**Atmospheric
Chemistry
and Physics**
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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

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Response to reviewer #1:

We thank the reviewer for his suggestions. Specific responses are given below. We recognize the problem in the performance in the NE corner over the domain. Some reasoning is given below under (10). We are continuously working on this – especially with respect to exploring the stochastic options in the scheme - to further improve performance.

Reponses to the individual suggestions:

1. These changes have been implemented. Additionally, on page 23850, line 17 was changed to state: " The other approach is based on interpreting"

2. Has been changed.

3. Has been changed

4. A sentence has been added just before equation (15).

5. This is actually an error in the manuscript. The normalized mass flux is defined in (13) and should be named η. This change has been made. A couple of sentences have been added to illustrate our procedure to get smooth normalized mass flux profiles.

6. The only reason for this is that the first author has been using degrees/day in all his previous work looking at vertical profiles of Q1 and Q2 which are usually given in deg/day.

7. This is a point that also was picked up in a similar way by reviewer #2. Unfortunately Fig. 3 – the 1d test – is somewhat misleading since it depends strongly on the provided sounding. In 3-d tests the convective parameterization will be automatically turned off if relative humidity is near saturation (95%) and vertical velocity is upward anywhere below the level of free convection. For the test sounding given in Fig. 2 we have an upward vertical velocity which is explicitly used in two of the ensemble closures. This is necessary to see the qualitative behavior of the parameterization at different resolutions. In a 3-d model run the scheme would most likely be turned off. A publication is in preparation by a different author to show this functionality in tropical cyclone simulations.

8. The sentence has been changed to make this more clear.

9. Has been added in the sentence.

10. There is a numerical explanation. G3d and GD use many subensembles, some have been weighted over water. This weighting is done in dependence of trigger func-

tions. For GF, the number of subensembles has been significantly reduced and the stochasticism has not been explored. We are aware of this issue and are working on implementing similar approaches. The fact that the total precipitation in this area is simulated best by GF-NS is not a positive outcome for GF-NS, since it would probably lead to significant errors for biases and rms errors (see also Figures 14 A and C later in the discussion), forcing all subsidence heating and drying to be in one grid box. Several sentences have been added in the document.

11. The subsidence spreading in G3d is only turned on at 5km horizontal resolution, so no model runs have been performed for G3d at coarser horizontal resolutions. This sentence had been added at the beginning of section 4.2.2.

12. We have added a phrase in the discussion in this paragraph stating that: "As expected, from 20 to 5 km, we see a smooth transition from non-resolved to resolved precipitation as this ratio increases from. . .."

13. We added a sentence at the end of the paragraph. 14. We changed the wording of the 2 sentences.

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