

Interactive comment on “An operational system for the assimilation of satellite information on wild-land fires for the needs of air quality modelling and forecasting” by M. Sofiev et al.

Anonymous Referee #1

Received and published: 17 May 2009

The authors describe the development and application of an operational system for the air quality assessment taking into account wild-land fire emissions through the use of satellite data. The paper is very interesting and it was a pleasure to read it and analyse it. It addresses one important and up to date research and technical issue, which is the need to include wild-land fire emissions into the air quality modelling activities. The developed methodology looks innovative and its application to specific episodes is a strong effort to show its usefulness.

However, there are some aspects of the paper that should be improved and/or better explained, namely:

- the authors developed an operational system to be applied in Europe. Hence, I was expecting a better description of wild-land fire characteristics in Europe, namely spatial distribution, type of consumed vegetation, size distribution of the fires, number of fires, etc. These statistics are available at the JRC site, based on the European Forest Fire Information System (EFFIS). This information is important to understand the representativeness of the selected fire episodes in relation to the main European wild-land fire characteristics. Without this we cannot understand if it is possible to apply the system everywhere in Europe.

- a better state-of-the-art would have been expected too regarding the nowadays use of satellite data to estimate wild-land fire emissions. The authors just touch this issue in the Introduction and thus it is difficult to really evaluate the added value of their approach.

- the authors mention the interest of the system concerning the atmospheric composition and the air quality, but in fact only PM emissions are clearly estimated. This should be clear since the beginning of the paper to avoid misunderstandings.

- within FAS-TA PM_{2.5} emissions are estimated and for FAS-FPR PM total emissions are estimated. This has to be very clear in the paper. There are several misunderstandings along the paper because of this.

- the authors identify potential difficulties when applying this operational system for small fires. I would like to have a discussion of these potential limitations in what regards the application of the system to different wild-land fire characteristics in Europe.

- the selected episodes are not described and this lack of episodes characteristics imply several doubts about the used approach and results.

- I also would like to have the comparison between FAS-SILAM system (considering wild land fire emissions and anthropogenic emissions) with air quality data from different monitoring stations.

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- several comments about the paper could be avoided if the authors just add "northern Europe" to the title and scope. In fact, the paper is very oriented to northern Europe and the application of the developed system to other parts of Europe it is not scientifically supported.

Substantial conclusions are not achieved, mainly because of the large uncertainties associated to the study approaches and assumptions.

In summary, I consider the papers interesting and worth to publish if major revision is done.

Following you can find more specific comments along the document:

- Abstract line 4 - what emissions fluxes are you evaluating? For the authors this is clear, for sure, but not for a common reader. line 7 - the same applies to the "empirical emission factors".

- 1. Introduction line 1- add a reference that supports the given figures. line 3 - replace Brasilia by Brazil 2nd paragraph, 1st sentence - are the authors referring to the globe or just to Europe? What is the scientifically basis for this sentence? last paragraph - specify again what emission fluxes.

- 2. Background 2.1 - line 4 - is it possible to differentiate wild-land fires from other type of fires? 2.1.1. - 1st sentence - why was 4 micrometer brightness temperature selected for fire-detection? What is the basis for this? 2.1.1 - last paragraph - T4 wasn't previously defined. I suppose it is the 4 micrometer brightness temperature, but it should be explicitly mentioned. - the empirical coefficient to yield PM2.5 emission flux is coming from where? How was it estimated? If we want to use empirical coefficients we need to know in what conditions they have been obtained. - what is NTR? 2.1.2 - is a short delay of 1-2 days acceptable for a system considered as operational? Can the authors comment this question in the paper?

- 3. Components of the Fire Assimilation System 1st paragraph - this paragraph and

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part of the second one make more sense if included in 2.1.2.

3.1 Connecting the FRP emission factors and the land use A better description of the chosen episodes is needed, namely in what concerns their location in Europe. Are they representative of what's happening in the different regions of Europe, in particular the ones more affected by wild-land fires? Figure 2 - I don't understand X axis. The text refers April-May and August 2006 episodes, but the axis show 2007! The authors mention that SILAM results for these episodes were compared with the PM2.5 in-situ concentrations. If FAS-FRP was applied, with the empirical coefficients to convert FRP to total PM how can results be compared to PM2.5? This has to be much better explained. Also, and because the selected episodes were not described at all, the reader doesn't understand why results were only compared to data from finish air quality stations. "The systematic deviations of both column-integrated and near-surface PM concentrations were attributed to the emission scaling". What do you mean by this? Please, elaborate a little bit more. Calibration of the system should be better described, namely the assumption that all systematic discrepancy between observed and calculated AOD results from errors in emission rates. After the calibration were the results compared again to ground-based observations? What was the outcome? The calibrated emission coefficients can in fact be applied to the entire European domain or should only be applied to regions with similar characteristics to the ones of the selected episodes? The comparison between the total fire emission values and the anthropogenic ones for Europe in 2006-2008 is not clearly described. How the authors estimated the fire emissions? Previously they were only concerned with selected episodes in 2006. Once again the authors seem to compare PM2.5 based on FPR data converted to total PM.

