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Interactive Comment

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 #2

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General comments:

The paper 'A scale and aerosol aware stochastic convective parameterization for weather and air quality modeling' by Grell and Freitas offers a contrasted picture. On the one hand, the chosen topic, the explanation of its importance and the description of the scientific challenges are very promising, even if not all existing similar avenues are mentioned. On the other hand, the obtained results appear very much linked to the particular modelling framework chosen for the study and lacking any deeper general character, at least in the way they are presented. The methodology of experimenting is not what is most at stake concerning this criticism. It is rather the fact that the paper

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lacks any support for a claim to more generality concerning basic issues (for the interested community) that would go beyond a mere ‘we justify our choices on the basis that the results are OK and shall carry on’. The paper is however announcing this lack of ambition (beginning of Section 2) and not suffering from many particular flaws. It seems difficult to require a more ambitious rewriting which would probably require the development of several additional diagnostic tools. I therefore recommend publication with minor revisions, leaving to the Editor the task of deciding which weight to give to my above criticism about the structurally weak impact I suspect that the paper will eventually have.

Specific comments:

A) In Section 2, the authors seem to consider that only three avenues have been proposed in the international community to deal with the conceptual problem of the ‘gray zone’. Yet as participant to the COST ES0905 Action (convection.zmaw.de) I can assure the authors that at least three other avenues are actively pursued and would, in my opinion, deserve being mentioned in the enumeration. Namely:

- The fact that there is nothing like a complete gap between ‘super-parameterizations’ on the one side and ‘classical mass-flux-type schemes’ on the other side. A hierarchy of schemes of intermediate complexity may be built, when going back to the roots of the parameterization process. The level where the minimum of arbitrariness seems (subjectively) tot be reached is the one where only entrainment-detrainment-type of exchanges between selected specific sub-ensembles of a given grid-mesh need to be treated (on top of radiative and microphysical contributions). When linked to a dynamical framework, such a ‘stylisation’ of the convective modeling yields the so-called NAM-SCA approach, see Yano et al., 2010: NAM–SCA: A Nonhydrostatic Anelastic Model with Segmentally Constant Approximations. Mon. Wea. Rev., 138, pp. 1957-1974.

- The conjunction of the ideas that (i) a maximum of prognostic character is needed to relax usual large-scale-type hypotheses made in classical mass-flux schemes, (ii) there is only one type of clouds from the microphysical point of view (and that the resolved vs. parameterised dilemma only exists when people insist on having 'closed' algorithms on each side), and (iii) that the key issue is therefore to imagine (like in radiation) a correct and sophisticated interaction between cloud geometry and basic physical processes (here, auto-conversion, collection, phase changes during precipitation and sedimentation). For details see Gerard et al., 2009: Cloud and precipitation parameterization in a meso-gamma-scale operational weather prediction model. Mon. Wea. Rev., 137, pp. 3960-3977.

- The concept that 'as most of the condensation is taken over by resolved processes when mesh-sizes diminish, the remaining part of the convective phenomena can equally well be treated by a sophisticated skewness-aware turbulence scheme than by the classical mass-flux/entrainment/closure approach, owing to the similarity between both concepts at their roots'. This path is very general (could even be combined with other approaches) and can be applied with many turbulence schemes, provided they rely on the said similarity, described in e.g. Mironov, 2009: Turbulence in the lower troposphere: second-order closure and mass-flux modelling frameworks. Interdisciplinary Aspects of Turbulence, Lect. Notes Phys., Hillebrandt and Kupka, Eds., Springer-Verlag, Berlin, Heidelberg, pp. 161-221.

B) When looking at Fig. 3, it is striking that, despite the correct transition in terms of overall intensity, even with 3km mesh-size, there is no sign of a transition in the shape of the forcing towards what will be the 'shallow resolved forcing' at 1km mesh-size. Yet, at 3km one is really finding, in my experience, the maximum of mixed cases (partly resolved, partly still needing a specific convective treatment) deserving all the attention of any parameterization claiming for scale-awareness. My impression is thus that the use of GF-A is not as successful as the authors claim it is. Of course, final results in terms of precipitation maps and scores are OK with its use. But, in eye-ball impression, I do not find them either superior to those of the G3d runs. The conclusion that the 'GF-A path' is to be pursued because of simpler implementation requirements than G3d when going to higher and higher resolution would be acceptable, if there was not this doubt that the GF-A parameterization does not work exactly as it should in the middle of the ‘gray zone’. The authors should, in my opinion, justify their proposal on a more rational basis and critically comment the strange result of Fig. 3.

Technical remarks:

C) Page 23848, line 4: there is a wrong break in the sentence.

D) The paper has a problem with the definition of (horizontal) resolution. Sometimes its ‘increase (decrease)’ is correctly linked to smaller (bigger) mesh-sizes, sometimes (e.g. page 23848, line 27) it is wrongly used with the opposite meaning. On page 23862 line 1, the term ‘grid resolution’ is used. I think this may be misleading and I’d recommend using the generally accepted ‘grid-size’ term. The authors should correct these (interacting) annoying sources of confusion.

E) There is a contradiction between the choice to treat only the liquid condensed phase (Page 23853, line 8) and the mention of ice detrainment (page 23860, line 2). Generally speaking the fact to have only a ‘warm microphysics’ is surely another handicap when trying to draw more general conclusions from this work (see above).

F) On page 23865 lines 24-25 it is not clear whether or not the choice of Box C applies to both compared schemes. One must assume ‘yes’, but the ambiguity should rather be removed from the text.

G) Page 23867, line 19: Table 2 (and not 1).

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