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

M. Sofiev et al.

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We would like to thank the respected Reviewer #1 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 2) 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 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, the 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 within ##.

General comments ##I strongly recommend the Authors to thoroughly revise their manuscript by drastically improving the quality of writing style and the organization of the manuscript. The goal is to provide a clear description of the methods and assumptions made to develop their parameterization. This will hopefully make the narrative flow, so that the reader can focus on the science of this work rather than guessing what points the Authors are trying to make. Another major concern is the literature review, which seems to be patchy and incomplete.##

See above

##Aside from clarity and organization, the major limitation of this paper is that it does not seem to introduce any sufficiently innovative science results. The parameterization of fire injection heights is indeed new. However, ACP may not be the best journal to publish it. I would strongly recommend to re-submit this work to a more applied journal, such as Geoscientific Model Development.##

We respectfully disagree. This topic is fully within the journal area and ACP has already published a series of papers on fires, including two our publications (one on injection height – the core methodology behind the current study). Neither we agree that the innovation is low. The presented dataset is the first-ever 4-dimensional injection profile from wild-land fires based on physical parameterizations applied to every observed MODIS fire over the globe during several years. Finally, we do not present the plume height methodology – it has already been published in ACP. We only provide its additional evaluation to justify the applicability of the globe. The paper is about the mean vertical distribution of the fire smoke.

Specific comments: ##Page 19211 Line 2. [: : :], but strong fires occurring under 'unstable' atmospheric conditions can send [: : :].##

Not only. In fact, the highest plumes usually appear when strong vertical updrafts are generated by passing atmospheric fronts. Such updrafts can reach much higher than ordinary convection, for example, in middle latitudes (deep convection is another story, again not exactly fitting into "unstable stratification" term). Therefore, "favorable" is more accurate term here than "unstable".

##Page 19211 Line 22. Lavoue et al [2000]'s fire injection parameterization is based simply on fire intensities; their method does not take into account atmospheric stability conditions. In addition, the Authors are missing many references here. For example, the works of Rio et al., [2010], Rafusse et al., [2012], Stein et al., [2009] should be cited and adequately compared to the presented methodology. Page 19212 Line 3. Val Martin et al. [2010] also evaluated the relationship between MISR plume height and MODIS FRP and atmospheric stability conditions. In addition, Tosca et al., [2011] evaluated an extensive MISR plume height climatology over Indonesia. This work should be cited as well.##

The list of references has been extended and more discussion added. We are aware about these works (Val Martin et al is actually quoted later), just did not want to occupy too much space: the paragraph is only after the representative examples.

Section 2.1 ##Although the calculation of the top-height of fire emission plumes, which is key in this paper, was described in Sofiev et al., [2012], it would help the reader understand the next steps if some details were given in section 2.1. For example, the semi-empirical equation was constrained using MISR plume heights, which they are not mentioned until section 3.1. Also, the determination of the semi-empirical formula has some uncertainties, which are not addressed at all. I understand that developing a

parameterization of fire emission injection heights is a difficult task, many assumptions need to be made and many uncertainties exist. For example, the semi-empirical equation does not take into account entrainment processes that the plume can undergo. In addition, MODIS FRP has many uncertainties (e.g. obscuration for clouds and dense smoke) and the meteorological fields used may not provide the most accurate state of the atmosphere at the location of the fire. The reader needs to be aware of all these caveats.##

A discussion on the methodology assumptions and limitations has been added. However, the publication is in the same ACP journal, open-access, so the interested reader can immediately get all details from the original paper.

##I wonder why the Authors use an equation derived using MISR plume heights without any screening, that is, using 'good', 'fair' and 'poor' quality plumes and later in section 3.1 the Authors only use 'good' quality plumes to evaluate further the approach on a global scale. I am familiar with the work presented in Sofiev et al., [2012] and the Authors explicitly said there that using the MISR 'good' quality plumes to derive the semi-empirical equation would results in statistically unreliable coefficients for the equation.##

The "learning dataset" for determining the coefficients needs to be large. However, for the evaluation of the methodology, the "control" dataset does not have to be that large. This simple reasoning is behind the standard practice in data assimilation technology: learning dataset is always much larger than the control one. Also, MISR dataset has somewhat increased during 2011-2012. As a result, it became possible to introduce the above filtering in order to reduce the impact of the MISR uncertainties to the methodology scores. Since we did not change the formulas and coefficients, this does not affect the methodology itself, only provides more objective assessment of its accuracy. Expectedly, it appeared somewhat better than originally suggested in Sofiev et al, 2012 where we had to use all to-date available MISR data.

