

Interactive comment on “Implementation of state-of-the-art ternary new particle formation scheme to the regional chemical transport model PMCAMx-UF in Europe” by E. Baranizadeh et al.

This manuscript presents the implementation of a new ternary H₂SO₄-H₂O-NH₃ parameterization, into the PMCAMx-UF model. The authors explore the ability of the model to reproduce observed number concentration during May 2008, when the intensive observation period of EUCAARI project took place. Apart from the testing of the new parameterization, sensitivity tests using the scaled Napari parameterization and sensitivity to the radiation scheme and natural emissions were performed. The topic and overall approach fits with GMD; therefore, I am in favor of accepting this work for publication in GMD after the authors have addressed the issues summarized below.

Major issue:

While several sensitivity tests are done, the paper lacks a proper statistics for each test. Figure 2 shows the results coming from ACDC-RADM-DE sensitivity study, however no information is given for the other studies presented in Table 1. It would be nice to see some numbers (r, over/under estimation factor, bias), to endorse the statement “Overall, we consider our results very promising: a NPF scheme based on first-principles theory and no artificial scaling is shown to be a promising alternative to semi-empirical approaches in the description of particle formation in large scale atmospheric models.”

Specific comments:

L82: Should be “Matsui et al., 2011, 2013”, not “Matsui et al., 2011a, 2013c”.

L88-90: Matsui et al., 2013 study, already mentioned by the authors, have also assessed the ability of WRF-Chem to reproduce the vertical profile of observed Aitken particles for South Asia.

L133-134: Should be “Yu et al., 2006”, not “Yu et al., 2006a”.

L210-213: I assume that if the H₂SO₄, NH₃, RH, temperature and condensation sink are not falling into the mentioned range, the Vehkamäki et al., 2002, parameterization is applied. Is that right?

L330-336: The authors show the scatter plots of predicted PNC using ACDC-RADM-DE simulation vs observed PNC in several size ranges. Yet, at line 303 they state that ACDC-TUV-DE is the baseline simulation. Do they have any particular reason not to present the results coming from the default simulation? As can be seen in Table 1 the differences between the ACDC-RADM-DE and ACDC-TUV-DE simulations are minors. Furthermore, they use the ACDC-TUV-DE simulation results for the following plots. A little bit confusing.

L373-379: Could you give an explanation why N₄ concentration increases in the upper boundary layer for ACDC-TUV-DE simulation? May you could present the particle formation rates for the ACDC-TUV-DE and Napari-TUV-DE simulation. Also, could you give an overestimation factor?

L384-389: An index of agreement will sustain “the scaled Napari NPF scheme agrees reasonably well with the observations throughout the atmospheric column” and “reasonably well” statements.

L433_435: The following sentence for a more scientifically sound expression should be rephrased: “We believe this is the first time that reasonable particle concentrations have been produced in a large-scale atmospheric model with a NPF scheme without any scaling factors or location/condition dependent semi-empiricism”.

Conclusion section: The authors should be more restrictive in using “reasonably well”, “are somewhat overpredicted by the ACDC-based NPF scheme”, “very promising” statements due to the fact that the lack of statistics throughout the paper does not sustain their claims.