



Interactive
Comment

Interactive comment on “Evaluating a lightning parameterization based on cloud-top height for mesoscale numerical model simulations” by J. Wong et al.

Anonymous Referee #2

Received and published: 28 December 2012

The paper "Evaluating a lightning parameterization based on cloud-top height for mesoscale numerical model simulations" by Wong et al. deals with parameterized lightning using the commonly applied lightning scheme by Price & Rind in the regional chemistry weather model WRF-CHEM.

The paper is well written and points to interesting results in the applied system; however for me it is relatively difficult to assess to which degree the results from this study can be applied to similar modeling questions, e.g. to other regional chemistry models. Hence, the paper is missing a “discussion section”, in which this question could be answered.

After clarification of some minor aspects, the paper merits publication in GMD.

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1. The Grell parameterisation of convection should be described in a little more detail (e.g. how the convective microphysics are treated) as it is crucial for the input to the lightning scheme.

2. Precipitation is used as a measure how well the convective activity is reproduced by the model. This is a little critical as lightning activity depends (in reality) more on other aspects of convective cells than the precipitation, i.e. the dynamical part of the convective clouds such as the vertical motion. Of course, the convective precipitation provides a reasonable estimate of the spatial distribution of convective activity and hence lightning. However, analyzing the mean convective mass fluxes and their spectra would shed more light onto the substantial changes when other horizontal resolutions are applied.

3. In Figure 2, the strong precipitation regions are difficult to distinguish, however, they are the most interesting ones for lightning activity. Even though the precipitation spectra match the observations well, the strongest extreme events are not captured, which might have some implications on the lightning results.

4. In Figure 3, the dotted line appears to be more or less on the solid one; hence all the precipitation is subgrid-scale. Even though convection will substantially contribute to total precipitation, it is a little surprising that none of the precipitation events are on the scales of the grid cells and therefore handled by the grid-scale cloud scheme, especially in the eastern part of the analysis region.

5. The interplay of the convection parameterization and the grid scale clouds is crucial for the determination of lightning. Even though precipitation seems to be dominated by subgrid-scale rainfall, the properties of the associated clouds on the grid scale can be of relevance for the lightning production. Starting at 36km this becomes even more prominent for the smaller scale grid cells with improved resolution. This interplay should be analysed in more detail.

6. The model overestimates the lightning substantially along the Eastern coast of the

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US, similar to the precipitation; is this due to too much moisture input from the Gulf of Mexico or caused by other meteorological features? On the other hand, can you provide a (physical) reason why the lightning activity in the central part of your analysis region is underestimated? According to the precipitation it is not obvious where this underestimation originates from? This potentially supports the aspect of point #5.

7. How well do the total simulated flashes match between WRF-CHEM and LIS/OTD data. Maybe the differences originate from the CG:IC ratio and its parameterization and not only from the convective activity.

8. Please provide a reason, why you use the (modified) LNB as a proxy and not the convective cloud top height, or the level of maximum detrainment. If the convection is parameterized, then this would be more consistent than neglecting all entrainment modifications of the buoyant parcel, and assuming simple or complex correction terms.

9. Analysing the resolution dependency you find a substantial overestimation with smaller grid size, such that you need an additional correction factor. You provide an argument that this is needed as the data from which the parameterization has been derived has been compiled on much larger scales. For me, this is not an obvious explanation, as already the 36km are substantially smaller than the original grid size, and hence a reduction of 1/3 of the grid size, does not substantially change the physically resolved processes, i.e. individual convective cells are still parameterized, whereas organized convective systems will be (partly) resolved in both WRF-CHEM simulations whereas they are still subgrid scale with respect to the original $8 \times 10^\circ$. Is the convection getting more intense with the smaller grid size? To which degree do the changes in resolved versus subgrid scale clouds contribute to this? How much does the vertical grid scale velocity change, i.e. what is the w_{\max} in both simulations?

10. Going to even smaller sizes, the skill of the precipitation prediction is substantially decreased overestimating rainfall, but the w_{\max} approach now underestimates lightning activity. How does this fit into the context of point #9?

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11. Using the Boccippio data, can you derive an estimate how an alternative fitting function for the IC:CG ratio would look like? This could be used as an alternative, if it matches the spectra better.

12. The last paragraph of the conclusion discussing LNOX would fit better as a motivation into the introduction and is not part of the conclusions, as none of the statements are discussed in the manuscript. Consequently, this does not belong into a conclusion section of a paper dealing with flash parameterization. This paragraph should be eliminated completely.

Interactive comment on Geosci. Model Dev. Discuss., 5, 3493, 2012.

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