Atmos. Chem. Phys. Discuss., 12, C13698–C13710, 2013
www.atmos-chem-phys-discuss.net/12/C13698/2013/

Atmospheric
Chemistry
and Physics
Discussions

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

J.-I. Yano et al.

jun-ichi.yano@zmaw.de

Received and published: 2 April 2013

Reply to Referee #2:

Summary of the Response:

We are afraid that the present Referee misunderstands the basic premises of NAM-SCA by assuming it is a type of CRM. NAM-SCA is indeed derived from a CRM formulation, but it is designed to work more like a "toy" for CRM, quoting from Referee #2. It appears to us that the Referee effectively call NAM–SCA flawed simply because it does not meet the present standard of CRM. That is the main understanding. [It will be

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

alleviated in revising the text for a final version]

The Referee also appears not to appreciate the basic point that when a model has a completely new algorithm (which is what NAM-SCA is), it is standard procedure to examine it starting from the simplest set-up, and then gradually moving to more complex cases (and then only if necessary). The extent of the complexity of the microphysics required for NAM-SCA still remains an open question (this is to be stated in the revised text). However, it appears to us that the Referee prematurely insists that we should use "perfect" cloud microphysics (with the inclusion of ice etc).

The Referee should especially be cautious with criticizing the simplicity of the NAM-SCA algorithm (upwind advection, etc), as this has been part of previous studies published in MWR after the review process (Yano et al., 2010). Though it is well possible that the review process has been flawed. However, if that is ever the case, the Referee must be concrete and precise. Both elements are clearly missing in the current round of the review.

Forwards:

We are pleased to see a very straightforward comment from the present referee, pointing to us that our manuscript is "poorly written", and the work itself is "conceptually and methodologically flawed". We would be very happy to accept these criticisms and seriously work on these issues.

However, we somehow miss the actual explanations on how the manuscript is "poorly written" and how our work is "conceptually and methodologically flawed". If the present reviewer could expand on these comments and suggest, we would much appreciate it. Unfortunately, as for now, without these reasons clearly explained, it is difficult for us to make such improvements.

In the main part, it seems to us, the reviewer's critics boils down to a point that the present model (NAM-SCA) is too simple (lack of sufficient complexity). However, here again, we are rather perplexed with the Referee's extensive negative remarks, because it seems to us that the Referee simply sets a standard subjectively in their own manner without providing objective grounds (with references). Arguments that the Referee *does* provide, appear to us being based on a misunderstanding as explained in each case below.

The present Referee tends to treat our NAM-SCA as if it were a "real" CRM. Thus, according to the Referee, this NAM-SCA is constructed in such a poor manner in the current standard of CRM development that the present work is not worthy to be considered for publication. This is a major misunderstanding. The unique nature of NAM-SCA must be well understood, which is that it seeks a middle point between a CRM (super-parameterization) and a standard parameterization. We've decided to call this methodology a "compressed super–parameterization", because NAM-SCA is "compressed" in respect to a standard CRM (super-parameterization) in terms of both the number of active meshes and the complexity of the physics. [This point is better emphasized in the revision by introducing a new paragraph in the abstract.]

In the original manuscript, we probably should have better emphasized the "compressed physics" in NAM-SCA in the same spirit as "mesh compression". This idea is already carefully discussed in Yano and Bouniol (2010) in introducing our microphysics (called a minimum microphysics). A point to be emphasized here is that these "compressed physics" are sufficient in order to use NAM-SCA for the purpose of parameterization. As far as we can follow, the present Referee does not refute this very point.

We also have to place more emphasis on NAM-SCA's unique capacity of dynamic mesh adaptation. As already emphasized in Yano et al (2010, MWR), and as originally presented therein, this dynamic adaptation methodology was developed originally by the leading author with the collaborators therein.

