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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Contrasting Local and Long-Range Transported Warm Ice-Nucleating Particles During an Atmospheric River in Coastal California, USA" *by* Andrew C. Martin et al.

Anonymous Referee #2

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General Comments:

The paper describes the measurement of ice nucleating particles (INPs) in precipitation collected during an atmospheric river event at two ground measurement stations in California – a coastal station (BBY) and an inland site (CZC). The central conclusion of the paper is that warm INPs (i.e., those that initiate freezing at termperatures warmer than -10 deg. C) are found to increase during the atmospheric river event at CZC, but no change is detected at BBY. FLEXPART trajectory modeling and radar measurements of cloud properties are used to provide context for the atmospheric state during the INP measurements, although the relevance of this ancillary modeling

and measurements to the central conclusion of the paper is not clear. Overall, the manuscript is quite lengthly with extensive discussion of the meteorological evolution of the atmospheric river. However, it is not clear to me how these details support the proposed science questions and the strong conclusions that are reached. Rather, it seems that the primarily piece of evidence to support the role of terrestrial aerosol as "an important source of warm INP during this atmospheric river" is that the CZC is inland and the BBY site is coastal, so any difference between the two must be caused by the intervening land surface. FLEXPART modeling shows that probability of trajectory air parcels residing within the terrestrial boundary layer is zero during the early part of the atmospheric river and "small, but non-zero" during the latter portions of the atmospheric river. Is such a small residence time sufficient to explain the marked, ten-fold increase in warm INPs? Also, the transition in language from the measured "small, but non-zero" conclusion on Pg. 16, Line 29-31 to "important sources of warm INP" on Pg. 1, Line 16-17 seems disingenuous. In sum, the authors ask important and bold science questions (Pg. 3, Lines 26-31), but the limited amount of data from this one case study and the weak interpretation of the FLEXPART and radar data do not support the authors' proposed answers to these questions. The manuscript length, lack of adequate description of some introduced quantities, and confusing internal referencing significantly detract from the readability of the manuscript. The data and results may be of interest to the readers of Atmos. Chem. Phys. as a much shorter discussion of an interesting case study; however, I do not think the results presented here allow the authors to convincingly address the proposed science questions. Therefore, I recommend that the manuscript be extensively revised, shortened, and reframed as a case study analysis before I could recommend it as suitable for publication.

Specific comments:

1) I don't understand the discussion on Pg. 3, Lines 22-25. How does the blocking of the radar return by the coastal mountain range ensure that hydrometeor information is indicative of mixed phase clouds? I get that this limits the radar signal to roughly 2.9-3.6 km in altitude. Are the authors saying that freezing conditions do not exist below 2.9 km? Similarly, is the temperature at 3.6 km always above -10 deg. C? From Table 2, it appears that there is a great deal of variability regarding the extent of the radar retrieval layer top and bottom.

2) There is insufficient detail provided in Section 3.1. What are the signficance of BBH and ETH? A brief description of how these quantities are obtained should be included so that the reader doesn't have to search out the reference citation to understand what they are. How and why are these data being using in this study? Also, why do we care about the LLJ, CBJ, and polar cold front? Here, the CBJ is described as a feature, while in Figure 4, the barrier jet is denoted as a time period. Basically, Pg. 6 is a laundry list of different parameters, but some additional context of why these parameters are important and how they are / will be used would be very helpful here.

3) The references to sections are confusing as all sections are numeric, while some references use letters. Presumably, 3a = 3.1, 3b = 3.2, etc. Regardless of that minor technical fix, the pointers included in a lot of places are very vague. For example, what are "significant kinematic features" in Section 3.1? Does it make sense to say that 2000 elements were released per layer for three consecutive hours surrounding the coastal barrier jet? What is meant by a kinetmatic feature (Pg. 7, Line 8)? On Pg. 7, Line 29, it's stated that the methods in Sections 3.1-3.3 are used to link INP source regions to clouds over BBY and CZC via means of FLEXPART simulations, but Section 3.1 is largely definitions. All of this internal referencing is very confusing and detracts from, rather than helps, readability.

4) What is the meaning of the sentences on Pg. 8, Lines 1-3: "...we can identify proxy regions for local INP sources using the terrestrial and marine boundary layers, but these methods cannot capture all possible LRT source regions. Thus, we must in part

make inferences about source after rejecting alternate hypotheses if the mechanisms examined are not supportive." What are these alternate hypotheseses and mechanisms?

5) The paragraph on Pg. 8, Lines 22-29 is very confusing and needs to be revised to be clearer. What is meant by the statement that the authors "sought to preserve the mixed phase temperature range as found by the soundings in Table 2"? Why are Chi-Square independence tests being performed? Why is a rule of thumb bing applied to the minimum expected population? The application of these statistical methods here (and throughout the manuscript) are not well described, and I don't understand why they should be done and are being done.

6) The discussion on Pg. 10, Lines 11-18 doesn't seem to match the graph. INP-10 at CZC seems to be between 1-4/mL on March 5th (where does 0.25/mL come from?). Similarly, on March 6, INP-10 at CZC are 10-15/mL (where does 3/mL come from?). Since there's only a few data points for BBY, I don't think it can be stated that "BBY only occasionally neared 2/mL".

7) Why are there so few data points for INP-10 in Figure 4? Do all of the time periods where there are no data points reflect that the concentration of INP-10's is below the detection limit? What is the detection limit? Points that are zero or below the lower limit of detection need to be added to the graph as well in order to evaluate trends. Otherwise, statistical and interpretative significance might erronously be applied to only a handful of otherwise insignificant data points.

8) On Pg. 10, Lines 23-25, it's stated that there are not precipitating hydrometeors during 15-21 UTC on March 5th, but it looks like the cummulative precipitation curves increase during this period. How can it be both ways?

9) The discussion on Pg. 12, Lines 5-9 is all highly speculative and not supported by any evidence in this manuscript. Please revise or strike this paragraph/conclusion.

10) What are the more exotic functions of temperature used/referenced on Pg. 12, Line 13? I don't understand how the authors are able to state that "it is likely that biological material contributed significantly to INP concentrations for T < -10 deg. C at CZC, but not at BBY." Where is the evidence!?

11) Where in Section 3.1 is it stated that the jet stream is located between altitudes of 6.5 and 11 km MSL as implied on Pg. 12, Line 33?

12) The sentence on Pg. 13, Line 14, "Table 4 presents the probability of element residence (section 3c) in the UTJ, AR, MBL, and TBL." is another example of sloppy internal referencing. Why is Section 3.3 being invoked here?

13) In the conclusions on Pg. 16, it is stated that terrestrial warm INPs are abundant and that marine warm INPs are not evident, but there are warm INP data points reported for BBY in Figure 4. If these are not marine warm INPs, where do they come from?

14) If the small, non-zero change in instantaneous element residence in the terrestrial boundary layer is really the driver of why the warm INP concentrations vary at CZC, then why do the INP concentrations not vary with the varying numbers shown in Table 4 – 6.2

Technical corrections:

Pg. 6, Line 4: IVT is not yet defined. It is defined on Pg. 9, Line 10, but only in passing.

Pg. 9, Line 15: Reference to Martin et al. seems out of place

Pg. 9, Line 18: Figure 5b is reference out of order

Pg. 12, Line 17: The reference to Section 2d (2.4) does not seem right. Should this be 2.5?

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