Answer to reviewer's comments

S. Turquety et al.

First of all, the authors would like to thank both reviewers for their comments, which have been very helpful in improving the quality of this paper.

Answers to each of the reviewers' comments are listed below in blue, following the original comment. We thank both reviewers for the precise corrections of the English. We followed all corrections and rephrasing suggested. We report here only the answer to more specific comments.

The main improvement in the revised version is a better description of peat burning in the emissions' model and in the text. Although peat burning is not important in the Euro-Mediterranean region, which is the main focus here, it is critical to correctly account for it in Western Russia, as pointed out by Reviewer #1, and more generally in boreal regions, as well as in Indonesia. The code has been adapted and is now better suited for application in these regions.

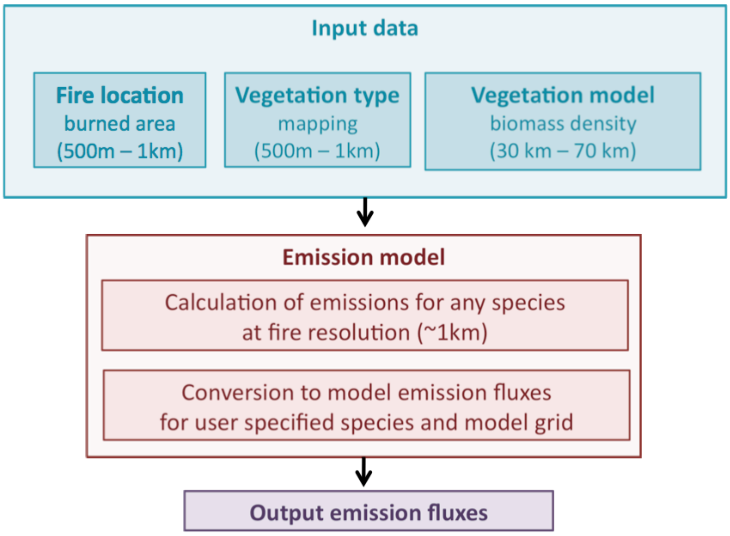
Anonymous reviewer # 1

General remarks

The manuscript would benefit from a rearrangement, so that the necessary input data sets are first described followed by application studies, e.g. section 4 should become a subsection of section 3, which makes the connection and logical order of the manuscript more clear. As the model development is in the centre of interest, section 5 should become section 2, than Fig. 6 would be the first figure, which clarifies the further sequence of the manuscript in describing the necessary input data sets. The original sections 2 and 3 would then be subsections of the new section 2, to be arranged in the appropriate order. This way the repetitions can be avoided, e.g. section 4.3 'Diurnal variability' and section 5.3 'Diurnal cycle' should be combined. Also sections 5.4 and 7.1 should be combined into one subsection. Section 8 should be a subsection of the new section 2.

The original outline was chosen to first introduce the input data, which does not correspond to developments specific to this work: the vegetation type mapping and the vegetation model used for the biomass density calculation (section 2) and the fire data (section 3). This is followed by the analysis of the input fire data in terms of patterns of fire activity in the region of focus here for the illustration/application (section 4). The model itself is then described (section 5) before detailing the results over the chosen region.

To make this choice sounder, and to address the request for a description of the outline in the specific comments, we have rewritten the last paragraph of the introduction. We also include the following figure to describe the general structure of the code.



“In this publication, we provide a full description of a new model for the calculation of emissions at high spatial and temporal resolution, developed in the framework of the APIFLAME project (Analysis and Prediction of the Impact of Fires on Air quality ModEling). The approach chosen is based on the Seiler and Crutzen (1980) classical approach. A general representation of the model is provided in Fig. 1. The APIFLAME emissions’ model was designed to allow calculation at high resolution, but also flexibility of the key fire characteristics. Input information required is the location of fires, the vegetation map for the region considered, and vegetation model simulations for the biomass density. The model then allows the calculation of emissions of any species for which emission factors are provided, as well as their conversion to gridded emission fluxes suitable for use in chemistry-transport models.

The input data used are first described. These include the vegetation maps and the ORCHIDEE global dynamic vegetation model (Krinner et al., 2005; Maignan et al., 2011) used for biomass density estimation, described in Sect. 2, as well as the satellite observations used for fire location, described in Sect. 3. An overview of fire characteristics over the Euro-Mediterranean region, our region of application, is then presented for the 2003–2012 time period, based on the input satellite observations (Sect. 4). Each part of the emissions’ model is described in Sect. 5. The emissions obtained in the Euro-Mediterranean region for key pollutants are presented in Sect. 6, and compared to estimates from other widely used inventories. A more precise evaluation at daily temporal resolution is done for the case study of the summer 2007, which was among the worst fire seasons of the past decades in Europe, and is discussed in Sect. 7. An analysis of the related uncertainty is undertaken using two complementary approaches: a comparison with other inventories, and the calculation of an ensemble of results obtained when changing the input information for burned areas and fuel load. Finally, the code structure is presented in Sect. 8. ”

Regarding sections 4.3 and 5.3 describing diurnal variability. The first one describes the observed diurnal variability of fire activity in the Euro-Mediterranean region (so the diurnal cycle in the data) and the second one describes the how the diurnal cycle is dealt with in the inventory (so in the emissions’ model). Therefore, both sections have to remain separate but we have modified the titles to avoid confusion:

4.3 Diurnal variability of fire radiative power

5.3 Diurnal cycle of fires’ emission fluxes

- page 5491, line 5-7: It remains unclear what are the new aspects of the model besides its flexibility, maybe high temporal and spatial resolution, as the classical approach to determine fire emissions is applied. Which look-up tables are provided for the flexible use besides those of the Euro-Mediterranean region? Maybe it is also the use or creation of new data sets? Please specify at the beginning of the manuscript.

