We thank the two reviewers for their constructive comments and suggestions that helped improving the paper. We answer the two reviewers in detail below, in blue. We also want to point out some major changes we made in the paper during the review.

1) While investigating the winter under-estimation after the smoothing step, we realized that we made a non-optimal choice for the extension of the MAX-DOAS substituted profile when regridding it on the FTIR altitude grid in Sect. 3.2.1. Actually, after the substitution step (eq 5 in the paper), we need to transform back this MAX profile on the FTIR grid, to be able to apply the FTIR AK (eq 6). For this step we used a regridding that was extending on the FTIR grid by aligning to the FTIR prior, above the MAX altitude height. But actually the FTIR prior is constant, and in winter the FTIR prior is much larger than the retrieved FTIR (see Fig A5a, now included as Fig 7 in the main part of the paper), this was strongly enhancing the MAXsubstituted_regridded_onF. In summer, it was the other way around, leading to a seasonal bias in the smoothed comparisons. Now, if instead of extending on the FTIR prior we extend on the retrieved FTIR profile, the seasonal bias disappears, so we updated the whole Sect. 3.2.1 and 3.2.2 smoothing part (Figures 8, 9, 12 and Table 4).
2) We revised Fig 11 where we compare the different MAX-DOAS MMF data to DS UV. The figure was originally done with all the available points but the strong impact of data sampling (especially in winter) lead to large differences in absolute and relative differences compared to Fig. 4. We decided to redo Fig 11(and A13, now A5), now only focusing on periods when all the datasets have valid data in a 15min interval.
3) Several figures in the appendix that were primarily diagnostic or redundant have been removed or merged. We removed A1, A2, A5, A6, A7, A8, A11, A12, A15 and simplified A4. We also moved to the appendix part of the discussions in Sect. 3.2. We hope these changes improve the readability and the visual density of the manuscript.

Reviewer 1

The authors present a valuable intercomparison study of formaldehyde (HCHO) vertical columns measured by three distinct ground-based remote sensing techniques. This work addresses a pertinent need for harmonized validation datasets, especially with the increase in satellite HCHO observations. The study is well-structured, and the methodological descriptions are clear. However, the current presentation reads more like a technical report than a scientific paper. Enhancing the readability by framing the study within a clear scientific question and discussing the implications of the findings would significantly strengthen the impact of this work. Given the technical soundness and the importance of the topic, I recommend Minor Revision after the following points are addressed.

We thank the reviewed for the presentation suggestion. We revised the abstract, introduction and conclusion keeping in mind the following scientific question “Are the HCHO retrievals consistent within the networks currently used for satellite validation (MAX-DOAS, FTIR, DS PGN)? Can we improve the MAX-DOAS?”

The abstract contains excessive methodological detail at the expense of clearly stating the core scientific findings and their implications. The introduction currently provides a good literature review but lacks a clear statement of the specific scientific gap this study aims to fill.

We simplified the abstract by removing many methodological details and revised the introduction stating more clearly the gap we want to fill.

The manuscript mentions that MMF and MAPA results often differ, leading to limited consolidated data output. However, it does not provide a clear, accessible explanation of why they differ, which is crucial for the community's understanding. In Section 2.1.1 and 2.1.2, the authors may want to include a concise summary highlighting the fundamental differences between the two MAX-DOAS retrieval approaches (e.g., optimal estimation vs. parameterization, use of a priori information). Consider adding a short paragraph or a table (in addition to Table 2) that contrasts the core philosophies, strengths, and inherent limitations of the MMF and MAPA algorithms. This will help readers understand the root causes of the observed discrepancies in HCHO VCDs.

We thank the reviewed and decided to add an introductory paragraph on in lines .

The results show a significant underestimation (~22%) by MAX-DOAS compared to direct-sun measurements, primarily attributed to the a priori profile choice. The authors may need to expand the discussion to explore why the CTM-based profiles improve the agreement in some seasons but not in winter. The season-dependent behavior may deserve more deep discussion. Link it to the actual seasonal variation in the atmospheric HCHO profile structure over the site. Explain why the standard exponential decay profile is particularly inadequate in certain conditions.

As discussed above (see point 1), we revised the smoothing during the review, and we don’t have such a season-dependent behaviour for the smoothed comparisons anymore. Moreover, we updated Fig 11 (see point 2) and this removed the wrong impression that using the model profiles was worsening the comparison in winter. However, winter is still a more sensitive period, with its small HCHO columns, the larger SZA (always above 60°, leading to larger errors for all the ground-based data, cf Fig A3), the smaller number of measurements, the increased sensitivity to data sampling and the larger differences between the two models. We added a discussion before discussing Fig 12, around lines 705-725 and in the conclusions (importance of winter).

The manuscript states the importance of the work for satellite validation but stops short of concrete recommendations. In the conclusion or a dedicated subsection, the authors may need to synthesize the findings into clear, actionable advice for the validation community.

We revised the conclusions adding some recommendations and discussions for the satellite validation.

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