



EGUsphere, referee comment RC1
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Comment on egusphere-2022-1211

Anonymous Referee #1

Referee comment on "Plume detection and emission estimate for biomass burning plumes from TROPOMI carbon monoxide observations using APE v1.1" by Manu Goudar et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-1211-RC1>, 2023

Review of Goudar et al. Plume detection and estimate emissions for biomass burning plumes from TROPOMI Carbon monoxide observations using APE v1.0" **submitted to GMD.**

Goudar et al. presented an automated way to determine CO emissions from fires using a combination of observational data and modeling. This is an interesting first study and the authors demonstrated well the potential of this approach although there are several limitations to this approach. The paper is sometimes hard to follow and could be improved for its structure and writing (especially in the introduced). It should be published in GMD, after few (mostly minor) corrections.

Comments

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.

Introduction

-p3: You list 4 methods but discuss only three. Please discuss the missing one: IME.

Section 2

-section 2.1.1: the choice of $r_{\max}=4\text{km}$ is puzzling. Will it not automatically discard the megafires from the analysis? Maybe this should be emphasized in the text?

-Figure 2. I don't understand this figure. What is the message? Is this supposed to be good? Many fire-counts are not considered by DBSCAN. Why? Because there are less fire counts than n_{\min} ? The selection of fires does not consider any criterion on the fire intensity (FRP). Why (not)? Also noticeable are the fire counts over sea. To what these pixels correspond?

-Figure 4 shows a relatively isolated CO plume but how is the plume detection working for the other plumes close to each other?

-p7, l 142: What is a 'connected region'. What is the CO VCD criterion related to this?

-section 2.3.1: Is the re-centering needed? Or is to facilitate the Gaussian fit? Please clarify

-p11 l219: what is the name of the model used for the simulations? Is it defined somewhere?

-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?

Section 3

-P12: Going from 622 to 196 plumes is in a way disappointing. Does that mean that only $\sim 1/3$ of the fires made a meaningful CO signal in the TROPOMI data? Please elaborate.

-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.

-Section 3.1.1: Generally, z_{lag} seems higher than z_c which is in contradiction with Fig 7b. It is confusing. Perhaps it is due to an unfortunate choice in the illustration?

-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.

Typos/text suggestions

-acronyms are sometimes defined multiple times. Please define acronyms only once.

-Both acronyms 'Tropomi' and 'TROPOMI' are used in the text. Please use one or the other throughout the text.

-several subplots /maps have no units. Please define the units for all figures.

-several figures or subplots would be better placed in the supplementary material: Figs 7c-e, Fig 8.

-P2, l27: 'CO in atmosphere and Shi et al.' -> 'CO in the atmosphere. Shi et al.'

-P2, l30: 'has been on increase' -> 'has been increasing'

-P2, l37: 'between two measurements' -> 'between the two measurements'

-P2, l50: refer to the use of VIIRS for methane cloud masking doesn't help the clarity of the text.

-P3, l74: 'deliberated'-> 'discussed'

-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?

-p4, l107: 'constrained' -> 'restricted'

-p6, l129: l129: Gaussian filter : is this a 2D convolution?

-Fig5d is not appearing in the manuscript.

-p18, l362: doesn't -> does not

-p18, l370: 'reliable' is subjective. You don't have any way to assess whether it is more reliable or not.

P18, l384-387: What about overlapping plumes from different fires? Isn't there a way to improve on this?