##Equation (2) is not an equation per-se; It shows the coefficients of Equation (1). Also, in Sofiev et al., [2012], N2o= 2.5x10-4 s-2 and not 2.5x10-3 s-2.##

Sorry for the misprint.

Section 2.2 ##This section is not very clear. Why is this section divided into problem statement and problem solution? More simply, it could be presented as whole and the text could describe the development of the vertical profile of the fire emissions. Overall, I found this section poorly explained and I had a hard time to understand the procedure and methods used to come up with the vertical profile of fire emissions.##

This section is revised to make it clear.

##Page 19213 Line 18. Sukhinin et al. [2005], Kaufman et al. [1998], and Sofiev et al., [2009] are not the only studies that have related fire radiative energy with emission rate. See Wooster et al., [2005], Freeborn et al., [2008], Kaiser et al., [2012], for example.##

Yes, we are certainly aware about these works, which are now added to the reference list. The reasons why they were left out is that we avoided species-specific emission scaling (as stated in the paper further and explained upfront in the revised paper) by computing the bulk-plume injection profile. Therefore, long discussion of the emission factors subject is hardly justified here.

##Equation (4). Does the approach square to two the emission rate Pf(t)? The explanation of the epsilon determination is unclear. Perhaps the Authors could include a simple graph to show the vertical distribution of the emissions based on the Briggs approach.##

No, Pf is not squared. In explicit form, it is present only once. Emission factors are not dependent on it. However, the plume profile is a function of Pf (about a cubic root of it), therefore the actual dependence of the vertical distribution on Pf is non-linear.

##Page 19214 Line 24. Is PM Particle matter? If so, what are the particle sizes considered, PM2.5, PM10, total PM? Please, also define PM.##

Done

##The Authors propose to use the emission factors proposed by Andrea and Merlet [2001] to scale the PM from the IS4FIRES. Akagi et al., [2011] provide a more comprehensive, updated collection of emission factors for fire species. The Authors indicate that profiles are computed for total emissions. Are those emissions PM or the sum of all the species emitted in the fire? What are the units?##

No, we are not using these factors, instead taking the total emission. The revised paper stresses from the beginning that we are not after particular species but rather after the bulk-plume injection profile. This eliminates the above-mentioned problems. In the revised paper, we shall still include a few sentences on the potential species-specific profiles and provide the corresponding references to emission factors.

Section 2.3.1 ##I suggest including a description of the MODIS FRP data: product level, spatial and temporal resolution, etc. In addition to MODIS, SEVIRI and VIRS, another satellite product that provides fire information is GOES WF-ABBA.##

The information on MODIS product details will be included in the revised paper. Regarding other instruments, we have certainly looked at them before starting the exercise. Our analysis of VIRS and SEVIRI products showed that they are less suitable than MODIS for the purposes of the study. SEVIRI has too coarse resolution and high detection limit, VIRS provides simple fire counts but not FRP. Therefore, we concentrated entirely on MODIS. WF-ABBA is another opportunity, which can be explored in further studies.

Section 2.3.2 ##The Authors should provide more information on the ECMWF meteorological fields: horizontal, vertical, spatial resolution, etc.##

Done

##A dry-parcel method is used to estimate PBL heights, which are evaluated with other approaches. Please explain what those approaches are. How well the dry-parcel method estimates the PBL height?##

The discussion and a few references are added

Section 2.3.3 ##The MISR plume height climatology does not provide continuous measurements for all the regions specified by the Authors from 2005-2008. For example, plumes in Siberia are provided only for August 2002, May 2003, July 2006 and April-July 2008, plumes in Africa for December, 2005 and January, 2006, etc, etc. The Authors should provide accurate information.##

Rephrased

##What is the spatial resolution of CALIOP aerosol product?##

Added

Section 3.1 ##The Authors state that the new MISR data were not available for the Sofiev et al., [2012] study. I am not sure that is correct. The MISR plume height dataset for North America, Africa and Siberia is publicity available since 2009. The Authors should reword this statement.##

That statement concerns only Africa and Borneo – the "new" datasets. Neither of these sets was available in 2010-2011 when the original work was made. Siberian and North American fires have been available and used there. Therefore the statement is correct. This is clarified in the revised paper.