The new aspect of the manuscript is both the high resolution of the input data used to detect the fire activity and vegetation cover, as well as the flexibility of the methodology. This allowed us to conduct an uncertainty analysis based on an ensemble approach. We think that the use of the classical approach by Seiler and Cruzten does not mean that the inventories cannot be improved and that no progress can be made on the understanding of key uncertainties on our knowledge of fire emissions.

Furthermore, the flexibility aspect is important for regional modeling since more precise databases are needed, particularly for vegetation mapping. In addition to providing different databases (one regional for Europe and two global), the code structure allows easy adaptability to new databases. Potential users of the code can use new vegetation mapping, or modify the emission factors without modifying the core of the code (only input data).

In addition to these specificities of the code, we provide a regional analysis of fires in the past 10 years in the Euro-Mediterranean region that had never been done before (to our best knowledge).

The strength of the algorithm in terms of high resolution and flexibility is already explicitly mentioned in the abstract, we only rephrased it slightly: “The applicability to high spatial resolutions and the flexibility to different input data (including vegetation classifications) and domains are the main strength of the proposed algorithm.»

We have added a sentence on the analysis over the Euro-Mediterranean region:

“A regional analysis of fire activity and the resulting emissions in this region is provided.”

We have also added the following sentence at the end of the 1st paragraph of the introduction:

“For this purpose, it was designed to allow calculations at high spatial and temporal resolution and to easily change the domain of interest and the input databases, depending on the region studied. (…). We provide a first regional analysis of fire activity and the related emissions. ”

The spatial resolution of the results should always be indicated in the figures to allow a better understanding of differences etc.

The results are always calculated at the same spatial resolution since emissions are calculated fire by fire. The results are then binned/gridded into grid cells for mapping (sum of emissions for individual fires). Regarding the maps, the spatial resolution is already specified.

To address the reviewer’s request, we have added a reference to Table 7 for comparisons of each model’s characteristics. But we think that writing each model’s resolution would result in too long captions in the end.

The manuscript lacks a discussion of how or if or if not peat fire emissions are included, e.g. peat bogs appear as a 'vegetation type', however, probably with much too low biomass load. Results of peat emissions are not presented and discussed and uncertainties of fire emission estimates due to potentially missing peat fire emissions are also not addressed.

We have added a specific paragraph about the handling of peat burning in all relevant subsections ("Vegetation" and "Fuel load" in particular). Wetlands were considered as a proxy for peatlands in the current version of the algorithm, except for the CORINE land cover database, which includes peat bogs specifically. We mention in the conclusion that peatland fires are among the most important improvements to be addressed in future versions, especially for applications in boreal regions and in Indonesia.

However, very little fires were detected for this vegetation type in the Euro-Mediterranean region, which is the focus region for the application presented here. This is why very little detail was provided in the discussion version of the manuscript. Indeed, peat fires can be important over the Eastern bound of our domain (i.e. Western Russia), therefore to address the reviewer's comments, we have decided to add a discussion specific to the case of the Russian fires in 2010.

We have included specific mires mapping for Russia at 1km resolution in the input databases, and we compared results with those obtained with MODIS landuse only (wetlands considered as a proxy for peatlands). We have also compared results for different choices for fuel load calculation.

This more precise specification of peatland location will have to be complemented by data for North America and Indonesia. We do not have access to such database for the time being but this will be refined in future versions of the algorithm. We state this more clearly in the revised version of the paper (conclusions), as detailed in the answer to the specific comments below (many concerning peatlands).

Specific remarks

- page 5496, lines 1-10: It would be helpful to include the section numbers, where the mentioned topics are presented. In addition, it is rather helpful to mention here as well in which section a comparison with other inventories is provided

This has been added, including a new figure to better explain what was developed specifically in this work (as described in the answer to general comments).

- page 5496, lines 6-12: How can peat fire emissions be derived from MODIS land cover classes?

MODIS and USGS do not include peatlands in their classification. Therefore, we have chosen to use wetlands as proxy for peatlands. This is a quite coarse approximation that will need to be improved in future versions of the algorithm using global or regional databases of peat burning. We have added a sentence to specify this.

In addition, we have tested the use of a specific mires mask in Eurasia at 1km resolution on the results obtained for the Russian fires in 2010. The general patterns are consistent between mires and wetlands, but the mires mask map is more detailed and comprises larger areas.

A short description has been added in the vegetation types section:

"For a more precise analysis of the impact of peat burning in Western Russia, the mires vegetation type (also classified in the wetlands) from the Eurasian mapping of Bartalev et al. (2003) was used."

- page 5496, line 27: Please explain L3 observations.

Definition of L3 observations has been added to MODIS vegetation data description.

- page 5497, lines 14-25: How do these resolutions of 70 km and 30 km correspond to the 1 km resolution mentioned on the first page?

We state that our inventory allows an evaluation of emissions at 1km resolution because we use fire activity observations at 500m to which we attribute a vegetation type (driving emission factor attribution and PFT for the biomass density calculation) using databases provided at 1km resolution. However, it is true that the biomass density has coarser horizontal resolution. We now explain this explicitly with the following sentence:

"The horizontal resolution of the biomass density is thus coarser (30 or 70km) than that of fire detection and vegetation mapping (500 m for MODIS burned area and vegetation map, 1 km for the CLC vegetation map), but a higher resolution vegetation mapping is used for the choice of PFT. »

- page 5498, line 3: How does ORCHIDEE determine the biomass density of peat?

Peatland are not represented as a specific plant functional type in ORCHIDEE. So, as shown in table 1 peatland is associated to a mix of forest and grassland. Then the biomass density is equal to the sum of that of forest and grassland weighted by their respective fraction.

