General Remark: The editor of this paper had asked for quick reports from the anonymous referees prior to publication of this article in the ACP discussion. In the course of the quick report manuscript evaluation, Referee #2 has already provided the interactive discussion comments. Following the editor's request, we have implemented some comments and modifications suggested by Referee #2 in the current ACP discussion manuscript (acp-2014-978). As page and line numbers given in the referee's comments refer to the original manuscript, we provide the corresponding page numbers in the current ACP discussion manuscript in squared brackets [].

Author Response to Anonymous Referee #2

The authors would like to thank the Referee #2 for the detailed review of our manuscript. The critical and well-founded comments and helpful suggestions will significantly improve the quality of the paper. In the following we list the individual referee comments together with our response.

1. This paper presents the results of a study aimed at tuning an empirical plume-rise model from Sofiev using MISR stereo height data, to provide injection-height input to the ECHAM6 model. The effort results in a significantly more realistic representation of plume heights than the original assumption of the ECHAM6 model. My comments manly focus on how the limitations of the empirical plume height parameterization are presented in the paper – although it is better than the original ECHAM6 assumption, a more realistic plume-rise model, perhaps validated in more detail regionally, might further alter the ECHAM6 simulations significantly.

We agree that the simplicity of the applied plume rise parametrization seems to represent a limitation of our study. There are two substantial arguments why we are convinced that a more realistic plume rise model implementation can not be expected to significantly alter the results of this study:

1) Val Martin et al., 2012 clearly pointed out, that more realistic plume-rise models do rarely improve plume height predictions. Our results generally support these findings. In Chapter 5, we demonstrate that the global plume height distributions of the Sofiev plume height parametrization and a modified version of the plume rise model by Freitas et al., 2007 are rather similar except for the lowest 1.5 km.
2) In the companion paper (acp-2014-979) of this two-paper series we show that even mean plume height differences of 1-2 km only have a limited impact on global parameters like aerosol optical thickness and atmospheric radiation.

In contrast to global climate modelling, regional and high resolution air quality studies can indeed be expected to benefit from the application of more advanced plume rise models. We also acknowledge these advantages in the conclusions section of part two of this two-paper series. But for low-resolution aerosol-climate modelling, the computational expenses for a fully analytical plume model are disproportionate to the potential improvements.

2. Introduction, P2, paragraph 1 [see acp-2014-978: P.6647, Paragraph 1]. *I guess you can define these terms any way you want, as long as they are applied self-consistently throughout the paper. But generally, "Plume height" means the height of an aerosol layer, whether it is at or near the injection point, or anywhere downwind; "plume-top height" usually indicates the top unambiguously. I agree that as commonly used, "injection height" is ambiguous; it sometimes means the top of an aerosol plume at injection, sometimes the median, and sometimes the profile. Perhaps "emission profile" would be better than "emission heights" when you mean the profile.*

These useful suggestions add to our considerations about an accurate and consistent nomenclature. We fully agree that the term 'emission profile' is more appropriate than 'emission heights'. The text passage has been modified accordingly in the ACP discussion manuscript [P6647, lines 16-17]. Moreover, we also state that the term 'plume heights' in this study is equivalently used to 'plume-top heights' [P6647, lines 15-16].

3. Introduction, P2, line 27 [P6648, lines 3-5]. *I think the plume-height uncertainty in the stereo-derived MPHP data is about 250 m [Nelson et al., Remt. Sens., 2013], which is not large for this application. The FRP-based methods are much more uncertain.*

Indeed, Kahn et al., 2008 and Nelson et al., 2013 provide a reasonably small uncertainty estimate of ± 200 m. We exchanged the references and replaced this sentence by a more appropriate statement in the ACP discussion paper, see page 6648, lines 3-5.

4. Introduction, P3, line 2 [P6648, line 16]. *Another key reference for high-elevation plume injection is Fromm et al. [JGR 2008, doi:10.1029/2007JD009153].*

We added this reference to the ACP discussion manuscript.

5. Introduction, P3, lines 15-16 [P6649, line 3]. *I think the FRP values included in the MPHP dataset come from MODIS, which might be worth mentioning.*

We applied these changes in the ACP discussion manuscript.

