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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Devastating Californian wildfires in November 2018 observed from space: the carbon monoxide perspective" *by* Oliver Schneising et al.

Anonymous Referee #1

Received and published: 22 February 2019

The paper by Schneising et al. reports on the detection of the carbon monoxide (CO) plumes from the 2018 Californian wildfires by the new TROPOMI satellite instrument. The study attempts to derive conclusions for the air quality burden by translating column enhancements into concentrations under the assumption that boundary layer height is known and that all measured CO enhancements reside in the boundary layer.

General comments

1. The style of the text is at the edge of what is acceptable for scientific writing.

It uses emotional and judgemental wording, (non-exhaustive) list of examples: title: "devastating"; abstract: "one of the most disastrous months in Californian history",

"destructive wildfires raging", "burnt to cinders"; introduction: "the town of Paradise was wiped out", "an unprecedented instance in history"; conclusion: "The analysed fires were the latest episodes of the deadliest and most destructive wildfire season the state of California has ever faced." Most of these statements can be removed without loss of any information.

Further, the manuscript is very short in making reference to previous work. More references to earlier CO work of the MOPPIT, SCIAMACHY, IASI, TES, AIRS teams are required.

2. What is the scientific value of the paper? The general CO detection capabilities of TROPOMI have been published before [e.g. Borsdorff et al., 2018a,b]. Air quality issues with wild-fire CO emissions are well-known. I would argue that the scientific value is the quantitative estimation of the CO burden (in units mg m-3) based on daily recurrent satellite data i.e. the evaluation of TROPOMI's capabilities for dense CO-related air quality monitoring. Comparison to the CAMS model could also be an added value since it might trigger model improvement. Currently, the methodological evaluation and model comparisons are too short and too vague to serve any of these scientific purposes.

Comments

P4, L11: Please add a discussion on errors coming from the assumptions on boundary layer height knowledge. Discuss how boundary layer height is determined. Please also add figures or tables for typical boundary layer heights. Are boundary layer heights of a few hundred meters (at midday) realistic (P7, L5)? These boundary layer heights need to be validated. If the boundary layer is so shallow, a large fraction of the fire emissions might reach above the boundary layer due to initial thermal rise.

P 4, L27: Please add and discuss a figure showing "the fact that the simultaneously retrieved gases, methane and water vapour, are not considerably increased compared to the pre-fire background abundances."

P4, L30: The CAMS comparison is too short to be of scientific value. Please add a quantitative discussion (e.g. average TROPOMI on CAMS resolution and calculate departures).

P9, L24: None of the statements in the second paragraph of the conclusion are actually conclusions based on the scientific results of the paper, but rather they are author interpretation of climate change impacts.

Figure 1: This figure could be dropped. The total column sensitivity of the solar absorption concept is standard scientific knowledge. Does the algorithm take into account that the near-ground sensitivity might be reduced due to scattering layers such as wildfire particulate plumes (or low clouds)? If not, what is the impact on the air quality derivations – does the satellite "see“ the entire column?

Figures 6 and 7: The figures are too dense with internal information. None of the acronyms (VC, SZA, VZA, dlnI/dx. . .) and few of the terms (sun-normalized radiance) are explained, the panels are too small, some of the panels are not even discussed (Temperature fit, . . .). Recommendation: Either remove the figures entirely or just show the relevant parts e.g. the CO panels.

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