

Interactive comment on “A compressed super-parameterization: test of NAM-SCA under single-column GCM configurations” by J.-I. Yano et al.

P. Stier (Editor)

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The manuscript “*A compressed super-parameterization: test of NAM-SCA under single-column GCM configurations by J.-I. Yano, S. K. Cheedela, and G. L. Roff*” has been accepted for the Atmospheric Chemistry and Physics Discussions and has undergone review by initially two reviewers and corresponding author responses in the public discussions forum.

The authors decided to submit a revised version of the manuscript for consideration for Atmospheric, Chemistry and Physics. The assessment was supported by an additional

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independent, Reviewer 3, in addition to consulting Reviewer 2 to assess the revisions made by the authors in light of the comments raised in the first round of reviews.

Both reviews of the revised manuscript raised significant issues. Reviewer 2 substantiates the question of the applicability of the model for the applied grid resolutions, limitations in the numerics and the treatment of sub-grid-scale processes. Reviewer 3 additionally points out limitations in the metrics used to assess the model performance and suggests significant rewriting of the manuscript to improve clarity and to justify the chosen numerical approach. Based on my own assessment, I have come to share the specialist referees' concerns that the manuscript in its current form provides insufficient evidence for the validity of the proposed modelling approach for the chosen setup.

Publication of a manuscript in ACPD implies that the editor and/or referees think that the manuscript is worthy of public review and discussion (rated at least “fair” with regard to the published [evaluation criteria of ACP](#) in terms of Scientific Significance, Scientific Quality and Presentation Quality). Publication in ACP is meant to imply that the editor, based on the referees, rates the manuscript as excellent or good in all of the review criteria. In this specific case, two specialist reviewers rated this manuscript for ACP below fair in all of the evaluation criteria. Consequently, I decided to reject the manuscript in its current form and invited the authors to consider re-submission of a substantially revised manuscript.

Disagreeing with the reviewers' recommendations and my decision to reject the manuscript, the authors have requested to make the review comments on their revised submission public and the reviewers have kindly agreed to this request in the spirit of a transparent review process. The reviews are copied below and the revised manuscript is provided as an Appendix.

Due to the interactive open access publishing approach of ACP, the original manuscript has already been published as a discussion paper in ACPD, which remains permanently archived, publicly accessible and fully citable.

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Philip Stier
Co-Editor
Atmospheric Chemistry and Physics

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Reviewer 2

Reply to the replies by the authors of “A Compressed Super-Parameterization: Test of NAM-SCA under Single-Column GCM Configurations.”

In my opinion the main issue with the presented work was and is the fact that NAM-SCA is used with grid resolutions that are outside its range of applicability. Due to the chosen setups NAM-SCA is not able represent important dynamical processes that are relevant for deep convection. (This is not a critic of the underlying model equations as the authors suggest in their reply.)

But before accused again by the authors of setting subjective standards let me start from a quote of the authors' own manuscript. In the introduction it reads “NAM-SCA, which is developed under a direct “compression” of a CRM, can contain complete physics just as any CRMs can. Thus, it embodies a much more solid physical basis than any conventional parameterizations.” What the authors are saying here is that NAM-SCA is a type of CRM (although the authors contest this in their reply to the reviewers). However, CRM is just a label (although well defined e.g. by Randall et al. in BAMS, 2003) and I come back to this point later. Much more important is the 2nd sentence of the quote that states that a solid physical basis is a requirement for any parametrisation development or the replacement for a parametrisation. I could not agree more strongly with this statement. However, this requirement is not met in the presented work. Since grid resolutions of 500m and coarser are used a treatment for subgrid mixing and transport is needed. For some background reading on the important spatial scales in cloud and convection related problems I recommend reading the already mentioned Randall et al. (2003) paper.

The authors claim that a treatment of subgrid dynamics is not needed by referring to papers by Margolin et al. (1999) and Yano et al. (2010) where the latter applies ideas from the former. However, close inspection reveals that the necessary condition for applying Margolin et al (1999) is not met in the presented work here. However, it is met

