This manuscript presents a model development effort by implementing aerosol-sensitive primary ice nucleation and machine-learning-based secondary ice production schemes into the EC-Earth3-AerChem Earth System Model. The study addresses a critical gap in representing mixed-phase clouds and their climatic impacts. The work is well-motivated, the methodology is largely sound, and the analysis is comprehensive, comparing multiple model configurations against a range of observational datasets. The findings that the combined aerosol-sensitive PIN and SIP scheme yields realistic INP and ICNC fields and modulates cloud-phase partitioning are valuable contributions to the field.
However, the manuscript requires revisions to improve clarity, justify methodological choices, and strengthen the interpretation and discussion of results. The current presentation sometimes lacks depth in explaining the physical mechanisms behind the modeled changes and in contextualizing the persistent model biases. The following specific comments aim to help the authors improve the manuscript.

The description of the nudging technique is brief. Please specify the nudging strength (e.g., relaxation time scale) and the vertical extent above the PBL where it is applied. Furthermore, while you mention that nudging "can also dampen aspects of the fast response," a more detailed discussion is needed on how nudging might specifically affect the evaluation of cloud microphysical responses to your new parameterizations, which are process-based and potentially sensitive to synoptic variability.

The choice of a 12-year simulation period (2009-2020) is justified for satellite overlap. However, for INP evaluation which uses specific campaign data, is this period representative of the climatological INP state, or could interannual variability (e.g., in dust emission or marine productivity) influence the comparison scores (e.g., P_t1)? A brief comment on this would be helpful.

You state that riming tendencies are not diagnosed and are internally estimated by RaFSIP. This is a crucial detail. The reader needs a clearer understanding of how this internal estimation works, as riming is a key driver for several SIP mechanisms. The reference to Frostenberg et al. (2025) is given, but since this is a "to be submitted" reference, the manuscript should include a more self-contained, concise explanation of the method or its core assumptions in the main text or a dedicated appendix.

**Lines 340-345:** The caveat regarding surface vs. aloft INP dominance is critical. This point should be elevated and expanded upon in the discussion (Section 4). What are the implications for your model's cloud impacts if mineral dust dominates ice nucleation aloft in many regions, while your combined scheme's superior performance is primarily driven by better surface INP representation from marine organics? Does this create a potential disconnect between INP evaluation skill and actual cloud impact skill?

**Lines 360-370:** The explanation for the limited impact of RaFSIPv1 is clear. However, the description of the "forestBRwarm" mechanism in RaFSIPv2 and other "additional pathways" remains vague. Please provide a more concrete, physical description of the primary mechanisms that make RaFSIPv2 effective independent of concurrent PIN. For example, how does the scheme handle seeder-feeder processes conceptually?

**Lines 420-450:** The discussion of persistent LWP/IWP biases and the role of COSP simulator artifacts is excellent and honest. However, the conclusion that "the main structural biases are not removed by representing PIN and SIP" is somewhat buried. This should be a central, clearly stated takeaway in the abstract and conclusions.

**Lines 490-510:** The analysis of radiative effects is comprehensive. However, the interpretation of the "net warming" introduced by RaFSIPv2 (Fig. 5i) needs strengthening. Is this warming primarily due to LWP decrease (as stated) leading to reduced cloud albedo? Link this mechanism more explicitly to the physical processes in SIP (e.g., conversion of liquid to ice via rime splintering, potentially leading to faster precipitation and reduced cloud lifetime/albedo). Also, discuss the climatic significance of a +10 $W/m^2$ TOA CRE anomaly at high southern latitudes.