

The authors would like to thank Prof. Krol (R1) for his time and his interesting discussions on our draft. Following the reviewers' comments, we have expanded the methodological assessment presented in the Supplementary information. Several of these additions directly address comments made by R1, including sensitivity analyses of NO₂ retrieval uncertainties in fire scenes, background selection, the performance of the synergy plume-masking approach and the possible effect of NO_x plume ageing on the MDR.

Please, find our point-by-point responses to major comments below. Afterwards, we have also addressed the minor comments that R1 made on the manuscript's PDF and in the supplementary PDF. Herein, all R1 comments are shown in black and our responses in blue. Please note that, when referring to line numbers in the revised manuscript or Supplement, we use those from the clean versions (without tracked changes) as, depending on the settings, Word may change the line numbering when tracked changes are displayed.

This paper addresses the 2025 fires at the Iberian Peninsula using TROPOMI CO and NO₂ data.

The main development presented in the paper is the “synergy” method to quantify CO and NO₂ emissions from individual fires. These top-down estimates are compared to bottom-up estimates from GFAS, GFED, and FINN.

A main finding is that the CO emissions from the top-down method are much higher (3x) than the bottom-up estimates. Concerning the NO_x/CO mole density ratio (MDR: NO_x emissions relative to CO emissions: a metric for combustion efficiency), the paper finds variability in time and space, which is sometimes consistent with bottom-up estimates.

Overall, the paper presents a new “satellite data only” methodology that holds some promise to independently quantify wildfire emissions in complex fire scenes.

The paper is well written and clearly presents the results. I have a number of small comments, which I add as annotated manuscript and supplement. I have some major remarks that could improve the quality of the manuscript further. These points are listed below.

1. Satellite data quality

TROPOMI columns are derived from fingerprints in the absorbed spectrum (UV/vis for NO₂, Short-wave IR for CO). Specifically, for NO₂, the sensitivity to near-surface NO₂ depends on the amount of scattering. Specifically, in the quoted Wang et al. (2026) paper it is written: “However, due to the lack of explicit fire-related a priori information in current satellite NO₂ retrieval algorithms, the resulting data products exhibit large uncertainties under fire conditions.”, and “Retrieved tropospheric NO₂ VCDs increase by up to 100 % at locations greatly impacted by fires and by about 80 % in surrounding areas. “. I am a bit surprised that this source of error is not discussed at all. These errors will both affect the plume mask and the MDR. For CO TROPOMI, errors related to fire scenes are likely smaller, because of the vertically flat averaging kernel and limited effects of aerosol on the CO retrieval.

We thank R1 for pointing this out. Indeed, the effect of scattering, particularly on the TROPOMI NO₂ fields, should be addressed. This is now done in Sect. S1.2 of the new Supplementary information. In the revision, we discuss previous findings and conduct a sensitivity study how errors propagate into our results. Overall, our tests indicate that

moderate perturbations of the NO₂ VCDs have little systematic effect on the retrieved CO emission rates, although the response of individual cases becomes more variable as the NO₂ perturbation increases. The MDRs are more directly sensitive to NO₂ retrieval uncertainties, since NO₂ enters explicitly in their calculation. We have also added references to these tests in the revised manuscript (L392-400 and L559-560).

2. Plume masking algorithm

The main issue in plume masking is to determine the CO and NO_x added to the background. Thus, a background is proposed (section 4.1.1). However, in the case of overlapping plumes, the background might be influenced by upwind plumes (which is OK, because you want to analyse the amount that is added by the new plume). Although this becomes clear from the paper (e.g. figure 5) and the supplement, it would be worthwhile to explain this better in the manuscript. Moreover, in case of an upwind plume, the size of the background (0.8 degree by 0.6 degree) will encapsulate likely only part of the upwind plume and hence lead to uncertainty. Since the “synergy” methods is central to the paper, it would be helpful to present a more elaborate error analysis for background selection (now figure 3 presents a plume-free background, while figure 5 presents a more complicated case). Concerning figure 5 and potential biases, it remains a bit unclear how the “total” result from the “standard threshold” method differs from the newly developed “synergy” method. Mentioning these numbers would be valuable information.

In the Supplement additional information is provided. However, the “standard”, “threshold”, and “synergy” methods are not very clearly explained. It would be good to start with the “standard threshold” method (e.g. used for NO₂) and then explain how the synergy method applies the NO₂ mask to CO satellite data, i.e. define the “standard threshold” method as standard.

We agree, in the original text (manuscript and Supplementary) we used “standard method” and “threshold method” while in fact we were referring to the same plume masking method. The new manuscript and Supplementary text have now been corrected so only two terms are used for the plume masking methods: (1) standard threshold method and (2) synergy method.

