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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ensemble Predictions of Air Pollutants in China in 2013 for Health Effects Studies Using WRF/CMAQ Modeling System with Four Emission Inventories" *by* Jianlin Hu et al.

Anonymous Referee #4

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General Comment

The authors showed an inclusive validation about the ability of CMAQ model to simulate the air pollutants (O3 and PM2.5) in China with using four different EI data in recent year (2013). They used the widely-used statistical indices for the validation and observations which covered wide areas in China. An ensemble method to obtain better prediction of air pollutants in China was proposed which is the main part of this paper. This paper is well within the scope of this journal, however, I noticed several issues in this paper which cannot be passed over to be published. I suggested that the authors should consider the following comments: two major and several specific comments.

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Discussion paper

Major Comment 1:

My biggest concern is the lack of carefulness in the manuscript. Several typos, mistakes in table and figure, and the insufficient explanations can be found which make the manuscript difficult to read and greatly damage the value of this paper. I pointed out some of those points in the specific comment below, and I strongly suggest that the authors consider those comments and should carefully and thoroughly check the manuscript again before revised submission.

Major Comment 2:

The authors set a goal of this paper on proposing a method for using the model simulation to health impact study and so the authors put “for health effect study” in the title. However, it was not clear which part of the manuscript was particularly dedicated for the health effect study. I concerned if the indices of air pollutants used in the manuscript: daily, monthly, and annual means, 1hourly and 8hourly O3, are appropriate for this purpose. I think more sentences is necessary to discuss the validity of those indices to be used for the health impact research, if they want to claim it as, at least, a part of heath effect study.

Specific Comments:

- Model description: There was no descriptions about the model domain. Figure S1 can be moved from the supplement to the manuscript since the abbreviation for the different regions in China were frequently used in the manuscript.

- E1-E4: How did you treat the observation from 422 sites? Are these data once averaged out to form the city average for each of 60 cities, and then calculate the statistical indices (MNB, MNE, MRB, MRE)? Please make it clearly described in the manuscript.

- L249-251: It is better to briefly describe the reason why different statistical indices are used for O3 and PM2.5.

- E6: A brief explanation of the method to minimize the function Q is necessary.

- Table 1: Are these statistical indices calculated using annual mean? not clearly described.

- L286-288: The description here is inconsistent with Figure1. Is this sentence correct?

- L295: Why were January and February omitted?

- L300-301: I couldn't understand the meaning of this sentence. Are there any typo or mistake?

- L302-304: It is difficult to see what this sentence said from in Figure 1.

- Figure 2: The explanation to properly see this figure is highly insufficient. What does the x-axis stand for? Is it the absolute concentration of observation or simulation? Furthermore, "goal" and "criteria" in Figure 2 should be explained somewhere in the manuscript. Otherwise the readers cannot take the messages properly from this figure.

- L311: typo?, a period -> comma?

- Figure 3: Are these indices (O3-1h, -8h) maximum 1h- or 8h- mean concentration in a day (=daily maximum 1h or 8h-mean O3)? If so, should be more clearly stated.

- L327-328: I don't think so. There were large differences between SOE and MEIC over the oceanic area east of China.

- L344: typo?, South Asia -> Southeast Asia

- L354 & L361: What is NCY?

- L362ïijŽtypo?, YRD -> PRD?

- Table2: This is too detailed information. It can be moved to supplement.

- L410-412: Why are the values referred here as the MFB of individual simulation (-0.25 – -0.16) different from those appeared in Table 1 (-0.32 – -0.21)? If the definitions are

different for both, it should be clearly written in the manuscript. I really confused here.

- L412-413ïijŽSomething wrong with English.

- L413-415: Same as the two comments above, why are the values of MNB of individual simulation (0.06 – 0.19) different from those appeared in Table 5?

- Table 3: The authors showed that the weighting factor of each EI can vary for different averaging time. in general, EDGAR and REAS have large weight for daily and monthly, and the other two Chinese EI were weighted large for annual time scale. I encouraged the authors to discuss more on the interpretation of it.

- Table 4: This is also too detailed information. If you only want to say how many cities out of 60 can improve their prediction with ensemble and do not intend to describe its regional differences, this table can be moved to supplement and it is enough to briefly describe the result in the manuscript.

- Table 5: This table showed that the weighting factor can vary large depending on the region. Table 3 demonstrated the factor also change for different averaging time scale. And the factor may be different for the different year. The purpose of this study is proposing an ensemble method for obtaining the better air pollutants concentration data for health effect estimation, from this point of view, how do the authors think the best way to calculate the weighting factor in China? Ned some more sentences on it.

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