Dear Anonymous Referee #2,

We would like to thank the referee for his/her constructive comments 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 with line numbers referring 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 sections of code. No effect on results.

Major comments:

1. unclear how "fully automated" is this method. Numerous thresholds are applied but theirvalues are not justified. Also: how universal are the numerous threshold values required by the method? How reliable are the results when applied to other locations and times?
   Thresholds were optimized for US and we observe that it generalizes well. So method can be applied without any visual inspection of intermediate data. We have applied the algorithm to different regions and times. Australia (Oct 2019) and Siberia (June-July 2021) analysis in the manuscript. Major changes have been made to the manuscript.
   Changed:
   i. The reasons for the choice of thresholds have been given (Sec. 2)
   ii. Added region encapsulating Siberia for months of June-July 2021 (mainly Sec 3)

2. unclear what percent of all VIIRS fires are detected and their emissions successfully quantified with this method. The quoted 97.9 % success rate seems too high (considering that many plumes seem to be rejected due to different reasons) and is not properly justified.
   This is the wrong perspective in our opinion. We do not want to claim that we can detect all fires detected by VIIRS. What the paper quantifies are the fires that can be observed in TROPOMI CO data with respect to pre-selected VIIRS data.
   Changed:
   - It is important to show the false positive detection by the algorithm and the 97.0 % gives the confidence level of the detected data. This is now phrased properly (Line 297)
   - The reasons for rejection have been explained in detail in the revised manuscript (Lines 276-297)
3. unclear how valid the plume height values and the emission values quoted in the manuscript are, since no attempt was made to validate those with respect to in situ measurements
   - No Changes: Reference to the GFAS data including its uncertainty is given in the manuscript. Sec 2.3.3 and lines 224-228
4. Specific issues: details needed for analysis replication are missing, e.g., filter size, filter formulation.
   - Changed: Filter criteria are explicitly mentioned in the revised manuscript (DD-1, DP-2, PD-1, PD-2 , EE-1 to -4, see Sec. 2)
5. Specific Issues: tools are not described and, thus, become black boxes to the reader.
   - No changes: References to all numerical tools used are given in the manuscript. These are available to the reader. The manuscript describes the main functionality of the tools. We think this is common practice and an appropriate approach. We do not know how else an algorithm description can be provided as the use of libraries is a common practice.
6. 16: "The emissions were severely under-predicted".
   a) With respect to what?
   b) Were there any in situ measurements used to validate the emissions calculated here?
   a) Changed: We have changed the way the results are discussed. (Lines 330-339)
   b) Not changed: To our best knowledge, there are no independent validation measurements (in-situ) available. On one hand, this represents the limitation, on the other hand, demonstrates the novelty of the data product.
7. 22: Please clarify "idealized cases"
   - Changed: Removed during the re-write of the manuscript.
8. 25-26: please provide a reference for "it is a weak greenhouse gas"
   - Changed: "Weak" is replaced by 'indirect' to represent what was intended, reference (Spivakovsky et al., 2000) is given. See line 26.

