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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Frequent Ultrafine Particle Formation and Growth in the Canadian Arctic Marine Environment" *by* Douglas B. Collins et al.

**Anonymous Referee #2**

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Frequent Ultrafine Particle Formation and Growth in the Canadian Arctic Marine Environment by Collins et al., 2017 ACPD

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Collins et al. (2017) is a great piece of work characterising aerosol in a difficult environment (the Arctic), on a challenging platform (an icebreaker) for two different years (2014 and 2016). I congratulate very much to the authors, and to the Canadian program NETCARE which should be an example of interdisciplinary studies to follow.

I think the paper should be well accepted in ACP, following few major revisions which I



am confident the author will be able to carry out.

1) It is mentioned a number of times in the text that the Arctic marine microbial communities are likely to be responsible for the large increase of new particle formation recorded in 2016 vs 2014 (41% and 6% of the time, respectively). For example, on page 15 line 16-18 "The source strengths of gas-phase precursors and reactive species, which are generally not well understood in the marine environment, are key remaining factors for explaining differences in ultrafine particle production". Reading section 3.3, one may get the feeling that this is the main reason for the large increase observed in 2016 relative to 2014, given other meteorological and physical conditions did not change substantially. I suggest to modify the abstract (a bit too general in the current state) and report - for example - important conclusion such as line 33 pg 13 "CS may not be a factor that directly limits the formation of UPF in this region". I think is important to stress that chemical precursors (likely coming from Arctic marine communities) may play an important rule in increasing UPF, and physical conditions (different CS, for example) may not be as important as chemical precursors availability. If that is the case, it should be stressed in the abstract, in the current stage too general and not representative of the discussions and conclusions presented across the manuscript.

2) Figure S5 should be a main part of the paper (maybe as new Figure 12) because it stresses a major difference across the two different years (differences up to 13-25% in sea ice concentrations) - therefore associating UFP events to open water, higher percentages of sea ice marginal zones, and less packed ice. On this regards, the authors should refer to a recent paper (Arctic sea ice melt leads to new particle formation, Dall'Osto et al., 2017a, Scientific Reports | 7: 3318 | DOI:10.1038/s41598-017-03328-1), where air mass trajectory analysis and atmospheric nitrogen and sulphur tracers linked frequent nucleation events to biogenic precursors released by open water and melting sea ice Arctic regions. Additionally, when discussing this (I leave to the author if prefer to discuss this in the result discussion part or in the conclusion paper) they should also discuss this potential source (polar open water and sea ice marginal Arctic

sea ice regions) and put into context another recent paper from NETCARE (Croft et al, 2016, Nature Comm, another possible source of UFP related to bird colonies).

Minor comments

- Section 3.3.2 - oceanic conditions. I congratulate to the authors for improving the paper with this interdisciplinary part, an interesting one. Whilst DOC - among other marine biological measurements - during different years were almost identical, Figure S6 shows interesting differences for nitrate (among other variables), suggesting phytoplankton production season was more advanced and well into post bloom phase during 2016 (relative to 2016). A recent paper (Dall′Osto et al., 2017, Antarctic sea ice is a source of organic nitrogen in aerosols, Scientific Report, DOI:10.1038/s41598-017-06188-x) also go in the same direction, showing sea ice marginal region with more advanced post bloom phase enhanced in UFP. It may be worth to stress that in polar regions (both Antarctic and Arctic) the biology is playing a role (and seems not Chl, but the stage of the bloom, is the key factor) and more interdisciplinary studies are needed.

- pg 3 line 30-25. Whilst the authors do a decent job in addressing the different chemical precursors, it may be more appropriate to cite only works carried out in Arctic regions (not Atlantic or other oceans) and not forget Sippila et al 2016 (Nature) and also to address recent new findings (Croft et al., 2016, Birds colony emissions) and marginal sea ice (Dall′Osto et al., 2017a, Scien Rep).

- Pg 11 line 2-5, it is possible to access the importance of coastal vs open ocean sources? As Rev 1 suggests, is this study more related to a specific environment, such as Archipelagos, and not to be extrapolated to open ocean and marginal sea ice zone Arctic areas?

- pg 15 line 23 - I think it is figure S5

-pg 15 line 22. I think the authors should improve this section and decide what is more appropriate (if include figure S5 as figure 12, and expand this section). I think maybe



presenting an average map for the two different seasons (2014 and 2016) but I am not sure the 1st of August is representative, I would use a longer period, or present figure S5 in the main text.

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