

Comment on egusphere-2026-3573:

Dust-driven changes in particle pH differentially regulate secondary inorganic aerosol formation in central China, Hongyu Zhang, Yating Zhang, Yang Xiao, Xiaohui Bi, Shenbo Wang, Qili Dai, and Yinchang Feng

General comments

This study conducted synchronous observations from seven cities in central China during a regional dust episode and examines changes in PM_{2.5} composition, modeled aerosol pH, sulfate formation rates, and nitrate gas–particle partitioning. The multi-city dataset is potentially valuable. However, I have fundamental concerns about the scientific contribution and the interpretation of the results. In particular, it is unclear what new mechanistic understanding is obtained beyond the established effects of crustal cations on aerosol pH and the known pH dependence of sulfate chemistry and nitrate partitioning; the results appear mainly to show how these established mechanisms operate under different urban atmospheric conditions.

Specific comments**1. Scientific novelty and significance of the knowledge gap**

The main knowledge gap identified in the Introduction is that intercity differences in the effects of the same regional dust episode on SNA formation have not been sufficiently investigated (Lines 96–99). This is a valid descriptive gap, but the manuscript does not clearly explain what general scientific insight can be gained by addressing it, particularly because several relevant mechanisms are already well established (Lines 80–95). Similarly, the stated objective of “clarifying” the effects of dust on aerosol particle acidity and SNA formation does not clearly define the specific scientific question being addressed in this study (Lines 111–117).

2. Distinction between observations and model-derived results

The bulk aerosol pH of PM_{2.5} is calculated using ISORROPIA-II, sulfate formation rates are calculated from kinetic expressions (Lines 193–202), nitrate partitioning is estimated from modeled pH and ALWC (Lines 203–221), and individual effects on Δ pH are evaluated through model perturbation (Lines 342–346). Nevertheless, these model-derived results are often described using causal wording, for example that dust “drove” pH changes (Lines 307–308, 368–371), higher pH “suppressed” sulfate formation (Lines 381–386, 393–397), and pH increases “promoted” nitrate partitioning and particulate nitrate accumulation (Lines 409–412). Observed results and model-derived results should be more clearly distinguished throughout the manuscript.

3. Regional dust versus local fugitive dust

In Section 3.1, the highest CM concentration, a relatively long dust event duration, and the second peak observed in ZZ are attributed in part to local fugitive dust resuspension (Lines 240–248).

However, this interpretation appears to assume that the concentrations of regionally transported dust were relatively similar across the seven cities. At the spatial scale considered in this study (tens to hundreds of kilometers from Fig. S1), such an assumption is not justified. Even within the same regional dust plume, dust concentrations can vary substantially at the spatial scale because of spatial heterogeneity in transport, arrival timing, boundary-layer dynamics, and deposition. Therefore, the observed spatial differences do not by themselves distinguish local resuspension from heterogeneous regional dust transport. Therefore, the final recommendation for long-term management of regionally transported crustal material and local fugitive dust sources described in the Conclusions (Lines 443–449) is not directly supported by the present analysis, which does not quantitatively separate local fugitive dust from regionally transported dust. I suggest that this recommendation be substantially revised to better reflect the scope of the analysis, or removed if it cannot be supported by additional evidence.

4. Interpretation of the single-factor substitution analysis

The single-factor substitution analysis evaluates model sensitivity, but it is unclear whether the calculated ΔpH values can be interpreted as quantitative contributions of individual factors (Lines 342–369). Aerosol thermodynamics is nonlinear, and the effects of the relevant chemical and meteorological variables are interdependent. Therefore, the calculated effect of one factor may depend on the baseline state and on the values of the other variables. The authors should clarify whether these contributions are additive and robust to the choice of reference state. If not, the analysis should not be used for quantitative attribution of the observed ΔpH . At most, it shows how modeled aerosol pH responds to changes in individual input variables under the selected reference conditions.

5. Interpretation of sulfate formation and SNA processing

H_2O_2 oxidation is not included because H_2O_2 was not measured (Lines 193–200), yet TMI-catalyzed oxidation is described as the dominant aqueous-phase sulfate formation pathway (Lines 373–380; 425–428). Without considering an important aqueous oxidation pathway, this conclusion appears too strong. TMI oxidation can only be described as dominant among the pathways considered in this study.

The authors state that, although SOR and NOR decreased during the dust event compared with clean days, they remained at “relatively high levels” (Lines 296–298). However, the basis for considering these values “relatively high” is not clear. In addition, the magnitudes of SOR and NOR do not by themselves indicate whether substantial local nitrate and sulfate formation occurred during the dust episode, because these ratios can also reflect transport and air-mass processing history (see also Line 418).

6. Discussion of modeling limitations

The major modeling limitations are discussed mainly in the Conclusions (Lines 432–442). Because these limitations directly affect the interpretation of the results, they should be discussed in

the relevant parts of Section 3. The Conclusions should also be revised so that the strength of the statements is consistent with these limitations.

Technical corrections

Several statements and definitions lack sufficient clarity or consistency. For example, “their contribution” does not clearly state that it refers to the contribution of SNA to PM_{2.5} mass (Lines 69–70), and TNH_x is used without definition (Lines 108–110). The meaning of $\epsilon(\text{NO}_3^-)$ should also be explicitly defined when it is introduced (Lines 203–221). In addition, an increase in $\epsilon(\text{NO}_3^-)$ indicates a shift of nitrate toward the particle phase but does not itself demonstrate increased nitrate formation; these concepts are not always clearly distinguished (e.g., Lines 409–412). There also appears to be an inconsistency regarding Ti: it is listed among the elements measured by the Xact 625 (Lines 142–144) but is later described as unavailable for the CM reconstruction (Lines 160–164). Table 1 and Figures 7 and 8 also do not appear to be cited in the main text. The manuscript should be carefully checked for similar ambiguities, undefined terms, inconsistencies, and missing cross-references.