

Interactive comment on “Long-term INP measurements from four stations across the globe” by Jann Schrod et al.

Anonymous Referee #1

Received and published: 30 August 2020

Review of “Long-term INP measurements from four stations across the globe” by Schrod et al.

General comments: The authors made enormous amount of efforts tackling the current challenges of the INP research community – wide spatiotemporal coverage of ambient INP measurements. This reviewer is impressed with a comprehensiveness of this work (for 1212 samples) as well as persistence and articulation of the authors, and supports publication of this manuscript in ACP. The results and discussions provided in this manuscript tightly fit in the scope of ACP. The reviewer has only technical (some are minor) suggestions to make (see below). But, the reviewer noticed different writing styles/tones involved over different sections (before/after Sect. 2.3.). Consistency in writing will improve the readability as well as importance of this paper even more.

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Specific and technical comments:

P1L4: → Unfortunately, only a few ...

P1L14-15: This statement introduces a multitude of perspectives – one may consider physicochemical properties have negligible impact on INP abundance/propensity, thereby ambient INP estimation could be rather simple than ‘complex’. This may be true and somehow supported by what the authors found (i.e., P1L9-11 & P1L18-19; great statements, by the way). Perhaps, incorporating this counter-thought (on top of what already exists) in an abstract and other parts in the main text would increase the readability/flexibility to both authors and readers.

P2L6-8: Depending on... - the reviewer is not sure if this statement is adding any meaningful aspects in this paper. The authors may consider removing this statement. The CCN is not discussed in tandem with INP much in this manuscript.

P2L9: Non-biological organics are deemed to be overlooked here. The authors may review Knopf et al. & Kanji et al.?

P2L19: Vertical distribution – very good point. This is somehow one of the things INP community has been missing for a long time in the reviewer’s opinion. This should be pointed out in the outlook section?

P2L21: ...in identifying globally relevant INP... → ...in identifying some or potentially atmospheric-related INP...

P3L1: The reviewer totally agrees with this statement. This statement is a nice complement to previous studies. Nice writing. P3L2-17: Perhaps summarizing the examined temperature and the n_{INP} ranges from these previous studies in a tabular format with minimum explanation instead of prolonged texts would increase the readability of this section.

P3L20-21: Yes. This is a very good motivation statement. Good job.

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P4L6-8: Seeing long/lat coordinates for these locations in their first appearances would be nice. The reviewer is aware these coordinates appear later on. This is just a suggestion from the reader's perspective. The authors can decide what to do.

P4L11: Please clarify what "semi-automated" really means. Please also clarify how the samples were stored while transporting here. Frozen at a certain temperature all the way? The reviewer is aware that the authors mention an insignificance of storage method on their INP characterization in P5L18-19. Perhaps, transportation and storage discussion can be combined here or P5?

P4L17: factors could include local dynamics, thermodynamics, large scale meteorology, and/or a combination of any?

P4L26-27: So is this correction incorporated/applied in relevant INP # in this study? Please state it if so.

P4L27-: It would be meaningful to have a discussion of all inlets configuration and properties (e.g., length, flow rate – if any, cut-size – if an impactor was used in part, transmission efficiency, transmitted aerosol particle size range etc.) from individual sites here (rather than in Sect. 2.3). Maybe, the authors can use a table summarizing the inlet config., characterization (if done/any). Also, listing previous INP research done at the sites would be meaningful info for the readers.

P5L2: "for use within an INP monitoring network" seems misleading – sounds like a strong promotion. The reviewer suggests altering this to → to collect aerosol particles at multiple field sites for subsequent offline INP analysis. This way, the tone would be reduced, and the point can be made for the concurrent work.

P5L12-13: Please elaborate the difficulties a bit further.

P5L13: Representativeness of local noon & short sampling time is questionable (the reviewer is aware that the discussion is given later on). On the other hand, the reviewer supports the best practice of pursuing consistency with this strategy employed by the

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authors for this study. Perhaps, such should be mentioned here to justify the strategy. The readers will understand.

P6 Sect. 2.3.: Very informative and detailed. But, this section seemingly better fits as SI in the reviewer's opinion. Especially, P6L20-P7L10 & P7L23-P8L20 seem not relevant to the main focus of this study. Putting a subset in SI at the least would even increase the readability – the reviewer's suggestion is based on the readers' perspective.

