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Geoscientific
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# *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.

**D. Goto et al.**

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We thank the referee #1 for the comments and suggestions. The Point by Point Clarifications to the comments and suggestions are as follows;

Response to comments of anonymous referee #1

[C1-1] 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 possi-



bilities 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.

[A1-1] Thank you very much for reading our manuscript and giving us useful comments for improving the manuscript. In this study, the main purpose is to show the model performance of simulating aerosols with the stretched-grid system. Basically, when we judged that our presented model has a good performance, the model captured important features (e.g., daily and weekly variations of the meteorological and aerosol fields, their magnitudes in daily, weekly, and monthly averages etc.) over the target regions and the simulated results were within ranges obtained by general regional aerosol-transport models, e.g., WRF-CMAQ. To show these results, we have compared Stretch-NICAM-SPRINTARS (NICAM-g6str in the revised manuscript) with other models, i.e., Global-NICAM-SPRINTARS or NICAM-g6 in the revised manuscript and sometimes WRF-CMAQ, as you suggested in [C1-4]. In the evaluation of the meteorological fields, we have showed the large-scale circulations of basic physical variables over East Asia (Figure 3 of the revised manuscript, suggested by the referee #2 on [C2-5]). The results indicate that the NICAM-g6str-simulated temperature and winds are consistent to the NICAM-g6-simulated and the NCEP-FNL-reanalyzed ones. We also compared the meteorological fields (temperature, RH, and winds) and precipitation in Figures 4-10 of the revised manuscript and Table 1. We have modified our comments on the analysis to clarify them. As for EC, sulfate, and SO2, we have modified the figures and added a new table to the revised manuscript to show the statistical parameters with the results of WRF-CMAQ by Shimadera et al. (2013). PM2.5 has been evaluated using both NICAM-g6str and NICAM-g6 simulations. Please read the details in the revised manuscript.

[C1-2] 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?

[A1-2] In the revised manuscript, the comparison between NICAM-g6str and NICAM-g6 indicates that the meteorological and aerosol fields obtained by NICAM-g6str are better than those obtained by NICAM-g6. It suggests that the higher horizontal resolution under the stretched grid system leads to a better performance. After the comparisons shown in the revised manuscript, we conclude that even the simplified aerosol module is applicable for the regional simulation if the module is coupled to dynamic cores with high horizontal resolution. We have added this point to summary in the revised manuscript. However, we also need to mention the shortcoming of our model in the revised manuscript, as you suggested; (1) no nitrate and ammonium in a fixed ratio and (2) prescribed oxidants. The point (1) is addressed in [A1-10]. The point (2) is described below. Prescribed oxidant for the use of sulfate formation is not probably crucial for predicting weekly- and monthly-averaged sulfate mass concentrations only by taking into account for diurnal and seasonal variations of the prescribed oxidant (which is based on our experiments). The statement is consistent with the results of the averaged sulfate mass concentration obtained by our model (NICAM-g6str) are comparable to those obtained by a regional aerosol-transport model, WRF-CMAQ. However, our model sometimes misses hourly variations obtained by WRF-CMAQ. Therefore, at least we can say that it may be important to predict hourly variations of the sulfate formation, especially during the daytime, because the oxidants largely depend on solar downward surface radiation and indirectly on clouds. We have added these comments on the revised manuscript.

[C1-3] 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.

[A1-3] Thank you very much for your comments. To show any evidence to support our statement, we need to compare the results obtained by Stretch-NICAM with those obtained by general regional models. Strictly speaking, we need to prepare both a regional model with the same platform as the Stretch-NICAM and the output of Stretch-NICAM as the lateral boundary conditions. Although we mentioned the detail in our answer ([A1-6] to [A1-8]), we did not show clear evidence for the superiority of our presented model to general regional models, in terms of transboundary air pollution. Therefore, we have deleted the sentence (L.2 of P.135 in the manuscript).

[C1-4] 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.

[A1-4] Thank you very much for your comments and suggestions to splitting our manuscript into two. Yes, we agree to your idea and we have rearranged our manuscript to mainly focus on model results of Stretch-NICAM-SPRINTARS without topics of health impact and scenario experiments. In addition, as you suggested, we have compared Stretch-NICAM-SPRINTARS (NICAM-g6str in the revised manuscript) with Global-NICAM-SPRINTARS (NICAM-g6 in the revised manuscript). Detail is found in the revised manuscript. Although you suggested a use of MIROC-AOGCM as a reference, differences in the dynamic and physics parts between MIROC and NICAM are not small, so it is difficult to properly evaluate our presented model (Stretch-NICAM-SPRINTARS). Therefore, we have selected the model, Global-NICAM-SPRINTARS, which has the identical dynamic and physics modules to Stretch-NICAM-SPRINTARS, although the horizontal resolution were different due to the insufficient computer time to integrate Global-NICAM-SPRINTARS with the finer resolution.

