Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-624-RC2, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "MICS-Asia III: Overview of model inter-comparison and evaluation of acid deposition over Asia" *by* Syuichi Itahashi et al.

Anonymous Referee #2

Received and published: 1 October 2019

Review of MICS-Asia III: Overview of model inter-comparison and evaluation of acid deposition over Asia by Itahashi et al.

The paper deals with an intermodel comparison of wet deposition patterns for East Asia given present day conditions. The models used in this study are chemistry transport models and meteorological data is provided by external model fields (for most models form WRF). In general this study is worth publication, however the gain of new scientific knowledge from this effort is rather small. Consequently, would have proposed that this manuscript is much better suited for GMD instead of ACP as a typical evaluation paper. The paper is decently structured and despite some minor required language improvements (proof-reading) could be easily published. The model and observation data description sections are adequate; however if it might be possible to add the

observation data file in the supplement more model applicants would benefit from this study.

Despite the model domain is very similar (if not identical) in the simulations and the models use unified emissions, substantial differences in the deposition maps occur. Unfortunately, the authors do not show a comparison of the simulated concentrations or tropospheric vertical columns of the species the study focusses on. Therefore, it is difficult to judge, whether the differences occur from slightly different assumptions in the wet deposition schemes, the simulated precipitation amount or the simulated concentrations of the trace species and their precursors. As the applied chemistry and aerosol schemes differ to a certain degree, this could already be a major cause for the differences in the wet deposition patterns.

The evaluation of the precipitation is unfortunately only superficial. As only monthly mean precipitation is compared with the simulation results, the corresponding frequency distribution, i.e. the number, duration and intensity of the events cannot be determined. However, this is crucial for wet deposition, as a few short but intense rain events result in less deposition compared to longer precipitation events of average intensity. This will also substantially impact the precipitation adjustment (see comment below!).

Analysing the wet deposition of sulphur, M11 shows a substantially higher deposition pattern in China. What is the reason for this? This is a typical example for a model inter-comparison study where data is compared, but the causes for the differences are not analysed in detail. Has there been an issue with SO2 emissions or conversion from S(IV) to S(VI)? Is there a bias originating from seasalt sulphate? Is total sulphur completely overestimated in this model? Or is it completely depleted, as wet deposition is so efficient? These differences require much more analysis for a consistent inter-comparison study.

Especially, when creating ensembles including such outliers, the ensemble mean can even be deteriorated compared to individual simulations. This does not appear to be the case in this study, as the M11 simulation compensates some of the low bias from the majority of the other simulations. A similar behaviour of overestimation is not as obvious for nitrate and ammonium, leading to the impression that this is not necessarily a consequence of the wet deposition scheme.

The weighted ensemble might be a better option to reduce the importance of outliers; however, it simply states that the models which show best agreement with the observations should be used for the ensemble mean. Consequently, it reduces ensemble spread and therefore does not cover the whole range of simulation results properly. Please state explicitly, what you hope to gain from the weighted ensemble mean.

Concerning the total deposition maps, the authors should clearly point out, that underestimated wet deposition can often be compensated by overestimated dry deposition and vice versa, as both processes depend on the atmospheric burden (or near surface concentrations).

I (personally) see the option of precipitation adjustment to improve the consistency of the simulation results with observations very critical. This adjustment does not include any kind of frequency distribution of precipitation events, the vertical extent of the precipitation (and hence the accessible fraction of the tracer vertical column for wet deposition). Also it does not include any kind of vertical redistribution by scavenging and subsequent evaporating precipitation and hence tracer release at lower altitude. Of course, I agree that with wrong precipitation amounts it will be impossible to fully match observations, but in my opinion not only the total amount of precipitation, but at least the central moments of the precipitation frequency distribution should be matched. As this correction is applied to the offline data, it could happen that an already strong precipitation event which might have a scavenging rate of 100% (i.e. all sulphate is already removed by the event) is supposed to remove even more sulphate (which is not available, as it is already depleted!). This is not discussed at all, implying that this precipitation adjustment is a useful measure to correct wet deposition for precipitation

biases.

Overall, I think that this study could be published after addressing the points above, but GMD would have been the better journal.

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