P10L15: Delete (incomplete).

P10L10-11, 16-12, and 27-29: The reviewer is impressed with these statements. Congratulations on finding these.

P11L1: Besides storage effects, inconsistency in inlet configurations and IN mechanisms can also play a role in the reviewer's opinion. If a proper inlet is not used for aerosol particles sampling, sampling efficiency of the sampler could be affected by local turbulence and other dynamic/thermodynamic conditions (e.g., sampler port get frozen/clogged). These points should be incorporated, otherwise the readers might be misled.

P11L7-8: Add reference(s) for bio-INPs that the authors are mentioning here or elaborate it.

P11L9-16: So what is the implication of such a strong IS dependence? Are the authors trying to point out the condensation/droplet freezing is more predominant as compared to deposition?

P11L19-22: This part is speculative. The reviewer sees lots of "may" words. But, it does justify that the sentence can remain speculative. Please introduce some references/citations to support the authors' idea at the least.

P11L28-29: Yes. The reviewer agrees.

P12L8: That said, → However (too informal for a scientific journal).

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P12L8-9: The source of INPs is important, but how aerosol particles are sampled at the sampling location through what sort of inlets is also an important source of potential data variation. See the reviewer's comment regarding an inlet above.

P12L16: which one is bimodal? Please clarify this in the text.

P12L19-19: distribution analysis with higher sensitivity at high Ts would be a good future work (may be incorporated in depth in an outlook section?).

P12L27-29: This sentence is running too long, diluting an important message. The reviewer suggest breaking it down and carefully reformulate this sentence.

P13L9-10: background air masses mean local ambient T and RH etc.? The authors may want to add "More discussion of insignificant role of local sources is provided in the next section" or something similar to smoothly guide the readers to e.g., P13L31.

P14L1-13: Though the reviewer finds this part (bio aerosol - INP - precipitation interactions) very interesting, some parts sound speculative simply due to the lack of sufficient data – e.g., rain intensity, wind/gust condition, rain duration etc. etc. What is discussed in this sub-section seems supplementary, not the main point of this study. The reviewer suggests either elaborate it rigorously or eliminate it completely.

P14L18: likely → presumably

P14L24: Then, the local source seems important... This seems contradicting to the point made in P13L9-10. Please clarify.

P14L29-31: Very good statement.

P16L5: Given → Due to

P16L5: ...atmosphere, the Arctic... (comma)

P16L13-27: The authors may consider mentioning about a more recent study by Rinaldi et al. (2020 - <https://acp.copernicus.org/preprints/acp-2020-605/>). The reviewer

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believes that findings of Rinaldi et al. (Ny-Alesund, Gruvebadet station through a semi-laminar flow TSP inlet during 2018) are consistent with what is presented in this study (2015-2017). Another place to potentiall add Rinaldi et al. is on P17L11 in addition to Welti et al. (2020).

P16L32: → ... anthropogenic Arctic Haze phenomenon during our study period. The reviewer supports the authors' view, but the authors may want to reduce the tone. Otherwise, it may sound like a personal attack even without an intention. Just a suggestion to be fair on everyone in our community.

P17L4-9 & P17L22: Very good summary – the reviewer's additional hope is a consistency in an inlet sampling system.

P18L17-18: The reviewer disagrees. The finer time resolution of INP measurements for prolonged period of time with a reasonable detection - perhaps by semi-autonomous technique as mentioned towards the end of this section by the authors - is an ultimate goal/outlook for ambient INP measurements in the reviewer's opinion. With a long(er) sampling time, researchers would overlook subtle change in INP episodes or local dynamic condition that has certain roles on INP propensity.

There are quite more important things to be listed as more specific future study ideas out of this study (e.g., inlet consistency, P2L19, P12L19-19 etc.). These could be addressed in this section.

Other general outlook can be made, but the authors may look through Murray et al. (2020 -<https://acp.copernicus.org/preprints/acp-2020-852/>), and adapt the authors' ideas on top? Just a suggestion.

P19L8: Möhler et al. may become publicly available soon. The authors may keep an eye on it, or touch base with Dr. Möhler.

The reviewer enjoyed reading this paper. Hope some of suggestions/comments made here help the authors (and future readers).

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