

Dear Anonymous Referee #1,

We would like to thank the referee for his/her constructive comments and recommendations on our manuscript. We have done few major revisions on the manuscript. Our replies to the comments are given below. The original comments of the referee are numbered, given in black and the answers are given in blue. The adjustments in the revised manuscript are specified in detail below where the page and line numbers refer to the revised manuscript.

### Software changes APE v1.0 – APE v1.1:

- (a) We have shortened plume length from 40km to 25km. (Filter PD-1 in manuscript)
  - a. Lead to increase in detected plumes from APE V1
- (b) Some minor bugs fixes and re-wrote few section of code. No effect on results.

### Comments

1. “One main comment is that the derived emissions are not compared to any other CO emissions estimates, so it is difficult to judge how good these are. I understand that this could perhaps be the subject of a separate paper, but this is not mentioned by the authors. I would suggest that the authors prepare supplementary material with the estimated CO emissions e.g., in the form of a spreadsheet so that the values could be compared with other emission values by others.”

#### Changed:

- a. Added to the manuscript in the results section, see Lines 339-341
  - b. We have added the suggested content to the manuscript as a reference [doi:10.5281/zenodo.7728874](https://doi.org/10.5281/zenodo.7728874). Additionally, the referee can view the plumes mentioned in the paper in <https://emhelium.users.earthengine.app/view/firespaper>
2. “p3: You list 4 methods but discuss only three. Please discuss the missing one: IME.”
    - a. Changed: We have added details on IME to the revised manuscript (Line 60).
  3. the choice of  $r_{max}=4km$  is puzzling. Will it not automatically discard the megafires from the analysis? Maybe this should be emphasized in the text?
    - a. Changed: The focus of the current version of APE is to quantify emissions of isolated hot-spot fires. This aspect is stressed now at several places in the manuscript (Line 74, 175-176, and 406) So, if mega fires mean spatially extended fires, these are not addressed with the presented version of the algorithm. However, strongly localized fires are addressed.
  4. Figure2. I don't understand this figure. What is the message? Is this supposed to be good?
    - a. Changed: We removed the figure. It causes more confusion than expected.
  5. Many fire-counts are not considered by DBSCAN. Why? Because there are less fire counts than  $n_{min}$ ?
    - a. No Changes: Yes, there are fewer fire counts than  $n_{min}$ . Results of this work in Table 1 also shows that even more than 10 fire counts do not lead to an atmospheric signal that can be detected in TROPOMI.
  6. The selection of fires does not consider any criterion on the fire intensity (FRP). Why (not)?

- a. No Changes: Yes. The assumption is FRPs might be low for a fire which is at its end. And the old fires might have strong plume signal. We need to investigate this further; we have planned future work on this subject.
7. Also noticeable are the fire counts over sea. To what these pixels correspond?
  - a. Not changed: Figure 2 is already removed. The reason for counts of sea is probably due to refineries or some burning events, whereas we cannot exclude false flags.
8. Figure 4 shows a relatively isolated CO plume but how is the plume detection working for the other plumes close to each other?

No changes: We think this is a misunderstanding. Plumes can and are detected even if they are next to each other. This is not a limitation of the data yield. However, if plumes are too close data will be rejected due to multiple fire sources because of difficulties to determine the atmospheric background (Filter PD-2 in the manuscript).
9. p7, l 142: What is a 'connected region'. What is the CO VCD criterion related to this?
  - a. Changed: Added an explanation in the manuscript. Lines 155-158.
  - b. CO VCD is not related to this as the detected plume is only used to draw plume lines.
10. Section 2: section 2.3.1 :- Is the re-centering needed? Or is to facilitate the Gaussian fit? Please clarify
  - a. Changed: Yes, it is to facilitate gaussian fit and has been added to the manuscript. Lines 206-207.
11. p11 l 219: what is the name of the model used for the simulations? Is it defined somewhere?
  - a. Changed: The model is a Euler forward model and an appropriate reference is added. Lines 239-240.
12. The authors attempt to account for wind variability in the horizontal and vertical dimensions. However, there is an additional flux term due to the partial derivative of the wind which is not accounted for (see the divergence method of Beirle et al., Sc. Adv, 2019). Can you quantify this?

No Change: Beirle et al.'s method aims to quantify multiple sources that have fixed geo-location and average it temporally. Our focus is on preselecting isolated plumes of single overpasses so that multiple sources are rejected as well as possible prior to the flux inversion.
13. Section 3: P12: Going from 622 to 196 plumes is in a way disappointing. Does that mean that only ~1/3 of the fires made a meaningful CO signal in the TROPOMI data? Please elaborate.