Regarding sensitivity tests performed on the background selection, as R1 mentioned, in Sect. S1.1.3 of the original supplement draft, we compared CO emission estimates inferred after applying the reference upwind background method, against estimates calculated using a fixed regional background. To assess the background selection even further as suggested by anonymous referee 2, Sect. S1.4 of the new Supplement also includes an additional sensitivity test of the retrieved CO emission rates based on the size chosen for the background region. This is now added in L380-384 of the new manuscript: “The sensitivity of our results to background selection was also investigated (Sect. S1.4). Changing the size of the upwind background region produced little systematic effect on the retrieved CO emission rates, whereas the use of a fixed pre-fire regional background resulted in systematically higher estimates (~39%). The latter reflects the elevated regional CO background during the multi-fire episode and supports the use of a scene-specific upwind background for the conditions investigated herein”

3. Plume chemistry

CO is relatively long lived, and emissions are estimated using the IME method. For NO_x the application of the IME method is questionable. Apart from the NO (emitted) to NO₂

conversion, also the lifetime of NO_x (~ hours) might bias the IME method. In line 536 “plume chemistry” is explicitly mentioned, so it is important to quantify potential errors due to (lack of) chemistry.

We clarify that, in our study, the IME method is applied exclusively to CO. Thus, there are not reported NO₂ or NO_x emission rates derived from IME. Nevertheless, we agree that NO_x chemistry could potentially affect the interpretation of the NO₂-derived NO_x/CO MDR. A sensitivity test on the MDR is now included in Sect. S3 of the new Supplement. The results suggest that the finite NO_x lifetime does not introduce a systematic bias in the MDR values reported in our work. In the new manuscript we have now added (L554-559): “Similarly, restricting the MDR calculation to the pixels closer to the hot spot (i.e, fresh plume) gives a slope close to unity relative to the full-plume calculation, indicating no significant systematic effect of plume ageing at the spatial scales considered here.”

Minor comments on the manuscript's PDF:

- L69 and L72 "van der Werf et al., 2025" has been corrected.
- In L74 “atmospheric chemistry” has been also added as uncertainty factor for observation-based approaches.
- L84: I think the CO emissions are always higher (hence the unit mol/mol for MDR), but indeed the flaming fires emit relatively more NO_x compared to smouldering fires. So the relative is important: maybe good to give emission factors as an example is this stage.

We agree and have revised the sentence to emphasize that flaming combustion emits relatively more NO_x with respect to CO. We note that the flaming–smoldering balance depends not only on fuel type but also on the combustion phase and temporal evolution of the fire. Since the discussion at that point of the manuscript refers mainly to changes during the lifetime of an individual fire, rather than to differences among fuel-specific emission factors, we have clarified the wording without introducing specific emission-factor values here (L86-87 of the new manuscript).

- In L89 “independently” has been corrected.
- In L97 “387 897 ha” has been changed by “0.388 Mha”. Similarly done in L98 and L99.
- L99: The sum of these numbers is higher than the 387 897 quoted above which is confusing.

The first number shown (0.388 Mha) in the original manuscript corresponds to the area burned studied in this work, while the numbers of 0.334 and 0.219Mha correspond to the total area burned within August 2025 in Spain and Portugal, respectively. The sentence has been modified (L99-101 of the new manuscript).

- L103, “Lands” has been corrected.
- Fig. 1: Confusing: 15 areas with different points? Total 46?

We have now clarified this in the new manuscript by adding L102-105.

- Fig. 2: Points area outside the gray period?

The gray line in the figure corresponds with the start and end date reported by EFFIS supervised MODIS and Sentinel-2. Note that, as detailed in their documentation (<https://effis-gwis-cms.s3-eu-west-1.amazonaws.com/apps/effis.viewer/userguide.pdf>, page 23): “The dates reported as Start date and Last update may not correspond to the date of ignition and extinction of the fire. “. Thus, the grey line in Fig. 2 should be taken as an approximation. For instance,

we found that for that specific wildfire (ID 8) the NRT data reports a period expanding from August 10th until 26th, while the supervised data reports the period shown in Fig. 2 (14th August). Although the NRT data contains the fire points, for consistency we show only the supervised data. Note however that this case was the only one we found with this considerable disagreement between NRT and supervised data.

In L114 of the new manuscript we have added: “It should be noted, however, that these dates should be only regarded as indicative of start and end dates of active fires (EFFIS, 2020).”. Thus, we have also included the EFFIS (2020) reference to the bibliography. We have also included “approximate” in the caption of Fig. 2.

- Fig. 2: I count 44 “dots” (not 46?).

The reviewer is right. The reason is that for two different hotspots (ID #10 and #12), there were two overpasses for August 14th. This possibility is now clarified in Fig. 2 caption.

- L155: not correct representation of units? This seems not the standard to me

All units have been changed from e.g. “g/day” to “g day⁻¹” format throughout the manuscript and supplement, including figures.

