Reviewer comments on **"Development of the DRoplet Ice Nuclei Counter Zürich (DRINCZ): Validation and application to field collected snow samples"** by Robert O. David, Maria Cascajo Castresana, Killian P. Brennan, Michael Rösch, Nora Els, Julia Werz, Vera Weichlinger, Lin S. Boynton, Sophie Bogler, Nadine Borduas-Dedekind, Claudia Marcolli, Zamin A. Kanji

Gabor Vali

**General:**

This paper is a good addition to the literature on INP measurements. Not fundamentally new, but every implementation of the drop-freezing technique, or of any other method, brings new challenges and new ways of solving them. The authors deal with those challenges reasonably well. This paper stands out with its focus on evaluating instrument-caused uncertainties. There are some parts of that evaluation that would benefit from a second look.

**Detailed comments:**

line numbers

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| 41-57 | In describing different approaches to INP detection it is useful to separate those that examine air samples and those that take water samples. |
| 81 | Does Bigg (1953) contain data and hail and snow samples? Please check. |
| 88 - 89 | The goals are stated in overly broad terms. The proposed measurement are expected to be relevant to MPC clouds but no claim should be made that they examine those responsible for ice formation in those cloud. There are other elements to that story beyond the INP measurements. Also, to what degree can these measurements illuminate 'fundamental understanding' of ice nucleation? |
| 97 | In addition to those cited, a design much like the one in this paper was described by Vali (1995; Principles of ice nucleation. Chapter 1 in: "Biological Ice Nucleation and Its Applications", R. E. Lee Jr, G. J. Warren, and L. V. Gusta, Eds., APS Press, The American Phytopathological Society., St. Paul, Minnesota, USA. 370 pp.; ISBN: 0-89054-172-8). |
| 103 | Is the foil seal enough to exclude ethanol vapors from getting into the samples and thereby producing a freezing point depression? |
| 123 -> | Since the position of the sample tray is fixed, and so is its dimension, why is such an elaborate process necessary for identifying the well locations? Also, if done this way, to what extent does perspective from the camera lens distorts the circular shape of the wells near the edges of the tray? |
| 131-132 | The meaning of " .... centered at the edge ... as the well center." could probably be |

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| | clarified better. |
| 135 | Random order and sorting seem unnecessary with the fixed geometrical arrangement of this setup. What is the rationale here? |
| 152 | Isn't the first instant of intensity change over a threshold magnitude sufficient to detect nucleation? If not so, why not? What possible reason exists for a significant peak in the signal, comparable to that caused by nucleation, prior to nucleation? |
| 157 | What is meant by "all recorded images"? |
| Fig 2. | It is unclear to me what mean intensity and normalized intensity refer to. Is it an average within the circle for a given well? Are they for a given well over repeated trials? Are the averages over many wells? |
| 190 | What is 'maximum standard deviation'? |
| 196 -> | The work here described in Section 3.2 is certainly well directed and quite extensive. However, it is surprising that nucleation temperatures of SA water are used instead of direct temperature measurements. It is the temperature of the water before, and at the instant, of nucleation that is most relevant. Direct temperature measurement of the water in the wells is not without its own difficulties (locating the sensors in the wells, sensor lead errors, etc.) but the variations in nucleation temperatures from well to well, even for SA water or other similar sample, are bound to be adding uncertainty to the calibration. What governed the decision to use nucleation temperatures to evaluate bias across the well-plate? To assess the quality of this approach it would be useful to know how much variation in nucleation temperatures was observed for any given well within the 20 repeat tests. The two sources of variations - within a given well and among different wells - should be both presented and the sufficiency of the use of the median for each well thus evaluated. |
| 226 | Section numbering is off. |
| 244 | This standard deviation refers to the distribution of observed freezing temperatures among wells? Again, please distinguish between single well repetitions and variations among wells. |
| 246 | The 50% fraction corresponds to the steepest point on the FF curve for SA water. But this is not a general result; other samples may have no such correspondence between the two measures. |
| 258 - 259 | Fig. 4a shows, as expected, that the standard deviation varies according to the slope of the FF curve (sample size effect). Assigning this patters to the influence of ethanol circulation is likely to be incorrect. More general point: to what extent in ethanol circulation predictable? This is a valid question in light of the flow being turbulent with the level control adding pulses of liquid. |

304 -> The background correction via Eq. (10) is valid, but it is surprising that the correction is finally presented in terms of FF, via Eq. (11). Fitting a correction equation to $k_{bg}(T)$ would be more direct and more readily applicable to a variety of samples with different volumes and/or dilutions.

342 -> Section 4.2 is well done. It is a good demonstration of the DRINCZ's capabilities. Was background correction applied?

373 -> Sections 4.3 to 4.5 introduce a topic beyond the description of the instrument. As has been amply shown in the extensive literature on the topic, analyses of snow samples are valid tools as inputs to the analyses of cloud processes, but with the attendant complicating factors partially discussed here. That current results vary within the range reported for other such measurements is due does demonstrate that the sampling techniques were adequate and that the atmosphere is relatively conservative in the range of INP contents of snow. They do not substantially reinforce the validation of the instrument per se; that validation is more clearly supported by the calibrations and by the illite sample results. It is not stated (or it escaped me) whether the freezing analyses were done in the field or in the laboratory. This would be relevant to possibly show that the instrument is rugged enough for field use and that different setups do (or do not) effect the results.