

Interactive comment on “Atmospheric new particle formation as source of CCN in the Eastern Mediterranean marine boundary layer” by N. Kalivitis et al.

Anonymous Referee #2

Received and published: 28 April 2015

General comments:

I recommend to publish this paper upon minor revisions.

The paper present very important findings to be able to understand how important new particle formation in marine areas is for CCN concentrations. Only superficial attempts have been made previously to elucidate this matter.

English language is very clear.

The abstract is short and to the point. The Introduction clearly presents the problem issue at hand, and clearly formulates what measurements are available the research

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questions addressed. Short and to the point sufficiently described method section. The result section is also condensed, and only accounts for the most important findings.

One discussion topic is omitted in the conclusions section (see below), but otherwise very useful conclusion for future research in this area, where future needs are clearly described.

Specific comments:

Abstract:

“(0.2–0.4 lower kappa between the 60 and 120nm particles)”. Unclear sentence, please rephrase.

Introduction:

“The probability by which an aerosol particle acts as a CCN at a given supersaturation depends primarily on its size and secondarily on its chemical composition (Dusek et al., 2006). The aerosol chemical composition may, however, have large impacts on the total CCN number concentration (Karydis et al., 2012; Padró et al., 2012).” Contradictory statement. In the first sentence you say that chemical composition influence CCN, and in the second sentence you write that, chemical composition may **HOWEVER** have influence on CCN.

Chapter 2.2.

Page 1148. How was the BC concentration determined from the absorption coefficients? Please write if you made some kind of own corrections, or used the corrections found in other literature, or if you just used the BC values that the instrument spits out without further correction.

Page 1149, line 2. “this” should be replaced by “which”.

Page 1149, line 4. “We made regular calibrations”. When did you actually do these calibrations? Please write the dates down?

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Chapter 2.3.

Page 11149. I never worked with scanning flow rate of CCNc: Is there a problem with double charged particles from the DMA in the CCN/CN versus flow rate curve when obtaining critical flow Q50? Could be worth mentioning if double charges sometimes play a role. One example is Snider et al., 2010: JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 115, D11205, doi:10.1029/2009JD012618, 2010.

Page 11150. About ME-2 and PMF. I associate PMF with a specific factor analytical tool. I think of ME-2 also as a specific factor analytical tool, but different from the PMF tool. Hence, I would recommend not to refer to your method as PMF, but rather as ME-2 throughout the paper (not only this chapter), and skip writing about PMF at all.

Chapter 3.1.

Page 11153, lines 10-12. Please indicate that this parameterization for the CCN vs N100 is valid for the NPF periods.

Figure 10. The labels for the different colors are missing.

Conclusions:

It is very important that you mention how often you have such regional new particle formation events, which can give high CCN production. Your referenced papers from Finokalia station clearly show that these kind of strong CCN-producing new particle formation events do not happen very often each year as compared to continental events. Please write how often and write a discussion about this. Otherwise, a reader, which only reads the abstract and conclusions might get the impression that these type of strong events happen very frequently during the year.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 11143, 2015.