Since the resolution of the model may not represent the large biomass density in peatlands very precisely, we have chosen to modify this and attribute a specific fuel load consumed when a fire is detected in peatlands (Cf. subsequent comment).

We have added a paragraph at the end of the section:

"There is no specific PFT for peatlands, so when a fire is detected in this vegetation type, the biomass density is derived from the densities of forest and grassland PFTs. Since this does not include the ground layer, fuel load may be strongly underestimated. Therefore, fuel consumption values from the literature can be used for this type of fire, as detailed in Sect. 5.1."

- page 5498, line 3: Please explain PFT.

The definition of PFT has been added.

- page 5498, lines 4/5: The sentence is unclear in this context – please rephrase and explain better what is meant.

To address the two comments above, we have added some detail in the text:

"Like in other dynamic global vegetation models, vegetation in ORCHIDEE is represented as a set of plant functional types (PFTs). Each PFT is represented in the model as a unique set of parameterization and parameters. 13 different PFTs are defined in ORCHIDEE mainly splitting vegetation between grass and trees. For trees there is a distinction between phenology (evergreen or deciduous), leaf form (needleleaf or broadleaf), and climate (boreal, temperate and tropical). For grass there is a distinction between natural grassland and crops and a distinction between C3 and C4 pathways for photosynthesis.

The fraction of each PFT is either calculated (thus variable in time) by the model depending on the climatic input forcing or prescribed. In order to avoid having an odd model initialization and thus unrealistic vegetation cover, for each grid cell the fraction of each PFT is prescribed using a vegetation map as input (Krinner et al., 2005). For Europe this PFT map is derived from CORINE land cover (CLC) map. “

- page 5498, line 8 and Table 1: It seems that peat land biomass density is set equal to forest and grassland. Please comment and discuss the associated uncertainties by referring e.g. to Levine, GRL (1999).

As described earlier, we have revisited the way peatlands are handled in the inventory.

Peatland is not represented as a specific PFT in ORCHIDEE. So, as shown in table 1, peatland is associated to a mix of forest and grassland. Then the biomass density is the sum of that of forest and grassland weighted by their respective fraction.

The reviewer is right to point out that the average fuel load derived from the ORCHIDEE simulations will not represent accurately the large values in peatlands. We have revised this in the inventory as follows, in order to be consistent with the high resolution approach proposed here.

If a fire is detected in a peatland, the fuel load consumed is modified using specific numbers available in the literature (weighted by the relative area in peatland in the vegetation database):

- In tropical regions, the values reported by Levine (1999) are used, resulting in 48.75 kg dry matter / m2 burned.
- In boreal North America or Eurasia, we used the values from Turetsky et al. (2011a, 2011b) for burning during the early fire season (6.8 kg DM /m2) or the late fire season (7.5 kg DM / m2), as well as values for burning in drained peatlands (33.2 kg DM / m2). Before 15 July, early season burning is assumed; after 15 August, late season burning is assumed; and in between, a linear increase is assumed (following the method used by Turquety et al., 2007).
- In other regions, we used the value reported for fires in Scotland by Davies et al. (2013) of 20 kg DM /m2.

Again, since the contribution from peat burning is negligible in the landuse attribution using either CLC or MODIS vegetation maps, this does not have a consequence on the results presented in the paper.

In order to test the sensitivity to the representation of peatlands, we have analyzed more precisely the case of the Russian fires in 2010 in Section 5.1.

We now include the above description in the paragraph on fuel loads (see answer to comment below).

- page 5499, lines 22-26: Please mention the revisit time, to specify the temporal resolution.

The revisit cycle is one to two days due to a large swath of 2330km. This has been added to the data description.

- page 5501: line 8: What about false detection induced by volcanic thermal anomalies?

It should be filtered by the data quality tests (specifications added to data description), but we also included an additional filter to avoid any false detection. This is now mentioned in the list of false detection filtering procedure.

- page 5501, line 4: Please add, if this is a new analysis or if the data have been analysed in a similar way before.

Similar post-processing/filtering of the data has been done before, although not using exactly the same procedure and tests. For example, in the construction of the area burned for GFEDv3, Mu et al. (2011) apply a filter for persistent active hotspots (similar to our step based on statistical analysis) at 0.5°x0.5° resolution. Kaiser et al. (2012) also use a specific data processing for the needs of their FRP analysis, building a FRP mask at 0.5° resolution. The false detections detected using these approaches are gas flares or other industrial activities, as well as volcanoes. The tests undertaken here are performed for each fire detected and were chosen for our specific needs.

We have added a sentence as introduction to this subsection to provide examples of previous approaches and mention that our approach is not "unique":

"This often corresponds to power plants, gas flares or other industrial activities, as well as active volcanoes. Previous analyses have used maps of persistent hotspots or high FRP to remove spurious detections, for example Mu et al. (2011) for the GFEDv3 inventory or Kaiser et al. (2012) for the GFASv1 inventory, both using masks at 0.5° resolution. In this work, we also developed a procedure including successive tests to avoid computing emissions at these locations. "

- page 5501, line 21: The first sentence is unnecessary, because its statement is without much meaning.

What we meant is that the variability in type of vegetation burnt does not contribute significantly to the variability in emissions in the end. We agree that phrased like this, this sentence can seam meaningless. We have rephrased it as:

"Variability of the fire activity is the main driver for variability in fire emissions, even if the type of vegetation burned is also a key factor in understanding the amount of trace gases and aerosols emitted."

- page 5502, line 9: Large area burned values are also visible in Russia, please comment.

