Review of ACP-2017-508_revised

In this revised version of the manuscript the authors addressed most of the comments I made to the original manuscript. The main points are clearer and their modeling results reveal some important aspects of entrainment and mixing processes in clouds. Now I think that this manuscript is worth the publication in ACP. However, I still think that the authors should clearly state the limitation of their results not to mislead the readers. English is improved significantly but the many typos I noticed should be corrected. It may not be necessary to review the manuscript again after corrections but the editor should check if the authors address my comments before making the final decision. Some specific comments are followed.

Major comments

One of the main arguments the authors make is the inappropriateness of the mixing diagram as a tool to analyze entrainment and mixing problem in clouds. Their argument is based on the fact that the mixing diagrams that can be drawn when equilibrium is reached in their model calculation are different from what is expected from the 'classical' mixing diagram for a particular mixing type, specifically inhomogeneous mixing at equilibrium state. This is misleading. When we draw mixing diagram, we do not assume anything. As the authors themselves state clearly several times, mixing diagrams of in-situ observation data just give us a snapshot of cloud microphysical relationships. We may assume equilibrium state only when we interpret the results, saying, for example, that such data scatter resembles something that can be expected from the final equilibrium state of inhomogeneous mixing or something that can reveal homogeneous mixing at its final stage. Even though mixing diagrams give us only the snapshot of different stages of entrainment and mixing process, they can still reveal some important information on the nature of entrainment and mixing process. That is the basic stance when we interpret mixing diagrams. In their response to my comments on the original manuscript, the authors showed two figures from Burnet and Brenguier (2007) that might demonstrate the difficulty of interpreting mixing diagram. The authors did not show another figure from Burnet and Brenguier (2007) that can indeed demonstrate clear difference of data scatter from the two figures the authors showed in their response to my comments, because this figure indicated inhomogeneous mixing unlike the two figures that indicated homogeneous mixing.

The authors should state the limitation of their model more clearly since this is the main reason why their results are different from observation. In real clouds, entrainment and mixing do not proceed continuously until the equilibrium state is reached as was postulated in their model. Intermittency certainly exists in real clouds as demonstrated in many observational studies by abrupt changes of droplet number concentrations near cloud edge regions but this cannot be generated with their model. Mixing diagram of in-situ observation data is a snapshot of cloud microphysical relationships that contains all these effects at an instance. The 'classical' mixing type idea is just one way of interpretation of mixing diagram. What if relative mean volume diameters do not change despite a large variation of relative droplet concentrations in a mixing diagram? A reasonable interpretation would be the dominance of inhomogeneous mixing for this cloud. What if relative mean volume diameters and relative droplet concentrations show a strong positive correlation? A reasonable interpretation would be the dominance of homogeneous mixing instead of ambiguity between homogeneous and inhomogeneous mixing. For inhomogeneous mixing would not continue until the equilibrium state is reached in real clouds and therefore mixing diagram would not become so similar between homogeneous and inhomogeneous mixing as the authors suggested with their model results. What if the data scatter does not suggest any of the 'classical' mixing type idea? A reasonable interpretation would be that some other processes must have been dominant.

The authors discussed some of these aspects in the last two paragraphs of Discussion and conclusion but their stance is still that mixing diagram is at fault. It is not that "classical mixing diagrams are plotted namely for equilibrium states." Mixing diagram is not plotted for anything but in the interpretation of mixing diagram we may adopt the concept of inhomogeneous or homogeneous mixing at equilibrium state. The authors should first emphasize the limitation of their modeling results more clearly and then the cautions we may take when we interpret mixing diagrams of in-situ observation data.

Minor comments

DSD is a collective term. So the word "DSD maximum" seems awkward at Line 313 and at several other lines. More appropriate expression seems to be the mode diameter of DSD. Similarly what does "DSD values" mean? Collectively it would mean total droplet

concentration. Make it clear.

There are many typos. One example is “within in the initially dry volume” at Line 326. These should be corrected.