[C1-5] 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.

[A1-5] Apologies for using the rainbow color without clear borderlines in figures. According to your suggestions, we have re-plotted all of figures using clear color bars.

Specific comments: [C1-6] Line 1 in Page 135 How is Stretch-NICAM-SPRINTERS potentially superior to general regional models? [C1-7] 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? [C1-8] 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.

[A1-6]&[A1-7]&[A1-8] Thank you very much for your comments. As you suggested, we need to add any evidence or references, which show problems of simulating transboundary air pollution using the general regional models. We have reconsidered this issue. One reference (Bhaskaran et al., 1998) pointed out the critical problem to simulate precipitation patterns related to intra-seasonal variability such as monsoon using the regional model through a dynamic downscaling technique. However, as far as we know, such problems for simulating the air pollution have not yet been reported. Although the lateral boundary conditions originally cause noises to perturb the airflow inside the domain, researchers have paid attention to minimize such noises by using various techniques (e.g., Nakanishi, 2001). So far, if users choose specific techniques and/or proper lateral boundary conditions, they may often escape problems to accurately simulate the transboundary air pollution. In contrast, our proposed method (Stretch-NICAM) is not required to consider such complex issues. In terms of the nesting technique and boundary conditions, we can safely say that Stretch-NICAM is absolutely superior to general regional models. In addition, as we mentioned in the manuscript, the model framework of the Stretch-NICAM-SPRINTARS is identical to that of the Global-NICAM-SPRINTARS without special modifications. This point is also very important for modelers to sophisticate the module such as aerosol module, because the model frameworks of regional models are usually different from those of global models, which often prevent expanding regional simulations to global simulations. This is based on the assumption that aerosol modules implemented to regional models are generally more detailed than those implemented to global models, because the spatial and temporal scales of the main target in the regional simulations are finer than those in the global simulations. Recently, owing to increases in the computational resources, global models have started to focus on the finer scales (Suzuki et al., 2008). To further develop the aerosol modules in the global models, the aerosol modules implemented to the regional models should be reflected to the global models, even though the implementation may be complex or perhaps troublesome. In the present study, since we cannot show clear evidence of the superiority to general regional models, we have removed the sentence (Line 1, P.135 in the manuscript) from the revised manuscript. However, we can safely say that our presented model can be applicable for simulating regional aerosols.



[Reference] Bhaskaran, B., Murphy, J. M., and Jones, R. G.: Intraseasonal oscillation in the Indian summer monsoon simulated by global and nested regional climate models, Monthly Weather Review, 126, 3214-3134, 1998. Nakanishi, M.: A Lateral boundary condition suitable for the one-way nesting scheme, Tenki, 49(2), 117-128, 2001, in

Japanese

[C1-9] 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.

[A1-9] Thank you very much for your suggestions. We have added this point to the paragraph of PM2.5 (section 3.2.3) in the revised manuscript; "At all sites, the possible underestimation of SOA may be a critical issue, as shown in the fact that the clear diurnal variation of PM2.5 during August 4-9 and suggested by previous studies (Matsui et al., 2009; Morino et al., 2010c). Morino et al. (2010c) implied that over the Kanto area SOA from anthropogenic sources, which were disregarded in this study, are large portion of total carbonaceous aerosols, even though WRF-CMAQ does not correctly reproduce such carbonaceous aerosols. More sophisticated SOA module, e.g., volatility basis-set approach proposed by Donahue et al. (2006) based on the categorization of organic vapors with similar volatility, is required for to produce SOA with higher accuracy."

[C1-10] 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.

[A1-10] Thank you very much for your comments. Surely, the nitrate mass concentration is comparable to the EC mass concentration, even in summer of Kanto area, although the nitrate mass concentration in summer tends to be lower than the sulfate one. As you pointed, disregarding nitrate may be too rough. In this study, however, we would like to show whether our presented model could be applied to a regional simulation as a general aerosol-transport model. Since the stretched-grid system in this study was used in the previous study for simulating tropical cyclones and tropical



