Reply to Referee Comment 2

by S. Turquety et al.

The co-authors thank the referee for carefully reading the manuscript and for the helpful comments. We have addressed all of the comments (copied in italic letters below to facilitate reading). Corrections to the text are reported in blue.

Major comments

First, some of the wording in the text has to be improved. Some sentences aren't clear..

Many parts of the text have been rewritten, as described in our response to RC1 and to the different comments. In addition, we have modified the structure of the text, more specifically for the analysis of the case study which raised a lot of questions. Section 5 now follows the following outline:

5.1) APIFLAME biomass burning emissions
5.2) Observations of atmospheric concentrations
5.3) CHIMERE-WRF regional CTM
5.4) Sensitivity of the simulated concentrations to the configuration of APIFLAME
5.5) Evaluation against observations
(a) Surface observations
(b) Satellite observations of aerosols (MODIS, CALIOP)
(c) Satellite observations of CO (IASI)

I suggest changing the title of the paper to state that this study isn't only evaluating the BB emissions, but also the CHIMERE model. I know in a number of studies BB emissions were evaluated by using the atmospheric models. However, this gives a false impression that different BB emissions can be evaluated accurately by plugging them into the air quality models. As the authors note, there are many uncertainties in the modeling of the plume injection height, tracer transport and mixing, and atmospheric chemistry in the air quality and atmospheric chemistry models. These uncertainties have profound effect on the performance of the atmospheric models. This point has to be made clear in the Abstract as well.

We agree that the comparison of CTM simulation outputs with atmospheric observations of the biomass burning plume is not a sufficient validation of the emissions. However, the illustration shows that the emissions calculated by APIFLAME allow a realistic simulation of the influence from biomass burning on regional air quality. The scope of the paper being primarily the description of the emissions model, we have removed the "evaluation against observation" from the title. To address the comments in RC1, we have added several sensitivity simulations and these now make up a large part of the results presented.

We have thus decided to change the title to:

APIFLAME v2.0 biomass burning emissions model: impact of refined input parameters on atmospheric concentration in Portugal in summer 2016

In the abstract, we have modified the last sentence:
"The overestimate compared to surface sites and underestimate compared to satellite observations point to uncertainties not only on emissions (total mass and daily variability) but also on their injection profile and on the modelling of the transport of these dense plumes."
to
The comparisons of the different CTM simulations to observations point to uncertainties not only on emissions (total mass and daily variability) but also on the simulation of their transport with the CTM and mixing with other sources. Considering the uncertainty on the emission's injection profile and on the modelling of the transport of these dense plumes, it is difficult to fully validate emissions through comparisons between model simulations and atmospheric observations.

It has to be emphasized that the APIFLAME2.0 can be used for the retrospective studies, not for forecasting. There are significant challenges in forecasting BB emissions, especially on the regional scales. Sometime it's assumed that using the new satellite data the forecasting of BB emissions can be easily done. The satellite FRP data provide information about the state of a fire intensity, unless the satellite scans are obscured by dense smoke or cloudiness. It's still very hard to forecast the spatial and temporal variability of the BB emissions for next hours and days.

As every model, it is possible to use APIFLAME in forecasting. We agree that the weak point in this case is the hypothesis of fires lifetime. Even if current observations of fire activity allow a real-time updating of past emissions, it has to be combined with other information to allow a realistic forecast. One possibility is the use of fire weather indexes, as in Di Giuseppe et al. (2017). We have added the following sentence in the conclusions:

APIFLAME may be used for near-real time applications (using MOD14 only). Forecasting the evolution of the emissions remains uncertain. However, the likelihood of a fire being controlled and extinguished increases when weather conditions are less favorable for its spread. A possibility is to modulate emissions with forecasts of fire weather indices computed from the forecasted meteorology, as suggested by (DiGiuseppe et al., 2017).

The burned area data is the primary source of the information to estimate the BB emissions in APIFLAME. I suggest adding a short description of the burned area dataset and associated uncertainties.

We agree that the description provided was probably not very clear and not well placed in the manuscript. We have moved and clarified paragraph:

The sentence in section 3.1: "During the processing of the fire observations, an estimated burned area is calculated for each burning pixel in the database as the pixel area actually covered by vegetation (Cf. section 3.2), as in the first version of API-FLAME (Turquety et al., 2014). "

is modified and moved to the beginning of Section 4.1:

"The fire observations described in Section 3.1 provide a date of burning at a resolution of 500m (MCD64 burned scars product) or 1km (MOD14 active fire product). The burned area is calculated for each burning pixel in the database as the pixel area actually covered by vegetation (Cf. section 3.2), as in the first version of APIFLAME (Turquety et al., 2014)."

