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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying atmospheric nitrogen deposition through a nationwide monitoring network across China" *by* W. Xu et al.

Anonymous Referee #2

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General comments:

This paper is a useful contribution to the issue of atmospheric nitrogen (N) deposition in China. It presents a 5-year monitoring data of dry and wet N deposition at 43 sites across China. However, the methods to obtain deposition fluxes are questionable which can result in large uncertainties to the conclusions made in this study. Some of the uncertainties are not necessary and could be constrained. I explained this in detail below.

(1) site locations:

Different from the air quality monitoring networks which focus on the protection of human health, the deposition networks are used to assess the pollutant impacts on

sensitive ecosystem and vegetation. Therefore, many networks, for example, CASTNET/NADP in US, CAPMoN in Canada, and IMPACTS in China, locate their sites in rural or protected areas in order to minimize effects from local pollution sources. In this paper, 10 out of the total 43 sites are located in urban areas, which are not homogeneously distributed in China. The comparison of regional averages including results from urban sites may not reveal the real spatial differences. For example, southeast China has two of the largest industrial centres and megacity clusters (the Pearl River Delta region and the Yangtze River Delta region) in China. Severe air pollution has been reported in this region (e.g., Chan and Yao, 2008). But this study shows the mean concentrations of reactive N species in southeast China were 15-25% lower than the national averages (Table 1). I suggest to remove all the urban results in the analysis.

(2) the method to derive the dry deposition flux:

This paper estimated the dry deposition flux by the inferential method where the modeled dry deposition velocity (Vd) is paired with the measured concentration. Vd results were extracted from the 1-year productions by a global CTM model (GEOS-Chem), which has a horizontal resolution of 1/2 x 2/3 degree. This resolution is in fact too coarse for this study. Many sites (for example, NC1-NC2, NC4-NC5, NE2-NE3, SE9-SE10, SW5-SW6-SW7) are too close from each other and located within the same grid. It means the same Vd values are used for different sites although they may have different surface characteristics. What is the height where the Vds were calculated? 70 m AGL? Vd for HNO3 is very sensitive to the calculation of Ra and the values computed at 70 m AGL should be much smaller than that at 2 m AGL. This also applies to the other species at some extend. The Vd calculations highly rely on the landuse types. Did you confirm the landuse map used in the simulation with the actual landuse types of the monitoring sites? They may not be consistent. In this paper, only 1-year simulation (2012) was used to derive Vds for the 5-year (2010-2014) period. This ignored the year-to-year variations in Vd. Several approaches can be taken to improve this. For example, you can run the GEOS-Chem model from 2010 to May 2013 and fill

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the gap in the period when GEOS meteorological data is unavailable using the means calculated from all the available simulations. Or you may try a single-point dry deposition model which can use the meteorological driving forces from various sources (e.g., field measurements if avaiable, MM5/WRF simulation).

(3) the sampling method of wet deposition:

This study utilized a bulk sampler instead of a wet-only sampler to collect the wet depositions. A well-known problem about the bulk sampler is that the wet deposition samples can be contaminated by the dry deposition. This is even worse in the north region of China where precipitation is not frequent and dry deposition may dominate the total deposition. As mentioned in the manuscript, the bulk deposition flux of N can be 20-39% higher than the wet-only deposition. This needs to be emphasized in the conclusion and abstract of the manuscript to remind the readers be careful when comparing your results with previous studies.

Specific comments:

P18372: The description of sampling methods is not clear. At the sites without power, how HNO3 was sampled? Was NO2 sampled using Gradko diffusion tubes at all sites? If yes, there is no need to mention this twice in the same section.

P18378L23: annual precipitation -> annual precipitation amount?

P18387L8: According to the references given, I think it means this study made improvements compared with previous work in China. It is better to clarify this.

P18387L20-25: The organic N species have been found as important contributors to the N dry deposition. For example, Turnipseed et al. (2006) reported that PAN accounted for 20% of the daytime NOy flux at a forest site. This should be included in the uncertainty discussion (section 4.4).

Supplement section S5: As stated in the manual of GEOS-Chem (http://wiki.seas.harvard.edu/geos-chem/index.php/Dry_deposition), the dry depo-

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sition for particles followed Zhang et al.(2001) scheme, instead of Wesely (1989), which is a dry deposition scheme for gases.

Technical corrections:

P18372L3: Sutton et al. (2001) -> Sutton et al. (2001))

P18372L11: delete "produced by"

Supplement section S1: South China (SC) -> Southeast China (SE)

References:

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