convective clouds over oceans (e.g., Satoh et al., 2010; Arakane et al., 2013), it was not adequately evaluated over the target region focused in this study (megacities over mid-latitudes). Therefore, for this purpose, we have compared representative primary and secondary aerosols in summer of Japan. We chose sulfate as a representative secondary aerosol. The global and regional modelings for sulfate, which is formed from SO2 in the atmosphere, are more deeply understood compared to modelings for the other secondary aerosols such as nitrate and organic aerosols (e.g., Barrie et al., 2001; Holloway et al., 2008; Hallquist et al., 2009; Morino et al., 2010a, 2010b). In addition, sulfate is the largest contributor to the total secondary inorganic aerosols (e.g., Zhang et al., 2007), and the sulfate mass concentrations are larger than that the nitrate ones in August 2007 over the Kanto area (Morino et al., 2010c). Furthermore, in summer over Japan and East Asia, the difference in the mass concentrations between sulfate and nitrate is higher than that in winter. Therefore, we disregard nitrate in this study. We have added them to section 1 of the revised manuscript and modified section 2.2 as follows; "The nitrate in this study is disregarded, primarily because the main objective in this study is modeling of sulfate as a representative secondary aerosols and secondly because the nitrate mass concentrations are lower than the sulfate ones with the target of August 2007 in Japan (Morino et al., 2010c)".

[References] Arakane, S., Satoh, M., and Yanase, W.: Excitation of deep convection to the north of tropical storm Bebinca (2006), J. Meteorol. Soc. Japan, 92(2), 141-161, doi:10.2151/jmsj.2014-201, 2014. Barrie, L. A., YI, Y., Leaitch, W. R., Lohmann, U., Kasibhatla, P., Roelofs, G.-J., Wilson, J., McGovern, F., Benkovitz, C., Melieres, M. A., Law, K., Prospero, J., Kritz, M., Bergmann, D., Bridgeman, C., Chin, M., Christensen, J., Easter, R., Feichter, J., Land, C., Jeuken, A., Kjellstrom, E., Koch, D., and Rasch, P.: A comparison of large-scale atmospheric sulphate aerosol models (COSAM): overview and highlights, Tellus, 53B, 615-645, 2001. Holloway, T., Sakurai, T., Han, Z., Ehlers, S., Spak, S.N., Horowitz, L. W., Carmichael, G. R., Streets, D. G., Hozumi, Y., Ueda, H., Park, S. U., Fung, C., Kajino, M., Thongboonchoo, N., Engardt, M., Bennet, C., Hayami, H., Sartelet, K., Wang, Z., Matsuda, K., and Amann, M.: MICS-Asia II: Impact

of global emissions on regional aiq quality in Asia, Atmos. Environ., 42, 3543-3561, 2008.

[C1-11] 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?

[A1-11] We intended to mention that "long-term simulation" in P.141 of the manuscript is "several thousands integration as an atmosphere-ocean coupled model", whereas the "long-term simulation" in P.134 of the manuscript means "several decades integration as an atmospheric model". So, we have modified the latter point (by adding "for decades" to section 1 of the revised manuscript). We have removed the former point from the revised manuscript.

[C1-12] Line 24 in Page 141 What is the horizontal resolution of MIROC-CHASER?

[A1-12] Actually this part has been removed from the manuscript, because we have removed the results of scenario experiments. In the standard experiment, we also used prescribed oxidant distribution from MIROC-CHASER with the spatial resolution of 2.8 degree and the temporal resolution of three-hourly averaged monthly averages. We have added the explanation to the section 2.3 of the revised manuscript.

[C1-13] 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?

[A1-13] Thank you very much for your comments. First, the source of the topography data used in this study and shown in Figure 1 is GTOPO30. So we have added the following comments to the revised manuscript (section 2.1 and the caption of Figure 1); "the topography used in this study is based on GTOPO30 (the horizontal resolution



is 30 arc seconds, that is approximately 1 km) courtesy of the U.S. Geological Survey." We have changed the grey contour in Figure 1 to clarify the values. Second, we need to add some explanation of how the topography does influence the meteorological fields at Maebashi (139.10°E, 36.40°N) and Kisai (139.56°E, 36.09°N). In Figure 6 of the revised manuscript, you can find that especially during the daytime at Maebashi and Kisai, the NICAM-g6str-simulated winds are not close to the observations, which show southeasterly winds (that is long sea breeze toward Maebashi Plateau surrounded on three sides by mountains around Maebashi). The observed winds are caused by daytime meso-scale thermal lows developed over the central Japan covering the Japanese Alps (Kuwagata and Sumioka, 1991). The Japanese Alps with the highest terrain in Japan can affect the local meteorological fields even around 100-200 km away (Kitada et al., 1998). Therefore, we can conclude that the horizontal resolution in this study using Stretch-NICAM cannot fully resolve the complex terrains of the Japanese Alps and the Maebashi plateau. These comments had been inserted to section 3.1 of the revised manuscript. We have also added the following comments to section 3.1 of the revised manuscript; "It suggests that it is inadequate to simulate the wind patterns and diurnal transitions near high mountains around the Kanto region, whereas it is adequate to simulate them around the center of Kanto region."

