

Interactive
Comment

Interactive comment on “Using surface remote sensors to derive mixed-phase cloud radiative forcing: an example from M-PACE” by G. de Boer et al.

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Review 2 (Bernhard Mayer)

Referee 1 did a great job in identifying all weaknesses of the manuscript. There is little to add and I essentially agree with his conclusions: The manuscript shows an interesting retrieval approach for mixed-phase clouds. The dataset itself is small and hence, in itself, not a major contribution for the scientific community. If the retrieval together with its uncertainties is properly described, then I would recommend publication, after the comments of referee 1 have been addressed.

We thank the reviewer for his comments. Hopefully the changes made, as described

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above, will increase the reviewer's excitement about the article.

Some specific points (most likely already addressed by referee 1):

Page 12490, line 20: This comment addresses the rationale of the manuscript: It is puzzling that the authors consider HSRL, Radar, and Microwave Radiometer standard equipment which - in combination with a radiative transfer model - may replace a pyranometer. In fact, broadband irradiance observations are two orders of magnitude cheaper and easier to operate than the instrumentation and retrievals presented here. In that context it also is surprising that the authors allow an uncertainty of $\pm 25\%$ for the surface albedo, instead of trying to observe it. Surface albedo is certainly not trivial to observe, in particular since a value representative of a larger area around the observation site is required for radiative transfer modeling, but it shouldn't be more difficult than measuring cloud microphysical profiles.

Perhaps we worded this in a manner that was easy to misunderstand. Certainly, ground-based radiometric measurements are more straightforward and cheaper than the suite of measurements and instruments described here. We are not suggesting the current method as a replacement for ground-based radiometric measurements. What we are doing is providing a framework that can be used in cases where the cloud measurements are readily available but the radiation measurements are less reliable. While this does not happen frequently, there are some datasets where this would provide radiative measurements to fill gaps in radiometric data, providing an increase in continuity. Additionally, as discussed in the text, this framework can serve as a testbed for retrievals when ground-based radiation measurements are available, allowing for the evaluation of individual cloud property retrievals.

Page 12494, line 10: Some information about the model is missing which is relevant for the study: Which parameterizations for water and ice clouds were used? Were it the ones in equations (3) and (4)? If not, why weren't consistent param-

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eterizations chosen for the analysis? The choice of the parameterization is particularly important for the ice clouds because an assumption about the particle shape must be made.

We're not quite certain what parameterizations the reviewer is talking about. No atmospheric model is employed beyond the use of RRTMG to calculate radiative transfer. RRTMG requires profiles of liquid and ice cloud water content and liquid and ice particle sizes, along with temperature, SZA and albedo information, to calculate the radiative profile. There is an assumption about ice crystal habit that goes into the determination of ice crystal size with the current set of instrumentation, and a sentence about this has been added to the text, along with a reference to Mitchell's 1996 paper that discusses these power-law relationships.

Is the model plane-parallel or does it use a pseudo-spherical or spherical correction? For the solar zenith angle range used in the manuscript (75 ° - 90 °) a correction for the sphericity of the Earth is required in order to obtain accurate results.

RRTMG assumes a plane-parallel atmosphere. We agree with the reviewer that this causes problems at SZA > 75 degrees, increasing direct solar attenuation, and reducing direct light at SZA > 90. Statements to this effect have been added to the text in sections 2.1.1 and 3.2.

Figures: The Figures are generally too small, at least in the “printer-friendly” version. Please make sure that they appear large enough in the final version. The “whiskers” and “open circles” are a real challenge for the human eye.

The amount of information included in figures such as Figure 3 is challenging to portray cleanly. Here, the main challenge is to ensure that the groupings of bars (case-by-case) are separated enough to be able to associate two bars with one another (see comment from reviewer 1), while still keeping the bars large enough to be visible. The figure has been formulated to be precisely one column wide. We suspect that the “printer-friendly”

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version messes with the dimensions somewhat due to the decrease in vertical extent available with the pages of this version. Therefore, the final figures should be larger and more legible. We will gladly work with the journal to ensure that the figures are of adequate size.

Figure 3: What causes the huge spread in case 6? This is not obvious from Figure 2.

After investigating this, we realized that there was an error in the netcdf file for this case. After re-generating all files, the variability has been dramatically reduced. All numbers have been updated to reflect the changes from this recalculation.

Figure 6: As admitted by the authors, showing the cloud radiative effect as a function of optical depth does not really make sense if the data were not separated into solar zenith angle bins. The number of data points is not large, but maybe 5 ° bins would be possible?

We have updated this figure, providing a row of figures using normalized shortwave cloud forcing to eliminate the dependence on SZA.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C7011/2011/acpd-11-C7011-2011-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 12487, 2011.

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