size distribution and concentration, the optical thickness of the cloud may be altered sufficiently to change the global energy budget and thus climate (Twomey 1977; Charlson et al. 1987). It is therefore vitally important that numerical weather and climate models simulate realistically the radiative properties of these clouds (Mitchell et al. 1989)." The suggested reference to McFarquard and Heymfield 2001 has been added.

Martin et al. study is limited to marine Sc: This is an important point, and we have

added a sentence to emphasize this in the introduction.

Section 3.1 : Partitioning Sc and Cu. We agree with this comment and have plotted the two cloud types separately in Fig 6 and 7.

p 5184, l 4: k would decrease towards cloud base in the maritime air-masses. This is indeed the case in Sc, as shown in Fig 5b) and also in Fig 8 of Pawlowska and Brenguier, 2000 (Tellus 868-887) or in Fig 5 of Wood (2000).

Section 4.2 : Drizzle impact. We have added more information on tests performed to compare with Wood (2000) results Contradiction of the Martin et al. result

This is a misunderstanding. What we want to say is that the k decrease with drizzle, mostly significant in pristine Sc, goes against the k decrease in continental or polluted high CDNC clouds. The sentence has been rewritten

Section 4.4: Dilution factor. We fully agree and are not happy with this term. Following Bower and Choularton (1992) we have replaced it by the “adiabatic fraction”. Then 1 means “adiabatic”.

p 5187: k factor and homogeneity of mixing. We agree that our explanation was not correct and leave the question open.

p 5188 l 24-28: Cu versus Sc sampling. This is a common challenge in airborne sampling. Because Sc have a cloud fraction close to unity, flying ascents and descents provides long cloud samples uniformly distributed from base to top. In contrast, such a procedure is not feasible in a field of isolated Cu because of the long clear air segments between successive clouds. It is therefore necessary to proceed with series of constant level legs, but then time is the main constraint and all levels are not uniformly sampled. The figure shows a typical example of sampling you get from Sc and Cu fields.

Section 4.5 : Fig. 6 and standard deviation. This is a plot error. Moreover, we found out in Fig. 6 and 7 that the <N> range from Martin et al. for continental clouds was also wrong. Both are corrected.

p 5189 l 26-27: Martin et al. and Cu. You are right. We changed the sentence.

Section 4.6 : Vertical stratification and radiative transfer calculations We agree but move the question to the end of the conclusion.

p 5193 l 10: Cloud system representative value. It is true that Martin et al. do not explicitly mention they are deriving a cloud system representative value, but when they indicate at the beginning of Sec. 5 that "these measurements have enabled them to produce a parameterization of effective radius of layer clouds for radiation schemes in large-scale numerical models", it is implicit, since at such a scale, all cloud volumes are considered, quasi-adiabatic as well as diluted ones. The paper, however, is very cautious and explicitly mention that entrainment effects are important and that they have been ignored. The issue therefore is not to criticize the Martin et al paper which is quite rigorous, but rather the usage that has been made afterward.

p 5194 l 112-17: Drizzle conditions. Maximum values of drizzle water content are now indicated in Sec. 4.2

p5195 l 5-8 Revise the conclusion of the discussion. We agree with the way it is expressed by the reviewer and add accordingly a sentence in the conclusion.

p 5195 l 17-18: “correlation” between k and N: replaced by “k decreases when N increases”.

p 5195 l 27 : correlation again : the sentence has been suppressed.

p 5196 l 2: We don’t know what the reviewer consider a systematic analysis, because we have shown here, step by step and on a large and diverse data set, how the retrieved k values progressively improves. The two main artefacts are due to the instrument and the data analysis. The discussion about averaging k instead of LWC and extinction to derive an unbiased k estimation is not only relevant to vertical stratification, but also to horizontal heterogeneity of the microphysical fields (lines 24-25 in page 5193). This second correction can easily be tested on previous data

sets by reprocessing the data. “Artefact” might not be the right word and we changed to “biased”; “morphological differences” as well, replaced by “different levels of dilution in the sampled clouds”.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C6822/2011/acpd-11-C6822-2011-supplement.pdf

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