Reply to anonymous referee #1

Overview

This paper presents UFS-Chem, a new forecast model with inline aerosol–radiation and aerosol–cloud interactions under the CCPP framework. Overall, the manuscript is well written and clearly structured. However, I have several major concerns that require clarification prior to publication.

Reply: We sincerely thank the reviewer for the careful evaluation of our manuscript and for the constructive comments. We appreciate your positive assessment of the manuscript and have carefully addressed all of the concerns raised. The revisions made in response to each comment are detailed below.

Major comments:

1. What is the computational cost for UFS-Chem compared with v12.3? It would be useful to provide a quantitative number.

Reply: We thank the reviewer for this helpful suggestion. We have added a quantitative comparison of the computational cost in the revised manuscript. Using the same physics suite and model configuration as GEFS-Aerosols v12.3, the earliest CCPP-based GEFS-Aerosols (UFS-Chem) implementation required approximately 22–23 min for a 1-day forecast, compared with approximately 24 min for GEFS-Aerosols v12.3, corresponding to a 4–8% reduction in runtime while maintaining comparable computational efficiency.

We have added this into the revised manuscript: "*Using the same physics suite and model configuration as GEFS-Aerosols v12.3, the CCPP-based GEFS-Aerosols implementation at the initial stage of development required approximately 22–23 min for a 1-day forecast, compared with approximately 24 min for GEFS-Aerosols v12.3, corresponding to a 4–8% reduction in runtime.*"

2. Why are all the discussion periods in August only? The balance between direct and indirect aerosol effects varies by season.

Reply: We thank the reviewer for this insightful comment. We agree that the relative contributions of aerosol direct and indirect effects vary seasonally. In this study, we focused on August because it is a period of active biomass burning and elevated aerosol loading in many regions, making it an ideal testbed for evaluating aerosol–radiation–cloud interactions and their impacts on weather and subseasonal forecasts. In addition, the multi-year fully coupled subseasonal simulations used in this study are computationally expensive, which precluded extending the experiments to all seasons within the scope of this work. We have clarified this motivation and acknowledged the seasonal limitation in the revised manuscript. Evaluating the seasonal dependence of aerosol feedbacks will be the subject of future work.

We have added this into the experiment section: "*This study focuses on August, when biomass burning activity and aerosol loading are typically elevated, providing a suitable period for evaluating aerosol–radiation–cloud interactions. Although aerosol direct and indirect effects exhibit seasonal variability, extending the analysis to additional seasons requires substantially greater computational resources and is left for future work.*"

3. Why do Figures 2–3 present the time period of August 2021, but Figures 4 and 6 are for August 2016? The inconsistency in evaluation periods is confusing. Also, why do Figures 2–3 focus on v12.3 rather than demonstrating UFS-Chem performance directly?

Reply: We thank the reviewer for this comment. Figures 2 and 3 serve a different purpose from Figures 4 and 6. They are included to document the major improvements introduced in the operational GEFS-Aerosols v12.3 relative to GEFS-Aerosols v12, since the CCPP-based GEFS-Aerosols (UFS-Chem) was developed from the GEFS-Aerosols v12.3 framework and these improvements have not been previously documented in the literature. August 2021 was selected because it includes an active wildfire event that clearly demonstrates the improvements in aerosol forecasting achieved in GEFS-Aerosols v12.3. In contrast, Figures 4 and 6 evaluate the newly developed UFS-Chem system. We selected August 2016 because complete initial conditions were available for all coupled components (atmosphere, ocean, sea ice, and wave), allowing consistent fully coupled UFS experiments. Reproducing the operational GEFS-Aerosols v12.3 for the 2016 period is also not feasible because the operational code is no longer supported outside its original operational computing environment. We agree that the use of different periods may be confusing and have clarified the distinct objectives of these figures in the revised manuscript.

4. Figure 4: I wonder why CYC_IWR generates such a huge bias near central Africa? Do you have any explanation for this?

Reply: We thank the reviewer for pointing this out. Upon rechecking Fig. 4, we found that an incorrect panel was inadvertently used for CYC_OWR during the final reorganization of the figure layout prior to submission because of a similar filename. As a result, the displayed CYC_OWR panel was incorrect, which led to the apparent discrepancy noted by the reviewer. We have replaced it with the correct CYC_OWR panel in the revised manuscript. We apologize for this oversight.

5. The transition from v12.3 to UFS-Chem incorporates changes to the coupling framework, physics suite (GFSv15→GFSv17), vertical levels (65→127), microphysics, land model, and convection scheme simultaneously. The paper cannot attribute benefits to CCPP coupling specifically versus the physics upgrade. Please acknowledge this limitation explicitly.

Reply: We thank the reviewer for this important comment. We agree that the current UFS-Chem configuration includes updates beyond the CCPP coupling framework. The objective of this study is to develop and evaluate the CCPP-based GEFS-Aerosols system (UFS-Chem), which enables tight coupling between physics and aerosol–chemistry processes, rather than to isolate the impact of individual model updates. We have added a statement in the revised manuscript acknowledging this limitation.

We have acknowledged this limitation in the revised manuscript by adding the following statement: *"It should be noted that the CCPP-based GEFS-Aerosols implementation is evaluated within the complete UFS-Chem framework. Therefore, the reported performance reflects the combined effects of the UFS-Chem configuration, and the individual contributions of the tight physics–aerosol coupling and other model updates are not isolated."*

6. Figures 10–12: Why does EXP1 improve the cloud fraction bias but worsen the TOA flux and SST biases, whereas EXP2 corrects all of them? Do the EXP2 improvements reflect better physics or a new balance of compensating errors?