##The Authors could reference the works of Nelson et al. [2008] and Val Martin et al., [2010] so the reader can be directed to more complete information on the MISR plume height uncertainties.##

Done

##The MISR plume height dataset provides with different definitions of plume height (e.g., best estimated median, maximum height, etc). What is the definition used here?##

Added

##The semi-empirical formula misses about 30% of the cases for temperate and boreal forest and tropical savannah in Africa, so the approach is not perfect. The Authors state that it can be applied to the whole world. I understand that the equation is as good as it gets. However, the Authors should smooth out the text and indicate that uncertainties exist.##

Done. The message was that the formula performs homogeneously over main types of land use and regions, i.e. can be applied to the globe with minimum chances for error explosions somewhere.

##Section 7 seems to be missing.##

Something with automatic numbering. Will be checked in revised paper

Section 3.2 ##Lines 13-14. The statement is not new, so a reference should be provided.##

Done

##Line 15-16. What do the Authors mean with 'their product'? Those lines are not clear. What is a LEO satellite?##

Rephrased

##Lines 27-28. Can a variation be as large as another? What variation do the Authors refer to? Do they mean that the diurnal variability of active fires is similar in VIRS and SEVIRI? This sentence is not clear and should be reworded.##

Done

##The Authors state that "Estimating of diurnal cycles directly from MODIS data is not feasible" and "Analysis of FRP could not be performed due to early saturation of VIRS infrared channels". Vermote et al., [2009] show the diurnal cycle of FRP using MODIS and VIRS as well as SEVIRI in different biomes (i.e, boreal Russia, Brazil and northern Africa, respectively).##

We will add the reference and some discussion on the subject. However, the demonstrated effects in Vermote et al paper are different from the declared above: (i) VIRS never had FRP product, as correctly stated in our paper, Vermote et al used "probability of fire detection for a given hour", whose similarity to FRP in terms of diurnal variation is far from being proven; (ii) Figure 3 of Vermote et al shows perfectly well that MODIS leaves large holes in the diurnal cycle even for the lucky region in northern Russia with over 10 overpasses per day (as we stated in the paper, usually there are just 4 of them and no diurnal cycle can be suggested from these). Therefore the claim that these two satellites are capable of providing the FRP cycle over any substantial area is far fetched. Among the instruments considered by Vermote et al, only SEVIRI has this capability – as stated in our paper.

##In addition, I wonder if the diurnal cycle of active fires is the same in equatorial regions as in extratropical regions. Fires over different regions and vegetation types have complete different regimes. Mu et al., [2011] show, using GOES WF-ABBA active fire counts, that the diurnal cycle of grassland fires have a different cycle than forest and shrublands in boreal and temperate America. The Authors use SEVIRI to determine the diurnal cycle of fires for grassland, forest and mixed. How did the Authors classify grassland, forest and mixed fires? Also, as far as I know, the SEVIRI domain only includes Africa and Europe. However, the Authors apply the diurnal cycle obtained over that domain to the globe. How accurate is that?##

SEVIRI actually includes some part of Northern and Southern America, so the analysis also covers those. Regarding the work of Mu et al, reference to which we now included for comparison, the difference between the regions in most cases is well within the uncertainty of the estimates (for example, see figure 3 there). This is why we did not go into differentiating of our three large land-use types between the different climatic zones etc. The revised paper will contain the annex with a list of land-use types attributed to

each of these classes.

##The Authors show the Fourier coefficients for the diurnal cycle of active fires in Table 1. However, nothing of this analysis is explained in the text. That is very confusing. Also, the Authors end up applying the mean diurnal cycle regardless of the vegetation type. Table 1 could provide at least the mean coefficients used in the analysis.##

Discussion is added, land-use types are fully separated in the new processing.

##Also, I am a bit confused on why the Authors use the total FRP diurnal cycle for the emission fluxes and FRP per-pixel diurnal cycle for the injection heights. Both cycles do not significantly differ from each other.##

Well, the total FRP changes by more than an order of magnitude, whereas FRP-per-pixel essentially stays within a factor 2. We think that this is sufficiently large difference to be taken into account.

