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Atmospheric Chemistry and Physics Discussions

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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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 it's uncertainties is properly described, then I would recommend publication, after the comments of referee 1 have been addressed.

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.

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 parameterizations 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.
- 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.

Figures: The Figures are generally to 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.

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

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?

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