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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The shortwave radiative forcing bias of liquid and ice clouds from MODIS observations" *by* L. Oreopoulos et al.

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We would like to thank the referee for his/her comments. They are identified below by the referee's own numbering.

General comments: 1) In Section 2, I found the discussion of the radiative calculations very confusing. In particular, I was not clear how liquid and ice clouds are defined in the calculations. Are you saying that for each 1x1 box, you define all the clouds within that box to be either liquid or ice depending on the mean cloud top temperature of the whole box? Is this true for both the PPH and ICA calculations? Or, do you derive a mean cloud top temperature separately for the ice and liquid portions of the cloud? This section needs to be clarified and the implications of this decision discussed. If the PPH calculations are all liquid or all ice, whereas the ICA calculations are not, then

some of the bias maybe due to that factor rather than to horizontal variability. If they are both calculated the same way, but using some mean-cloud top temperature, then the relative contributions of ice or liquid to the SWCRF bias may be estimated incorrectly. Also, in the summary of the paper is the first time that “mixed” or “undetermined” clouds are mentioned –treatment (or neglect) of these clouds should be discussed earlier. On a related note, the use of the cloud fraction in the albedo calculations needs to be explicitly mentioned as well before it is discussed in the section on defining SWCRF biases. On page 10342, where you discuss the different methods of calculating albedo, presumably the PPH albedo is calculated using the mean tau, mean reff, *and* gridbox mean cloud fraction? And similarly for the IPH calculations.

We are sorry that the referee found the discussion of the radiative calculations confusing. Here are some clarifications: No, we do not define all clouds to be either of liquid or ice phase based on the mean cloud top temperature (we would have given a temperature threshold to separate the phases if we were doing this). Each cloudy pixel within the 1° degree gridpoint is assigned a phase based on a variety of tests in the MODIS phase determination algorithm: liquid, ice or undetermined (we had incorrectly also included mixed phase as a possible outcome in our text, but have now corrected this). A mean cloud optical thickness, effective radius and cloud fraction is calculated for each of these cloud phases (this is now explicitly stated) by averaging the retrieved values of only the pixels with the appropriate phase (in the case of cloud fraction, the number of pixels of a certain cloud phase is divided by the total number of pixels – clear and cloudy – belonging to the 1° gridpoint). There is no separate gridpoint mean cloud top temperature (CTT) product for liquid and ice phase (we stated that explicitly in the text, lines 7-9 of p. 10341); only a mean CTT for all cloudy pixels. For this reason we use 2D histograms of optical thickness – CTT which exist separately for each phase to obtain the mean CTT of liquid and ice cloudy pixels. The mean cloud properties, but not the cloud fraction, is used for the PPH albedo calculation of each phase , i.e., the albedo is only for the cloudy portion of the gridpoint. We made clear (we refer explicitly to their binnings) that separate 1D optical thickness and 2D histograms

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of optical thickness-effective radius are available for each phase. These, along with the CTT mean temperature of the individual phase are usde for the ICA albedo calculations which correspond once again to the cloudy portion of the gridpoint only. The cloud fraction (common for the PPH and ICA calculations) only enters the SWCRF bias calculation (Eq. 3). We now make this explicit in the text on two occasions. Finally, undetermined cloudy pixels which typically make a very small fraction of the retrievals are ignored, i.e., no PPH or ICA calculations are performed on their means or histograms.

2) This paper is an extension of work first reported in an earlier paper (Oreopoulous et al. 2007) and that paper is cited extensively in discussing the methodology, etc. However when I tried to compare the results presented in this paper to those in the earlier paper, I was confused about which numbers I should even be comparing. It would be useful to explicitly compare the results in this paper to those in the earlier paper. If the estimates for SW forcing of liquid clouds have changed it would be useful to discuss whether this is due to changes in the methodology or to a larger sample being examined.

The previous paper did not deal with ice clouds and presented the PPH bias of liquid clouds more in terms of the albedo bias rather the SWCRF bias, so there are not many results to compare to. Moreover, the SWCRF bias analysis was conducted somewhat differently this time around. This is why with respect to the previous paper we only compare directly ratios of overpass to daytime and diurnal SWCRF biases. In the previous paper we essentially used the “CF/no FO” methodology (we mentioned this in p. 10347) which is not prominent in this paper. Also, in Oreopoulos et al. (2007), when calculating the global averages, the zero contributions of non-illuminated areas were not taken into account yielding higher values. This is one of the reasons the current values (black bars in Fig. 3 –overpass) are smaller than their counterparts previously reported (white bars of Fig. 5 in the previous paper) for July and January liquid clouds. Additional reasons for the smaller values in the current paper are the influence of effective radius variability which reduces the bias (included in Fig. 3, but not in the old

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Fig. 5), Collection 5 vs. Collection 4 differences in optical property retrievals and cloud fractions (smaller in Collection 5), and (perhaps) interannual variability. Because of all these reasons the shapes of zonal SWCRF bias July and January liquid curves in old Fig. 7b and new Fig. 5 match, but not the magnitudes.

Minor comments: 1) Introduction; The authors jump right into discussion about the magnitude of the PPH bias in the opening sentences of the paper. A sentence or two about why the reader should care about this bias (e.g. that it affects global climate modeling results) would be useful to motivate the study.

We agree and have expanded the introductory paragraph accordingly.

2) Introduction; P 10339, line 6, The use of the word "assumed" in the sentence starting "This so called Plane-Parallel Homogeneous (PPH) bias" is confusing; as it sounds like you are assuming a value for the bias rather than stating the value of the bias in previous results.

We reworded to avoid the possibility of confusion. In our original wording the word "assume" was obviously used under its meaning of "take on".

3) Introduction; Please define what you mean by cloud radiative forcing when it is first introduced, as it can have multiple definitions in the literature.

Done. We use the definition of the AMS glossary.

4) Section 2, p. 10344, line 10-12. Does this sentence mean that gridpoints with no illumination are not included in the averages, or that they are included but with a value of zero?

Included with a value of zero. The reviewer is right that the sentence as originally written can be interpreted both ways. We have now rephrased as follows: "Zonal and global averages of the gridpoint monthly values are trivially estimated as in Oreopoulos et al. (2007), but in this case gridpoints not receiving solar illumination are taken into account with zero contributions to the averages in contrast to the previous paper where

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they were ignored."

5) P. 10347, line 13 Replace "where indeed" with "were indeed"

Done.

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