Indeed, the large 2010 fires in Russia are also clearly visible on the maps, with large areas burned, large persistence but low frequency. We have added the following sentence:

"Large events (large burned areas, but with low frequency) are also observed in North-Western Russia."

Then with the interannual variability:

"Fires in Western Russia were particularly strong in 2008 and 2010 (strong event in the Moscow area for the latter)."

- page 5502, lines 11-24 and Table 2: What about Russian fires? Are they not included in the European fire database?

Yes, Russia is in the EFFIS reports. However, we have chosen not to evaluate this since we only have a small portion of this large country in the domain presented here. An analysis specific to Russia should be performed. We mention this in the paper.

"For the case of Russia, the selected region for this analysis does not include the whole countries. Since EFFIS reports total numbers, Russia has not been included in this comparison."

- page 5503, line 25 and Fig. 4: Category 13 (peat) is missing, please add.

Contribution from peat is very small. We have decided not to include this (not visible in the figure). But we added a comment in the text (see answer to following comment).

- page 5503, line 27: A discussion of uncertainties associated with peat fires needs to be included, in particular for the 2010 fires in Russia.

Almost no fires are detected in this category using the default approach (peat bogs in CLC and wetlands in MODIS). Zooming the Ukraine - Western Russia region on the region of the fires, we end up with no (0.) contribution from peat burning.

This illustrates the need of a more precise representation of peatlands in the vegetation attribution since we know that a large fraction of the fires in Russia in 2010 were located in peatlands.

We have added a discussion in the text rather than a new class in the figure (readers would not be able to see anything).

"Peatland burning is not mentioned in Fig. 4 because its contribution on average over the domain is negligible. For the 2010 fires in Russia, the large event in the Moscow area was partly located in peatlands, which contributed to 30 % of the CO emissions according to Konovalov et al. (2011). For this event, no fires are detected in the MOD12 wetland category. This highlights the need for a more precise database specific to peatlands in this region."

- page 5506, line 8: Which resolution is meant with fire resolution?

We mean the resolution of the MODIS detection, i.e. 500m or 1km resolution. This has been specified.

- page 5506, line 20: Please explain DM.

DM stands for dry matter; this is defined in the above paragraph of the manuscript.

- page 5507, line 1-5: If ORCHIDEE does not determine peat biomass load (a question raised above), then peat biomass load should be included based on available data sets – please discuss in the manuscript.

We have added a paragraph specific to peatlands in this section, Cf. answer to previous comment on the ORCHIDEE description.

"In the case of fires detected in peatlands, since the ORCHIDEE simulations used here do not have information on the ground layer organic matter, and since peatlands do not correspond to a specific PFT, we have chosen to specify the fuel consumed using values from the literature. In tropical regions, a fuel consumption equal to 48.75 kg DM m-2 is used, following values reported by Levine (1999) for Indonesian fires. In boreal regions of North America and Eurasia, fuel consumption of 6.8 kg DM /m2 is used for early season burning (before 15 July) and of 7.5 kg DM / m2 for late season burning (drier fuels after 15 August) using results from Turetsky et al. (2011a). Between mid-July and mid-August, a linear increase is assumed, following the approach used in Turquety et al. (2007). In other mid-latitude regions, fuel consumption of 20 kg DM /m2, reported for fires in Scotland by Davies et al. (2013), are used. Note that uncertainties on these values are very large. For boreal fires for instance, Turetsky et al. (2011b) report fuel consumption values of 33.2 kg DM / m2 in drained peatlands."

- page 5508, lines 1-19 and Tab. 4 and Tab. 5: Both tables are unnecessary, as some of the presented numbers could be inserted into the text. In line 4-6 and Tab. 4 it is only indirectly clear that these values are for European conditions, this should be stated clearly.

We have followed the reviewer's suggestion and removed table 5 and inserted discussion on the numbers in the text. However, we think Table 4 includes important information for validation of fuel loads, and mentioning numbers in the text would become unclear. We have chosen to keep it. This part has also been rephrased to address comments by reviewer # 2.

- page 5512, lines 23-28: Please include a discussion of the year 2010, where GFAS produces much higher emissions. Does GFAS take peat burning into account?

We have included the following discussion:

« A noticeable exception is the case of the Russian fires during the summer 2010, for which GFASv1 emissions are significantly higher. During the large fires in North-Western Russia (latitude between 52 and 58◦ N and longitude between 35 and 55◦ N), 11.3 Tg CO were emitted according to GFASv1, and only 2.7 Tg according to APIFLAME, and 1.9 Tg according to GFEDv3. Kaiser et al. (2012) provide a full analysis of this case study. Large FRP values were measured, leading to large emissions. Moreover, fires were detected in peatlands, that are included in GFASv1 through specific land cover type and specific conversion factor between FRP and fuel consumed (much higher than for other vegetation types). Peat burning is taken into account in GFEDv3 but mainly for Indonesia, and no contribution from peat is obtained for this event. As already mentioned, the APIFLAME inventory uses the MOD12 vegetation map in Russia, with peatland assumed in wetlands but no fire falls in this category during the Summer 2010. CO emission factors are also different. In GFASv1, peat burning emits 210 g CO kg-1 while in APIFLAME, we use a value of 182 g CO kg-1 from Akagi et al. (2011). A test has been performed using a mires mask to locate peat burning in Eurasia. 4% of the fires are then attributed to peatlands, resulting in emissions between 3 and 10 Tg CO depending on the fuel consumption value used (average numbers or consumption in drained peatlands). Additional work is clearly needed in boreal regions to better ac- count for the specificities of ground layer burning, including peat burning. »

- page 5513, line 6 and Table 8: CLC category 13 (peat) is missing. Why? Please add the results to Tab. 8

Cf. answer to previous comment: no fires are detected in peatlands in the area considered using our vegetation mapping. This is clearly something that will need to be improved for Russia for instance.