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in Yano et al. (2010). More specifically, Margolin et al. (1999) present large eddy simulations (LES) of the planetary boundary layer without an explicit treatment of subgrid scale (SGS) turbulence. The authors of that study conclude that they can simulate a realistic turbulent spectrum for the resolved scales even without SGS model. Their results agree favourably with observations although the from theory expected $-5/3$ slope in the power spectrum in the inertial subrange is better reproduced when an SGS model is included. This concept of LES without SGS model was already introduced by work of Boris (Springer, 1990) and Boris et al. (Fluid Dynamics Research, 1992) and is now better known as ILES (implicit LES). In ILES the SGS model is replaced by the implicit numerical diffusion of a non-oscillatory transport scheme. To my knowledge nobody has ever shown or claimed that this concept can be extended to coarser, non-LES scales. For a review of the method and results I refer to the 2007 Journal of Fluids Engineering special issue on Computing Turbulent Flow Dynamics with Implicit Large Eddy Simulation. Although probably common knowledge let me define LES here. According to Moeng and Sullivan (2003), LES is a simulation that explicitly calculates large eddies that contain most of the turbulent kinetic energy while approximately representing the effects of smaller eddies. While the exact spatial resolution limit for LES depends on the actual conditions of the simulated flow LES resolutions are limited to several tens of metres. Both, Margolin et al. (1999) and Yano et al. (2010), fall into that resolution limit with 50m horizontal spatial resolution and 30m and 20m vertical resolution, respectively. The new aspect of Yano et al. (2010) is that the same boundary layer structure can be reproduced under a smart grid coarsening in some regions. My main point is that – whatever NAM-SCA might be labelled – it is certainly not run in LES mode in the study that is under review here. The ILES concept simply does not apply here. If the authors would restrict the simulation to the LES scale I would be happy to consider this study for publication. Of course, even the coarsest model will produce some entrainment/detrainment on the resolved scales, however, there is absolute no physical basis that a severely underresolved flow will represent real atmospheric conditions without some kind of representation of subgrid dynamical processes. In the

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contrary, there is plenty of evidence (e.g. Randall et al., 2003) that suggest that this cannot work.

Even in the LES limit there is a strong argument by Margolin et al (1999) against NAM-SCA in its present form and I quote: “Second-order accuracy is essential as first-order schemes are overly diffusive and fail to model physical reality. [...] In the donor-cell limit [i.e. first-order upwind] , the dissipation is excessive and the flow becomes laminar.” The same argument is also made by other authors e.g. Aspen et al (Comm. App. Math. And Comp. Sci., 2008); in addition, see also the aforementioned special issue on ILES. Since NAM-SCA is based on a first-order upwind scheme it is consequently not applicable in the ILES limit. However, although Yano et al. (2010) never show turbulent spectra they are able to reproduce the expected thermodynamic structure so that the argument against first-order upwind might not weigh so strongly.

Alternatively, the authors could include some SGS treatment (turbulence or boundary layer scheme) to extend the applicability of the model beyond the LES scale. Good examples of such CRM model configurations are given in Guichard et al. (QJR Met. Soc., 2004) and the follow-on study by Grabowski et al. (QJR Met. Soc., 2006). Although not mentioned in the manuscript both of these studies are highly relevant to the work here. They compare single-column models (SCMs) to CRMs for convective test cases. All CRMs include SGS models, see e.g. table 6 in Grabowski et al. (2006). They conclude (and I quote from their abstract): “The CRMs featuring horizontal grid length around 500 m are capable of capturing the qualitative aspects of the benchmark simulations, but there are significant differences among the models.” Note that this statement is even only for qualitative aspects of the simulation, not quantitative aspects. This puts another upper limit on the usable spatial resolution of 500m which is the highest spatial resolution in the study under review here. Even if we stretch the CRM approach to 4km grid resolution in line with Cheng and Cotton (J. Hydromet., 2004) this excludes the NAM-SCA runs with 8km and 16km resolution for which the authors quote the smallest RMS errors. So, if the authors would include a suitable

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SGS treatment the presented study could be extended to 4km and might be considered for publication. Of course, limitations on the domain size which I won't discuss in detail here still need to be considered.

However, already at 4km the CRM approach where individual clouds are assumed to be explicitly resolved (Randall et al., 2003) is more often than not violated. This brings me to the next point, the cloud microphysics. A microphysical schemes that do not parametrise cloud fraction and only produce cloud fractions of either zero or one become inconsistent with observations and loses its physical basis when individual clouds cannot be explicitly resolved. Since the microphysical scheme in NAM-SCA does not parametrise subgrid cloud fraction NAM-SCA cannot be used for grid resolutions coarser than 4km. Following authors like Guichard et al. (2004) or Grabowski et al. (2006) this resolution limit should even be lower at 2km or below.

Finally, in the reply to my first review the authors dismiss all of the above arguments by saying that it wouldn't matter how a model produces quantities like Q1, Q2 and surface precipitation as long as it produces the correct results. As an example let me quote from the authors' reply: "However, as long as the goal is to obtain a correct domain-averaged precipitation rate, it does not matter whether the rain is convective or drizzling (from a very weak stratiform cloud)." I disagree very strongly with this statement for many reasons. Firstly, any approach that produces the correct results for the wrong reasons is clearly without physical basis and therefore also contradicts claims by the authors themselves that the presented study has a solid physical basis. Secondly, if true one could use any model for anything as long as it produces the expected results for certain metrics. There wouldn't be any limits to our imagination. Thirdly, even a method that produces the correct mean results for the wrong reasons for a number of atmospheric cases (two cases in the present study) its variability and sensitivity is most likely wrong and cannot be trusted. Thirdly, such a framework cannot be used as a basis for further development contradicting claims by the authors e.g. in the introduction: "NAM-SCA, which is developed under a direct "compression" of a CRM, can

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contain complete physics just as any CRMs can." Improving the representation of ice processes in the cloud microphysics (or adding aerosol cloud interaction as quite a few groups try to attempt) is pointless if the model produces drizzle instead of the observed convective precipitation.