Reply: We thank the reviewer for this insightful comment. EXP1 includes only aerosol direct radiative effects, whereas EXP2 additionally incorporates aerosol indirect effects through aerosol–cloud interactions. The inclusion of indirect effects modifies cloud microphysical properties and cloud radiative forcing, resulting in a more consistent representation of the coupled aerosol–cloud–radiation system and improvements in cloud fraction, TOA radiative fluxes, and SST. However, we acknowledge that the current diagnostics do not allow us to fully distinguish whether these improvements arise from a more realistic physical representation or from a different balance of compensating model errors.

We have added a discussion of this limitation in the revised manuscript: "*Although the inclusion of aerosol indirect effects improves cloud fraction, TOA radiative fluxes, and SST, the present analysis does not isolate whether these improvements result from a more realistic representation of aerosol–cloud interactions or from a different balance of compensating model errors. Additional evaluations using cloud microphysical properties and process-oriented diagnostics will be needed to further assess the underlying mechanisms.*"

7. Figure 11: A better evaluation for the indirect effect implementation would be to compare against cloud liquid water path and droplet effective radius.
Reply: We thank the reviewer for this valuable suggestion. We agree that cloud liquid water path and cloud droplet effective radius are more direct diagnostics of aerosol indirect effects. The primary objective of this study is to present the first implementation and evaluation of inline aerosol indirect effects within the tightly coupled UFS-Chem framework. As this is the initial implementation, the current UFS-Chem configuration does not yet provide all of the cloud microphysical diagnostic variables needed for a comprehensive evaluation of aerosol indirect effects, including cloud droplet effective radius and cloud droplet number concentration. In response to the reviewer's suggestion, we have added a comparison of cloud liquid water path among the Control, EXP1, and EXP2 experiments for the 2016 case in the Appendix to further illustrate the impact of the aerosol indirect effect. We acknowledge that a comprehensive observational evaluation of aerosol indirect effects would provide additional insight; however, such an evaluation is beyond the scope of this implementation paper because it would require additional cloud microphysical diagnostics that are not yet available in the current UFS-Chem configuration. We have added a statement in the final section of the revised manuscript identifying expanded aerosol–cloud diagnostics and a comprehensive evaluation of aerosol indirect effects as priorities for future UFS-Chem development.

Minor comments:

Figure 6: please add quantitative annotations (global mean bias) on each panel. It is hard to tell which experiment performs better as presented.
Reply: Added.

Table 4: It would be more effective to present Table 4 as a scatter plot. Also, do you have any explanation for the few stations where v12.3 degrades?
Reply: We thank the reviewer for this helpful suggestion. We have added scatter plots comparing the Day-1 AOD forecasts with AERONET observations in the Supplement (Fig. A1) to complement the statistics presented in Table 4. Although GEFS-Aerosols v12.3 shows overall improvements relative to v12, a few stations exhibit degraded performance. GEFS-Aerosols

v12.3 incorporates several major updates, including the transition and updates of fire emissions and anthropogenic emissions, fixes to the dust emissions and AOD calculation, and improvements to the wet deposition treatment. While these updates generally improve the overall forecast skill, their impacts vary regionally, and local degradation at a few sites is likely associated with changes in emissions, aerosol transport, and removal processes. We have added a brief discussion in the revised manuscript.

L288: add a reference for the Baker–Schepanski map
Reply: Added.

L288–289: What is the sea salt emission scheme? Does wave model coupling influence sea salt production?
Reply: We thank the reviewer for this question. In the current UFS-Chem configuration, wave model coupling does not directly influence sea salt production. Sea salt emissions are calculated using the parameterization of Gong (2003), with modifications following Jaeglé et al. (2011), based primarily on the near-surface wind speed. The wave model is not coupled to the sea salt emission scheme. We have clarified this point in the revised manuscript.

L306–307: Wave model uncoupling for the 6-year experiments: does this affect SST or surface flux results?
Reply: We thank the reviewer for this important question. In the 6-year experiments, only the wave model was uncoupled because wave initial conditions were unavailable for long-term simulations, while the atmosphere, ocean, and sea-ice components remained fully coupled. Therefore, SST continued to evolve through the coupled atmosphere–ocean system. Although wave coupling can influence sea surface roughness and air–sea momentum and heat fluxes, its impact is much smaller than that of the atmosphere–ocean coupling and is not significantly affect the SST or surface flux results presented in this study. We have clarified this point in the revised manuscript.

L376: What are the mass-to-number conversion parameters for mapping GOCART mass to CCN/INP?
Reply: We thank the reviewer for this helpful comment. We have added a brief description of the mass-to-number conversion used to map GOCART aerosol mass mixing ratios to the IFA and WFA number concentrations (CCN and INP number concentrations) in the revised manuscript. The implementation follows Cheng et al. (2025), in which the IFA number concentration is derived from the five dust size bins, while the WFA number concentration is computed from sea salt, sulfate, and hydrophilic organic carbon (OC2) using the prescribed conversion coefficients of the Thompson aerosol-aware microphysics scheme.

Typos: Table 1: First column, sixth row: “GEFS-COCART” -> “GEFS-GOCART"; “CDES”-> “CEDS”
Reply: Revised.