We have added the following sentence:

"The contribution from peat burning is negligible in the Euro- Mediterranean region discussed here. This might be due to the MOD12 vegetation map (wetlands as proxy for peatlands) used outside of the region covered by the CLC vegetation map. »

- page 5513, line14 and Tab. 9: It remains unclear which area is used for the GFED and GFAS inventory for comparison with the estimates determined in this manuscript.

Please indicate in Tab. 9, that the first four columns represent estimates determined in this manuscript – this is at least my indirect understanding.

The regions are specifically indicated in the text. They are the same for the APIFLAME and GFED results. Calculations for other inventories were undertaken using the actual datasets, and not only the numbers provided in the publications.

The caption of Table 9 (now Table 8 with removal of Table 4) now includes more detail:

"Average (2003–2012) total annual emissions in Gg for different pollutants and regions of the Euro-Mediterranean (Euro- Mediterranean: latitudes between 36 and 48◦ N, divided into 3 subdomains: West from 10◦ W to 5◦ E, Central from 5 to 20◦ E, and East from 20 to 35◦ E.). The average total emissions from the GFED and GFAS inventories are provided for comparison (total within the Euro- Mediterranean domain)."

Please also delete in the caption of Tab. 9 the sentence about parenthesis – there no parenthesis in the table.

Done

Again, are peat fires and their inadequate representation in the inventories a possible reason? Please discuss.

We have added a discussion about peatlands in many areas of the manuscript. We mention it again here but without much detail because we do not think it is relevant for these average numbers considering the few number of fires detected in peatlands in Europe on average. Furthermore, GFED does not include peat fires in this region.

- page 5514, line 25 and Fig. 7 and Fig. 8: For Greece and Ukraine-Russia Fig. 7 and Fig. 8 values seem to be different for the estimate determined in this study, but also for others, even if the presentation of monthly versus daily values is taken into account. Please correct or explain. In addition, please add the starting day into the caption of Fig. 8.

These two figures were done using the exact same input database and areas. We have added the starting day into the caption of Fig. 8, as suggested.

- page 5516, line 2: Please include a discussion of the limitations of the estimates conducted in the manuscript, in particular concerning peat areas.

We have added specific discussion of peat burning in many sections, and added a paragraph at the end of section 7 on the ensemble (Cf. answer to the following comment).

- page 5519, line 3: Please add a discussion of uncertainties associated with peat areas.

We have added a sentence to mention that the databases used do not allow a precise uncertainty evaluation regarding peatland burning. But it is not a major source for Europe on average. In the considered area, it could be very significant in Western Russia (case of 2010 for instance). We discuss uncertainties based on the values available in the literature:

"Although peat burning is not a major issue in the Euro-Mediterranean region, it becomes important in Northern and Eastern Europe and in Russia. As detailed in section 5.1, the fuel load consumed depends on the depth of burning and the dryness of the available fuel. Available observations show a strong variation across regions and time of fire season, from 6.8 (early season fire in boreal regions) to 48.75 kg DM m−2 (Indonesia) in the values used here, hence showing a spread of more than a factor of 7 in fuel consumed only."

The manuscript also includes a sentence in the conclusions about the issue of ground layer burning, especially for boreal regions and peatland fires.

« Future developments will also include a parameterization of ground layer burning and peatland fires, to allow applications to boreal regions in particular."

Anonymous reviewer #2

Comments

Introduction

The Introduction very good, it is recent and it covers all significant aspects of biomass burning emission modeling that are pertinent to evaluating the air quality impacts of wildfires. The uncertainties associated with estimating fire emissions, especially with respect to air quality, are identified and described. The background provides information on similar studies that are relevant to the work. The Introduction is well referenced; important references are included and I can think of no necessary references that are missing. The Introduction is well order and flows nicely. There are some, mostly minor, English usage errors that need to be corrected.

P5493, L14-18: These two sentences are unclear and need to be rewritten.

We have rewritten them as:

"Air quality assessments report the compliance with limit concentrations for a series of pollutants in terms of a number of exceedances of daily and yearly limit values. In the directive 2008/50/EC (EC, 2008) of the European Commission on ambient air quality and cleaner air for Europe, PM10 (particulate matter with diameter lower than 10 µm) exceedances that have a natural origin can be subtracted from the total number of exceedances that have to be reported. Forest fires can fall in this category: they can explain significant exceedances that are not attributable to monitored anthropogenic emissions."

2. Vegetation susceptible to burning

P5496, L2: The text mentions 14 vegetation classes, but Table 1 lists only 13 classes.

This is clearly a mistake. The 14 classes include sparsely vegetated areas, but it is not accounted for, as stated in the following sentence. We have corrected this.

P5496, L26: Should "L3" read "2"?

Cf. answer to 1st reviewer: we have explained what is meant by L3 in the description of the MODIS MCD12Q1 product. Level 2 observations correspond to retrieval of geophysical quantities from the measured radiances, and Level 3 (L3) to an additional level of analysis, here the constitution of a yearly climatology. We now mention this in the text.

P5497, L5: change "are" to "may"

We have chosen not to modify this word since it changes the meaning of the sentence. The VCF product is always used in the area burned processing procedure.

P5498, L6-12. Please clarify how biomass is assigned. Is the biomass assigned to a burned pixel based on the average biomass value(s) of the appropriate PFT(s) in the Orchidee 30 km x 30 km grid (or 70 km x 70 km grid for global) that contains the burned pixel?

We have clarified this in the text.

“The biomass density for a specific fire then corresponds to the sum of the biomass densities of all contributing PFTs in the ORCHIDEE grid cell where the fire is located (nearest neighbour approach).

