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Interactive comment on "MATCH–SALSA – Multi-scale Atmospheric Transport and CHemistry model coupled to the SALSA aerosol microphysics model – Part 1: Model description and evaluation" *by* C. Andersson et al.

Anonymous Referee #1

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General comments:

The article reports on the implementation of the aerosol model SALSA in the regional chemistry-transport model MATCH (Multi-scale Atmospheric Transport and Chemistry). The technical representation of processes relevant for the description of the aerosol life cycle in the model is documented in detail. In addition an evaluation of the model results by means of comparisons with observations is presented. The manuscript thoroughly describes the model components and their coupling as well as the set-up chosen for performing reference simulations. The results of comparisons

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of the simulations with measurements are highlighted in detail. It is particularly valuable that many suggestions for further model improvements are provided. For these reasons, the article is of particular value for the atmospheric modelling community.

Since the major focus is the description of a new model system as well as the evaluation and interpretation of the resulting model results, the paper is well suited for publication in GMD. The paper is generally of good technical quality. It is well written and - in most parts - well organized. Model concepts and evaluation results are clearly presented. Relevant literature is referenced thoroughly. Unfortunately, the discussions of model discrepancies and possible reasons and model improvement solutions are provided in separate sections what affects the readability of the article. I recommend publication after the following comments have been addressed by the authors.

Major comments:

1) Section 4 of the article focusses on comparing model results with measurements. Unfortunately, possible reasons for deviation of model results from measurements and corresponding suggestions for model improvements are mostly provided in section 5. This considerably affects the readability of the article since the reader already expects such information when reading section 4. Some explanations are provided in section 4 but the corresponding discussions are comparably sparse. For instance, the overestimation of PNC in Melpitz due to nucleation is briefly explained in section 4.2.1, but reasons for underestimation at other sites are not discussed. As another example, in section 4.2.3 it is discussed that the reason for the maximum occurring at too small sizes may be too little condensation onto nucleating particles in the model. However, the reader misses a subsequent discussion why condensation is too inefficient. The reader misses such information when reading section 4 but is surprised to find such details in section 5 later on. To enhance readability of the article I would suggest skipping section 5 and discussing the reasons for discrepancies and possible model improvements directly in the context of the model comparisons with the observations (section 4). A summary of the major improvements needed could be included in the Conclu-

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sions section. If the authors decide to refrain from merging section 4 and 5 in this manner, I would urgently change the title of section 5 since 'Identified issues' sounds somewhat meaningless. A possible title could be 'Major reasons for discrepancies and suggestions for model improvement'. Choosing such a title would show readers of the previous sections that this important information is given later in the paper.

2) It should also be discussed in the manuscript how the results of MATCH-SALSA compare to other European-scale aerosol model results described in the literature. This would show whether the discrepancies found by the authors are model specific or common features of regional aerosol models. If some of the discrepancies occur also in other models they could be due to external forcings, such as an underestimation of emissions or general lacks of knowledge e.g. about SOA formation. Such analysis would help to evaluate the overall quality of MATCH-SALSA.

Minor comments:

1. Abstract: The acronym SALSA should be explained.

2. Page 3269, line 20: The statement 'PNC was not described' should be discussed in more detail. Since fixed particle sizes were assumed PNC could have been derived from total mass of the respective particles. The authors probably mean that prognostic equations for PNC were not included.

3. Page 3269, line 25: Is this really an iteration (i.e. are the different operations passed multiple times within each time step)? If not, the term 'integration' might be more appropriate.

4. Figure 1: It should be specified which parts of the flow chart show MATCH and which parts characterize SALSA operations. It is also not clear why output from the aerosol microphysics module is needed as input for the meteorological part of the model. This should be specified in more detail and Figure 1 should be modified accordingly.

5. Page 3271, lines 15-16, '. . .and a few heterogeneous reactions for nitrogen com-

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pounds are included in the model.': Since no reference is provided here, some more details should be added (which nitrogen compound react on what kind of surfaces? Which uptake coefficients are used?).

6. Page 3272, lines 13-14: '. . . size bins with a constant internal volume ratio.' should be explained in more detail. What is meant exactly?

7. Page 3272, line 15, 'are that are': Skip first 'are'.

8. Page 3272, line 25, 'shrinkage of particles': It should be explained how particles can shrink in the model. Since semivolatile species as nitrate or ammonium seem to be neglected, this could only happen due to water evaporation. Or are other mechanisms relevant here? Are the simplified treatments of nitrate and ammonium (see next point) capable to simulate shrinkage?

9. Aerosol nitrate and ammonium are included by means of a simplified treatment. Since these compounds can be quite important, possible consequences of this simplification need to be discussed. The simplified treatment should be explained in more detail.

10. Page 3274, line 6, '. . . sub-cloud scavenging is neglected for these species': It should be discussed why this simplification is justified.

11. Page 3276, lines 3-8: In the description of the size distribution settings it is mentioned that different bins are used for soluble and insoluble particles but mixtures of these particle types seem to be not considered. In the beginning of section 2, however, the authors mention that also mixed particles can be represented in the model. This is also suggested by Figure 2 where 'aged' particles are mentioned. Hence it is not clear how aged or mixed particles are considered in the model runs. It seems that the bins termed soluble here include also the mixed particles. This however would imply that the model is not capable to represent purely soluble particles. This should be explained in more detail in the manuscript

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12. Page 3277, lines 1-2, ‘The emitted sulfate mass was distributed over particle sizes in the same manner as OM.’: It should be explained how these compounds are distributed over the different particle sizes and appropriate references should be given.

Editorial changes:

1. Figures 1 and 8: Some fonts used are hardly visible even when the figure is enlarged. Larger fonts need to be used.

2. Page 3279, line 2: Replace ‘is general’ by ‘in general’.

3. Figure 5: The legend (description of colour bars) is hardly visible and should be enlarged. The colours are hard to distinguish and should be replaced.

4. Figure 6: The legend (description of colour) is hardly visible and should be enlarged.

Interactive comment on Geosci. Model Dev. Discuss., 7, 3265, 2014.