**Atmospheric
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Techniques
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Interactive comment on "Ground-based retrieval of continental and marine warm cloud microphysics" *by* G. Martucci and C. D. O'Dowd

Anonymous Referee #1

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General Assessment

This paper describes a multi-instrument retrieval of cloud microphysical parameters in the framework of CLOUDNET and beyond, with the stated goal of improving climatologies of cloud (shortwave) albedo (if I understand the motivation/introduction correctly). I agree with the authors that it is important to study cloud albedo and its dependence on various cloud microphysical parameters, which in turn are affected by various aerosol effects. But I doubt that the paper offers a solution to better global observations of this parameter, for two reasons: (1) While ground stations do offer long time series of important parameters, there are not enough of them to be globally representative; I would argue that a better use of ground stations would be to validate and improve

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existing satellite retrievals, or to establish local climatologies; (2) Since the main motivation for this work seems to be the radiative effect of clouds (albedo), it is astonishing that the authors chose this particular combination of instruments. I will now explain:

Shortwave cloud albedo depends primarily on cloud optical thickness (not cloud thickness, as stated by the authors) and effective drop radius. In the near-UV, it also depends on cloud top height. (In gas absorption bands, e.g., water vapor bands, it also depends on gas concentration.) The most important parameter: cloud optical thickness (column-integrated extinction), is related to the second moment of the drop size distribution, or $<r^2>$; the second most important parameter, the effective radius, is the third divided by the second moment, $<r^3>/<r^2>$, which is also explained in the paper. With the instrumentation used by the authors, the available moments of the cloud drop size distribution are $<r^6>$ (radar); $<r^3>$ (microwave). The second (and most important!) moment, $<r^2>$, is supposedly provided by the lidar/ceilometer, but section 3.2 and Fig. 3 are (at least in this version of the manuscript) inadequate to explain how the full vertical profile of extinction can be derived even in regions where the lidar is completely attenuated. It is also not clearly described how the assumed lidar ratio is justified; literature where extinction is derived from the backscatter ratio without assuming a lidar ratio should be consulted and cited (check numerous papers by Eloranta), and a better justification should be given why it can simply be assumed. Also: How was the lidar/ceilometer calibrated by a sunphotometer, and which parameter was calibrated? This calibration certainly does not apply for cloudy conditions, because sunphometers determine aerosol optical thickness if the direct beam is not attenuated - they are therefore inadequate for the retrieval of cloud parameters (except in the so-called zenith-pointing mode introduced for AERONET: see Chiu, J. C., Huang, C., Marshak, A., Slutsker, I., Giles, D. M., Holben, B. N., Knyazikhin, Y., and Wiscombe, W. J.: Cloud optical depth retrievals from the Aerosol Robotic Network (AERONET) cloud mode observations, J. Geophys. Res., 115, D14202, 2010.). The most confusing statement is that about the extrapolation of lidar-derived extinction profiles from the lower regions of the cloud into the higher regions where the lidar signal is attenuated. In sum: Although

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the most relevant parameters for cloud albedo are optical thickness and effective radius, the second, and most important, moment of the size distribution is problematic for the aforementioned reasons. A more appropriate method for determining cloud optical thickness would be the method by Chiu et al., JGR, 2010, or by McBride et al., ACP, 2011. Both methods use ground-based shortwave radiance measurements, which by definition are more appropriate for climate-relevant cloud observations because they determine second (optical thickness) and third over second moment (effective radius) directly.

Despite all of these severe issues, I do think that this paper has substantial merit, albeit not for studies of the cloud radiative effect. It warrants publication after major changes. Most importantly, I highly encourage the authors to change the motivation of the paper. Rather than claiming to improve climatologies of, e.g., albedo, I would suggest to focus on cloud process studies and satellite validation work (a predecessor study would be that of Brandau et al.,2010). An obvious application would be a comparison with CloudSAT and CALIPSO active retrievals, rather than trying to match MODIS observations which are based on visible, near-IR and IR passive retrievals and thus have less in common with the techniques or wavelengths used in this paper.

Major Comments

(1) The description of MODIS retrievals is erroneous (p4843,l17-20): It is not calculated from the emission at two wavelengths, at least not in the daytime retrieval. In the daytime retrieval, MODIS obtains optical thickness and effective radius from the combination of *reflectances* in two channels in the very-near infrared, and in the near-infrared. Also, the sentence on line l19-20 (region that is 'responsible' for emission) should be revised. The dependence of the MODIS cloud retrievals on the vertical profile is described by Platnick, S.: Vertical photon transport in cloud remote sensing problems, J. Geophys. Res., 105, 22919–22935, 2000

(2) In the introduction (most importantly on page 4827, l9-17), terms "uncertainty" (of

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which parameter?), forcing, albedo, absorption, and emission, are used extensively, but it is unclear which of these parameters the authors are intending to improve. At one point, they talk about the balance between shortwave cloud absorption and longwave absorption; at another, the point out the importance of cloud albedo. Cloud albedo, cloud absorption, and cloud emission, *all* play a role in the cloud energy budget, and the wording of the introductory paragraphs are confusing in this regard. Cloud forcing is yet another parameter which is not directly related to cloud albedo; while cloud albedo is F_up/F_dn, cloud forcing is (F_dn-F_up)[cloudy] - (F_dn-F_up)[clear]. I am not sure that the authors are fully aware of these 'ingredients' of the clouds' energy budget.

(3) Two clouds were singled out in the analysis. Is there a reason for choosing two particular clouds rather than a larger data set?

(4) There are many assumptions and parameterizations used throughout this paper. And yet, none of these is questioned in the sensitivity analysis of the paper. For example, the ramifications of fixing the lidar ratio, or formula (6) with various underlying assumptions (such as that of a Gamma cloud drop size distribution) are not considered in the error analysis. How is it possible, that, e.g., equation (6) still holds even for a bi-modal distribution (when drizzle is present)?

(5) Water vapor super-saturation (p4828, l15) does not play an important role in determining cloud albedo. Rather, it is water vapor concentration that determines cloud albedo, and that only in water vapor absorption bands.

(6) Since albedo is one of the motivations of the paper, what would the resulting uncertainty be in this parameter, or in any other relevant parameter (forcing, absorption, . . .)?

Minor comments:

p4827,l24/25: aerosols –>aerosol

p4827,l25-27: Revise sentence, it is unclear

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p4828,l6: droplets –> droplet

p4828,l14: cloud thickness –> cloud optical thickness; how about effective radius (see comments above)?

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