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Interactive comment on “Production and growth of new particles during two cruise campaigns in the marginal seas of China” by X. H. Liu et al.

Anonymous Referee #1

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This manuscript investigates atmospheric new particle formation in a polluted marine environment. The paper addresses an important topic and reports on observations that are original enough for a scientific publication. The analysis conducted in the paper has, however, a few weaknesses that require more work before I can recommend publication. The detailed comments are given below.

Main issues

The authors motivate their research with the potential connection between marine new particle formation, CCN production and climate (CLAW hypothesis). The problem with this approach is that the CLAW hypothesis can only be investigated in a remote marine environment where the anthropogenic influence is minor. The investigation of this

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paper has been made in a polluted marine environment and, as also the authors state in there paper, the observed new particle formation events seem to be associated with continental pollution. The character of marine new particle formation is very different between the remote marine boundary, polluted marine air, and some coastal environments such as Mace Head. The authors should make a clear difference between these different marine environments and put their investigations into a correct context. The same should also be kept in mind when analyzing the results (e.g. section 3.1).

The paper does not define its scientific goals properly. It is only stated that "to improve understanding on these issues..." (line 22 on page 3045). Which issues? What are the concrete scientific questions this study aims to answer?

The causes for new particle formation and growth (sections 4.1 and 4.2) have been analyzed solely based on the CMAQ model results. This is problematic for several reasons. First, CMAQ does not include marine aerosol precursor emissions. Second, CMAQ simulates PM2.5 mass, but neither the particle number size distribution nor the distribution of chemical species over different particle sizes. Third, there is no separation between SOA of different volatility in CMAQ. Because of this, CMAQ simulation results are only indicative of causes of new particle formation and growth and should be interpreted with extreme care. For example, the presence of ammonium nitrate or SOA in PM2.5 does not guarantee that the same species would contribute to new particle formation and growth. Several studies indicate that semi-volatile SOA is very inefficient in growing newly-formed particles (see Riipinen et al. 2011; Ehn et al. Nature 2014).

Minor/technical issues:

Lines 4-14 on page 3045: The authors might consider citing the recent overview by Kerminen et al. (2012, Atmos Chem Phys 12, p. 12037) on CCN production associated with atmospheric nucleation here.

Past tense should be preferred in sentences like to in line 6 of page 3046, and lines 9-10 of page 3047. Please check out throughout the text.

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I do not understand the first sentence of section 4.1. Is this a general statement? If yes, then a present tense rather than past one is needed.

Page 3056, line 2: "much low mixing ratio", improper wording.

Page 3056, line 6: 3 ppb does not sound a very low SO₂ concentration to me. In many continental locations, there is plenty of gaseous sulfuric acid even at much lower SO₂ levels. It is the balance between sulfuric acid sources and sinks that determine its concentration, not just the SO₂ concentration.

I am not convinced about the particle shrinkage discussed on pages 3051 (line 1) and 3056 (lines 14-22). Are the authors sure that the observation represents particle shrinkage? It might also be due to slight changes in measured air masses, especially and the new particle formation and growth seems to take place in a plume of continental outflow. Furthermore, it is definitely not only the Kelvin effect that matters in partitioning SOA between particles of different size (e.g. Riipinen et al. 2011), as claimed on lines 14-22 on page 3056.

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