*

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Line–by–line Response:

The following is our line-by-line response to the Referee's comments. The numbering below corresponds to the paragraph numbers for the Referee's comments. In responding to the 4th paragraph, the sentence numbers are also indicated.

1. Thank you for a good summary of the present manuscript. We are also happy to know that the present Referee also appreciates our choice of case studies.

2. We would like to improve our "poorly written" manuscript making "reading and understanding difficult". However, unfortunately, since the present Referee does not make any more concrete remark in the following [or do we miss something?], there is no way for us to modify the text accordingly.

The number of figures could be an issue. We personally think that the number is not too many. However, we will directly follow the instruction of the Editor on this issue: the number of figures will be reduced exactly below the threshold number which can be specified by the Editor. All the removed figures are simply marked as "(not shown)". Alternatively, we may refer to our response to the Referee #1, where some of the revised figures are listed. If the Editor suggests to move extra figures to a supplementary material, we will also follow this instruction. [However, we are afraid any of those procedures will substantially reduce the readability of the present manuscript.]

3. We very much appreciate the Referee's very straight remark that the present manuscript is "conceptually and methodologically flawed". However, unfortunately, in the following comments, we totally miss their explanations on how it is flawed both "conceptually and methodologically", respectively. For this reason, we cannot respond to this main comment.

As for a clarification of our own understanding, here we assume that "flawed" means "defected", and more precisely in certain logical sense, both the concepts and methodology of the present work are "logically defected", but how? Of course, this is a very serious remark, we would like to understand more precisely where we made our mistakes. We are looking forward to hearing from the present Referee for a more careful explanation on those two matters.

4. (1) The Referee's first sentence in this paragraph, again, well summarizes the main intention of the present work, however, with a minor misunderstanding: it is *not* "supposed to", but *actually* replaces cloud and convection parameterizations in global atmospheric models.

(2) However, we do not understand what the second sentence intends to say: at what conceptual level NAM-SCA is not a CRM? Without a clear answer on this matter from the Referee, we cannot further comment on this remark. Here, we simply emphasize again that NAM-SCA takes an approach of a middle point between CRM and a parameterization.

(3) We are also deeply puzzled with the third sentence: in what sense exactly the Referee wants to say our cloud physics is "incomplete and not suitable for the purpose of simulating convection"? We agree with the present Referee that our cloud physics is "incomplete" *in the absolute sense*. As Yano and Bouniol (2011) point out by carefully reviewing the existing hierarchy of cloud microphysics schemes, there is no such thing as "complete" cloud microphysics, because certain details are inevitably missed out by the very nature of the problem. We always have to make a compromise: here, we make the choice of a minimization of the microphysics under the spirit of compressed physics.

In this very respect, we emphasize the simple fact that this "minimum" microphysics is sufficient for realistically simulating tropical convection as clearly demonstrated by Yano and Bouniol (2010). Though the two reviewers for this article are critical with our microphysical treatments (the comments are available on Web), neither of the reviewers argues that we failed to successfully simulate a tropical squall line.

(4) We would like to sincerely request a clear reference establishing that "treatment of

subgrid-scale mixing is crucial for the evolution of convective clouds". As Yano et al (2010) clearly demonstrate, NAM-SCA without subgrid-scale mixing can successfully simulate a dry well-mixed convective boundary layer. Margolin et al. (1999) strongly argue that subgrid-scale mixing is not necessary for turbulence simulations in general, if a proper advection scheme is chosen. If Margolin et al.'s argument is fundamentally flawed, we would like to know why.

(5) Yes, it is well known that the upwind advection scheme is highly diffusive, however less well known is that it is diffusive in a highly selective manner. If the main purpose of the simulation is in vertically-coherent convective plumes, the upwind advection scheme works extremely well as the series of our work using the upwind advection scheme for NAM-SCA demonstrate. Especially Yano and Baizig (2012), who closely examine the dynamics of a single plume under this framework.

