**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Global mapping of vertical injection profiles of wild-fire emission" *by* M. Sofiev et al.

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Received and published: 11 November 2012

Response to the referee report #2.

We would like to thank the respected Reviewer for the detailed comments. We also would like to apologize if the presentation of the material was not always up to the reviewer's expectations.

Responses to the general comments (same as for Referee 1) We have identified two main directions of the paper improvement, which answer all main comments of both referees. 1. The paper is being thoroughly revised in order to improve the presentation style. The literature review has also been extended. We are thankful for the specific suggestions coming from the comments of the referees. 2. The selection of just two

years (2001 and 2008) for the analysis was mainly dictated by limitations of processing capacity and available meteorological data. As both reviewers pointed out, this point is potentially a significant drawback of the analysis, which might question the representativeness of the obtained dataset. Therefore, we decided to process the whole MODIS dataset for 2001-2011. Apart from processing of the complete period, he analysis will also include the relation of diurnal variation to the land-use type.

Below, we provide responses to all criticism raised in the report and describe the corresponding changes in the paper. The Reviewer's comments are also included in Italic.

General comments ##Notwithstanding the pleasure reading the paper it was not an easy reading, because the authors don't always present all the needed information and the readers have to do an effort to clearly understand the full content of the work and results. Anyway, the work is very interesting and deserves to be published after some revisions, due to some drawbacks that still need to be solved improving the paper.##

The paper is undergoing a thorough revision of the text in order to ensure the good presentation level.

##One of the limitations of the presented global mapping methodology regards the absence of information from some regions of the world. In fact, the plume top height approach was evaluated for some regions of the world, according to available data, but this evaluation is still missing some important regions with different fire characteristics. Authors have to deal with this and to reformulate their language assuming that the global map they present has a lot of uncertainties for these regions of the world. ##

See above – we are processing all available data for 2001-2011.

Specific comments

##Section 3.2 is not clear enough. It has to be improved clearly explaining the use of VIRS for fire counting several times per day and the use of SEVERI data. The reader has to do a strong effort to understand the relationship you're trying to establish and

suggesting. ##

The section is being revised

##Moreover figure 2 FRP diurnal variation (total and per pixel) has to be explained and supported. Ignoring vegetation type based on the presented results (figure 2) doesn't look a good assumption. I don't agree with your last section sentence "Owing to similarity of the profiles for different land-use classes (Fig. 2) and foregoing spatial grid, the extra uncertainty is believed to be small". Can we base our scientific approach on "believes"?##

The vegetation types are included in the new processing.

##Probably it's my problem, but I didn't understand why using a gap closure and spatial smoothing approach. Can you, please, be clearer about the need of this procedure? If no fires were detected why are you artificially filling in the empty cells? In fact, later you only present results without this step.##

Explanation will be added. Smoothing is excluded as a technicality that is not adding-up much to the dataset and is needed only for some specific model applications.

##Page 19211 Line 26 – please, specify the information needed by referred methods; the readers need to better understand why your methodology is an added value.##

Added

##Line 29 – what do you mean by "climatological" injection profile?##

The wording is changed.

Page 19212 ##Lines 13-15 – replace section 4 by section 3, section 5 by 4 and 6 by 5-##

Something went wrong with autonumbering. Will be checked in the revised version.

##Page 19213 Why separating problem statement and problem solution? It doesn't

help the reader to understand the approach.##

The section is being revised and separation eliminated.

##Line 11 – why monthly? If daily data are available why not estimating daily values and based on this higher temporal resolution aggregate to other periods of time.##

There is not enough statistics – too few fire cases in many parts of the globe. Secondly, there is no physical processes that would have regular trends visible at daily basis. Therefore monthly values are more appropriate.

Page 19214 ##Lines 2-4 – please, provide more bases for the assumption of a similar shape of the vertical profile of emission for all fires. Page 19215 Lines 9-13- please, provide information about the pixel size of the MODIS data and about the temporal resolution. Lines 22-24 – please, provide spatial and temporal resolution of the used ECMWF data##

Done

Page 19216 ##Lines 8-10 – this evaluation has to be expanded to other regions of the world that are not considered yet, but they're very important in terms of smoke impacts (e.g. south Europe, Latin America). Before advancing to a global map of vertical injection profiles you need to be sure that all different fire regions in the world have been considered.##

We respectfully disagree. It is not the region of the world but objective features of the place and environmental conditions that govern: vegetation type rather than geography, actual meteorology of each particular fire case rather than climate zone, etc. In that sense, our validation is quite exhaustive: it covers dense and sparse forests, open grassland and mixed vegetation, praires, etc. Weather-wise, it included a wide range of conditions from shallow boundary layer with suppressed turbulence up to very hot conditions and deep convection. Extrapolation along these lines is evidently allowed – and covers practically the whole globe. We added the corresponding discussion.

##Lines 11-15 – please, better describe the type of used data from CALIOPE.##

Done

Page 19217 ##Line 2 – specify the meaning of "good" plume height retrieval;##

We added the reference: this interval is explained in previous work Sofiev et al (2012). It is the uncertainty of MISR data themselves. Essentially, inside this interval our predictions are statistically non-distinguishable from the observed values.

