Atmos. Meas. Tech. Discuss., 7, C4300–C4302, 2014
www.atmos-meas-tech-discuss.net/7/C4300/2014/

Atmospheric Measurement Techniques Discussions

Interactive comment on "The Ice Selective Inlet: a novel technique for exclusive extraction of pristine ice crystals in mixed-phase clouds" *by* P. Kupiszewski et al.

Anonymous Referee #1

Received and published: 22 December 2014

This manuscript presents a novel design for a ground-based inlet that extracts ice crystals from mixed-phase clouds using a combination of an impactor, a phase-discriminating flow tube, and a counterflow virtual impactor. To my knowledge this is only the second phase discriminating inlet, after the ice-CVI referenced in this paper. The paper is of importance to the field, is well written, and the topic is appropriate for AMTD

Unfortunately, this manuscript falls far short of the standard of a peer-reviewed paper. The central issue is that there is an entire section missing, which is any sort of experimental validation of the technique. The paper starts with a presentation of the concept

of the technique – which is theoretically sound. There is an extensive discussion of the WELAS instrument used to observe exiting (that is, in theory) ice crystals. The new inlet was then brought to a location of mixed phase clouds, a mountain-top Swiss site. There is one plot of observations and one plot of optical output after the inlet.

This paper is not publishable until there is a comprehensive section on inlet testing and determination of transmission. I can find no other paper on this type where a set of controlled lab experiments were not performed. In essence the reader is asked to believe the theory translates perfectly to performance. Why is there no data using e.g. lab-produced droplets to show the phase discriminator fully evaporates these and the CVI rejects them? Why is there no laboratory preparation of ice crystals where they are shown to be transmitted through the phase discriminator and passed through the CVI? These tests would allow (1) quantification of some portion of the inlet artifacts and (2) quantification of the transmission efficiency.

As it stands now, the authors attempt to suggest that each of the inlet segments will work exactly perfectly ... except that they have neither tested all together or tested the phase discrimination setup. Indeed, the authors mention an observation of highly rounded ice residuals by the optical detector is not convincingly separated from slightly aspherical droplets as would be the case in a turbulent flow. They argue this is sublimation, which maybe it is. Put more directly, there is no presented evidence that refutes the transmission of droplets through the inlet interspersed with ice crystals. Since the target of the new inlet is mixed phase clouds, which can be (greatly) dominated by droplets over ice and often with similar size, it is stunning that some type of testing wasn't performed before field deployment and certainly before attempted publication.

In conclusion, this manuscript describes a novel inlet of importance to the study of clouds and the aerosols upon which they form. Rejection is suggested, however, since the authors have skipped what should have been a relatively simple and absolutely critical step of a laboratory validation of a new technique. As currently constituted, this paper does not convince that the inlet works as theoretically devised.

Upon resubmission: (1) there needs to be a comprehensive laboratory study with quantification of transmission of droplets and ice residuals – show the former is not transmitted experimentally while the latter is (2) instead of the final optical figure field data should quantify the transmission of droplets versus their presence in the cloud as well as the transmission of ice crystals. This should be presented analogous to the experimental data to show the inlet works at the different pressure and more rigorous field conditions.

Ideally, a comparison should be made to the current "state-of-the-art" ice-CVI which, per referenced publications, has been located at the same research site during these field studies.

Suggestions and references:

In researching the CVI, I note that the authors of this manuscript do not fully consider the pressure effects noted in Boulter et al., 2006. While the paper is referenced here the authors seem to perform some CVI checks on transmission but don't explain differences when operated at the altitude of the mountaintop station (that is to say, apparently the lab settings were transferred exactly which would not appear to be correct?). Boulter et al., 2006 suggested significant differences. This pressure dependence should be quantified.

Pekour et al., AS&T, 2011 have performed a study of CVI artifacts. It is surprising this isn't referenced here, nor are the artifacts described in detail. It would be good to know if these artifacts are observed with this inlet.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 12481, 2014.