9. 91-92:
   (a) Could missing fire counts (due to missing VIIRS pixels because of, for example, clouds/smoke) result in fires and, thus, in plumes not being identified by the automated plume detector?
   (b) Some CO plumes may only be detectable downwind from the fire, if clouds/smoke mask the fire, which is not uncommon.
      a) No changes: Yes, that is a possibility.
      b) Yes, some plumes are detected in downwind from the fire. This is one of the reasons why we detect plumes within certain pixels from the fire source pixel (see Lines 168-169).
10. 98: Please correct to "The minimum number […] has been set to n_{min}=10" Also, why 10?
    - Changed: Thresholds like n_{min} are tuned for US fires and verified for Australia and Siberia. (Sec.2.1.2, line 121 and sec 3)
11. 105: a granule would be much larger than 41 x 41 pixels; please consider using "subset" instead of "granule" here as well as in lines 107, 108, 109, 110, 111, 114, 128, and Fig. 3 caption. (A granule would have whatever size is covered by a whole TROPOMI file.)
    - No changes: We believe 'granular data' is data that is in pieces, as small as possible to be more defined and detailed. So, we think it is the right wording.
12. 110: how were the 80% and 85% thresholds selected? Also, please explain the meaning of QA>0.5
    - Changed: The threshold was selected to disentangle the plume signature from the background Added to the manuscript. See lines 116-120.
13. Fig. 2: "fire-counts that were not clustered" was it because there were less than 10 fire counts within a 4 km radius? Please explain
    - Changed: See item 10.
14. Also: why 41x41 pixels? Why 7x7 pixels?
    - Changed: A motivation is added to the manuscript. If a large number of pixels are bad in 41x41 pixels and if they are around the fire source, then no plume will be detected. That is the reason for 7x7 pixels. (liness 111-113)
15. 116: please explain "gold standard data"
    - Changed: Removed and reworded (lines 126-128)
16. 121: "Thus, the watershed algorithm segments the regions into valleys and mountains (CO enhancements) based on a given marker" Valleys suggest low CO regions and mountains high CO regions, i.e., plumes. However, line 125 seems to say that what the algorithm does is to look for low/high boundary zones, i.e., zones of maximum slope change. Please clarify.
    a. Partly changed: The section has been reworded for clarity (Sec 2.2)
    b. The algorithm requires two inputs, gradient map and marker image containing seeds of high and low CO, to segment (see changes in Sec 2.2)
17. How does this method perform compared to simply calculating the background value in a TROPOMI scene and then selecting clusters of pixels above that value?
    - No change: We think the referee underestimates the heterogeneity of the CO background field. Due to the moderately long lifetime of CO, the background for a location is always changing and it can contain emission features from other sources and long range transports. A simple subtration of background does not work.
18. Also, it looks like only one of several plumes in this 41x41 TROPOMI subset is detected, even though several fires are shown in the same 41x41 subset in Fig. 3; please clarify what happened to the other plumes in this subset, including the largest of them all, in both size and CO value: did the algorithm identify all of them or just one of them?
    - Changed: We understand from this and from few follow-up comments below that our explanation is not clear. Thus, we have done necessary additions to the manuscript. Furthermore, a pseudo-code of APE algorithm (Appendix B) is added to clarifies the idea further. Also have explicitly mentioned this in lines 124-125.
    - Each extracted 41x41 granule corresponds only to one fire source.
    - All plumes were detected since they were assigned to different data granules.
19. Fig 4c-4d: plumes detected are much shorter than the actual plumes.
    - Partly changed: Quantification in downwind direction is difficult due to lower enhancements in downwind direction and heterogeneity of the CO background. Therefore, we only consider strong enhancements in the plume. See lines 170-172.
20. 128-162: Please clarify if this example illustrates the process followed to either 1) identify a single plume in the 41x41 TROPOMI subset; the process is then repeated for each of the remaining plumes in the subset or 2) all plumes in the 41x41 TROPOMI subset at once. If 1) is true: please clarify text. If 2) is true: most plumes are missed, please discuss.
    - Changed: See item 18 (above).