- L155: Below you discuss uncertainties, but not for GFAS? Why?

Note that GFAS does not seem to provide any uncertainties assessment. This is clarified in the new manuscript in L172.

- L161: I guess units is then g/day per gridbox. At least same unit as GFAS (incl m2)

“per grid cell” has been added (also in Table 2).

- L169: 1x1 km2? Corrected, also in Table 2.
- L192: Is this the case of #8 in figure 2? Yes (now clarified in L209).
- Table 2. Units and temporal resolution.

Units are now clarified. Since units and temporal resolutions are provided in the table, all units have already that temporal resolution (i.e., per day or per hour).

- Fig. 3: The “background” works well for region 3, but will fail for #7 (&8) because plume #3 is located in the background...?

We understand RC1. However, that is not the case. By using the upwind method for the background selection, we subtract possible emissions from fires located upwind of target fire. In the example, when studying the emission from wildfire #7, emissions from wildfire #3 are part of the background region of fire #7 and therefore subtracted from the CO fields from #7. This is now better clarified in L232-235 of the new manuscript.

- L260: Include then in the table above. PBLH now included in Table 2.
- L269: Per grid box? For clarity, “per grid cell” and “per km²” have now been added for GFED and FINN units respectively.
- L273: What do you mean by densities? And “retrieve”? Calculate?

It has now been corrected “GFAS provides emissions per area”.

- L279: Yes, so the only difference is that units are ‘per grid cell’, while GFAS gives per m2, but to not make this to big!”

We considered useful to distinguish equations for different inventories to give a full comprehension of our methodology.

- L326: look at the recommendations how to present units. [Corrected](#)
- Fig. 5c: Here it seems that part of plume in panel (b) is in the background

As mentioned above, that is indeed to goal of our chosen upwind method for background subtraction of each fire (i.e., to account for possible contributions from fires upwind).

- Caption of Fig. 6: Maybe good to mention that these numbers refer to the ‘synergy’ method”

In L315 of the original manuscript, we mentioned that all the CO estimations reported in the main manuscript were computed using the synergy method. We have now clarified this better in L342 of the new manuscript.

- L381: two times the (double as high is incorrect I think). [Corrected](#).
- L394: maybe some reference to Table 3 would help the reader to see some of the unlying causes.

Thank you for the suggestion. We have added L450 int the new manuscript.

- L400: “Although maybe not the right word here? Next to the moderate correlation, there is also a bias? [Corrected](#)
- L409: GFFEPS acronym. Now “Global Forest Fire Emissions Prediction System” has been added as definition to GFFPS acronym (L471). Similarly, to assess RC1 in L424 of the original manuscript, in L492-493 of the new manuscript we have added Quick Fire Emission Database (QFED) and Global Biomass Burning Emission Product (GBBEP).
- L413: Indeed the 13:30 overpass time of TROPOMI came directly to my mind! This is maybe the "most active period" for fires, while inventories report "diurnal averages" (actually, do they? please add information in the relevant sections).

Indeed, the temporal resolution of the inventories used in our work are daily means (as indicated in Table 2 of the original manuscript). That is the reason behind our comment (L414 of the original manuscript) “Although their specific explanation referred to GFFEPS, the broader issue is also relevant to this work, since TROPOMI estimates are based on conditions close to the overpass, whereas the inventories used herein provide daily emissions or temporally distributed estimates that must be related to the overpass time for direct comparison.” Please, refer also to our response to the major comment 2 (temporal sampling) of anonymous referee 2, where we clarify that the TROPOMI estimates should be interpreted as overpass-centred, short-timescale emission estimates, whereas the inventories provide daily emission estimates. See also L477-484 of the revised manuscript.

- Title of Fig 11. [The figure title has been changed.](#)
- L491-492: The maximum in the TROPOMI derived NO_x/CO is however only 20: so what is the value of this statement?

We agree that the original wording could be misleading. We did not intend to suggest that the maximum inventory MDR is too low relative to TROPOMI. We meant that the inventories MDR seem to show an upper limit. We have now deleted “Fig. 12 also shows an upper plateau in the inventory ratios, even for FINN and GFED. Several fires reach similar maximum inventory NO_x/CO values, whereas the TROPOMI-derived ratios vary more continuously. This behaviour likely contributes to the moderate correlations and suggests that the upper range of biome- or fire-type-specific emission ratios used in the

inventories may be too constrained or not fully representative of the observed combustion variability.”. Instead, we have added L699-701 on the new manuscript.