6. Section 2.1, P5 [P6651], *Eq. 1. I understand what you are doing here, and it is certainly an improvement over injecting everything into the BL, or mixing it uniformly to some arbitrary height. But on physical grounds, we know (and also observe) that smoke injected above the BL tends to accumulate in layers of relative stability above the BL [e.g., Kahn et al., JGR 2007, doi: 10.1029/2006JD007647; Val Martin et al., ACP 2010]. So I'm wondering if using the model stability profile rather than picking arbitrary model layers above the BL might be a better strategy, requiring less subsequent adjustment.*

Prior to this study we tried to extend the original Sofiev formula by a function which takes into account the entire stability profile of the troposphere. We analysed all available 'good quality' plumes of the MISR Plume Height Project data set combined with meteorological data from NOAA-CIRES 20th Century Reanalysis. However, no significant correlation could be found between plume height and CAPE (Convective Available Potential Energy) respectively Convective Inhibition (CIN) which represent integrated measures of the vertical stability. For this reason we did not further modify the original Sofiev parametrization except for nocturnal plumes, for which we apply the atmospheric stability of the nocturnal boundary layer for the calculation of night-time plumes, see page 6652, lines 20-23. As described on page 6652, lines 8-15, we also tested the two-step iteration scheme proposed by Sofiev, which distinguishes between Planetary Boundary Layer (PBL) and Free Troposphere (FT) plumes, but we did not find any improvement in the prediction of global plume height distributions, see Chapter 3.

7. Section 2.2. This is just a comment; I include it just to offer one general perspective on the approach. As you know, the main factors affecting plume rise in most cases are dynamical heat flux, atmospheric stability structure, and entrainment. Similar factors dominate cumulus convection (the main heritage for plume-rise modeling), except latent heat release replaces dynamical heat flux as the primary factor generating buoyancy in cumulus convection. So you account for the heat flux parametrically with the P_f term in equation 2, and the atmospheric stability structure with the H_pbl and N_ft terms. Three free parameters allow each term to be tuned (a) to empirically relate FRP to dynamical heat flux, and (b) to account for the stability structure profile, with entrainment and other possible contributing factors (wind shear, latent heat release, etc.) subsumed in the parametric fit. This is probably not the most direct way to represent the main factors involved, but as you mention, uncertainties in both the parameters and the parameterizations leave more advanced models poorly constrained, and the model applied here does provide enough flexibility so the available validation data can be reproduced, as Sofiev has shown in previous papers.

We fully agree that the Sofiev plume height parametrization does not represent a full physical plume model. Nevertheless, in global climate models the coarse horizontal and vertical model resolution represents the decisive factor which strongly limits a potentially superior performance of more advanced plume models, see discussions on page 6653, lines 1-11 and page 6666, lines 14-20.

8. Section 2.3, P6, lines 23-24 [P6653, lines 13-15]. I think that unlike the MINX plume heights, the MODIS thermal anomaly data in the MPHP dataset is just copied from the MODIS product. If so, you might want to make that clear in the description here; also relevant to P8, lines 2-3.

We complemented our statement in chapter 2.3 with the additional information suggested.

9. Section 2.3, P7, lines 14-15 [P6654, lines 11-13]. *Note that satellite observations have detection limits, so small plumes often go undetected in the MODIS thermal anomaly data, which skews the data set. Also, FRP can underestimate the fire radiant energy flux due to partly filled pixels [e.g., Peterson et al. Remt. Sens. Env. 2013, p. 262-279], due to the opacity of overlying smoke, and other factors. I see now that you get to some of this later in the paper…*

The opacity of overlying smoke and partly filled pixels is mentioned on page 6657, lines 17-24 together with the Peterson et al., 2013 reference. Following your comment, we reformulated one sentence and added the reference Kahn et al., 2008 which describes the smoke-opacity problem.

10. Section 2.3, P7 [P6684]*, Figure 1. This is a neat figure in that it gives a qualitative sense of plume distribution and FRP diversity. But it is really difficult to see what the height distributions within the cluster actually look like. You might think about how that information might be conveyed, either within the figure, or as a separate plot or table.*

We added your statement that the figure only aims to provide a 'qualitative sense of plume distribution and FRP diversity', see page 6654, lines 8-9. However, at this point of the paper we do not aim to include more detailed information about plume statistics for Figure 1 as the visualization only shows an excerpt of the entire MPHP data set. More detailed information on plume statistics for the MISR data set are provided later by Table 4, Figure 3 and Figure 4 in the results section.

11. Section 2.4, P8, lines 18-19 [P6655, lines 12-13]. *I would think that a more important reason not to have too many FRP bins is that plume injection height is only loosely correlated with FRP (e.g., Val Martin et al. 2012, Figure 7), so the added FRP-bin precision would represent an over-interpretation of the associated injection height accuracy.*

The FRP bins scheme which we introduce represents a conceptual approach to implement a global FRP data set into a global coarse resolution model. However, this scheme could potentially also be applied for regional modelling combined with a more advanced plume rise model, if high resolution input data is available. For FRP bins which describe fires of 0-100 MW, the differences in plume heights predicted by the Sofiev parametrization are indeed only a few meters up to some hundred. Thus, the importance for global modelling approaches turned out to be very limited. This is one result of our study.