(…)

The horizontal resolution of biomass density is coarser (30 or 70 km) than that of fire detection and vegetation mapping (500 m for MODIS burned area and vegetation map, 1 km for the CLC vegetation map), but a higher resolution vegetation mapping is used for the choice of PFT.”

It would be interesting to mention how the ORCHIDEE Landcover compares with CLC. For example, how does the total percent of forest cover compare over the CLC domain? I realize a comparison is not straight forward and difficult to interpret. However, a simple mention of how does the total percent of forest cover compares over the CLC domain may be of interest to the reader.

CLC landuse is used in both ORCHIDEE simulations, so there are no inconsistencies. However, there may be some when MODIS landuse is used. We have followed the reviewer’s suggestion and included some quantification for the example of forest cover:

“Since ORCHIDEE simulations were performed using the CLC vegetation classification as a reference, a good consistency is expected between dominant PFTs in a given ORCHIDEE grid cell and vegetation type attribution. For example, the fraction of forest in the PFT distribution of the global ORCHIDEE simulation over the Euro-Mediterranean region is equal to 17%, which is close to the 16% obtained for forest vegetation types in CLC, and the 19% in MOD12.“

P5498, L16-17. Please provide a better description of the biomass components that are subject to burning. Does the “litter” include down dead woody debris? In many forests down dead woody debris can be a very significant portion of total biomass consumed by a wildfire (ref). Or is down dead wood included in the “wood” pool? Please define the “wood” pool. Is the “wood” pool is live wood only or does it include dead wood (standing dead or down dead wood debris)? Does “wood” include all above ground non-foliage of trees (boles and branches)? Does “wood” include woody shrubs?

Indeed litter includes all the dead plant material that is not already decomposed. So it includes leaves and all the dead woody material. We consider in fact 3 kind of litter pools corresponding to specific rate of decomposition: the metabolic litter pool with a rapid turnover, a structural pool corresponding the structural part of leaves and small branches and a woody litter that correspond to the branches and trunk both standing or down. The wood pool corresponds only to wood of living trees. Wood includes boles and branches. There is no specific representation of shrubs that are considered as "small trees" and then like for tree the above ground biomass of woody shrubs is split between wood and leaves.

This description has been added to the text:

“Litter includes all dead plant material that is not already decomposed (leaves and all the dead wood material). The wood pool corresponds only to wood of living trees (bole and branches). There is no specific representation of shrubs. They are considered as ”small trees” so that, like for trees, the above ground biomass of woody shrubs is split into wood and leaves.»

P5498, L20. Please specify the origin of the seasonal cycle. Presumably this is the ORCHIDEE seasonal cycle of carbon allocation, please clarify.

Indeed, the seasonal cycle of each pool is calculated from ORCHIDEE. It is based on the balance between net productivity that is allocated to the different pools based on dynamic allocation rules and the biomass turnover. This turnover is separated between a seasonal turnover (e.g leaves, fine roots) and a long term mortality of wood that in these simulations is considered for each year as a constant fraction of the total biomass (depending on the PFT).

This has been specified in the text:

“It is based on the balance between net productivity that is allocated to the different pools based on dynamic allocation rules, the turnover time of the biomass, and the decomposition rates (Krinner et al., 2005). The turnover is separated between a seasonal turnover (e.g leaves, fine roots) and a long term mortality of wood that in these simulations is considered for each year as a constant fraction of the total biomass (depending on the PFT). A slight increase in the vegetation biomass during the last decades is associated with the response to increasing CO2, accounted for in the simulations (Sitch et al., 2013).»

3. Remote sensing observations of fire activity

The authors describe 2 fire detection data types (active fire detections and burn scar detections) but it is unclear how these are combined to provide a map of burned area used in the emission calculations. In particular, are the active fire detections used in mapping the burned area? If so how are they combined with the MCD45 and MCD64? Is the Wiedinmyer et al. MODIS VCF approach applied to the hot spot pixels? How is double counting handled (see Wiedinmyer et al., 2011)? When there are overlapping detections by hot spots & burn scar, which date is used?

We show both datasets for comparison and because the algorithm may use any of the two. However, there are some inconsistencies in fire location and timing between the two detections. We have chosen not to combine them (which would require a significant amount of work) and used the burned area product as a reference. We now state this more clearly in the text. We have added the following paragraph before subsection 3.1:

“However, the emissions are calculated based solely on the area burned detection. No combination of active fires and burned area is undertaken in this study.“

3.1 If the confidence levels of the active fire products were used in this study, please note that these are fields provided in the data products.

We have added a sentence about this, with the threshold values used in this analysis.

In subsection 3.1: “In our analysis, only observations with nominal to high confidence level (quality index greater than 7 on a scale from 0 to 9) are used. In addition to bad detections, this excludes low confidence fires as well as non-fire thermal anomalies (e.g. volcanoes).”

In subsection 3.2: “Only the highest quality data are included in this analysis (quality assessment index equal to 4 on a scale from 1 to 4).”

P5499, L26 – P5500, L2: FRP provides information on the fire “radiant heat energy”, insert “radiant” between “fire” and “heat”. Also, this sentence is awkward, suggest a rewrite such as: “The FRP provides direct information on the fire radiant heat energy and provides a measure of fire intensity that has been linked to the fire fuel consumption rate”

We have rephrased as suggested.

3.3 Clarify which fire products these false detection tests applied to? Hotspots only (MOD14, SEVIRI), burn scars only, or both? The false detection tests seem designed for hot spots. Specify if any if (and if, how) the confidence levels of the remote sensing fire products were used to eliminate detections.

We found suspicious fire locations in all products, so this additional filter is applied to all datasets. We have also added a sentence about confidence levels used in the sections describing the different fire products (see comment above).