- Titles in Fig. 13: POI? POI (point of interest) has been changed to “hotspot ID” (Table 1).
- L526: From the CO emissions (in panels (a) and (b)) it seems that magnitudes differ specifically during peak events

We agree that the largest differences in absolute CO emission rates seem to occur mainly during the peak emissions, as already discussed in the original manuscript in the preceding paragraph (L510-515) for both Jarilla and Arganil. We did not intend to attribute the lower TROPOMI-derived MDR specifically to enhanced smoldering combustion (i.e., relatively higher CO than NO_x), but rather to mention this as one possible interpretation. We have now clarified this in the new manuscript L565-567.

- L527: Well, this sounds like a long stretch! If you claim this (certainly interesting), you should try to obtain information on the fire-fighting activity in this period. Now it sounds like an ad-hoc argument.

We agree that the original wording was speculative. Our intention was to point out that the evolution of large wildfires is not controlled only by fuel characteristics and meteorological conditions, but can also be affected by firefighting intervention, a factor that is not explicitly represented in the emission inventories considered here. However, we agree that linking firefighting activity specifically to the lower MDR observed around the TROPOMI overpass would require event-resolved information on the timing and intensity of those operations, which is not available in the present study. We have therefore removed this specific phrase and added L605-610 on the new draft: “Firefighting intervention may also influence the temporal evolution and combustion conditions of individual fires. Such activities are not explicitly represented in the emission inventories used in this work, although their effects may be indirectly reflected in the satellite fire observations on which those inventories rely. In contrast, the atmospheric columns observed by TROPOMI at the overpass time integrate the actual state of the plume and therefore may also reflect any preceding influence of firefighting activities on the fire emissions. However, the effect of firefighting activities cannot be evaluated with the information available.”

- L536 (plume chemistry): this I have not seen being analysed...what would be the effect on plume chemistry on NO_x/CO MDR?.

Regarding plume chemistry, the effect of the lifetime of NO_x on MDRs has now been assessed. Please refer to our responses to your major comment 3 and to Sect. S3 of the new supplement material. See also L545-560 of the revised draft.

- L552: with reference to figure 13 it is fair to remark that specifically the inventories seem to miss peak events

Thank you for the suggestion. L634 is now added the new manuscript.

- L583: For NO₂ there is an issue with the averaging kernel. There might be a bias because the AK used is not fire-aware (i.e. has a too low NO₂ abundance in the prior profile). Thus, in reality, NO₂ columns might be higher.

We agree, each gas may have independent retrieval issues. The sentence has now been modified in L668 to “...but also to analyse complex emission scenes by taking advantage of their different chemical lifetimes and complementary spatial information.”. Please refer also to our response to your major comment 1.

Minor comments on the Supplementary's PDF:

- Sect. 1.1.1: Here uncertainty in TROPOMI should maybe be larger (aerosol, NO₂ AMF, ..)

Now investigated in the new supplementary information (Sect. S1.2)

- Sect. 1.1.1: PBL to PBLH. [Corrected](#)
- Sect. 1.1.3: "We compute the average VCDs for the whole region..."?? [In the new version of the Supplementary \(Sect. S1.4\), we have now described better that sensitivity test.](#)
- Fig. S2: Here, some more context is needed. The "orange" points are quite a bit higher than the "mean" green dashed line. So, logically, the enhancements are larger. But the "orange" points are often also higher than the "blue" mean values on that day. Does this mean that plumes are indeed "stacked" and that the background gets high due to one fire and then another fire adds CO and NO_x to it?

[We clarify that the orange crosses in Fig. S2 of the original Supplementary text \(Fig. S3 in the revised one\) are background CO VCD values, not plume enhancements. As mentioned above, that Section S1.4 of the new supplement has been modified to explain better our results.](#)

- Sect. 1.1.4: "we used the nearest wind field from ERA5 to the hotspot". wind field nearest to the hot spot. [Corrected](#)
- Sect. 1.2.1: the threshold (what is exactly the threshold method??). I tend to loose track here: I read (1) threshold method, the standard method, and the synergy method. In the end to differences are not 100% clear after reading this.

[Please, refer to our response to your major comment 2.](#)

- Sect. 1.2.2: "make easy to isolate" to "facilitate easy isolation of the plume". [Corrected.](#)
- Sect. 1.2.2: "lower CO emission rate values (10%)". Is this due to a lower background? Or due to slightly different plume areas?

[As shown in Fig. S7 of the original Supplementary material, the synergy method generally selects fewer plume pixels than the standard threshold method because the synergy plume mask is constrained by the shorter-lived NO₂ field. Now clarified in L190-192 of the revised Supplementary information.](#)

- Sect. 1.2.2: "is expected closer" Sharper gradients are expected for NO₂ close to the hotspots. [Corrected, as well as the other corrections suggested and typos found by R1 in that section.](#)
- Sect. 1.2.2: "NO₂ density" It would be instructive to also show the NO₂ plume (i.e. the red in panel (b)?). [Included \(now it is in Fig. S9 of the revised Supplementary information\).](#)