

Interactive comment on “APIFLAME v2.0 trace gas and aerosol emissions from biomass burning: application to Portugal during the summer of 2016 and evaluation against satellite observations of CO (IASI) and AOD (MODIS)” by Solène Turquety et al.

Anonymous Referee #1

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This study presents an assessment of the APIFLAM v2.0 biomass burning and emissions model over a case study of forest fires that occurred in Portugal, 2016. It compares the modelled results from burnt area and emissions of Carbon Monoxide and aerosol particle matter (PPM) with different observed datasets and quantifies the uncertainties with a set of sensitivity experiments. There are several major issues with this paper, and major revisions are needed before the manuscript can be published.

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1) The way the manuscript is written, and structure is very confusing, making it hard to read through. I would recommend this to be reviewed by an English native speaker and I would advise restructuring the text in some of the sections in order to improve readability and conclusions.

2) Section 4 shows the methodology for merging burned area with radiative fire power (RDP) data in order to increase the temporal variability which is crucial for estimating emissions for this study. It is shown how the RDP of small fires contributes to an increase of the burnt areas estimates. It would add value to this analysis to include an assessment on how this could impact fire emissions (e.g. using datasets such as GFAS).

3) It is mentioned in L288-289 that for MIDIS IGBP “to limit uncertainties in APIFLAME, fires detected in areas of chaparral or mediterranean vegetation types and classified in shrubland but also savanna are attributed to chaparral”. Further along in L295-298 the authors state that for CLC forcing the biome of temperate forest allows a better agreement between the results of the experiments. Why not use a consistent approach and use the different datasets to understand the sensitivity to uncertainties regarding the land surface categorization?

4) In section 5.4 it is said that Secondary Organic Aerosols (SOA) have a very low fraction and can be discarded as a justification for not accounting for these. However, this is later presented as a limitation of this study affecting the bias of the modelling approach.

5) This study uses climatologies as boundary conditions for the chemistry and aerosol species. This could have a significant impact on the background levels of these species and contribute to the bias presented away from fire regions. Considering this, what would be the impact of using real time boundary conditions in the conclusions here presented? This is only pointed as a limitation of this study.

6) In the paragraph between lines 440 and 457 it is mentioned that regional fire emis-

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sion reports from Alves et al. (2011) are larger than the ones used in this study. Considering that there are specific emission factors in the literature for the study region why was not do an assessment of the sensitivity of these fire emissions compared to the values proposed by other authors and the ones used in this work? Would the conclusions of this study be affected by the choice of the emission factors and land cover database? These are topics that are mentioned in this paper but are not explored, which would add value to this paper.

7) It is stated that if not explained by excessive emission factors, the overestimate of CO concentrations in simulations can be explain by other factors such as the temporal variability of emissions, problems in representing the Planetary Boundary Layer or injection of fire burning plumes. Do these factors have a greater influence than the uncertainty in the plant functional types? This is especially relevant since the plant functional type determines the emission factor and the fuel load available for emissions. Having an analysis of these contribution would significantly increase the value of the results presented here.

8) In section 5.5 it is mentioned that the analysis presented will focus on the increase above background in order to remove the bias in the background CO and PPM. This should be expanded to all the analysis, including surface stations.

9) There is no agreement between the comparison with surface and satellite observations and the reasons presented here to explain the differences diverge. This analysis lacks clarity.

Other comments:

Throughout the whole document the word enhancement is used in the context of increase. Enhancement usually relates to improvements in quality not in increases, of for example fire contributions.

L178-179: It is mentioned that “a matrix of correspondence between the MODIS IGBP

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and the CLC vegetation types is provided.” Does this follow any of the correspondence matrix available in the literature? For example, Pineda, N., Jorba, O., Jorge, J., and Baldasano, J.: Using NOAA AVHRR and SPOT VGT data to estimate surface parameters: application to a mesoscale meteorological model, *Int. J. Remote. Sens.*, 25, 129–143, 2004

L188-L189: It is mentioned that “This option has been developed after strong discrepancies in the daily variability in fire activity” . . . Could you please reference a study providing details on this, or summarise the outcomes? This would allow a better understanding of the improvements given the discrepancies found in this study. L266: The analysis focuses on regional transport (around 100 km away from fires) and not long-range (> 1000 km)

L277-279: The reference to the results for southern France are solely mentioned here and do not add relevant information to this study if this is not used as a comparison between the two case studies further along.

L300: “Considering all experiments, it is equal to 23% on average during the main fire event”. What is equal to 23%? Is it the average coefficient? What information does this provide? The paragraphs around this statement should be re-written in a clearer way.

L368-369 the way this is written implies that fire emissions are also assumed to be more intense during the day and an emission scaling is applied. Is this also applied to fire emissions? Please re-write this paragraph in a clearer way.

L396: The paragraph starting in this line is very confusing to read and mixes two distinct ideas, please reformulate this.

L465-468 Were there any significant dust events prior or during the period of study that corroborate this statement?

Paragraph stating on line 470 suggests that having a better representation of the injection heights is important for the transport and vertical distribution of the emis-

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sions. Would the authors consider adding a comparison with LIDAR data (e.g. from CALIPSO)?

L502-509: Does this mean that due to the lack of sensitivity of the satellite to these levels, this satellite data is not fit for purpose?

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