Section 3.3 ##This section is very confusing. It describes a smoothing procedure, which is not applied to the results presented in the paper. What numerical dataset the Author s refer?##

We apologize for the confusion. The smoothed datasets were requested by AEROCOM community, with whom we discussed the details of the study. These datasets are available from IS4FIRES Web site. Anyway, the section has been removed from the revised paper to avoid confusion.

Section 4 ##I am not convinced entirely by the results. For example, the parameterization returns one of the highest injection heights over the Middle East and south of the Caspian Sea. In my understanding, those regions are classified as semi-desert shrub and prairies. Injection heights from fires for those biomes are not expected to be high, in particular over semi-desert vegetation.##

As seen from total number of fires, these regions are still prone to substantial number of events with high FRP reported by MODIS: grass can burn very strongly. It is also the very deep boundary layer that contributed to high injection. In that sense our analysis is fully transparent: high ABL and FRP lead to high plumes. Discussion on Northern Caspian region will be added. Persian Gluf may indeed have a caveat: errors in MODIS products can affect some grid cells. For that region, Kaiser et al (2012) argued that the very strong FPR actually comes from oil refineries, which are not always properly masked out in MODIS products. We are considering to eliminate these points from the revised dataset – at least warn the reader of possible contamination.

##In addition, it is not expected that extremely high fires over Siberia and the boreal regions be represented in their map as the Authors stated. Extreme high fires over the boreal region are very episodic. The semi-empirical equation that estimates injection heights was constrained by MISR plume heights. The MISR overpass time is around 11:00-13:00 over North America and fires over that region are not at their maximum intensity [Kahn et al., 2008; Val Martin et al., 2010]. Also, the MISR plume dataset screened out pyrocumulus clouds associated with intense fires, so the 'infamous extremely high' plumes are not taken into account in this approach.##

This is not correct: the approach is by no means related to MISR overpasses or limitations. The MISR dataset is used only to identify the coefficients of the formula and then entirely excluded from the consideration. As shown in Sofiev et al (2012) the MISR dataset still contains significant fraction of high fires, i.e. the methodology is calibrated and validated for those plumes too. Calculations of this paper include absolutely all fires that have been recorded by MODIS within the considered years. What is true, as already pointed out in our paper, is that the episodic character of huge events makes their contribution to global budget and mean injection profiles small – which is reflected in our dataset.

##In Figure 4, the Authors show February and August to demonstrate the seasonality of the injection heights. It would be more accurate if they show complete seasons, for example, the average injection heights for June, July and August for summer and December, January and February for winter.##

The figure has been modified

##In addition to Figure 5, the Authors could add another figure showing the vertical injection height distribution of fire emissions obtained for single grids at different vegetation types (e.g, boreal forest, Australian forest, African savannah, etc). These profiles will help see the difference obtained for the injection height profiles in different parts of the world.##

Will be added to the revised paper

##Page 19221 Lines 1-2. "[: : :] more than 50% of the fire emission is confined within the lowest 1–2 km, i.e. within the ABL, consistent with previous studies [e.g., Labonne et al. 2007; Kahn et al, 2008; Sofiev et al 2009; Val Martin et al, 2010].##

Done

Section 5.1 ##Page 19221 Line 11-18. It is not clear from the text and the figures how the Authors evaluated the plume-top formulations.##

Did not understand the comment, sorry. This discussion explicitly refers to the evaluation of the formula in the "Preparatory step" section.

##The last paragraph in this section mentions the disconnection between the plume formation time and the MODIS FRP time as a source of uncertainty. How did the Authors determine the 15-30 min disconnection? It is also not clear how they come up with the 20-30% difference in FRP using the parameters in Table 1. I would suggest including the possible effect of uncertainties in MODIS FRP and the ECMWF meteorological fields on the outcomes of the presented method.##

Explanations and extra sources of uncertainties are added

Section 5.2 ##The Authors use years 2001 and 2008 to determine the injection height profiles and mention that they provide enough coverage and are representative of the interannual variability of the fires. Fires in Africa, South America and Indonesia are mainly from agricultural activities or from deforestation and have similar inter-annual variability. However, fires over forest regions such as Siberia or northern America have a strong inter-annual variability. In fact, year 2001 was a low fire year over Alaska, Canada and Siberia, whereas year 2008 was a low fire year for Alaska and Canada, but exceptionally high fire for spring in Siberia. As the Authors point out in Line 3 page 19223, year 2001 only contains MODIS data from Terra whereas analysis in year 2008 contains MODIS Terra and Aqua. The reviewer wonders why year 2001 was picked instead of, for example, year 2003, in which MODIS Aqua was fully operational during the whole year. Would the same results be obtained if several years were used in the analysis (e.g. from 2003 to 2008) and the average of those profiles were used to provide a global injection height map instead? I believe the global dataset would result more robust. In fact, a variability (or standard deviation) could be computed for each grid cell and an uncertainty in the results can be reported.##

As pointed out above, we are extending the analysis period and including the land-use types.