21. 129: “First, high frequency components of the CO-image are reduced by a Gaussian filter” Please explain, is that to remove noise? What is the size of the filter, is the size constant for all plumes, how was it selected
    - Changed: This is used to reduce the noise.We added an explanation in manuscript. Lines 141-142.
    - Standard deviation of Gaussian filter $\sigma = 0.5$ (in pixels) (added to manuscript) and yes, the size is constant for all plumes. This was an emperical choice.
22. 130: “the elevation map Ielev is computed using a Sobel operator” Describe with an equation what the Sobel filter does. Also, “elevation” seems incorrect here, since the Sobel filter would highlight zones of maximum change in slope in the input. Consider changing to Isobel or similar.
    - Changed : Referred as I_grad in the manuscript and have included the equations to Sobel opeators. Line 144-147.
23. 134: stating that Imark is initialized with zeroes would suffice, no need for an equation.
    - Changed : done.
24. 136-140: for clarity and simplicity, consider rewording to “[…] clear CO enhancement. Pixel Imark(i,j) is considered CO enhanced (i.e., Imark(i,j)=2) if Is(i,j) is either above the median of Is or above the mean of the 15x15 pixels centered at Is(i,j). Otherwise, Imark(i,j)=1. For our example in Figure 4 [...]” (no equations needed). Also: why 15x15 pixels? Is this size fixed, or does it change from plume to plume?
    - Changed: We have changed the paragraph and reworked on it. See from lines 151.
    - 15 x15 is again empirically chosen to account for background variability. This has been added to manuscript (line 152-155).
    - This remains the same for all plumes.
25. 140-141: the meaning of the last sentence in the paragraph is unclear. It looks like the result of the step that was just described (where Imark is populated with either ones or twos) is illustrated by panel 4d, not 4c. What is panel 4c? How is it relevant? Please comment on the plumes present in 4a and 4b but absent in 4d; one of the absent plumes was the largest of them all, in both size and CO values.
    - Changed: Rewritten for more clarity in the manuscript. Lines from 155.
    - Each plume corresponds to a fire source and is detected separately. See item number 18 for further clarity.
26. 142: how does this new tool work?
    - Changed: ‘label’ algorithm identifies all connected pixels with the same value and creates a region. A description has been added to the manuscript. See lines 156-158.
27. How is this plume detection algorithm better than a simpler approach, such as identifying groups of CO pixels with values above that of the background? It looks like the latter would have sufficed to identify all the plumes in this 41x41 TROPOMI subset, while this plume detection algorithm (at least according to Fig. 4) missed most of them.
    - Changed: See item 17 and 18.
28. 169. (Here and elsewhere in the manuscript) wind velocity from ERA5 data is expressed in the manuscript as “u”. Usually (and that includes the ERA5 dataset) u represents the E-W component of wind; v would represent the N-S component. Is u in the manuscript really the E-W component of the wind? Shouldn’t the wind velocity be calculated according to the plume’s direction? Please clarify
    - Changed: Added new symbol in the manuscript (see equation 2) to get rid of confusion.
29. 175 Please clarify “The plume line results from a second order curve fit through the pixelcenters of the identified pixels”
    - Changed: The vague wording of “identified pixels” to “detected plume” in the manuscript.
    - Plume line is the solid black line in figure 4a, along the plume. See Line 196.
30. 176 Why 2.5 km?
    - Changed: To reduce interpolation errors. Added to the manuscript. Lines 197-198.
31. 177 Why 500 m?
    - Changed: Oversampled to get smoother CO distribution. Added to the manuscript. Lines 199-200.
32. Fig. 5 caption: c): why missing value at 0 km from the source?
    - No Change : The pixel at the fire source has a qa value < 0.5. Usually, we do find the pixels near the fire have bad quality due to smoke.
    - Changed: Figure has been changed. Figure 4 in revised manuscript.
33. 182. Please explain what are the terms H0, H1, and A0
    - Changed: H0, H1 refer to background and A0 to intensity. Added in the manuscript, Line 213.
34. 203. Fig. 6 shows two distinct plumes approximately 100 km long each, resulting from two fires 50 km apart. According to the text, both plumes were rejected by this algorithm because they were too close to each other. How close is too close? What’s the minimum plume size detectable with this method? These and other limitations of the method presented in this manuscript should be discussed both in the abstract and in the conclusions sections.

- Not Changed: 'Close' cannot be quantified in distance but in terms of a too heterogeneous background. This is done in the manuscript. Section 2.3.2.