In section 4.1 it's stated that high FRP points correspond to larger burned areas. While this assumption is true in general, sometimes the MODIS instruments report very high FRPs (∼1000MW) for small agriculture fires, for example when the overpass time coincides with the intensive flaming phase.

We thank the referee for this comment. It is correct to say that it is not always the case, we just want to say it is the most commonly observed case. We agree that the approximations associated with this very simplified method are important and we have added a specific comment on this subject:

"A larger burned area is therefore associated with fires of greater radiative intensity. This may not be true if the satellite overpass coincides with the flame phase of the fire. Even a small fire can then have a high FPR. However, intense fires are expected to burn more fuel. The reported burned area should therefore be analyzed either as a larger area or as a larger burning fraction. This follows the same logic as the merging of burned scars and active fires. "

One of the key uncertainties in modeling of the BB emissions is the estimating flaming and smoldering emissions. This topic isn't discussed in the paper. What emission factors (EFs) did the authors use to estimate the emissions of CO and other chemical species? For smoldering or flaming phases?

We have used EF from tabulated reports available in the literature, as details in section 3.4. The numbers used do not separate flaming and smoldering phases. However, the reported combustion efficiency shows that flaming or smoldering may be favored depending on the vegetation. Since the inventory aims at being used in air quality models at resolutions of several kilometers, both flaming and smoldering phases should be mixed in one grid cell so that using average values seems relevant. Although it would be very interesting to be able to construct separate tables of EF for the flaming and the smoldering phases, it would then be difficult to have a systematic estimate of the fraction of flaming .vs. smoldering for each location and time in the region considered. This would require to develop methods using models and data at higher horizontal and temporal resolution.

We have added the following paragraph to Section 3.4 on emission factors:

"Although emission factors strongly depend on the phase of combustion (flaming favoring CO_2, NO_X and SO_2, smoldering favoring CO, CH_4, NH_3, NMVOC and organic aerosols for example), the reported EF used in the model correspond to average numbers for different fire-type categories. Since the inventory aims at being used in air quality models at resolutions of several kilometers, both flaming and smoldering phases should be mixed in one grid cell so that using average values is relevant."

When the plume rise parameterization is used, how the emissions are partitioned in CHIMERE? What vertical distribution was used for the BB emissions in the model with Sofiev et al. plume rise parameterization?

A short description is added to section 5.3:

The injection profile is derived by assuming homogeneous mixing below this maximum height. Menut et al. (2018) have tested different shapes of injection profile in CHIMERE for the transport of biomass burning plumes in West Africa and show very low impact on the simulated concentrations.

L.405: The studies show that as the semi-volatile POA species emitted by fires evaporate partially, more SOA forms downwind of fires. Overall these two processes compensate each other. How these processes are parameterized in CHIMERE?
How the AOD is calculated from the CHIMERE model output? In the literature the reported aerosol extinction coefficients for smoke vary from one study to another. Was any hygroscopicity assumed for the modeled BB aerosols?

As it is not really the scope of the paper to fully describe the CHIMERE model, we added in the text a reference to the article of Couvidat et al. (2018). This article extensively describes the modeling of organic aerosols and the chemical mechanism leading to the formation of SOA.

The thermodynamic module ISORROPIA v2.1 (Fountoukis and Nenes, 2007) is used for inorganic aerosols and the module SOAP is used for organic aerosols (Couvidat and Sartelet, 2015).

and for the calculation of the optical properties:

"The optical properties of aerosols are calculated by the Fast-JX module version 7.0b (Bian and Prather, 2002). Fast-JX is used in CHIMERE for the online calculation of the photolysis rates."

Some text and figures can be moved to SI, e.g. Figure 3, Tables A1-2.

Done.

Using the MISR data to constrain the injection heights in the model is a reasonable approach. However, the MISR typically misses the most intensive stages of the fire evolution (occurring during the afternoon hours). This will lead to underestimate of *the fire injection heights overall.*

We thank the referee for this comment and we have added a sentence about this limitation. We checked if there were any CALIOP observations above the fire region during the fire event (day time overpass being in the early afternoon) but the CALIPSO track was too far. CALIOP observations have been included in the revision for the evaluation of the height of the transported plume.

The following sentence has been included in section 5.2:
"Fires are usually at their highest intensity in the afternoon (corresponding to highest injection height). As the MISR observation is performed in the morning (Terra equator crossing time at 10:30LST), the fire plume height deduced will be quite conservative and will not correspond to a maximum. "

Minor comments

L. 540: Move "The ability. . ." to the Introduction.
Done.