Again for this reason, we would like to sincerely request a clear reference stating that the upwind advection scheme is not suitable for moist convective simulations, and in what sense and how.

(6) Here, under the NAM-SCA formulation, entrainment and detrainment are explicitly treated by resolved circulations. We can even diagnose the "effective" entrainment and detrainment rates from a given circulation. Such a careful analysis of the entrainment-detrainment rate is presented in Sec. 6 of Yano and Baizig (2012).

(7) The resolution-dependence of the results is shown in Figs. 11–12, 14–28 in various formats. Especially, the time–space section of the precipitation field shown in Fig. 15 is probably the best demonstration among them: it shows how well NAM–SCA can simulate the propagating features of precipitation systems during GATE, and how the simulations only weakly depend on resolution.

(8) Here, we repeat our main emphasis that NAM-SCA is a middle point between super-parameterization and a parameterization. The lead author's key question has been how to obtain something equivalent (or almost) to a conventional parameterization under a

more robust physical basis, as extensively discussed in Yano et al. (2005). However, in order for a scheme to function as a parameterization, the level of complexity must be substantially reduced. That is the key concept behind "compression". Thus both the total mesh number and the physics must be strongly compressed in order to approach to a level closer to that of a standard parameterization. This is the drastic difference from the "conventional" super-parameterization.

(9) Yes, for the purpose of highly compressing the physics, only the cloud physics is considered, and the cloud physics itself is also limited to warm processes.

(10) Note that using a smaller fall velocity for precipitation at sub-zero temperature leads to a substantial improvement of the stratiform representation. See the improvement in Fig. 1 of Yano and Bouniol (2011) compared to the original result without this treatment as shown in Fig. 6 of Yano and Bouniol (2010).

(11) We *do* know that ice processes play a very important role in cloud physics (i.e., details of the full cycle of the water budget: see below) of deep convection. Fig. 23 of Fovell and Ogura (1988: compare with their Fig. 14b) is a classical example demonstrating this point. That is the reason the lead author is also devoting himself to an ice physics problem (Yano and Phillips 2011).

However, whether the ice processes are crucial for successfully simulating deep convection for a parameterization purpose is a different question: see for example, discussions in Grabowski (1998) on this issue. In the case of Fovell and Ogura, when the ice physics is considered, most of cloud water grows into hail via snow phase, but it reaches the surface as rain. Thus, the total condensative heating (just freeze and melt again in the whole cycle: thus an intermediate stage is not our concern, the ice process is not important) does not change by adding the ice process. The total rainfall does not change substantially either by adding ice. [An increase by 10% is negligible compared to the other errors associated with convection parameterization.]

Indeed, the present study clearly demonstrates that a model without ice can reasonably predict both Q_1 and Q_2.

(12) Though the present scheme uses much simpler cloud microphysics than many of the current GCM clouds schemes, we should also note that it uses far more sophisticated cloud microphysics within convection than most of the current convection schemes. Details are given below as an appendix for both ECHAM and ACCESS (UM).

(13) Lohmann and Roeckner (1996) is definitely a pioneering work implementing a sophisticated microphysics into a GCM cloud scheme. [Lohmann et al. (2008) that the present Referee appears to be referring to is just a recent update.] It clearly leads to improvements in cloud properties as the original paper demonstrates. However, unfortunately, a systematic study is still missing to demonstrate a degree of sophistication required in order to obtain satisfactory cloud representations in GCMs, and especially its interplay with a cloud fraction scheme, which is not well formulated in general [for example, by a careful feedback analysis based on a tangent linear method].

(14) Thus, we emphasize that NAM-SCA clearly uses far more sophisticated microphysics within convection than in the default convection schemes both in ECHAM and ACCESS. In short, NAM-SCA improves microphysics within a GCM enormously especially considering the importance of microphysics in deep moist convection.

