**General Comments**

The manuscript evaluates the WRF-Chem model's ability to simulate chemical species over Réunion Island, focusing on key species such as formaldehyde, methanol, isoprene, Iox (isoprene oxidation products), monoterpenes, acetone, NO₂, NOₓ, OH, and O₃. While the study offers valuable insights into VOC and NOₓ simulations in a tropical setting, significant improvements are needed, particularly in defining clear scientific objectives and providing a robust statistical evaluation of model performance for both meteorological and chemical species. Substantial revisions are required.

**Major Comments:**

1. The manuscript provides excessive detail on data collection methods, particularly PTR-MS measurements. Since the primary focus is on model evaluation, these descriptions should be condensed, retaining only essential information relevant to data analysis and model validation.
2. The study introduces updates to chemical mechanisms (Sections 2.2.3.1, 2.2.3.2, Table 2, and sensitivity run S4). If these updates represent novel developments, they should be explicitly highlighted in the abstract and conclusion. Otherwise, proper citations should be included.
3. Despite significant differences in simulated precursor concentrations (Fig. 18), O₃ predictions remain relatively stable. The authors should provide a thorough explanation of this discrepancy.
4. The use of sequential 2-day simulations (Lines 158–160) is unsuitable for evaluating monthly trends. It is recommended to conduct daily 48-hour simulations, starting at a consistent time (e.g., 00Z or 12Z UTC), and assess either Day 1 or Day 2 results to account for numerical model performance variations over different forecast hours.
5. The description of biogenic emissions in Section 2.3.2 should be streamlined unless significant modifications were made to MEGAN 2.0.4.
6. Sensitivity run S1: The over-prediction of NO₂ and NOₓ at the LP station (Lines 598–600) is attributed to surface-level emission injection. Instead of reducing NOₓ emissions by a factor of five, a more appropriate approach would be to inject emissions at the plume rise height.
7. Figure 8: Evaluating meteorological simulations based on a single-site comparison is insufficient. Are additional meteorological observation sites available? Including statistical metrics (e.g., correlation coefficient, RMSE, mean bias) would strengthen the evaluation.
8. Figure 10: Include a simulated vs. observed O₃ comparison at the Le Port station to assess the impact of NOₓ and VOC predictions on surface O₃ levels.
9. Lines 368–369 mention 18 air quality monitoring stations. The evaluation should incorporate all available sites with statistical metrics such as correlation coefficient, root mean square error (RMSE), and index of agreement (IOA).
10. Lines 33–34: The statement attributing a 6 ppbv O₃ overestimation to the exclusion of halogen chemistry is overly simplistic. Ozone overprediction can stem from multiple factors, including uncertainties in emissions, vertical mixing, model resolution, and atmospheric chemistry.

**Minor Comments:**

1. Line 23: Define “a.s.l.”
2. Line 12: Add Piton de la Fournaise’s location to Figure 3.
3. Line 176: Define MEGAN and include references.
4. Line 190: Spell out MVK and MACR.
5. Line 230: Clarify the meaning of “higher resolution data (0.75° x 0.75°).”
6. Line 233: Spell out “BIGALK.”
7. Line 241: Define “EDGAR.”
8. Line 242: Define “HTAP.”
9. Line 354: Avoid duplicate definitions (e.g., ISOPOOH on Line 196).
10. Line 560: Define NOₓ (=NO+NO₂) upon first mention.
11. Lines 578–579: List stations categorized as “other stations.”
12. Line 582: Correct “cyan” to “blue” (also check Line 588).
13. Line 591: No green dots appear in Figure 2—did you mean red dots?
14. Lines 613–614: The statement that NOₓ model agreement issues stem from O₃ overestimation is confused, as NOₓ is one of major precursors of O₃ formation rather than being driven by O₃ levels.
15. Line 629: Should reference be “Fig. 14”?