To conclude, due to the coarse resolutions used in this study NAM-SCA is clearly not a physically well-based substitute for a conventional convection parametrisation as claimed by the authors. Thus, the manuscript does not merit publication in its present form. As discussed above, the required changes would be so drastic that they would result in a very different paper.

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Reviewer 3

Review of "A compressed super-parameterization: test of NAM-SCA under single-column GCM configurations" by Yano, Cheedela and Roff

Recommendation: reject

Although this manuscript presents some interesting and relevant results, I cannot recommend its publication in the current form. I feel the manuscript must be significantly rewritten. At the moment it reads more like a dissertation or a project report documenting systematic studies using NAM-SCA model, rather than focusing on specific scientific problems and their solutions. The reading is quite heavy, with many caveats and sidetracks, not really relevant to the main thrust. The numerical results presented do not go deep enough to consider the manuscript as an advancement of the science of moist convection and/or its representation in large-scale models. At the same time, I am sure some readers might have problems with the design of the NAM-SCA model (numerics, thermodynamics, treatment of subgrid-scale processes, etc.) and some expansion of this aspect to defend the strategy would be desirable. Just referring to previous papers is not sufficient in my view. But the manuscript is already long, with 28 figures, and any further expansion is not possible in my view.

I do not like the NAM-SCA model and I am concerned with some of the statements in the paper, "compression" and specific numerical aspects, for instance. Below I have a list (not exhaustive by any means) that substantiates my above statements.

Because of the limited science at the current stage, I see two options: 1) strengthen the science by removing unnecessary caveats and discussions (see comments below) and resubmit to ACP; 2) focus on implementation issues and submit to the Copernicus GMD journal. In the first case, expansion of the science component is needed. In the latter case, strengthening the discussion of the model and presenting comprehensive set of model diagnostics would be desirable.

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Specific comments:

1. I am little uncomfortable with the "super-parameterization" term. As far as I can understand from what is provided in the paper, the analogy between the real cloud-resolving model (CRM) and NAM-SCA model is rather far-fetched.
2. I do not think abstract can have footnotes.
3. I do not understand how the grid length can be 16 km and the model domain can be 32 km. Does this mean that the model has just two columns? From that point of view, the opening sentence in the 3rd paragraph of the abstract ("32-km domain with 16-km grid length works well") seems absurd to me. BTW, there are several papers in the literature reporting single-column-like tests of the CRM (e.g., Khairoutdinov and Randall, JAS 2003). Are results reported in the current manuscript consistent with those? I am not sure.
4. I am not sure if the way RMS differences are considered in this paper is the most sensible. Because of different time steps in various tests, I would suggest that the model precipitation is first averaged out to the common time interval for all tests (say, 6 or 12 hours) and then compared. The time period may be selected the same as in the observational data shown in Fig. 1.
5. An important difference between the two cases considered is the domain size: about 1,000 km by 1,000 km in the GATE case and (roughly) 250 km by 250 km in TWP ICE case. This obviously has a strong effect on the forcing characteristics. I think the forcing is more variable and with larger spikes for the smaller domain size. I think this deserves a comment.
6. Section 3 can be significantly tightened up. Section 3.1 and 3.2 can be shortened and sections 3.3 and 3.4 (that present material only tangentially related to the main thrust) can be removed from the paper, together with figures 2-4.
7. I feel an attempt should be made to reduce the number of figures. For instance, are

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all Figs. 7-10, 13-14, and 19 needed? Should showing just a couple and then moving to more comprehensive analysis (e.g., as shown in Fig. 16 and several similar figures) be more appropriate?

8. One can argue that Q1 and Q2 are not the best measures of the model performance. Should other measures, more directly related to the meteorology, like the differences in the mean precipitable water, relative humidity, temperatures, etc., be considered as well? This is done in section 4.5, but I feel this section should be expanded (in expense of the previous sections) and results need to be put in the context of previous studies. For instance, there is a consistent signal from CRM studies that decreasing model horizontal resolution leads to systematic changes of model solutions (e.g., Pauluis and Garner JAS 2006). Are results from NAM-SCA model consistent with those other studies?

9. Overall, I like the comprehensive way color plots of the type in Figs. 11 are developed and used. However, I am struggling with the main message, as already stated in 3 above. This is puzzling and needs to be understood. Current paper does not provide any clues for this strange result.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/12/C14036/2013/acpd-12-C14036-2013-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28237, 2012.

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