As a whole, we do not understand why the Referee concludes "NAM-SCA does not accurately represent relevant dynamical processes". NAM-SCA is based on the full nonhydrostatic anelastic model, a standard dynamical core used for many CRMs. Thus there is no reason to believe that it *does not* represent dynamical processes properly. A finite volume approach based on SCA has been extensively tested under various configurations (Yano et al., 2010, Yano and Baizig 2012), and it has proved to be robust.

5. We absolutely agree with the Referee that the convective-scale circulations would not be properly represented under a resolution much cruder than the order of magnitudes of 1 km. However, the Referee should clearly realize that the main goal here is to use NAM-SCA as a substitute for convection parameterization. The goal of con-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

vection parameterization is not to reproduce convective dynamics accurately, but only for predicting the grid–box averaged effects of convection (i.e., Q_1 and Q_2). For this purpose, there is nothing to forbid us to use a very crude resolution. Note further that a nonhydrostatic anelastic model (NAM) itself can run consistently with any horizontal resolution. Thus there is no fundamental physical inconsistency in taking a case with a very crude horizontal resolution.

The main point of this exercise is already well-summarized in the last paragraph of the current version of Sect. 4.2.3. To quote in full length: "Here, it is important to reiterate the main point: in spite of the fact that all these runs simulate the mesoscale convective organization well, their overall performance on thermodynamic tendencies as measured by Q_1 and Q_2 never exceed that of the case with just two segments, in which no realistic convective sub-domain feature is simulated. It further suggests that inclusion of realistic convective elements, such as mesoscale organization, is not necessarily a crucial ingredient for improving convective parameterization. The success of the two-segment case rather suggests that a very crude scheme may work better than a more complex one as long as it provides a more consistent description of convection processes."

6. We look for RMS errors of the three key variables concerning the standard convection parameterization: Q_1, Q_2, and precipitation. We sincerely request the Referee to explain us clearly: what is wrong with using this metric in order to measure the performance of the scheme, if we take the major outputs from the scheme that are to be used as inputs for running large-scale models?

Yes, it is also a great surprise for us to find that the case with 16km resolution and the 32km domain often works the best as far as these measures are concerned. In this case, the convective dynamics are not at all properly represented within the model. Since this is a key question, we have carefully examined this point to find out that only drizzling is going on. However, as long as the goal is to obtain a correct domain–

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

averaged precipitation rate, it does not matter whether the rain is convective or drizzling (from a very weak stratiform cloud).

7. As we have discussed so far, we do not find any basis on the Referee's argument that more sophisticated microphyscs must be used in NAM-SCA. As both our earlier and present work shows (along with many other CRM studies, especially from earlier periods) clearly demonstrate that the basic dynamics of deep moist convection (e.g., a squall-line system) can be well described without any ice physics [again refer to Grabowski 1998]. Moreover, for the purpose of parameterization, which is what NAM-SCA aims for here, we are only concerned with the grid-box averaged quantities. For this purpose, we do not need extremely sophisticated microphysics, as the present study clearly demonstrates. And the level of complexity of microphysics already far exceeds those adopted in many of the current convection parameterizations including both ECHAM and UM (ACCESS).

Here, we are not sure what kind of improvements that the present Referee demands. NAM-SCA dynamics is based on the nonhydrostatic anelastic formulation, a very standard, consistent formulation for the convective-scale dynamics. Its finite-volume implementation under SCA is carefully discussed in Yano et al. (2010). The Referee does not point out any fundamental flaw in our finite-volume formulation either.

The upwind advection scheme could be diffusive: if a horizontal advection of a horizontally isolated feature is the main point of interest, then we would agree that this would be a very bad choice. However, our main interest is rather vertical advection within a horizontally isolated feature (i.e., a convective tower). In this case, the upwind advection scheme works rather well as demonstrated by the present study as well as our earlier studies (Yano et al. 2010, Yano and Baizig 2012).

