

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2021-455

Anonymous Referee #2

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Referee comment on "Long-term trends and drivers of aerosol pH in eastern China" by Min Zhou et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-455-RC2>, 2021

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This manuscript uses a long-term dataset of hourly observations of inorganic aerosol constituents and water-soluble gases from Shanghai to quantify the drivers of aerosol pH variation over a decade during which substantial air quality regulations were introduced in China. The authors examine long-term trends from 2011-2019, which allows them to assess the impacts of the Air Pollution Prevention and Control Action Plan introduced in 2013. They also project how future emissions reductions may change aerosol mass loadings and pH. This is a very novel and interesting analysis.

Overall, my main concern with the manuscript is that the methods section is much too brief. The authors need to provide a lot more explanation of how they generated the data that are presented in Figures 1, 3, 5, and 6. Since each of these figures represents perturbations or trends to some previous averaging period, the way in which the data are averaged (and perturbed) needs to be explained more clearly. For the long-term trend in Figure 1, the approach seems fairly obvious, but the way in which the seasonal and diel cycles are formulated in Figure 3 and 5 is quite confusing.

Specific comments:

P1 L33-36 It is hard to understand the meaning of this sentence. Are the authors suggesting that NH<sub>3</sub> and NO<sub>x</sub> emission controls are not going to be effective in the time period leading up to 2050, or that they won't be effective after that point? Further – is it appropriate to examine the values just on their own – what would happen if only SO<sub>2</sub> reductions were implemented? Presumably NH<sub>4</sub><sup>+</sup> and NO<sub>3</sub><sup>-</sup> would increase much more.

The data in Figure S2 look much more tightly correlated in the later years. Can the authors comment on whether this reflects improvements in the accuracy/precision of the measurements or whether the relative importance on the measured ions to the overall ion balance may have changed?

The sensitivity tests mentioned in Section 2.3 (lines 143-152) and in Figure S4 are not sufficiently well-described. What did the authors alter and what did they hold constant in each test? The results appear to span a different range of ALWC for each variable. In general, I did not find this section added much to the manuscript, and only made me confused about the method. I suggest removing this section unless it can be much more clearly explained.

The language in Section 3.4 is a little confusing because the authors describe the changes in absolute amounts of particle and gas phase ammonium and nitrate somewhat interchangeably with their partitioning ratios and it becomes hard to keep track of what metric is being described. It might be more clear to focus first on the absolute particle phase amounts and then explain the changes in the context of the partitioning. The sharp increase in the particle phase partitioning of nitrate between 2040 and 2050 is quite difficult to understand in the BHE scenario – what explains it?

Figure S6 – The caption should explain why are the rates of change only calculated starting in 2013 when the data record starts in 2011. And why a separate slope is calculated for the latter part of the record.