Moharreri et al., AMTD, "Aircraft testing of the new blunt-body….", 2014

Review by Jeff Snider, University of Wyoming

Overview

This manuscript builds on a series of published papers from the Clarkson group. The aim is perfection of aerosol inlets, making them less prone to produce aerosol particles that result from the breakup of hydrometeors, or avoidance of those particles. Because this manuscript is following others, there is a clear desire to make the point that the design/development/testing is a success. From everything I can see here, I am convinced that this is a valid conclusion. Having said that, I still have reservations; specifics are provided below.

What is the set and setting for this research? As I see it, the type of measurements proposed (cloud interstitial), are fraught with hassle because of the largely unknown, and variable, effects of shatter. The Kleinman et al. paper (ACP, 12, 207-223, 2012) is a case in point. What did we learn from those interstitial measurements, how can this new technique improve on those studies, and what is the vision going forward? Related to this, I see the need for the development of observational tools, as described and improved on here, and for models which evaluate the myriad of processes (entrainment/mixing, Bergeron process, activation, and etc.) known to influence the budget of cloud interstitial aerosol particles. Without buy in, from modelers, I feel that this type of effort will wither. Clearly, that would be unfortunate.

Much of my criticism stems from my own ignorance of the findings in Moharreri et al. (2013). The latter is a basis for this manuscript. I recommend that, in the introduction or later on, the authors summarize the *relevant* findings in the Moharreri et al. 2013 paper. I recommend this type of summary for the work of Craig et al. (2013a and 2013b), as well.

Specifics

Figure 3 – The droplet concentration is about 70 cm^-3, so the interstitial aerosol would be expected to decrease by 70 cm^-3, relative to outside the cloud, if the inlet is excluding the cloud droplets, if shatter is minimal, and if the background (adjacent to the cloud) is representative of what entered the cloud through its base. I am surprised that there is not more discussion of these points. For example, the Figure shows an interstitial decrease (SMAI) (relative to outside) which is many times greater than 70 cm^-3. As I comment below, it would also be good to have the LWC and the drop-to-total mass ratio for this cloud pass, and as well for the other passes.

P2668-L19-20 This phrasing is confusing: "..suggested that shatter particles in that size range will entrain the aerosol sample." It seems, to me, that the word "entrain" is being used to indicate something other than the entry of one fluid into another. I recommend that the authors elaborate on what it is that they are describing here.

The transition from Section 2.1.1 to 2.1.2 is abrupt. What is motivating the need for pressure measurements? In Section 2.1.2 you conclude that "..flow around the blunt-body separates." It's not

clear what is implied by “separation” or how it is diagnosed from Figure 5. Does “separation” imply that even if the shatter particles are not ejected out of the boundary layer, they would be sampled?

Figure 6, when compared to Figure 5, looks like a mirror image. Why not present Figure 6 and Figure 5 similarly? Also, it is not clear if the hole, seen in Figure 1, allows air to flow down the centerline of the body.

P2671-L4 In my opinion “..large droplet shatter..” should be replaced with “drop shatter.” Further, I recommend that definitions be provided, up front, that follow the standard cloud physics definitions (cloud droplets<20 um radius and drops>20 um). See, for example, the Cloud physics textbook by Rogers and Yau.

Figure 9 – The solid curve makes sense, but the dashed curve, defined in the caption as “..the size dependent fraction of shatter particles that will be present in the sampling flow region of cross flow sampling inlet” is not clear. I recommend that the latter be explained and discussed, in the text.

Figure 11 – Its not clear why the effect of the body (Figure 11a) causes the efficiency to exceed unity. Is the body acting like a CVI, causing the larger particle to concentrate? There is another thing about this. We need a diagram, or a statement, telling us that the UHSAS the HDDMA sampled from the BASE-II’s sample inlet, and that the PCASP and F300 are open path and wing-mounted.

P2672-L26 Presumably, drops, and droplets, coexisted in these warm cloud samples. Of relevance is the mass in these two hydrometeor size fractions. There are two products provided by NCAR. One is the drizzle water content (DWC), the other is the cloud water content (LWC). A useful statistic would be DWC / (DWC+LWC). I recommend that you report the DWC / (DWC+LWC), and the LWC, for this particular cloud segment and for the segments in Figures 3 and 4. Future researchers will need some grip on the cloud conditions you encountered.

P2673-L7 and L8 These lines describe what Craig et al. (2013b) did. Here, the BASE is compared to the SMAI (as a ratio, the “normalized enhancement”), without factoring in the ambient aerosol concentration. It’s good that you are explaining what Craig et al. did. However, the transition to your “Normalized Enhancement” needs to warn the reader that you are not using the ambient aerosol as a reference. Related to this, the caption of Figure 12 uses “CN Enhancement” not the “Normalized Enhancement” used to label the plot.

Other points

Adequate heating of the sampler, and of the slot, so that they do not clog with ice, will be a concern for future work. Specifically, where were the heaters installed? Also, was icing a problem?

I understand that the suction flow is designed to keep the boundary layer attached. If the flow is turbulent, which it seems to be (Reynolds number ~10^6), then one might desire more suction than that provided in front of the sample inlet. Have the authors considered a broader slot, perhaps circumscribing the perimeter of the body?