3.2 Cross-calibration of the TA-based FAS against FAS-FRP 1st paragraph - line 4 - replace stroger by stronger Figure 3 - 2006 fire season: the figure doesn't represent Europe, just northern Europe! The emissions represented are from what? What do you mean by relative units? It would be very interesting to compare these maps with

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fire area maps. At least the spatial distribution should be similar. Without this type of comparison it is difficult to evaluate the system performance. When discussing the limitations of FRP the authors mention the distinguishing between the types of burning. It is the 1st time this kind of concern is mentioned. Are you distinguishing the different types of burning within FAS-TA? A better explanation is needed. Figures 4, 5 and 6 - units should be given in the graphs, not only in the captions. Figure 5 - the polynomial fitting is shown for forest land use type. The main text doesn't specify the type of land use. Hence, equation 2 only regards forest land use or the 3 considered land use types? I'm afraid Figure 6 in the text is not correctly mentioned. See the last paragraph of 3.2.

3.3 Operational setup of FAS at FMI 2nd paragraph - Do you really think you're computing a mini-ensemble? Aren't you just averaging? Once again, how did you estimate PM2.5 based on FRP? Previously only total PM was explained. The calibration from PM2.5 to a full list of chemicals has to be described. Just giving the Andreae and Merlet reference is not enough. At the end of the paragraph the authors mention the scaling of FRP to PM2.5 and then the other species. Please, clarify all this issue. 3rd paragraph - Is there any basis for the assumption regarding the fire development? What is the forecasting period (24, 48 hours)? Can you apply the diurnal variation assumption to the entire Europe? There are already diurnal variation profiles for some countries in Europe (and also in USA). It is not clear for me the assumption regarding the injection height. "Therefore, for the European fires we assumed that 50% of the emission is injected in the lowest 90 m while the rest is homogeneously distributed from 90 m to 1.1 km". With this assumption it looks that you are considering the European fires as mainly small or moderate fires. Do you have any information to support this? Can you apply this assumption uniformly over Europe? I don't think so. At the Discussion section you try to fundament this injection height assumption, but you're still assuming there European wild-land fires are small or moderate in terms of fire intensity. I would like to see some scientific or technical references to support this.

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- 4. Examples of the simulations and comparison with the MODIS and ground-based observation 1st paragraph - why only fire emissions were considered in the model simulations? You mentioned previously that anthropogenic emissions are also taken into account in FAS. Only with fire emissions it is difficult (or even impossible) to compare model results with observed data. Looking to Figures 7 and 8 it looks only northern European fires were simulated. Can you consider them representative of the other regions in Europe? Figure 8 - the presented distributions result from near-surface concentration from the operation system or are column integrated, like the satellite observations? 2nd paragraph - can the authors explain why they didn't consider all the emission sources? I strongly advise them to repeat the simulations in order to be able to correctly compare the results. The comparative analysis is too weak and possible strong conclusions are not achievable. 4th paragraph - "Figure 8 ... forecasts for 2008 of fire-induced ". Do you mean 2008 or one day in 2008? I don't understand the next sentence "Comparing the absolute levels with Figure 7 ". line 4 of the 4th paragraph - "from" is repeated. Please, try to explain why SILAM overestimated the values presented in Figure 8 and don't forget that the simulations were already missing the other emission sources. 5th paragraph - please, explain what do you mean by "moderate scatter". Figure 9 correlation coefficients are low. You don't mention them explicitly in the main text. 6th paragraph - I don't understand your approach. If you have simulation results with all sources considered why you didn't show their results instead of the ones you presented in Figures 7, 8 and 9? Figure 10 - why don't you limit the analysis area as you did before? Considering the whole Europe (and averaging) you cannot really evaluate your approach for wild-land fire emissions inclusion in the system. Units are missing in the figure. I would like to see the comparison between the simulations results (considering wild-land fire emissions and anthropogenic emissions) with air quality data from different monitoring stations.

- 5. Discussion - 7th paragraph - we cannot see differences between emissions estimates in figures 7 and 8. What we can see is the effect of these differences.

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