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Interactive comment on "Ensemble Forecasts of Air Quality in Eastern China – Part 1. Model Description and Implementation of the MarcoPolo-Panda Prediction System" *by* Guy P. Brasseur et al.

Anonymous Referee #2

Received and published: 15 September 2018

General Summary This paper describes a newly developed ensemble forecasting that is being used to produce daily deterministic and probabilistic forecasts of air quality in China. This is a timely study in light of the fact that air quality has become a really serious environmental concern in Asia. Air quality forecasts, such as those described here, enhances the ability of air quality managers to warn the public in advance of the forthcoming air pollution episodes. The ensemble system is described in detail with both the capabilities and shortcomings for a period in March 2017. I think the paper is suitable for publication in GMD but have some minor specific comments that are listed

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Discussion paper

below.

Specific comments

Line 115: Change “include assimilated data” to “assimilate data”.

Line 145: I guess you mean to say “Numerical weather forecasting at seasonal scales. . .” here.

Line 152: In addition to meteorological forecasts, I think it would be useful to drive a single model with an ensemble of emission scenarios and chemistry.

Line 158, 161 and 162: I suggest naming all the processes instead of leaving the reader with the curiosity of what “. . ..” mean here.

Lines 177-190: I suggest defining all the acronyms (e.g., WRF-Che, WRF-CMAQ, SILAM etc.) upon their first use here.

Line 207: Change “aata” to “data”.

Line 214: Suggesting adding NOx to ozone-CO-NMVOC.

Line 237: Could you please provide a brief summary (2-3 lines) of the overall performance of IFS over March-May 2017?

Section 2.2: Please provide information about at what resolution CHIMERE forecasts were produced.

Line 275: Change to Fast et al., 2006.

Line 319: Spell out STEAM.

Line 433: Could you say more about how anthropogenic emissions are adjusted every week? Do you employ a machine-learning approach?

Line 440: I guess you mean “ideal” profiles and not “idea”.

Line 453: All these papers focus on the U.S. It is okay to cite these papers but it would

be useful to add few references for applications of CMAQ over China.

Section 5.1: Can you say something about the role of representativeness errors in model-observation discrepancies? Will the model performance change if you isolate the comparison only to rural sites?

Line 754: Is better performance of IFS related to assimilation?

Lines 735-758 are the same as 760-784. Please remove the duplication.

Lines 811- 812: It is well known that models have difficulties in reproducing nighttime concentrations of air pollutants including ozone. How does the model perform for daytime ozone? Section 5.2 provides some information about the daytime performance in three metro areas but it will be good to examine and discuss spatial patterns of daytime ozone in particular.

Line 906: Change RSME to RMSE.

Figs. 8 and 9: I am somewhat puzzled by the PM2.5 panels in Figs 8 and 9. For Beijing, ensemble median (Fig. 8) is lower than the observations for March 5-10 while all models show higher PM2.5 values than the observations in Fig. 9. I also suggest using the same color for observations throughout. Fig. 8 shows observations in black and Fig. 9 shows in red. Adding legends to Fig. 8 will also be useful. I was also expecting the spread will be higher in Fig. 8 because IFS has such large value of PM2.5. Similarly, all the models are lower than observations for ozone (Fig. 9) but the median of the models in Fig. 8 is higher than the observed ozone. Please check the plots carefully and revise the discussion.

Line 935: Do you want to say that WRF-Chem-MPI meteorological simulations are driven by IFS?

Line 958: Even the WRF-Chem-SPS does not agree with other models for odd-oxygen.

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