##What type of strong fires burned in the Middle East in 2008?##

See above

Section 5.4 ##Page 19225 Lines 5-10. Please cite previous work that investigated the export of biomass burning out of the coast of Africa. For example, work from the TRACE-A or AMMA campaigns (Mauzerall et al 1996; Fiedler et al 2011).##

Done

##The Authors state that CALIOP measurement at night did not differ from daytime and that is 'evidently wrong'. As the Authors well explain later, CALIOP may measure aged smoke at night remaining from the previous day at night. The reviewer suggests "smoothing" this point.##

Done

Section 5.5 ##The Authors state that tundra regions over northern Eurasia have low fuel loads. The reviewer asks to provide a reference for this statement. Tundra regions have deep layers of organic matter that can potentially burn, and in turn fuel loads over those regions are very large [Kasischke et al., 2005]. Perhaps the Authors meant to say fuel consumption instead of fuel loads or that usually are more surface fires than crown fires.##

Yes, thank you for the correction. We meant the low rate of fuel consumption, of course, which is due to low amount of quickly-consumable fuel.

##Page Line 7-9. The Authors state that fires over the North America boreal region are predominantly of low intensity and injection height. This seems to contradict previous work. For example, Val Martin et al. [2010] showed that plume heights over forest and shrubland in the boreal region were the highest and the most intense out of all the biomes studied over North America with the MISR plume height dataset. In addition, Ichoku et al. [2008] using MODIS FRP show that fires over Alaska, Canada and Quebec regions are the most intense in the world on a regional scale.##

This analysis is not correct. Everything again comes down to the problem of rare episodes with strong events versus numerous moderate fires than emit the bulk of the smoke. Our dataset provides the injection profile for the bulk of the smoke, so that the strong events are visible there only to the extent they contribute to the regional multi-annual emission. Please note that Ichoku et al stated that fires "of categories 4 to 5 (each) constitutes less than 0.5% of all fires detected in the different regions". For the emission injection profile this fraction is of little importance – as shown by our computations. We added this discussion to avoid confusion.

##As a final remark, I do not fully understand why the Authors compare their injection heights and vertical distribution of fire emissions with CALIOP and AEROCOM. The most comprehensive dataset of plume heights that exist up-to-date is the MISR plume height climatology developed at NASA JPL. The Authors use that dataset to develop a parameterization, but there is nothing available to validate this parameterization on a global scale. The spatial coverage of CALIOP is limited and it typically measures smoke far away from the source, so it is difficult to used CALIOP data to look at injection heights from young plumes, which it is what the Authors' product is meant to provide. Likewise, the AEROCOM global injection height distribution is subjective and was developed to recommend injection heights in atmospheric chemistry model studies, whose accuracy has never been verified. Therefore the validation of the methodology presented here seems to be questionable.##

This is not correct. We demonstrated that: (i) our methodology agrees with MISR – by development fit into the learning subset and validated over the control subset, (ii) it agrees with another space-born global dataset fully independent from MISR: CALIOP, which is still global despite only a fraction of the Earth surface observed, (iii) it has both similarities and differences from the other global injection height dataset of AEROCOM, which is the only other global dataset recommended for usage by the biggest group of global modelers. One should also keep in mind that CALIOP, being indeed difficult to handle in specific episodes and individual plumes due to low coverage, is sufficient when space- and time- averages are taken for the gridded data: the number of observations in the covered grid cells gets very large already at monthly level (the lidar is a high-frequency instrument). There are no other comparable datasets that can be used for evaluation of our dataset, therefore our exercise comprises the state-of-the-art.

Figures ##Figure 1 caption is not correct for the temporal description of MISR plumes. North America plumes are for 2002, 2004-2007; Siberia and Africa plumes are for only a few months in several years (see text above); Borneo plumes are also for agriculture peatland fires in addition to tropical forest fires, from 2001 to 2009, unless the quality screening removed several years of data.##

Corrected.