Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-29-RC1, 2022

Comment on acp-2022-29

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Referee comment on "Technical Note: A High-Resolution Autonomous Record of Ice Nuclei Concentrations Between -20 to -30 °C for Fall and Winter at Storm Peak Laboratory" by Anna L. Hodshire et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-29-RC1, 2022

The goal of obtaining INP measurements continuously in time and over the range of temperatures covered by tropospheric clouds is a very desirable one indeed. When accompanied by other physical and chemical data about the atmospheric aerosol such measurements could be very helpful in unraveling the many uncertainties which are obstacles to usable descriptions of ice initiation in the atmosphere. From that perspective the paper is a welcome contribution. The site selected for the measurements here reported is not ideal but has advantages. Past use of the site for similar studies helps to provide some perspective.

This paper is formulated as a Technical Note but it is hard to tell the difference in emphasis between trying to accomplish two things: to demonstrate the accomplishment of continuous operation of a modified CFDC, and to show results obtained during the period of operation. Neither goal is quite accomplished satisfactorily.

The modifications of the CFDC are described in some detail but in a way that is hard to comprehend without intimate knowledge of the design and operation of the instrument. The basic theory of operation of the CFDC instruments is well documented in the literature. However, intimate details of the operation, specifically the cycling needed to maintain the ice coating on the walls, the avoidance of ice shedding from the walls and the drop/ice crystal thresholding are not well explained in this paper and are hard to track down in earlier papers. Thus, the technical details given for various changes are somewhat in the air. More importantly, it would have been very helpful to see more objective measures of assurance that the device was functioning correctly throughout the long sampling period. The comparison with a previously tested device is a good sign but, clearly, those tests were done with operator assistance while autonomous operation is another matter, specially in the case of an instrument that needs periodic rebuilding of the critical ice coatings of the inner surfaces.

The change to aluminum construction was done to provide surfaces that are more suitable for the formation of the ice layers without periodic treatment of the surfaces. This is a good step if it works, but raises questions about how the aluminum surface may have changed during the operating period and how that may have affected the ice coatings. Were the ice layers uniform over the whole surface? Were there patches without ice? Were there any controls, or specifics of the data that could be used to judge the constancy of the required conditions. This is readily done for wall temperature but is more subtle with respect to the saturation value accomplished. Perhaps there is no problem here but the readers should be provided by some assurances.

Regarding the long-term continuous record, the paper presents only a glimmer at the results. Statistics over the whole period are given in Fig. 4. The authors suggest that the 'public dataset maybe of interest ... ". This is likely to be true but the value of the dataset can't be judged from this paper. Would the overall statistics be much different if derived from intermittent sampling, say, on daily basis? How can the reader evaluate the benefits of continuous sampling? It was tempting for this reviewer to actually obtain some of the data and look for answers to the questions posed above. But, it is perhaps best done by someone with direct experience with CFDC instruments.

The analyses presented are reasonable but add little real substance. The derived surface site density values are speculative because the INP sizes are not known and may be quite different from the assumed values. The comparison with the prior parameterizations is quite short of meaningful correlations and are hard to judge because data for all temperatures are shown in saturated plots (Fig 5).

The two points are, in fact, related. Examinations of the detailed data may provide some basis for judging the adequacy of instrument performance through consistency versus erratic behaviors.

Overall, what is presented in the paper does not indicate definite problems but neither does the material show robust reasons for accepting the results. The somewhat surprising lack of INP concentrations on temperature add to justified curiosity about the validity of the data. Does shorter term data show temperature dependence at any time? Or, are all INPs in the dataset activated at temperatures at or above -20°C, the highest temperature at which measurements are made?

It appears that the authors' main focus was making an instrument capable of continuous operation. They are likely to have accomplished that goal, or came close to it. However, for measurements that are highly sensitive to instrumental conditions it is desirable to have some controls monitoring those conditions. If such information is not available and can't be re-created in retrospect, relevant disclaimers or caveats may be necessary.

To demonstrate the nature of high-resolution data (as in the title of the paper) more detail than here given would be beneficial. The title should perhaps also indicate the

temperatures for which data have been obtained.