This manuscript presents a comprehensive modeling study that evaluates the driving factors controlling aerosol impacts on surface ozone (O_3) response, across two seasons (winter vs summer), to two emission reduction phases that have strategic policy shifts. By separating aerosol effects into aerosol-radiative interactions (ARI) and heterogeneous chemistry (HET), the authors show that summertime O_3 responses are primarily HET-driven, while wintertime responses are mainly driven by ARI. The study also demonstrates how meteorological variability contributes to summertime O_3 responses and projects how these processes may behave under air-quality control strategies. The topic is timely and of clear scientific and societal significance: it advances understanding of the nonlinear nature of photochemical O_3 production and multi-pathway effects of aerosol on this process. It is also of societal significant as the conclusion is informative and understanding the driving factors will help guide emission reduction policy to be more effective and comprehensive. The modeling approach is generally appropriate and carefully implemented. However, the manuscript would benefit from major revisions to improve clarity and to remove ambiguous or potentially misleading wording. I recommend major revision; my detailed comments follow.

Major comments:

1. The manuscript alternates between two different pairwise comparisons – (a) anthropogenic emissions vs. meteorological variability, and (b) aerosol-radiative interactions (ARI) vs. heterogeneous chemistry (HET) – without clearly stating how these four factors relate to each other. This creates a sense of disconnection in the abstract lines 19-24, the reader sees that "anthropogenic emissions and meteorology dominate winter and summer O_3, respectively" immediately followed by a discussion of ARI vs HET. Please clarify and explicitly state the conceptual framework that links the four factors.
    - In the Abstract, add a short sentence that explains the two comparisons used, like "we separate changes in O_3 into those driven directly by emissions/meteorology and those mediated by aerosol processes", or after the sentence "anthropogenic emissions and meteorological variability respectively dominated winter and summer O_3 increases" (line 19), follow immediately with a short clarifying sentence linking that conclusion to the ARI/HET result.
    - In the Introduction, define the four factors and their roles: anthropogenic emissions and meteorological variability are external drivers that change precursor concentrations and transport; ARI and HET are aerosol-mediated

mechanisms that modify photochemistry and how these mechanisms mediate O_3 response to precursor (NOx) decrease or meteorological variabilities.

- In the Results or discussion sections, organize the presentation so that readers first see the partitioning of O_3 responses into contributions from emission reduction vs meteorology variability, and then – for the emission-driven portion – show how aerosol processes (ARI and HET) modulate the response. Could add a schematic to make the logic explicit.

2. The Abstract's wording (Lines 22–24) that frames the Phase I–Phase II change in terms of "radical scavenging" is misleading and risks oversimplifying heterogeneous chemistry (HET). Radical uptake by aerosol (i.e., HO_2 scavenging) is a loss pathway for radicals: a reduction in aerosol mass or aerosol liquid water will generally reduce this loss and therefore tends to *promote* ozone formation. Thus the statement that the "weakening of this effect during Phase II reduced O_3" is unclear: if the radical-scavenging loss decreases further in Phase II, that would not by itself explain a reduction in O_3. Instead, the reversal in the net HET effect between Phase I and Phase II likely reflects changes in the *net balance of multiple heterogeneous pathways* (for example, reduced radical uptake *and* changes in aerosol-mediated production or recycling of reactive nitrogen species such as HONO or ClNO2), together with changes in aerosol liquid water content and the magnitude of aerosol reductions.
    - Reword the Abstract lines 22-24 to avoid implying that radical scavenging alone explains the Phase I → Phase II sign change.
    - In the Result section 3.2, when discussing HET roles, include discussion that separates HET into its component effects: radical scavenging, heterogeneous production of reactive nitrogen like HONO and ClNO2, or at least a discussion of the chemical mechanisms used in the model parameterization of heterogeneous chemistry. In addition, a chemical diagnostics for the ozone production/loss terms during phase I and Phase II could also be useful as this allows readers to see which HET component could explain the change of sign of HET impact between phases.

3. The manuscript correctly notes that uncertainties in heterogeneous chemistry parameterizations could influence the results, but the current treatment is not stated and does not make clear how robust the paper's conclusions are to variations in those parameterizations. Again, HET processes directly modulate

reactive-nitrogen recycling (e.g., HONO formation, N_2O_5 hydrolysis), radical budgets (HO_2 uptake), and hence O_3 production regimes; therefore, more explicit discussion and, where possible, quantification of the uncertainty introduced by HET assumptions is essential. By adding discussion of HET impact with more details, it would help.

4. Choice of O_3 metric (daily mean vs MDA8): The authors justify using daily mean O_3 on the grounds that MDA8 "may underestimate full-day aerosol effects." I disagree that daily mean is a superior diagnostic for separating daytime vs nighttime processes: opposite-signed changes during day and night can cancel in the 24-h mean, obscuring mechanism interpretation. Therefore, it would help if the authors provided mean diurnal cycles of O_3 (and key chemical drivers such as $P(O_3)/L(O_3)$) for baseline and each phase. These plots will (i) show whether daytime and nighttime responses compensate, (ii) allow comparison with observations for model evaluation, and (iii) improve mechanistic attribution.

Minor comments:

5. Figure 3 caption – panel references need correction. The panels currently cite (b) and (c) which doesn't match the description, please correct.

6. Figures S7-S8: the y-axis is labeled "O_3", but plotted quantity is the change in O_3, please relabel to change of O_3 (ΔO_3).

7. The manuscript contains several sentences that are unclear and would benefit from careful English editing. For example, the sentence: "*Therefore, the commonly used MDA8 O_3 may underestimate full-day aerosol effects*." is ambiguous. If the intended meaning is that using only MDA8 can miss aerosol impacts that occur outside the daytime 8-hour window, especially at night, please reword.

8. For figure 7, which presents changes in HO2 concentrations, it'd be clearer to express HO2 in molecules/cm^3 or ppt, as these are the standard units used for radical species. Using these units would also avoid displaying values with multiple leading zeros (as in ppb) and help readers to better assess the relative magnitude and atmospheric significance of the simulated HO2 changes.

9. Lines 14 – 29: the font size of Abstract does not seem consistent; lines 14-19 font size seems smaller than those of lines 20-29.