

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2021-819-RC2>, 2021
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Comment on acp-2021-819

Anonymous Referee #2

Referee comment on "New particle formation in coastal New Zealand with a focus on open-ocean air masses" by Maija Peltola et al., Atmos. Chem. Phys. Discuss.,
<https://doi.org/10.5194/acp-2021-819-RC2>, 2021

The manuscript presents analyses of comprehensive measurements of aerosol number size-distributions from 1 nm to 500 nm, together with supporting data on meteorological variables and airmass back-trajectories.

The dataset presented is valuable and the results provide information on atmospheric new particle formation (NPF) occurrence from the Southern hemisphere and at a marine airmasses influenced area. This represents an environment type from where there are only few previous similar analyses in the literature, as most atmospheric NPF studies are concentrated on Northern hemisphere continental areas. The manuscript is within the scope of Atmos. Chem. Phys. Besides the comments given by Referee #1, I have listed below my additional comments. After considering these comments, I would recommend the manuscript to be published.

General comments

I suggest adding to Section 2.1 a map, which shows the location of the measurement site in New Zealand, with major cities indicated. This would help the reader to have a clearer picture of how the site is situated with respect to sources of at least some anthropogenic emissions (which was also pointed out by Referee #1). The sector used for the marine air mass arrival direction (120-220°) could be indicated in this figure, since marine airmasses are one of the focus points of the study.

In Section 2.3.2 (page 7, lines192–194) it is stated that the median of the ratio of the concentrations of the CPC and SMPS was used to correct the concentrations of the SMPS.

How much does this ratio of the concentrations vary? I would expect it to depend on which size particles the peak of the size-distribution is. Can you estimate, how much uncertainty it introduces to the calculated particle formation rates, when you use a single correction value to the whole SMPS data timeseries?

Section 3.1.1 (page 10, lines 286–289) discusses the influence of time that airmasses spent over land on the NPF characteristics. In the text, you refer to Fig. 3 which however shows the fraction of time spent in marine air. This feels a bit difficult to understand, how do you see the results presented in the text from Fig 3. Are the marine and land-based airmasses complementary to each other (or can there be airmass which is not classified to either of those categories)? I would suggest to present in Fig 3 results of NPF characteristics as function of the time-over-land, which would more clearly relate to the discussion in the text.

Page 11, lines 296–297: Please give in the text the altitude which was used as the threshold between boundary layer and free troposphere. HYSPLIT model output could also be used to estimate the boundary layer height at each location along the trajectory (based on the input meteorological data). This could be potentially provide more insight into the division of boundary layer vs. free tropospheric airmasses, but I do not require that this type of further analyses needs to be added here.

In Figure 4, please specify are the lines medians or averages of the data. I strongly recommend adding some measure for the variability of the data, for example an area showing the 25th and 75th percentile (if the lines shown are median values) or standard deviations (in case of averages). Based on this, the statistical significance of the differences between conditions on NPF and non-NPF days should be shortly discussed in the text.

In Figure 6d, the diurnal variation of N_{1-10} seems to have a clear pattern which is clearly different from the diurnal pattern in land-influenced N_{1-10} : in marine air there seems to be an early morning peak (around 05:00) and this could be actually linked to the example case shown in Figure 11 (high N_{1-3} and N_{1-10} concentrations after midnight on 9th Nov 2020). This pattern should be more clearly highlighted in the text, since the marine NPF observations are one the most interesting findings of this study.

In Figure 7, the second order polynomial fits should not be used. In my opinion they do not represent the trends of the datasets (as is also said in the manuscript), and there is also no physical reasoning why the particle concentrations would follow a second order polynomial of the time-over-land. It's also potentially misleading to interpret anything based on the extrapolation of polynomial fits, as is done from Fig. 7c on line 426 ("... according to the fit it would only start decreasing after 101 h"). I suggest binning the data based on time-over-land and analyzing the trends in the distribution (medians and 25th–75th percentile) of the binned data (similar to the suggestion from Referee #1).

The discussion related to Figure 13 on the differences between conditions during the cases of high N_{1-10} ($>500 \text{ cm}^{-3}$) and low N_{1-10} ($<500 \text{ cm}^{-3}$) seems to overstate these differences. The differences in temperature, RH and wind speed are highlighted in the text (lines 599–600) but for example ozone is said to have small difference between these cases (line 596). Only looking at Fig. 13 I would not draw this kind of conclusion. Have you done any statistical test, which could tell if there are statistically significant differences at any of the studied variables between these two cases? For example, two-sample t-test could be used here.

Minor comments and technical corrections

Page 3, line 62: "... can contribute between 92–49 % ...", I suggest writing this the other way around as "49–92%"

Page 4, line 93, "... marine masses ...", should be "... marine air masses ..."

Page 5, lines 134–135: Why is the measurement range of ions reported as mobilities (electrical mobility is the primary variable of the NAIS in both particle and ion mode, as well as in SMPS)? It's not logical to write some of these in diameters and some in mobilities; I suggest stating also the ion measurements in nanometer size range.

Page 6, lines 144–145: Reference format should be without the dates "Chambers et al., 2014"

Page 9, line 266: "pre-exiting", should be "pre-existing"

Page 13, line 357: The end of the sentence "... more intense than over land" should probably be "... more intense than over sea".

Page 19, line 508: I suggest repling "... particle emitting precursor ..." with "... particle forming precursor ..." (a precursor is related to formation, not emissions)

Page 20, line 520: The highest ion concentrations in Fig. 10b are in southwestern, not northwestern airmasses.

Page 24, Figure 13 caption text: Add the explanations for the circles, box widths etc. (similarly as in Figs. 5, 6 and 8).