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Geoscientific Model Development Discussions

Interactive comment on "Application of a global nonhydrostatic model with a stretched-grid system to regional aerosol simulations around Japan" *by* D. Goto et al.

Anonymous Referee #1

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This paper describes application of Stretch-NICAM-SPRINTERS to Kanto region in Japan. The stretched-grid system embedded in this models realizes more efficient simulations over target regions in finer resolutions. In addition, it is superior to general regional models because it does not need to apply a nesting technique and boundary conditions.

It appears that Stretch-NICAM-SPRINTERS has a great possibilities in its concept. However, I do not have any impressions only from this paper that this model has a good performance. It is too ambiguous how the authors have judged that this model was capable of simulating meteorological fields and anthropogenic primary and secondary particles. If the authors judge so, the concrete criteria should be shown.

The stretched-grid system should be one of advantages of Stretch-NICAM-SPRINTERS. On the other hand, the treatment of aerosols is much more simplified than general regional models (no nitrate, ammonium in a fixed ratio, prescribed oxidants, etc.). This paper does not describe that a good performance (which the authors judged) was obtained from the former or the latter. Are the authors indicating that the simplified treatment of aerosol enough to represent aerosol over Kanto region? In addition, the authors describe Stretch-NICAM-SPRINTERS are potentially superior in simulations of transboundary air pollution. However, any discussions of transboundary air pollution are missing.

I do not think that the assessment of the public health impact is suitable to be included in the current form in this paper. I suppose the main objective of this paper is to show advantages of Stretch-NICAM-SPRINTERS. If the authors are willing to include this part, the advantages of Stretch-NICAM-SPRINTERS in the results should be clearly stated for example by comparing results with those obtained by other models (e.g. MIROC-AOGCM). The current results may cause confusion because mortality in 2030 would increase whereas PM2.5 concentration decreases. It is due to changes in the age distribution. In this case, I think it is necessary to describe how to predict future population and its distribution in ages in details. However, explanations on the population data used in this study are almost missing in the current manuscript. I also think such a discussion would be a topic to be described in a separate paper focusing on the assessment of the public health impact.

Most of figures are too obscure to recognize if the description in the main text is valid. Especially, the described features in the horizontal distribution over Kanto regions are hard to be recognized in contour figures.

Specific comments:

Line 1 in Page 135

How is Stretch-NICAM-SPRINTERS potentially superior to general regional models?

Indeed, a nesting technique or boundary conditions are necessary in regional models. Does it mean that a nesting technique or boundary conditions have any problems to represent transboundary air pollution accurately? Are there any references which imply such problems? Or, is it just complicated to apply a nesting technique or boundary conditions? Is it appropriate to determine that Stretch-NICAM-SPRINTERS is "potentially superior" only by this reason?

Line 25 in Page 136

Again, it is not clear that how the stretched-grid is more suitable for the current study compared with general regional models.

Line 4 in Page 138

Anthropogenic SOAs from toluene and xylene are disregarded in this study. However, Morino et al. (2010c) implied that anthropogenic SOAs are important during FAMIKA. Potential influences on simulated PM2.5 should be discussed.

Line 19 in Page 138

According to Morino et al. (2010b), 1-3 micrograms per cubic meters of nitrate were observed at FAMIKA. This magnitude is comparable to or even more than EC. Although nitrate is not abundant in summer, just disregarding nitrate is too rough.

Line 11 in Page 141

Stretch-NICAM-SPRINTERS cannot be used for a long-term simulation. However, the sentence in the line 4 in the page 134 says that the stretch grid overcomes the limitation (requirement of vast computer resources for highly resolved calculations). What a temporal scale is expected to apply Stretch-NICAM-SPRINTERS?

Line 24 in Page 141

What is the horizontal resolution of MIROC-CHASER?

Line 26 in Page 143

How can be judged that poor performance at Maebashi and Kisai is due to the topography? What is the detailed configuration of the topography data used in this study?

Line 12 in Page 144

It is very difficult to recognize the overestimation of the precipitation in the Sea of Japan, Kyusyu, and the main island of Japan in Fig. 9.

Line 20 in Page 145

The overestimation of the simulated precipitation shown in Fig. 9 may cause the underestimation of the simulated sulfate concentrations at Hedo. However, the sentences in the line 14 in the page 144 says that all results generally shows similar patterns of the occurrence of heavy precipitation in the East China Sea especially near Okinawa in which Hedo is located. They may cause confusions.

Line 4 in Page 147

Why do the offline oxidants not alter sulfate concentrations so much?

Line 11 in Page 147

Why is PM2.5 included here? It is also one of the validations of Stretch-NICAM-SPRINTERS described in the section 3, isn't it?

Line 10 in Page 148

Indeed, when the results of PM2.5 obtained by Stretch-NICAM-SPRINTERS are used in an estimation of health impacts due to PM2.5, the bias should be minimized. However, Stretch-NICAM-SPRINTERS has been immediately applied to estimate health impacts in the subsequent subsection without minimizing the bias. That is obviously inconsistent.

Line 19 in Page 148

It is very difficult to recognize that sulfate mass concentrations over the Kanto region decrease from the present to 2030 in Fig. 18 (and Fig. 14?).

Line 25 in Page 148

Why is the largest sulfate mass concentration in Ibaraki unrealistic? As shown in Fig. 11, the highest observed sulfate concentration is at Tsukuba, which is certainly located in Ibaraki. It is not strange that concentrations of secondary components are higher in downwind regions than source regions. It is very surprising that large differences among prefectures are found in MIROC with coarser resolutions while differences among prefectures are very small in NICAM in sulfate concentrations shown in Fig. 19. Are there any reasons?

Fig. 6

(d) Ayase -> Adachi

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