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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A scale and aerosol aware stochastic convective parameterization for weather and air quality modeling" *by* G. A. Grell and S. R. Freitas

Anonymous Referee #1

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This paper reports on the evaluation of two distinct strategies for improving the stochastic cumulus parametrization of Grell and Devenyi (2002) to take into account the effect of grid refinement and aerosol concentration in the parametrized convection and rainfall. This is an important step forward in the quest for convective parameterizations that are capable to perform in the so-called grey zone (between roughly 1 km and 20 km) where the grid cell size is too small to justify the assumptions under which most convective parameterizations have been derived and too large to fully resolve convection; The parameterization finds itself competing with the resolved dynamics and the results are often not better than when the parameterization is switched off! The authors

compare two distinct strategies on how to overcome this problem. One, proposed recently by Arakawa and co-authors (2011), consists of simply adding a counterweight to the parameterized convective fluxes so to make them weaker or insignificant when the convective area fraction, within the grid cell, is close to unity. The other approach amounts to not restricting parametrized convection into one single cell but redistribute the subsiding air into the neighbouring cells. The two strategies were tested for the case of organized tropical convection over South America, including three types of events: a midlatitude cold front, diurnal cycle, and South Atlantic ITCZ. The results are sensitive and comparable improvements were seen with both approaches compared to the case when the scale aware feature is turned off, especially in the cold front and diurnal cycle convection but both methods seem to perform worse than the non-scale aware parametrization in the S-A ITCZ region, though the authors do not seem to be bothered by this! Nonetheless, in terms of mean quantities (mean and RMS errors) the results obtained with the two scale-selective strategies are superior. Maybe it is because, for this particular experiment, convection in the ITCZ part of the domain is much weaker. Moreover, the authors conducted an idealized study comparing clean and polluted conditions for their aerosol aware implementation. Many significant differences were noticed but the simulation is perhaps too idealized to make sensible conclusions on whether the aerosol aware scheme is better or worse.

I recommend major revision to address these issues and the following more specific comments before it can be acceptable for publication.

Specific comments:

1. Page 23849 (line 15): "The three ideas discussed here ...". the third idea, namely of Kuell et al. (2007) is only mentioned to say in the end " ... we refrain from testing this method...". Since this third strategy is not at all tested, it will be better to change the first sentence in parag. 15 to " The two ideas discussed here ..." and then in Page 23851 simply say "there exists an interesting third approach but

it wont be considered here for the reasons already mentioned, etc.”

2. Page 23851, top: The way the “subsidence is distributed among neighbouring cells” is not clear. Which neighbouring cells are being targeted? How many? Is the distribution among all neighbouring (with or without the “updraft” cell) uniform?

3. Notation: Overbar in Eqns 7-8 and subsequent refer to ensemble average quantities but in (1)–(6) it refers to Reynolds averaging. Some precaution should be taking here. Either use different notations or carefully warn the reader.

4. Eqns 16-18: Replacing the cloud base mass flux with its ensemble average is not justified? Is there evidence that the stochastic variability of mb is independent of those of other variables such as vertical profiles and/or convective fluxes? Maybe not!

5. Eq (20): mu is not defined...it becomes clear a few lines later that it is an entrainment rate but it is not clear how it is set in the model. Also what is the difference between mass entrainment rate and entrainment rate: how do en, delta and mu differ? Which quantities actually define the other and how?

6. Fig3 and elsewhere: Why drying has units of degree/day

7. Page 23862: The fact that the heating and drying rates are larger @1km resolution, compared to 3km resolution is not explained. It is against the statement on top of the page: “Heating and drying tendencies decrease with a decrease in grid resolution” ... Also do you actually mean increase in grid resolution, i.e, decrease in Delta x?

8. Page 23862, end of line 16: Typo? “through significantly” —> though?

9. Page 23862, line 17: “subsidence is significantly decreased” where can we see this?

10. Figures 5 and 6: Is there a physical explanation why parameterized convection persists near the N-E corner even with a 5 km resolution, especially for GF-A and GF-NS but no much so for G3d? This detail is not mentioned at all in the paper. In fact according to the obs in Fig8, this corresponds to South Atlantic ITCZ convection for which the modified scheme seem to perform worse than GF-NS!

11. Page 23864, bottom paragraph: Is it surprising that GF-A and G3d yield similar results at 5 km resolution, given that at such grid scale most of the convection is handled by the grid? Why results with coarser grids 10km and 20km are not being discussed for G3d?

12. Discussion on top of page 23865 stipulates that, “even @5km resolution, for the case of daytime convection with GF-NS the parameterization captures most of the rainfall but for the scale aware schemes the grid handles most of the rainfall.” This is very intriguing indeed. Is there an explanation for this?

13. Bottom of page 23865: it will be helpful if differences in results between the two methods were also discussed.

14. Reference to figure 8 comes after Figure 9!

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 23845, 2013.