35. What are the dashed lines in Fig. 6?
    - Changed: Figure removed. They represented transects.
36. 207. Please explain briefly why is the uncertainty in injection height about 500 m.
    - Partly Changed: We quote Sofiev et al. (2012), who showed how the IS4FIRES injection height deviations from MISR Plume Height Project (MPHP). Has been added to the manuscript. Lines 228-230.
37. 224. Why 6 hours?
    - Changed: This is chosen based on diurnal cycles. Added explanation to the manuscript. Lines 248-249.
38. Fig. 7 b) values at distance=0, 2.5, and 5 km from the source (i.e., at the fire source and close to it) are missing; please explain. The text states elsewhere (e.g., l. 366) that plumes go higher away from the source but the opposite behavior is shown here.
    - Changed: See item 32.
    - Figure 7 has changed in the manuscript as it is an unfortunate choice in the illustration. Now, Figure 5 in revised manuscript.
39. 234-245. Please quantify what proportion of plumes are rejected due to: lack of GFAS injection height, disagreement between Lagrangian particles flow direction and actual plume direction, wind velocity below 2 m/s.
    - Changed: Details added to the manuscript. See section 3 and Appendix B shows all the rejected cases in detail.
40. 260: "To conclude, presented automated algorithm can successfully detect plumes and compute emissions for ≈ 97.9% of the cases." It looks like the percentage of plumes detected is much lower that that. How was this figure calculated? A few sentences earlier the text says "the plume detection algorithm [...] identified 196 plumes among 622 cases" and lists numerous cases which were not successfully processed due to a number of reasons. The detection rate quoted does not seem feasible, unless relevant qualifiers are missing.
    - Major changes: Section 3 has been rewritten. Now we describe how data are filtered in detail.
    - We agree with referee and we now speak of "confidence level" of the APE algorithm.
41. 264 How is it decided what is the number of transects along the downwind direction to beconsidered? Does this number change from plume to plume, or is it universal?
    - No changes: Number of transects depend upon the plume size and does change from plume to plume.
42. 3.1.1. As expected, emissions calculated using plume height Zlag and Zc differ when the heights themselves differ. Unclear if results vary from Australia to USA; thus, please consider using the same symbol/color for data points from both locations. Unclear if all 4 panels are relevant; some seem redundant. Consider showing one panel with height difference (between zlag and zc) versus emissions difference and another panel with height difference (between zlag and fix z=100 m) versus emissions difference.
    - Changed: We have made new figures and added new data from Siberia. Additionally, text in manuscript has been updated. See Sec 3 and Sec. 3.1.
43. 277-287. Unclear where the discussion is going until the last sentence "although the overall effect of the Lagrangian estimate of the plume height on the emission estimate is minor, we could identify several cases where the emissions estimate become more reliable." Consider starting the paragraph with this sentence and add a very brief description of relevant data.
    - Changed: We have re-organized the manuscript and this has been included. From line 319.
44. 295. 10% change in emissions seems to be much smaller than some of the emission uncertainties discussed later on (e.g., l. 344-345). Also, a 10% variation in emissions was qualified as "minor" elsewhere (l. 19). Please discuss.
    - Changed: We now motivate that it is difficult to scale 100m winds to compute emissions at plume heights (injection and varying plume height). This part of manuscript has been re-written. Paragraph starting at line 332.
45. 325. "It should be noted that this uncertainty has been reduced to 3.4ppb in the newer versions of L2 product". How much is that in percent value?
    - No Change: Depends on background concentration and means typically 3-10 %. We do not want to use relative errors in the manuscript.
46. 328. Please clarify "as the pixel size of TROPOMI is high".
    - Changed: Removed in the re-write. Here intent was to understand the changes in emission with change in resolution. And in that context it is referred as large pixel sizes compaed 1km pixels.
47. 339. Table 2: please include percent differences.
    - Changed: Also it is now Table 3.