4. Fire activity

P5503, L12-13: Please specify which pixel? Is this the pixel of the land cover map?

This is actually not the successive detections within a given pixel but within a given 0.1°x0.1° grid cell. This has been corrected in the text.

5. High resolution emission model

P5507, L 10-12: This line should be removed. In the reference to Yokelson et al. (1996) I believe the author is confusing combustion completeness / burning efficiency (the fraction of biomass consumed) with combustion efficiency (CE, the fraction of combusted carbon that is released as CO2). CE is related to the relative mix of flaming and smoldering combustion. CE approaches 1 for pure flaming combustion. Fire behavior and fuel characteristics are important factors behind the relative mix of flaming and smoldering combustion, and hence CE. However, the fraction of fuel consumed (combustion completeness / burning efficiency) does not directly relate to the relative mix of flaming and smoldering combustion. For example, organic soils can burn completely, right down to the mineral soil, but do so by mostly smoldering combustion and with very low CE (e.g. Geron and Hays, Atmos. Environ., 64, 192-199, 2013). Similar behavior, high combustion completeness by mostly smoldering combustion occurs for large woody fuels (e.g. rotten logs).

Indeed, there was confusion in the concepts introduced by this sentence. We thank the reviewer for pointing that out. We have removed it.

5.1 Fuel Load

The authors define fuel load (F) as the as the amount (kg dry mass per m2) of vegetation that is consumed by fire for a given vegetation class. F is the product of biomass density (B) and burning efficiency (β), both of which vary by carbon pool. The biomass density also depends on vegetation class. Are the values in Table 3 the burning efficiency (β) that is applied in Eq. 2? Or are these “available biomass” that are then multiplied by the burning efficiency alluded to at P5508, L1? In which case, the variable β in Eq. 2 is the product of value form Table 3 and some other “burning efficiency” number from Hoelzmann et al. (2004). This must be clarified.

We thank the reviewer for pointing out the misleading notations. Indeed, we often referred to “fuel load” where it should “fuel consumed” in the text. We have corrected this. β in the text stands for the fraction allowed to burn, so $B \times \beta$ provide the available fuel load. We have rewritten part of this paragraph to clarify the procedure. In order to be clearer in the different steps, we have added a variable C for combustion efficiency, which depends on vegetation type. We also now include the numbers used of combustion efficiency.

Table 4 & 5. Are these available biomass or biomass consumed? They should be biomass consumed as this is more useful. Regardless, please clarify in Tables. I assume the fuel loads in Table 4 and Table 5 are the load of fuel consumed (F in Eq. 2). But at P5508, L14-15 the authors refer to “available biomass” in Hoelzemann et al. (2004). The authors need to clarify if this is the biomass consumed or the biomass available for combustion. The term “fuel load” is often used to describe the amount of biomass available for combustion, while “fuel consumption” is usually used to describe the amount of fuel consumed by fire (fuel consumption = fuel load x combustion completeness). The authors use “fuel load” as the amount of fuel consumed which is confusing. Please consider using “fuel consumed” or “fuel load consumed” to refer to the amount of vegetation consumed by fire.

These tables correspond to available biomass, as this is what is provided in Hoelzemann et al. (2004). This is now stated more clearly.

To address comments from Reviewer #1, we have removed Table 5 and we provide the values directly in the text.

5.2 Emission Factors. The Akagi et al. (2011) review is an appropriate source for EF. The authors note that an emission study of wildfires in forest of Portugal (Alves et al., 2011) reported significantly higher EF for smoldering compounds compared to the recommendations of Akagi et al. (2011). I note here for the authors that the extra-tropical forest EF in Akagi et al. are weighted heavily by prescribed fires in the southeastern US, fires which tend consume only small amounts of smoldering prone fuels (down dead wood and duff/organic soil) . In fact the Alves et al. (2011) findings are consistent with a recent emission study of wildfires in the US which found higher EF for smoldering compounds (CO, CH_4) (Urbanski, 2013).

We thank the reviewer for this remark. We have added the additional reference, and will mention in the conclusion that emission factors will be updated with recent findings.

“In a recent analysis on forest wildfires over the Northern United States, Urbanski (2013) also find higher emission factors than Akagi et al. (2011), of 135.4 g kg^{-1} for CO and 23.2 g kg^{-1} for $PM_{2.5}$. This suggests that values for extra-tropical fires in Akagi et al. (2011) are too low.”

P5510, L25: FINN supplements Hoelzemann for some regions. (See Table 2, Wiedinmyer et al., 2011), however for the region examined in this study FINN does use Hoelzemann.

We have nuanced this statement in the text by modifying the original sentence to:

“For FINN, the fuel load consumed is based on the tabulated values provided by Hoelzemann et al. (2004) for Europe (not for all regions).”

P5512, L15-17: For the statement “If only summer-time emissions are compared (largest values), the emissions based on either one of the vegetation databases are 2.5

larger than both GFEDv3 and GFASv1." please specify if this is compared to CLC or MOD12 based emissions.

Calculations based on both CLC and MOD12 give the same average differences for summer.

P5512, 17-18: The sentence beginning "This indicates. . ." needs be rewritten. Do the authors intend to state that: "outside the wildfire season, during periods of low fire activity, the GFASv1 emission values are significantly higher than the other estimates"; or that: "the difference between summer and non-emissions is significantly smaller for GFASv1 compared to GFED and APIFLAME v1.0"?

I zoomed in on Fig. 7 Euro-Med panel but could not really tell is GFASv1 is larger than GFED and APIFLAME. Either seems likely. One would expect an FRP based method to overestimate fuel consumption (and hence emissions) for agricultural burning (which dominates the spring & fall fire in the region), since these fires burn mostly by flaming combustion while forest fires will have significant fuel consumption from smoldering combustion and FRP is poor for estimating fuel consumption by smoldering. Also, forest fires will often have peak intensity in the late afternoon after the second daytime MODIS overpass.