Following your suggestions, we have already added a statement in this paragraph of the ACP discussion manuscript to clarify that the FRB bins scheme only represents a conceptual framework to efficiently calculate injection heights, see Page 6655, lines 10-11.

12. Section 2.5, P11 [P6658], top paragraph. Although MODIS FRP can be underestimated due to smoke opacity at 4 microns, other factors also contribute to the underestimation of injection height that are not due to underestimation of the Fire Radiative Power (i.e., the TOA 4 micron radiance) itself, such as low fire emissivity.

We extensively scanned the literature, but unfortunately we were unable to quantify the FRP underestimation due to low fire emissivity any further. We will mention in the revised manuscript that such observational estimates would be highly desirable for future applications.

13. Section 3, P13, lines 6-7 [P6659, lines 20-21]. This conclusion is very similar to that for the Freitas model, as studied by Val Martin et al. (2012) with a range of fire area and heat flux input combinations.

In the ACP discussion manuscript, we explicitly mention the similarity of our results to Val Martin et al., 2012, see page 6659, lines 21-23.

14. Section 3, P13, lines 14-15 [P6660, lines 4-6]. This might be related to the degree to which the FRP must be scaled up to adequately represent the buoyancy produced by the fire. The 30% uncertainty is a small part of the actual uncertainty in the buoyancy that FRP is used to represent (e.g., Equation 6).

Basically we agree to your statement. However, the implicit representation of buoyancy in the Sofiev plume height parametrization does not enable us to further analyse the uncertainty in buoyancy and FRP in the framework of this study. We will add a statement in the revised manuscript to stress the fact that buoyancy, meteorology and heat release represented by FRP are not completely separable in reality as the Sofiev representation might suggest.

15. Section 4, P18, line 6 [P6663, lines 8-10]. From P12, lines 7-8 [P6659, lines 1-2]: The assumed diurnal cycle "distributes 80% of the FRP constantly during daytime (8–18 Z local time) and the remaining 20% during nighttime (18–8 Z local time)." How applicable is it to the entire global distribution of fires, especially those that inject above the boundary layer, and how strong is your conclusion that the diurnal cycle of FRP is of minor importance (e.g., Abstract, line 19)? I realize it primarily affects the vertical smoke distribution of the largest fires.

We apologize for a mistake in our references list. The original manuscript was lacking the reference Zhang et al., 2012b, JGR, doi:10.1029/2012JD017459 which is different from Zhang et al, 2012a, ACP, doi:10.5194/acp-12-8911-2012. We replaced the wrong citation by the correct one in the ACP discussion manuscript. In Zhang et al., 2012b, Figure 7, it is shown that a similar diurnal cycle in FRP applies to many regions although some regional differences exist. Our application of a diurnal cycle as 8-18 Z vs. 18-8 Z based on the statement by Zhang et al., 2012b (P9, 3.3) is a crude simplification, but it sufficiently represents the impact of a diurnal cycle on the parameters we discuss.

16. Section 4.2, P20 [P6690]*, Figure 7. I'm wondering what is assumed about the amount of smoke injected by the smaller and larger fires. Wouldn't this affect the appearance of this figure, as it seems that both amount and vertical distribution should influence the weighting of contributions to the curves?*

In Figure 7, the mean relative emission distribution has been weighted with FRP and thus also with the emitted aerosol mass, see Section 4.1. E.g. if the total FRP of a fire is 1 MW, the emission profile is weighted by a factor of 0.01 compared to a fire with 100 MW which is weighted by a factor of 1.00. The weighting is explicitly mentioned on page 6662, lines 2-7.

17. Section 4.4, P22, lines10-11 [P6666 ,lines 10-11]*. From my reading of their paper, Mims et al. (2010) had only a few plumes, not a statistical sampling. I think Diner et al. (2008) reported on an early version of the North America dataset analyzed more completely in Val Martin et al. (2010).*

Thank you very much for pointing out the Diner et al., 2008 paper, we added it to the Val Martin et al., 2010 reference. Nevertheless, we keep the Mims et al., 2010 reference, because it includes plume heights of fires in vegetation types different from those in North America.

18. Section 6, P24, line 16 [P6668, lines 6-8]*. This conclusion probably says more the way the meteorology and FRP are represented in the empirical parameterization than on the physical factors that affect plume-rise; if you agree, it might be worth stating. Same for the conclusion stated on P25, lines 9-11* [P6668, lines 20-22].

We agree and we rephrased the first text passage in the ACP discussion manuscript according to your suggestions. For the second conclusion on potential future changes in FRP, we replaced 'will only marginally increase…' by 'might only marginally increase…' to express the speculative nature of this statement.