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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.

Anonymous Referee #2

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A global non-hydrostatic model with a stretched grid system is used to simulate aerosol distributions around the highly populated Kanto region of Japan during the month of August 2007. The stretched grid system uses a fine mesh (allows high resolution) over the target region increasing to larger mesh (lower resolution) on the opposite side of the globe. This type of grid appears very promising as it eliminates the need for nesting techniques and boundary conditions required in regional air quality models. Simulated meteorological and aerosol variables are evaluated against a range of ground-based measurements and the application of this modelling system for air quality forecasting is advocated. The model is then run in a future climate scenario set-up to assess the impact of future aerosol emissions on mortality in Japan.

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Discussion Paper

The quality and content of this manuscript needs to be greatly improved before publication should be considered. The results and conclusions drawn in the first part of the manuscript in which the Stretch-NICAM-SPRINTARS is run and evaluated for August 2007 are in my opinion inconsistent. The authors conclude that the "simulations of Stretch-NICAM-SPRINTARS are generally successful in simulating the air pollution over Japan and are adequate as a new regional model for simulations over the Kanto region". However, there are clear shortcomings in the current simulations. Omission of nitrate aerosol, simplified sulphur and SOA chemistry are major barriers to a skilful air quality forecast. Indeed Figures 10-12 highlight the clear underestimation in aerosol fields and the model clearly misses a number of peak SO2 and PM episodes. Poor performance in precipitation fields will seriously affect the aerosol transport within the simulations in particular the impact of trans-boundary pollution from China. While the authors highlight various developments/improvements which should be conducted in future work to improve the quality of these simulations these points/the limitations of the current simulations should also be emphasized when discussing the results in Section 3.2.

There is no mention of the global performance of the model. Is it capable of producing the large scale circulations required for an adequate simulation over the target region? Perhaps an evaluation of large-scale circulations against reanalysis could be performed. I am surprised given that the model is nudged that there is such discrepancies in the circulation. 2D spatial plots of the circulation compared with reanalysis or satellite observations would give a nice depiction of the models ability in capturing the general flow.

From the current evaluation it is not clear whether the simulations using the stretched grid model are superior to a more conventional nested uniform grid regional model. An evaluation against a regional model would put the current study in much better context. Furthermore the authors claim that the computational cost of running the stretched model is 256 times smaller than a global model with a uniform grid of the

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same high resolution as in the target region. Given the application to regional air quality the authors should really be comparing the cost to a regional model over the same target domain as used in this study.

The scenario experiment is badly described and therefore difficult to follow. In its current form I find it superfluous to the manuscript as the results are very provisional and should be clearly declared as such. If the recommended improvements to the first part of the manuscript were made this would make a perfectly reasonable paper on its own without needing the future scenario experiment. It reduces the impact of this paper. The model configuration and method used to calculate the mortality rate is poorly referenced and insufficiently described. For example, I assume MIROC-AOGCM simulations are used to nudge Stretch-NICAM-SPRINTARS in August 2030 but this is not at all clear from the model description. What is "x" used in the calculation of D(x)? From the text I deduced that it doesn't refer to a NICAM grid point as the authors refer to a "NICAM grid" and "grid x" separately. Where were the population distributions taken from ? I would recommend a total rewrite of Section 2.4 before publication is considered.

Large sections of the manuscript are poorly written and lack clarity making it difficult to follow the experimental design and subsequent evaluation. Given the focus on air quality a more detailed description of the aerosol scheme, in particular the sulphur chemistry is required in Section 2.2. I would recommend splitting Section 2.3 into 2 separate sections 1) Design of Experiment and 2) Observations. Furthermore the quality of the figures is very poor making it extremely difficult to follow the description in the manuscript.

Some specifics:

Section 2.2. L17: Are the authors assuming that all sulphate is in the form of ammonium sulphate?

L20: "The nitrate concentrations...can be disregarded" This is a confusing statement.

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Do the authors mean nitrate emissions are low enough in summertime in Japan to be disregarded in this study (in which case suitable references should be provided) or that nitrate is not represented in these simulations? Please rephrase for clarity.

Section 2.3: How long of a spin-up was allowed in the Stretch-NICAM-SPRINTARS simulations?

Section 2.4: "Therefore we combined Stretch-NICAM-SPRINTARS with MIROC_AOGCM by nudging..2026-2035". These sentences are badly constructed and very unclear. Please rephrase.

Section 3.1: The description of Figure 8 does not reflect my interpretation of the same figure, where there are large discrepancies between model and observations. It is clear from Figures 8 and 9 that the model overpredicts the precipitation in the target Kanto region.

Section 3.2.1, Last sentence: There is no evidence in the manuscript to support this statement that the simulations of trans-boundary pollution is well simulated. Remove or provide evidence.

Figure 17 shows a clear underestimation in the extinction coefficient below 1km however they are within observational uncertainty. This should be stated as well as an explanation for the large uncertainty in the observations should be given.

Section 4.2: The role of nitrate in future emission scenarios is expected to increase and potentially outweigh SO2 emissions in terms of contribution (see for example Bellouin et al. JGR 2011 or Bauer et al. ACP 2007). Increasing emissions in Asia will therefore impact trans-boundary pollution in Japan and impact results found here. The limitations of this scenario study needs to emphasized.

Figure 19: I find it interesting that the MIROC-AOGCM shows higher regional variability in sulphate concentrations than NICAM given its coarser resolution. Do the authors have an explanation for this?

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Thank you for the opportunity to review this paper.

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