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Interactive comment

Interactive comment on "New Particle Formation in the South Aegean Sea during the Etesians: importance for CCN production and cloud droplet number" *by* P. Kalkavouras et al.

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We would like to thank both reviewers for their comments and recommendations. We believe that we have corrected and improved the paper by incorporating their comments, in the revised version. The figure proposed by the first reviewer was a very good idea where we had to clarify several points of 'our story' to provide sufficient context. We reran the simulations at higher resolution, replaced figures and modified the discussion, accordingly. The main changes are the following: Following both Reviewers' comment, regarding the model's estimation of the simulated new particle formation, we reran the model by ignoring NPF process. In the revised manuscript, section 3.5 is divided in 2 sections: 3.5 is called "Impact of NPF events on CCN production" and 3.6

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Discussion paper

“Impact of NPF events on cloud droplet number.” We followed first reviewer suggestion to use for the two types of northern flow the terms: Etesian Flow (EF) and Moderate Surface Flow (MSF), in order to have a more concise wording. We also followed the same formalism in the revised Tables and Figure captions.

Reviewer#2 The manuscript presents measurements of the number size distribution and chemical composition of submicron aerosols at two islands in the Eastern Mediterranean. The analysis is based on a measurement period over two weeks in the summer 2013, during persistent transport of continental airmasses from north to the sites. A chemical transport model and airmass back-trajectories are used to identify the source areas and transport routes of aerosols to the sites. Using case studies of two new particle formation (NPF) events the contribution of NPF to both the cloud condensation nuclei (CCN) and cloud droplet (Nd) concentrations is assessed. The results for CCN and Nd are based on Köhler theory and parameterizations. I agree with the comments presented by the anonymous referee #1, and would like the authors to address my further comments below. After addressing these comments I can recommend the manuscript for publication in Atmospheric Chemistry and Physics.

General comments: Page 5, lines 6–7: Is it known what are the possible reasons for the underestimation of organic matter concentrations in the model results; could it be due to underestimation of primary emissions or underestimation of SOA formation in the model? The biases are probably related to the underestimated POA emissions but also to the limitation of the RADM2 mechanism regarding the treatment of monoterpene emissions (Tuccella et al., 2012). This information is now included on page 8, lines 4-5. WRF-Chem simulations over the Aegean Sea during Etesian flow revealed that the simulated SOA, formed from anthropogenic and biogenic emissions, contributes respectively to less than 5% and almost negligibly to the OM (Bossioli et al., 2016). The importance of secondary aerosols over the area has been pointed out in earlier works (Athanasopoulou et al., 2015; Fountoukis et al., 2011)

Page 5, lines 8–12: Care should be taken when using the HYSPLIT model with the

GDAS 0.5_ input data: the back-trajectory results might differ from those obtained with GDAS 1_ input data due to the differences in the airmass vertical advection calculation method between these two datasets (see e.g. Su et al., 2015). Perhaps the authors could check that their back-trajectories shown in Figure 2 remain the same if using the GDAS with 1_ resolution as input meteorological data.

There are no significant differences, especially at low levels. The differences are mainly noticed on the 24th, but they do not change the hypothesis that air masses are better mixed throughout the boundary layer, covering a broader area over Asian Turkey. See attached figure.

Page 8, lines 6–7: Why are coagulation losses not included in the calculation of the formation rate of nucleation mode particles? This should be fairly straightforward to calculate based on the measured size distributions, and including the coagulation losses would make the calculated formation rates more readily comparable to literature values (which typically account for coagulation).

Both coagulation flux and condensational growth are now included in the calculations. The text has been modified accordingly (page 12, lines 10-14).

Page 10, line 4: Where does the 3 hour difference in the comparison between particle observations at Santorini and Finokalia come from? Based on the particle size distribution data in Fig. 8 the particle formation at both stations seems to start at 9 a.m. on 23 July, and the only appreacable difference in the particle concentrations in Fig. 4 seems to be in the nucleation mode concentration (i.e. intensity of particle formation). Regarding the discussion on the CCN-sized particles and the calculated hygroscopicity parameters, it would be interesting to see how the results differ on days without new particle formation. This type of comparison between NPF and non-NPF days would put the results presented in the manuscript better into context with regard to the importance of NPF to CCN and cloud droplet number at the Aegean Sea. Where there during the campaign any such non-NPF days for which the parameters of Table 3 could be calculated and reported for comparison with the two NPF days?

We agree with the reviewer that this was not clear in the text. The air masses spent 3-4 h to reach Finokalia after Santorini, according to HYSPLIT (Fig. S3 left panel), on 23 July. The 3-h transit timescale is in agreement with the prevailing wind speed (about 10 m s-1; Fig. S1) and the 120 km distance between Santorini and Finokalia. For this reason, we claim that the air masses reaching Finokalia earlier (Fig. 4) are probably due to a local nucleation event initiated at Heraklion (Crete).

Throughout the non-NPF events (MSF period), the CCN concentrations decrease by almost 48% and 23% at Santorini and Finokalia respectively, compared to the levels during the NPF events. We have added this information on page 17 (lines 22-24) but we decided not to change Table 3.

Minor and technical comments:

Page 2, line 30: The sentence starting with "Short-lived events of small number young Aitken particles" is difficult to understand, consider revising it. Does "small number" refer to low concentrations?

This sentence has been replaced by (page 3, lines 18-19): "A few short-lived particle formation events (18–25 nm) were first recorded at Finokalia by Kalivitis et al. (2008), arriving with low speed from the west, during autumn."

Page 3, line 4: should be "prior to reaching "

Done

Page 7, line 2: "non-refractive" should be "non-refractory"

Done

Page 8, line 21: A more recent reference for NPF event classification is Kulmala et al. (2012).

Done

Page 10, line 3: In the sentence “ : : : have trace a lower number of : : :” the word “trace” should be omitted.

Done

Page 10, line 21: As also suggested by the other referee, Section 3.5 could be divided into two parts, one dealing with CCN concentrations and another dealing with cloud droplet concentrations. That would make this section more readable.

Done

Page 13, line 30: “: : : have a similar to ozone behavior : : :” should be “: : : behave similarly to ozone : : :”

Done

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2016-330/acp-2016-330-AC2-supplement.pdf

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