

Interactive comment on “The Importance of Biological Particles to the Ice Nucleating Particle Concentration in a Coastal Tropical Site” by Luis A. Ladino et al.

Anonymous Referee #5

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General Comments:

“The Importance of Biological Particles to the Ice Nucleating Particle Concentrations in a Coastal tropical Site” by Ladino et al. describes efforts to characterize the INP population and biological particles at a tropical site. These data are valuable to the community due to a lack of data in such environments. My major comment is that more information on the INP measurement detection limits, blanks, and uncertainties is needed because the paper heavily relies on these data. I have several other concerns in the interpretation of results, described below. In general, the authors motivate the study by describing the need to characterize marine INP sources in the tropics (“Very

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few studies to sample INPs have been carried out in tropical latitudes, and there is a need to evaluate their availability to understand the potential role that marine aerosol may play in the hydrological cycle of tropical regions”), but I am not convinced the method deployed can measure [INP] for a remote region and I think the concentrations reported could not possibly be explained by marine aerosol. Overall, I think there are a few things that need to be clearly stated and supported consistently throughout: 1) What were the [INP] and their variability (and their detection limits)? 2) What size range corresponded to the highest [INP]? 3) what meteorological conditions, air mass histories (back trajectory and PCR results) corresponded to the highest [INP]? 4) What is the hypothesized origin of these very high [INP] and biological particles? If the authors can build up the results discussions around some clear points, I think it will be easier to follow along.

Comments:

Abstract:

Should mention the freezing mode and INP temperature range measured during this study in abstract.

L18 – I think a better way to say this is similar to how it was stated in the results section, something like “The high concentrations of INPs at warmer ice nucleation temperatures ($T > -15^{\circ}\text{C}$) and the supermicron size of the INPs suggest that biological particles may have been a significant contributor to the INP population in Sisal during this study”.

Introduction:

L55: The modeling studies listed determined specific regions where oceanic sources dominated the INP population due to an absence of other types, like mineral dust.

L58: Bigg, 1973 was the first to report such a study (his data are used in Schnell and Vali, 1975).

L67 – the range may also be from different species, right?

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Methods:

L142 – please provide information on the measurement detection limit and how measurement uncertainty was determined. Where were blanks collected and how were these accounted for?

Results and discussion:

Fig S3- Why are there no particles larger than 1 micron at a coastal site? Is this consistent with other studies? Are the y axis units correct?

L250 – Did you compare particle composition between the cold front/marine air mass periods and the other periods? These back trajectories shown in Figure S1 suggest that the air masses actually originated from the US Central Plains. So, you would expect a mixture of aerosol composition I think.

Fig 4 – It would be best to show the data from this work as points versus a shaded region so that the variability in [INP] is fully illustrated/reported. Are these samples background corrected? Are the [INP] for all the stages combined or each individual stage? For temperatures lower than $\sim -25^{\circ}\text{C}$, the “bluish” region flat-lines at about 30 L^{-1} – is that the upper detection limit of the INP measurement? Same with lower detection limit. This figure suggests to me that the range of detection of this method is from 0.1 L^{-1} to 30 L^{-1} . Is it possible for this method to observe the concentrations reported for remote marine environments (dark blue shade, DeMott et al. (2016))? These detection limits should be noted in the methods and in the figure caption.

L260 –Should also note that the [INP] reported here are up to 3-4 orders of magnitude higher than [INP] reported for marine boundary layer measurements reported by DeMott et al., 2016.

L270 – “The Sisal data corresponds to particle diameters ranging between 0.32 μm and 10 μm where 16 out of the 29 samples fulfilled the size criteria.” – please clarify what is meant by this?

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L283 – what time of year were Rosinski’s measurements made?

Fig 6 – Should there be standard deviation bars on these? Also, if one were to use Figure 6 and Figure S3 to determine a number fraction (which should be done as an analysis), the number fractions are bogus. Are the units of Figure S3 correct (maybe they should be per cubic centimeter)? How do you have higher [INP] than total particle counts in the same size bin?

Fig 7/L309 – Are these results for the entire study or a specific period? Please add this detail to figure caption and text. If the entire study, why not look at individual events? They were 48 hours sample, so perhaps show a timeline? Is it not possible to look at carbon or oxygen with this method?

L328 –show the timeline in the supplemental to support this statement?

Figure S4 – “Daily profile” or is this the average of two days of data (i.e., two points averaged for each time bin)?

Fig 8 – why do only some [INP] points have horizontal lines? The y axis on the top two panels have errors for the lower limit label. What are the measurement uncertainties for [INP] and bacteria/fungi?

L365 – were offshore chlorophyll a concentrations elevated during this study?

L366 – I think it’s great to show the utility of this method for showing the air mass history (i.e., terrestrial versus marine). I suggest pulling this forward in the introduction, as this a unique approach to identify air mass origin (e.g., In this study, we use PCR to confirm air mass history and its influence on [INP]) and also reference any other papers that have attempted this (if applicable).

Conclusions:

L387 – The dates of the study should be specified here (for those who read only the conclusions..)

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L390 – Should report the range of [INP] for a given temperature

L390 – also similar to [INP] measured from U.S. Central Plains (harvesting aerosol), as you mention in the text. I think it should also be clear that the [INP] are high for a marine environment (i.e., comparison to DeMott et al., 2016 marine measurements).

L406 - Could you comment on the representativeness of these measurements for modeling efforts? I.e., would you expect these [INP] to change for different seasons based on Rosinski's work? What size bins would you expect to reach cloud level and therefore what [INP]?

Technical comments:

L2 – “are referred to as ice nucleating particles (INP).” Should be: “are referred to as ice nucleating particles (INPs).”

L3 – “mid- and high-latitude oceans” – I think there is general consensus that bubble bursting at the ocean surface (regardless of latitude) is a source of aerosol

L11 – may be helpful to add the latitude here since your reference “similar latitudes”

L18 – “Biological particles were likely found to be very important” should be “Biological particles were found to be likely important”

L19 – “A variety of bacteria and fungi were identified.” – identified as what?

L20 – “Although the majority are of terrestrial origin, some of them are clearly oceanic.” – majority of what? What is “them”?

L95 – fix lat/lon format

Fig 1 – would be beneficial to add a scale bar to this photos

L138 – is stage one 0.18 micron or 10 micron?

L276 – Are these error bars a standard deviation? Please define in figure caption

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Table 3 – does this source correspond to all of the Genus listed?

References

Bigg, E. K. (1973). Ice nucleus concentrations in remote areas. *Journal of the Atmospheric Sciences*, 30, 1153–1157. [https://doi.org/10.1175/1520-0469\(1973\)030<1153:INCIRA>2.0.CO;2](https://doi.org/10.1175/1520-0469(1973)030<1153:INCIRA>2.0.CO;2)

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