Changed: Details on how plumes get filtered are now added to the manuscript. Sec 3, P12, L283-287. This happens due to three reasons: Some data will have no meaningful signal because of the TROPOMI detection limit, short plumes that cannot be interpreted with CFM, and plumes have multiple fire clusters in them. See Table B3 in appendix for details.
14. The discussion on errors should be expanded. The error characterization based on standard error (Eq.5) does not account for any systematic error and mixes random errors and real CO flux variability, so it is not a very good metric. I would propose including a table summarizing all error sources and estimating them.
  - a. Partly changed: Our philosophy is to use the standard error to characterize errors that this quantity is sensitive. In addition, we discuss other error sources like TROPOMI CO biases, injection height, and wind speed. The section is revised and hopefully also improved.

- b. Yes, we agree. We changed the Uncertainty estimation section. See Sec 3.2.
15. Section 3.1.1: Generally, zlag seems higher than zc which is in contradiction with Fig 7b. It is confusing. Perhaps it is due to an unfortunate choice in the illustration?
- Changed: Yes, an unfortunate choice in the illustration. Added a new Fig. Now it is Fig 5b in revised manuscript.
16. P13, l273: the author states: "a relation between plume height rise and these two variables can be expected as higher FRP means higher temperature which heats up the air, leading rise of the warm air." However, this process of self-heating is likely not accounted for in the Lagrangian modeling. In fact, the approach presented here is in fact limited to a certain range of fires not too low (because of the limit of detection of the satellites) and not too big (because self-heating and other non-linear processes are not well represented). Therefore, Fig 9b is misleading. The differences are very small, but it does not mean  $E_c$  is good because the  $E_{lag}$  is not representing all the physics.
- a. Partly changed: Yes, that is true. And we do not include heating in Lagrangian simulations, but we assume that the ERA5 velocity fields incorporate this heating effect as ERA5 assimilates the skin surface temperatures from satellites. And this has been added to manuscript (Lines 248-249)
- b. Including heating in Lagrangian simulations is considered to be future work.
17. -P4, l91: 'Mostly, an emission plume created by a burning' ->'Essentially, a plume emitted by a fire'. The sentence states that a fire in a single VIIRS pixel cannot be detected by TROPOMI. Why not? On what is based such statement?
- No Changes: What we meant here is that it is difficult for a single pixel (0.14 sq km) to create a plume spanning a few TROPOMI pixels. The limitation is the detection limit of TROPOMI because of the pixel size and precision of the measurement. We observed 5562 fire clusters (at least 10 fire counts) and good TROPOMI data is only observed for 1327 fire clusters because of this limitation.
18. -p18, l370: 'reliable' is subjective. You don't have any way to assess whether it is more reliable or not.
- Changed: We agree on this. This text has been rewritten.
19. P18, l384-387: What about overlapping plumes from different fires? Isn't there a way to improve on this?
- No Changes: We believe that something like Beirle et al. method is possible. Our approach was first to get something in place that works in an automatic way for the 'simpler' cases and to improve the approach later if possible. So, we foresee this for the next iteration of the APE algorithm.
20. TYPOS/text Suggestions
- a. -acronyms are sometimes defined multiple times. Please define acronyms only once.
- i. Changed.
- b. -Both acronyms 'Tropomi' and 'TROPOMI' are used in the text. Please use one or the other throughout the text.
- i. Changed.
- c. -several subplots /maps have no units. Please define the units for all figures.

- i. Changed.
- d. -several figures or subplots would be better placed in the supplementary material: Figs 7c-e, Fig 8.
  - i. Changed: 7c-e are removed. Fig 8 is still left in the manuscript. Now Fig 6.
- e. -P2, l27: 'CO in atmosphere and Shi et al.' -> 'CO in the atmosphere. Shi et al.'
  - i. Changed.
- f. -P2, l30: 'has been on increase' -> 'has been increasing'
  - i. Changed.
- g. -P2, l37: 'between two measurements' ->'between the two measurements'
  - i. Changed
- h. -P2, l50: refer to the use of VIIRS for methane cloud masking does not help the clarity of the text.
  - i. Changed: Removed from the manuscript.
- i. -P3, l74: 'deliberated'-> 'discussed'
  - i. Changed.
- j. -p4, l107: 'constrained' -> 'restricted'
  - i. Changed: Paragraph has been reworded.
- k. -p6, l129: l129: Gaussian filter : is this a 2D convolution?
  - i. Changed: More details have been added to the manuscript. Lines 142-143.
- l. -Fig5d is not appearing in the manuscript.
  - i. Changed.
- m. -p18, l362: doesn't -> does not
  - i. Changed. During the rewording of the paragraph.