Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-489-RC3, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The dual-field-of-view polarization lidar technique: A new concept in monitoring aerosol effects in liquid-water clouds – Case studies" *by* Cristofer Jimenez et al.

Anonymous Referee #2

Received and published: 4 August 2020

This manuscript brings new observational skills to the problem of quantifying aerosol-cloud interactions using surface-based instrumentation. It brings an impressive array of new technical developments to simultaneously quantify cloud microphysical properties, updraft velocities, and sub-cloud aerosols. The downside of this paper in this reviewer's mind is that (i) the uncertainties in the retrievals – this paper and others like it – are large enough that they are not useful constraints on the problem; and (ii) the view of the cloud system as a vertical column in which one can learn about microphysics is a flawed one. That being said, I don't have a problem with the paper being published after some major revisions are made. These should persuade the reader of the utility of the approach. I offer some thoughts below.

Major comments

1) The ground-based system looks up at the clouds, and has the advantage of measuring aerosols beneath the clouds rather than between the clouds as with space measurements. But there is a significant over-interpretation of the relationship between below-cloud aerosol, updraft and drop concentration because the cloud system is almost never static and advection/diffusion mixes different drop concentrations together. Thus, a metric like the activated fraction doesn't make sense. It is also no wonder that the "ACI metric" is so noisy to the point that one wonders how useful it really is. Even without the uncertainties in Nd and particle extinction, the scatter is so large (Figure 11). This is a general criticism of this approach. You need to articulate much better what it is good for?

2) Since this is an "example paper", you should clarify what the applications are. For various reasons it's not an approach that teaches us about cloud microphysics. The system sees some integrated view of all processes and trying to sort that all out in the presence of advection and entrainment is not a feasible approach for elucidating processes – especially given the large uncertainties.

It could be useful for constraining ACI in climate models, or as an adjunct to satellite studies that attempt to do so but with poorer accuracy. But then how well would you need to measure ACI so that it would be useful for this purpose. And you would need to consider the big difference in measurement/modeling scales. Quaas et al. (2009, ACP) addresses some of these issues by comparing ACI metrics at a ground site with ACI metrics in models. Frankly, I find this question of usefulness very challenging because there are so many uncertainties. If I had the answer I would be happy to share it.

3) For case 1, the speciation in the mixed phase cloud seems useful. My major concerns relate to the physical interpretation of the 2nd case. Section 4.2 rambles and provides no clarity. Numbers are reasonable but with the large uncertainties, what do we learn?

Examples: By means of the new dual-FOV polarization lidar technique, cloud and aerosol information can be derived with high temporal resolution which allows us to resolve different phases of cloud evolution and life cycle and to investigate the impact of individual updrafts on the droplet nucleation rate, droplet growth and corresponding evolution of the effective radius, and the Nd–NCCN relationship in very large detail. This cannot be true. Individual updrafts don't reflect on activation in the column. What the system measures overhead is the net result of upstream activation, mixing, condensation, collision-coalescence (perhaps), and all other processes that shape the size distribution on its way to the volume that you sample. Updrafts vs. Downdrafts. If you want to sort by updrafts vs. downdrafts, you would need to take into account the typical size of large eddies. The 'instantaneous' ($\sim$ 60s) sample likely measures the effects of upstream updrafts and downdrafts given the size of typical eddies (Fig. 8). A weak decrease of the cloud extinction coefficient is visible when going from updraft to downdraft conditions in line with the dissolution of droplets. (I assume you mean evaporation of droplets.) One can't make such a statement because evaporating drops will be larger than growing drops, all else equal, because of asymmetries associated with solute and curvature terms (Korolev 1995, JAS). So perhaps all else is not equal. But again what do we learn? New droplet formation and growth of existing droplets by water uptake led to a slight increase of the cloud extinction coefficient in many cases. A reasonable but not at all useful statement. A weak reduction of the mean effective radius during upward motions may indicate new droplet nucleation in the presence of existing droplets. Again, reasonable but not at all enlightening given the uncertainty in drop effective radius. Maybe it indicates lower liquid water content, which could come from a variety of processes or from uncertainty in the measurement. 4) Regarding uncertainties: The lowest part of Sc clouds (which case 2 appears to be) is adiabatic. Why do you assume sub-adiabatic? How much influence does this have? How big of an error does the extinction/backscatter ratio have on the sub-cloud CCN?

You mention 500 m below cloud base as being "dry". Is this really so? Hysteresis in the efflorescence curve will hold on to water vapor down to small RH, especially in the

presence of organics.

The choice of CCN proxy has a very large effect on the metrics. What value is the right one? (E.g., Shinozuka 2015). I don't believe there is an answer to this question. And because of this I don't see a way to use this approach for quantification.

5) I appreciated the clarity in Fig. 5 showing source of uncertainty in Re. Fig. 6 left me confused. One of the Frisch approaches is to calculate Re from Z assuming a fixed Nd. In such a case you can tune Nd to get the Re over the range that you want. So the statement "Good agreement was found" is misleading when it comes to the comparison with radar and fixed N. Radar is so sensitive to large drops and I would put more faith into optical measurements. Did you look at deriving mean Re from cloud optical depth and liquid water path? It's a simple, straightforward approach to retrieving the Re from exactly the moments of interest (2nd and 3rd). It's unpretentious and quite solid if the uncertainties are known.

Other comments

6) Please provide historical context to the work that has been done trying to quantify ACI from the surface. The paper should quote early and usually similarly flawed work by various authors including Kim, Schwartz et al. (2003, JGR), Feingold et al. (2003, GRL), Garrett et al. (2004, GRL), Sena et al. (2016, ACP) to name some that come to mind. Some used optical depth and microwave radiometer to measure layer-mean cloud drop size; others used radar and microwave radiometer. Some used surface aerosol measurements, and others lidar profiles. Please give a little recap of the advantages and disadvantages of these older approaches.

7) I was frustrated by references to figures and equations in Part 1. E.g., table 1 references equations in Part 1! You shouldn't expect the reader to have all of those details, or to have to read two papers in parallel.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-489,

2020.

C5