To repeat, Fig. 15 (along with the other figures) is a good demonstration how NAM-SCA can simulate convective dynamics during GATE. If this can only "resemble" convective clouds (for a wrong reason?), the Referee should provide a crystal clear explanation what is fundamentally flawed with the present formulation.

Yes, the present NAM-SCA must be considered something closer to a "toy" than a real CRM. To repeat, we do not intend real, serious convective–scale simulations, but rather we are looking for a physically well-based substitute for conventional convection parameterization. The present study clearly demonstrates that NAM-SCA works very well for this very purpose. This demonstration is the real originality of the present study. To our best understanding, the present Referee does not dispute this very point, thus indirectly appreciating the merit of the present study.

Appendix: Convective Precipitation Schemes

ECHAM (Johannes Quaas, personal communication, March 2011: "Convection microphysics in ECHAM6", http://convection.zmaw.de/Discussion-Documents.1851.0.html):

Convective precipitation rate is assumed to be proportional to the cloud water content, l:

$$K(p)l$$

The proportionality, $K(p)$, is defined by

$$K(p) = \begin{cases} K_c & \text{if } p_B - p \geq p_0 \\ 0 & \text{otherwise} \end{cases}$$

with p_B the cloud base pressure; $p_0 = 150$ hPa over ocean and $p_0 = 300$ hPa over land. A resolution dependent constant is here set $K_c = 10^{-4}$. ECHAM also consider re–evaporation of precipitation water below the cloud base.

UM (ACCESS) (Rachel Stratton, personal communication, March 2011):

When the cloud water/ice, l, within convection exceeds a minimum value, l_{min}, convection begins to precipitate, and it is given by

$$(l - l_{min})\frac{M}{g}$$

Here, M is the convective mass flux, g is the acceleration of the gravity. A height–dependent minimum (kg/kg) is defined over ocean by

$$l_{min} = \min(10^{-3}[2 + 0.5\tanh(\frac{1500 - h_c}{1000})], 0.5q^*)$$

where h_c is the cloud depth at a given level, q^* is the saturation specific humidity of the environment. The above value is multiplied by two over land; l_{min} is also limited to the range between 2×10^{-5} kg/kg and 3×10^{-4} kg/kg.

References

Fovell, R. G., and Ogura, Y.: Numerical simulation of a midlatitude squall line in two dimensions. J. Atmos. Sci., 45, 3846-3879, 1988.

Grabowski, W. W.: Towards cloud resolving modelling of large-scale tropical circulations: A simple cloud microphysics parameterization, J. Atmos. Sci., 55, 3283-3298, 1998.

Lohmann, U., and Roeckner, E.: Design and performance of a new cloud microphysics scheme developed for the ECHAM general circulation model. Clim. Dyn., 12, 557-572, 1996.

Lohmann, U., Spichtinger, P., Jess, S. Peter, T., and Smit, H.: Cirrus cloud formation and ice supersaturated regions in a global climate model. Environ. Res. Lett., 3, 045022, 2008.

Margolin, L. G., P. K. Smolarkiewicz, and Z. Sorbjan, 1999: Large-eddy simulation of convective boundary layers using nonoscillatory differencing. Physica D, 133, 390–397.

Yano, J.-I., and D. Bouniol: A minimum bulk microphysics. Atmos. Chem. Phys. Dis., 10, 30305-30345, doi:10.5194/acpd-10-30305-2010, 2010

Yano, J.-I., and D. Bouniol, Interactive comment on "A minimum bulk microphysics" by

J.-I. Yano and D. Bouniol. Atmos. Chem. Phys. Discuss., 10, C14638-C14654, 2011

Yano, J.-I., and V. T. J. Phillips, 2011: Ice–Ice Collisions: An Ice Multiplication Process in Atmospheric Clouds, J. Atmos. Sci., 68, 322-333.

Yano, J.-I., and H. Baizig, 2012: Single SCA-Plume Dynamics. Dyn. Atmos. Ocean., 58, 62-94.

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