[Reference] Kuwagata, T., and Sumioka, M.: The daytime PBL heating process over complex terrain in central Japan under fair and calm weather conditions, Part III: Daytime thermal low and nocturnal thermal high, J. Met. Soc. Japan, 69(1), 91-104, 1991
Kitada, T., Okamura, K., and Tanaka, S.: Effects of topography and urbanization on local winds and thermal environment in the Nohbi Plain, coastal region of central Japan: A numerical analysis by mesoscale meteorological model with a k-e turbulence model, J. Applied Met., 37, 1026-1046, 1998.

[C1-14] 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.

[A1-14] Apologies for the unclear color maps. In the revised manuscript, we recalculated NICAM-g6str using longer spinup time (one and a half months) than that in the previous manuscript. As a result, the precipitation distribution was somewhat different from that obtained by the previous simulation. In the revised manuscript, we have added the following comments to section 3.1; "During early August 2007, mainly due to passing of a typhoon over the western Japan, Okinawa, and Korea, the August mean precipitation in the western Japan is larger than that in the eastern Japan, especially the Kanto region (Figure 8 of the revised manuscript). The monthly mean precipitation is estimated to be more than 200 mm/month over the western Japan, whereas that is estimated to be less than 50 mm/month over the eastern Japan. In the NICAM-g6str results, the horizontal distribution of the August mean precipitation in the East China Sea, the Sea of Japan near the Japan coast, and Korea are closer to those derived from MSM and GSMaP than those by NICAM-g6. In the Kanto area, however, the NICAM-g6str-simulated precipitation with ranges of 50-200 mm/month is overestimated compared to the MSM and GSMaP results. The NICAM-g6-simulated precipitation over the Kanto area is also much overestimated, with ranges of 100-200 mm/month."

[C1-15] 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.

[A1-15] Thank you for your suggestions. Figure 8 of the revised manuscript shows that the heavy precipitation area with more than 500 mm/month is found in the East China Sea especially near Okinawa by both the observations (MSM and GsMAP) and simulations (NICAM-g6str). However, when the temporal variation of the NICAM-g6str-simulated precipitation and the predictive value of daily precipitation using NICAM-g6str are compared with those obtained by the observations in Figures 9 and 10 of the revised manuscript, at Okinawa (Panel (u) of Figure 9 in the revised manuscript)



the timing of the NICAM-g6str-simulated precipitation is different from that obtained by the observation and the predictive value using NICAM-g6str is the lowest among all of the sites. Therefore, the underestimation of the NICAM-g6str-simulated sulfate at Hedo of Okinawa may be caused by the possible underestimation of the sulfate from transboundary air pollution, which might be caused by large uncertainty of the simulated precipitation related to the typhoon, as the referee #2 pointed out in C2-3. We have modified this part section 3.2.1 of the revised manuscript.

[C1-16] Line 4 in Page 147 Why do the offline oxidants not alter sulfate concentrations so much?

[A1-16] Thank you very much for your comments. In this sensitivity tests for oxidants, the SO2 oxidation by OH radical strongly depends on the OH concentrations as well as the cloud cover area, whereas the SO2 oxidation by ozone and hydrogen peroxide mainly depends on their concentrations, the cloud cover area, and the cloud water content. The cloud distributions are modulated by some feedbacks of the sulfate formation through the aerosol direct and indirect effects. These various pathways can determine the magnitude of the sulfate formation. As a result, the sensitivity of the OH radical concentrations to the simulated sulfate concentration is smaller than that we expected and that to the SO2 emissions, as shown in Figure 16(b) of the revised manuscript. We have added these comments to section 3.2.2 of the revised manuscript.

[C1-17] 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?

[A1-17] As you suggested, we have modified this part (PM2.5) to section 3.2.3.

[C1-18] 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. How- ever, Stretch-NICAM-SPRINTERS has been immediately applied to estimate health impacts in the subsequent subsection without minimizing the bias. That is obviously inconsistent.

[A1-18] We actually did a bias correction by multiplying twice PM2.5 values for the application. However, we have deleted the application issue from the revised manuscript.

[C1-19] 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?).

[A1-19] Apologies for using unclear color maps. We have corrected all of figures.

[C1-20] 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?

[A1-20] Thank you very much for your comments, but we have removed this part from the revised manuscript. The strong peak in the coarse resolution causes the higher variability in the sulfate concentrations.

[C1-21] Fig.6(d) Ayase -> Adachi

[A1-21] Thank you for your correction.

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