##Is it important to know the type of fires included in the dataset? Are they large fires mainly? Can't you provide information about the area burnt by each fire? I know you want to be independent from this parameter, but it is important to know the type of information included in the dataset.##

Well, this is hardly possible: the burnt area is formed by many fires, possibly occurring during several days over different parts of the land patch. Therefore attributing them to a single fire is impossible. Fortunately, it is not needed for the methodology. To provide the reader with some feeling of the problem, the revised paper will include the histogram of the FRPs of the MISR fires and the histogram of all fires observed by MODIS.

##Line 10 – the sentence "Eqs. (1)–(2) can be used over the whole globe" is too strong South European fires or Latin American fires were not included in the dataset and they also have to be testes before generalising the use of Eqs. (1)-(2) to the whole globe.##

Please see above. We restated the sentence to reflect the main point: extrapolation goes along the similarity of physical characteristics rather than along longitude and latitude.

##Line 15 – "their product" means the product of what? Also FRPtotal (total regional) needs a better explanation.##

Explanation added

##Lines 18-19 – this sentence is not clear enough, please try to improve it.##

Restated

##Line 25 – specify the meaning of "LEO"; this diurnal cycle analysis seems to be based on data from the equatorial region. Can you generalise these results to the whole globe?##

Well, not quite: SEVIRI reaches up to ∼60 latitude, so it is only the boreal zone that is out of consideration. Forests there are constrained by the part of Russian forests that are still inside the SEVIRI domain, which is admittedly not too much fraction but seemingly sufficient from statistical point of view. Tundra fires are indeed excluded – there is no analogous conditions inside the SEVIRI view area. Fortunately, fires there are anyway small and from the point of view of injection height are almost always inside the boundary layer. The explanation is added.

Page 19218 ##Line 10 – specify SEVERI domain##

Added

##Table 1 – needs a better explanation##

Explanation added. The discussion of the diurnal cycle determination and implications is extended – see above.

##Figure 1 – the variation of FRP, total and per pixel, for the different types of vegetation has to be explained. It is strange to see such a different behaviour between total and pixel FRPs for each type of vegetation. Temporal variation of FRP per pixel for forest shows some peaks that have to be explained. Lines 9-18 – see my comments above##

Indeed! That seems to be a very interesting phenomenon, which we are only beginning to explore. Discussion is added but the issue deserves a separate study. It looks like there are at least two different types of fires: those, which shut down in the evening (night-time intensity is zero) and those, which survive through the night with barely a

factor of two reduction of the fire power. Peaks then refer to morning ignition of these "short-term" fires, whose lifetime may be just a few hours or even less.

##Line 20 – Why do you call your approach as a bottom-up?##

Well, because we go fire-by-fire counting their injection profiles hour-by-hour, and then aggregate them in space and time. We will check the language of the revised paper with a native speaker.

##Figure 3 – Even knowing that's not the purpose of the paper I would like to see a quantitative evaluation of the number of recorded fires by comparison with country provide information. At least for some areas of the world this information is available.##

A short comment is added.

Page 19219 ##Section 3.3 – please see my doubts above. Line 19 – what do you mean by "particularly low and particularly high fires"? Please, better introduce Figure 4 contents. Figure 4 – I would prefer a comparison between night and day injection heights (for instance for 90% of mass) instead of comparing 50 and 90% mass injection heights.##

The panels of 50% height are replaced with night-time 90% maps.

Page 19221 ##Section 5.1 – the identified problem of using FRP density for fires with different burnt areas is important and should be solved, even with high fire area esti-mation uncertainties it would have been better to take them into account, at least for a comparative study.##

We agree and presently working in this direction.

Page 19222 Section 5.2 ##Your rough representativeness approach is too weak. You're just using two years and you don't even describe them in terms of fire characteristics and/or meteorology (for different areas of the world). These two years could not rep-resent at all the typical fire characteristics and a longer time period is needed. Fig

6 – Something is missing in my understanding of the described approach. Years are different in terms of fire characteristics and behaviour. To obtain a "climatological" injection profile, as mentioned at the beginning of the paper, you have to use more years, two are not enough and trying to quantify representativeness only based on these two years is not scientifically sound.##

Please see above: the revised paper will include all fires over 2001-2011.

Page 19223 ##Lines 12-13 – you need a reference to support this or you need to show this correlation.##

Explanation is included.

##Line 27 – I would avoid sentences like: "In-average, one can use the ABL-injected fraction of 50–60% as a rough estimate." This value depends on the region and on the meteorology.##

The sentence has been removed

Page 19224 Sections 5.4 and 5.5 ##The attempt to evaluate fire injection heights using results from other authors is appreciated. However, this comparison should be done more carefully and not only as a generic comparison of maps.##

The problem is that the maps are strongly different in many regions, so that any quantitative comparison (RMS, correlation, bias, . . .) does not lead to any interesting results. Since the AEROCOM map does not seem to be rigorously justified it was not possible to compare the underlying assumptions either. So, we were left with qualitative considerations.

##The use of CALIOP observations and their potential link to your results have to be better explained/supported.##

The CALIOP discussion is extended.

##It is strange to mention several times a table from another pape; if this table's infor-

mation is so important it should be included in the current paper.##

Restated. We would prefer to avoid importing this table in our paper as its origin is not very clear and some of its values are confusing.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19209, 2012.