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

Anonymous Referee #2

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Reviewer's Comments on "Devastating Californian wildfires in November 2018 observed from space: the carbon monoxide perspective" by Schneising et al.

General Comments

This manuscript by Schneising et al. presents some interesting observations of satellite-observed CO over California during a recent episode of extensive wildfires along with a brief analysis. However, due to several major limitations of this work, the presented results are truly only useful for qualitative analysis and are therefore of limited scientific interest. For this reason, I can not recommend publication in ACP.

The first issue is the lack of any discussion of the retrieval algorithm or the expected

error characteristics. References are provided to two ESA technical reports, but these appear not to be peer-reviewed, nor publicly available. A detailed presentation of the retrieval algorithm (in the peer-reviewed literature) is essential for establishing the provenance of the TROPOMI-WFMD CO products. While the general aspects of the algorithm might be similar to published algorithms developed for SCIAMACHY, some details will certainly be instrument-specific. Such details are extremely important. Similarly, details of the filtering methods (based on CO fit and water vapor absorption) will also influence the scientific interpretation of the data and should therefore also be discussed fully. (For example, how were the thresholds for CO fit and water vapor absorption determined?)

The second major area of concern is the lack of any proper validation results; the only mention of validation is a reference to an unpublished technical report. The history of satellite remote sensing demonstrates clearly that satellite products can not simply be taken 'at face value'. For satellite CO products, validation should preferably exploit in-situ CO vertical profiles measured from aircraft. If that approach is not feasible for some reason, the authors could either exploit ground-based FTIR CO retrievals or other satellite CO products. (These latter methods are less optimal than in situ-based methods because of issues related to averaging kernels.) Comparisons of satellite CO products with surface measurements of CO concentration are generally inadequate for validation because of the variability of CO in the middle and upper troposphere.

Beyond these two major issues, it is not clear how the TROPOMI-WFMD CO product relates to the TROPOMI-SICOR product (as developed by Borsdorff et al.). Do the two algorithms give the same results? Are there other issues which might make one product preferable over the other? Is the TROPOMI-WFMD CO product routinely generated and publicly available? These are inevitable questions that will concern potential users.

My advice to the authors is to strongly consider writing and submitting a validation paper (to AMT or another appropriate journal) which directly addresses these issues. Such a paper is an essential prerequisite to the quantitative use of satellite CO data.

Publication of that paper would pave the way for this paper and increase its significance.

Specific Comments

In several places, word choice could be improved to be less sensational and more scientific. For example, in the title, 'Devastating' could be 'Severe'. Similarly, the expression 'burnt to cinders' is gratuitous.

p. 2, line 8. Need reference for physiological effects of CO on humans.

p. 2, line 15. The text in this paragraph suggests that MOPITT and IASI CO retrievals are generally insensitive to CO near the surface, but this is overly simplistic. In fact, publications document that both of these instruments can provide useful sensitivity to CO near the surface in daytime scenes over land (i.e., in conditions of high thermal contrast). For example, see "Sensitivity of MOPITT observations to carbon monoxide in the lower troposphere," JGR, 112, doi:10.1029/2007JD008929 (2007) by Deeter et al., and "IASI's sensitivity to near-surface carbon monoxide (CO): Theoretical analyses and retrievals on test cases," JQSRT, 189, doi:10.1016/j.jqsrt.2016.12.022 (2016) by Bauduin et al. MOPITT is also equipped with near-infrared channels which can boost surface-level sensitivity in some scenes.

p. 4, line 1. If scenes with low clouds are tolerated, what effect does that have on the retrieval vertical sensitivity (averaging kernels)?

p. 4, line 13. The assumption that all of the pyrogenic CO remains in the boundary layer is dubious. Can it be assumed that pyroconvection out of the boundary layer does not occur? After the first day or so of burning, it is likely that CO in the boundary layer will start venting into the free troposphere, thus affecting CO concentrations in the free troposphere throughout the region. Finally, there seems to be no consideration of the uncertainty of the boundary layer height.

p. 4, line 28. The evidence that light path lengthening (in the presence of smoke) is insignificant is not compelling. The most credible way to prove this claim would involve

validation results. If the evidence for this claim is based solely on retrieved amounts of methane and water vapor, those results should be presented in the manuscript to allow the reader to judge whether in fact retrievals of those gases 'are not considerably increased compared to the pre-fire background abundances.'

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