Indeed, it is difficult to tell just by looking at the figure how the comparisons looks away from the burning season (i.e. summer), and this statement is not clear. We have added some numbers in the text to be more specific.

"During spring, when the fraction of cropland fires is higher, APIFLAME-CLC is 3.2 times larger than GFEDv3 and 39% lower than GFASv1, APIFLAME-MOD12 is 3.3 times larger than GFEDv3 and 27% lower than GFASv1, while GFASv1 is almost 28 times larger than GFEDv3 on average over the region. This indicates that during periods of low fire activity, the GFASv1 emission values are significantly higher than the other estimates."

P5512, L23-28: The authors should elaborate on the difference in fuel loading between GFED, and CLC, MOD12. For example, is it related to differences in the area mapped as agriculture vs. forest?

There are clear differences in the fraction of vegetation burned in each approach, which quite directly result in different fuel consumed and so in different emissions. We have chosen to discuss this further on the case of the summer 2007, for which we show carbon emissions in the section describing ensemble results (previously 7.2, now X).

In this section, we have added the following paragraph:

"Regions with largest differences also often correspond to regions with largest differences in vegetation attribution, especially in the fraction of forest, woodland and shrubland with respect to cropland, grassland and savanna. For example, during the summer 2008, fires in Eastern Europe attributed to cropland account for 93% of the carbon emissions in the APIFLAME inventory using CLC vegetation map, for 99% if MOD12 vegetation is used, and only 75% in the GFEDv3 inventory (for which vegetation partitioning is provided). Larger contribution from croplands also explains the large differences in Southern Italy in 2008 and Turkey in 2009. The fuel load in croplands is likely higher in the APIFLAME inventory than in GFEDv3. CO emission factors are also larger for crop burning, increasing the discrepancies."

And in section 7.2:

"Vegetation attribution may also explain part of the differences in the fuel load consumption differences among inventories. Since fuel load consumed is larger in forest and cropland vegetation types, the carbon emissions are expected to be higher if more fires are attributed to vegetation types including carbon from forest and agriculture PFTs (forests, shrubland, woodland, cropland). As already mentioned in the previous section, regions with largest differences in carbon amounts also often correspond to regions with largest differences in vegetation attribution. In Eastern Europe during the summer of 2007 for example, the carbon emissions calculated by the APIFLAME inventory with CLC vegetation correspond to 67% forest and shrubland, while this fraction is equal to 39% with the MODIS vegetation and 49% in GFEDv3. In Greece, the fraction of carbon emissions in the APIFLAME inventory in forest and shrubland is equal to 62% with CLC, while it is 45% with the MODIS vegetation. This fraction is equal to 79% for GFEDv3, suggesting that the vegetation mapping is not the only issue. For the full Euro-Mediterranean region, forest, woodland and shrubland account for 37% of the carbon emissions in the APIFLAME inventory with CLC, 20% of the emissions with MOD12, and 53% of the emissions in GFEDv3. Cropland burning accounts for 56% of the carbon emissions in APIFLAME with CLC, 72% with MOD12, and 41% in GFEDv3.»

7.2 Ensemble approach

P5517, L9: Does “the regions considered here” refer to the 6 regions shown ion Fig. 10 or all 9 regions considered in the study? Pleas clarify.

We refer to the six subregions of Fig.10, this has been added.

P5518, L8-12: This sentence is unclear and needs to be rewritten, probably as two sentences. Specify that this statement refers to GFED and note the region. Is it global? Why the reference to North America and burned area? For example, possible rewrite beginning something like: “Using a Monte Carlo approach, van der Werf et al. (2010) estimated the average uncertainty in annual, global GEFD carbon emissions. . .”

We do not refer to North America but to the Northern Hemisphere (our region being located in the Northern Hemisphere).

We have rephrased as:

“Using a Monte Carlo approach, van der Werf et al. (2010) evaluated the uncertainty on the average, annual global GFED carbon emissions to 20%.

For their analysis, they assume uncertainties on biomass density of 44 and 22% for grassland and forest, respectively, as well as uncertainties on area burned (values provided by Giglio et al. (2010), equal to ~ 10% in the Northern Hemisphere).”

P5518, L 22: “kilometric” is unclear. Do you mean scales of ~ 1 km or 10 km or 100 km? Please specify.

We have specified that we mean up to about 1km resolution.

P5520, L 23: Does “A large fraction of the fires” refer to burned area or actual number of fires? Please clarify. For example, “A large fraction of the fires detected occur in. . .”

This refers to the number of area burned detections. This has been specified.

P5522, L12: Change “database” to “map”. The authors need to differentiate between the mapping of vegetation / land cover and the attribution of fuel loading based on the class assignment of the map. The authors found large uncertainty related to the mapped vegetation, but because they used essentially the same biomass model, they didn’t evaluate the impact of fuel loading. Urbanski et al. did considered different fuel loading databases but didn’t examine the influence of the vegetation mapping independently. van der Werf included an uncertainty estimate for fuel loading, but did not consider uncertainty associated with mapping.

We agree with the reviewer that the sentence as it is written may be misleading. We have rephrased the second part as follows:

“This source of uncertainty had not been considered independently in previous uncertainty analyses based on a Monte Carlo approach. Urbanski et al. (2011) evaluated the impact of different fuel loading databases, implicitly including vegetation type, and van der Werf et al. (2010) considered uncertainty on the fuel load values but not on the vegetation mapping.”

Table & Figures

All suggested corrections and precisions have been addressed.