48. 351. How universal is this method? It seems to have many steps requiring thresholds which seem to have been selected based on specific examples. Would the same thresholds result in the desired results if the method was applied to other regions, other time periods?
    - Changed: APE generalize well as demonstrated for the Australia and Siberia case. See also different periods of the ensembles. See Sec 2 for filters and Sec 3 for emission estimates.
49. 355. Please clarify: 97.9% of what? Many plumes were rejected based on proximity to other plumes, lack of injection height data, … Such high percentage seems off.
    - Changed: See Item 40. We now speak of confidence level of the detected plumes.

Typos, Minor/Grammatical comments

50. line 5: please explain "APE"
    - Changed.
51. 7: please explain "VIIRS"
    - Changed.
52. 10: "IS4FIRES"?
    - Changed.
53. 15: should month names be spelled without abbreviation?
    - No. Changed accordingly.
54. 30: "CO emissions due fossil fuel burning has been on increase", consider rewording to "CO emissions due to fossil fuel burning have increased"
    - Done.
55. 32-33: please consider rewording "Thus, it becomes essential to understand the effect of CO on air-quality and climate by measuring it accurately on a global and local scales which helps to quantify CO emissions" for readability
    - Done.
56. 65: please correct "using the the wind"
    - Done.
57. 74: since the present tense was consistently used before (lines 71-74), please consider rewording to "The study results are deliberated in Section 3". Consider using "discussed" instead of "deliberated".
    - Done.
58. 80: "extracts TROPOMI CO data"
    - Done.
59. 87-88: please explain acronyms as they are introduced
    - Changed in the whole manuscript.
60. 90: Should "Furthermore" be "From now on" or similar?
    - Changed.
61. 92: also 5.5 x 7 km^2, since all but one of the cases analyzed here postdate August 2019.
    - True. Now included in manuscript.
62. 95: "low density areas"?
    - Yes, changed.
63. 104: "for part of data granule S5P_OFFL_L2__CO_____??? over Australia"
    - We have added Orbit number in the manuscript. It is also a unique identifier along with the product version. Line 109.
64. Fig. 4: the map shown in panel (b) seems to display zones of maximum slope change or, as the text states, zones of "gradient" change. However, panel (b) is labeled "Elevation map", which does not seem very appropriate. Please reword.
    - Reworded in the manuscript.
65. Changed 125: Ielev does not show a continuous variable (like elevation, or CO value) but it rather shows where the maximum change in that variable occurs. Consider renaming it to Iedge or similar.
    - Changed : To I_grad in manuscript. See line 137.
66. 149: Where does "14" come from? Eq. 3 is not needed, since it does not add to what's already in the text.
    - Changed: Removed.
67. 164. Please provide reference for the cross-sectional flux method.
    - Changed: Added to manuscript. See line 183.
68. Fig. 5 caption: please correct typo, "transaction" should probably be "transect"
    - Changed.
69. 197, 198. "remove overlapping fires". Fires like those shown in Fig, 6 are not overlapping, both the fire sources and the plumes appear quite distinct. Consider rewording "overlapping" by "closer than … km".
    - No changes. See item 34.

70. 220. does “on the right-hand side” refer to equation 8? If yes, then consider rewording to “The velocity v” or similar. If not, please explain what does it refer to.
    - Changed as suggested and referred to the equation (see line 242).
71. 244. Please clarify if “the velocity at the TROPOMI measurement time” refers to wind velocity.
    - Changed: Yes. Added to manuscript Line 261.
72. 265 Is “w.r.t” acceptable in a manuscript?
    - No. Changed.
73. Fig. 9. The blue crosses and blue dots are too similar to tell them apart. Consider using other symbols or separate colors instead.
    - Changed.
74. 332. “TROPOMI”
    - Changed.
75. 363. 22 out of how many cases? Alternatively, please provide a percent value. Otherwise, “22” alone is not informative.
    - Changed.