Author Response to Anonymous Referee #1

The authors would like to thank Referee #1 for his/her constructive comments. They helped to improve the clarity of the paper. In the following we list the referee comments together with our response.

General Comment 1:

Section 4, in particular, feels a bit too long, especially considering that many of the data presented are also shown in tables and figures. The authors may consider trimming parts of this section to help the reader.

We carefully checked section 4 for potential text passages which could be shortened without loosing essential information. We deleted / replaced the following lines (page and line numbers refer to the discussion manuscript):

- P6662, line 26, deleted '…which shows significant regional and seasonal variations…'
- P6663, lines 1-3, deleted first sentence 'In the tropics and subtropics plumes deeper …'
- P6663, lines 16-23, deleted entire text passage 'Interestingly, mean plume heights …'
- P6664, lines 21-22, deleted entire sentence 'Scenario SOFIEV-MODIFIED simulates …'
- P6665, lines 5-6, replaced 'Besides PBL height, the SP also takes into account FRP and Brunt-Väisälä Frequency leading to a less pronounced …' by 'The SP, which also takes into account FRP and Brunt-Väisälä Frequency, shows a less pronounced …'
- P6665, lines 25-27, deleted sentence 'In North America HAM2.2-STANDARD …'

We agree with the referee that the information provided by the listed text passages is either already included in the tables and figures or is not substantial for the conclusions of our paper. Therefore, we substantially shortened Section 4 by deleting these text passages in order to improve the general readability of the paper.

General Comment 2:

The authors rely heavily on the modified Sofiev plume height parametrization for their analysis. This modified SP model uses FRP as a major driver. As the authors are probably aware, there is not conclusive evidence that FRP is inherently tied to plume height (numerous studies contradict each other). This is probably due to saturation or obscuration remote sensing effects. Nevertheless, I have hesitations with this heavy reliance on FRP, and a word or two on this in the Summary or Conclusions section would probably help clarify.

As FRP is strongly correlated with the heat flux of a fire and thus with fire-induced atmospheric convection, FRP should naturally impact plume heights. Among others, Freitas et al., 2007 (ACP) have used the analytical PRM model to demonstrate the heat flux dependence of plume heights for specific atmospheric conditions and fire size, see Fig. 4 in Freitas et al., 2007. The authors show that increased heat fluxes directly entail stronger plume rise.

In our study we only see a limited increase in plume heights with FRP due to the specific representation of FRP in the Sofiev formula. For the majority of low or moderate intensity fires (0-100 MW) and the common range of Brunt-Väisälä Frequencies, a doubling in FRP is vastly damped by the exponent γ in equation (2). In contrast, as shown in Table 5, for the uppermost 10 percent deepest emission injections, a doubling in FRP entails substantial plume height changes of 300-500 m on average.

However, we fully agree that the reliance of plume heights on FRP *measured by remote sensing techniques* is much more uncertain in reality then the theoretical approach might suppose. The presumably most important sources of uncertainty in the FRP-plume height relationship are expressed on P6654, upper paragraph. Prior to this study, we tried to quantify this relationship for all plumes of 'good quality' in the MPHP data using NOAA-CIRES 20th Century Reanalysis Data. We found a weak but significant Pearson correlation of 0.25 for FRP and plume heights which is implicitly included in the computational data set that was used for the tuning of the SP described in Sofiev et al., 2012.

Nevertheless, we agree that a short statement on the uncertainties connected to the use of FRP as a driver for plume height calculations might improve the conclusions section. We will add the following statement after P6668, line 8: 'The reliance of plume heights on FRP in the Sofiev parametrization represents a very simplified approach which provides reasonable statistics on the global scale, but it may fail for the prediction of individual plumes.'

Specific comments

1) Page 6650 – lines 20-25. For the initial simulations you do no nudge, but for all subsequent simulations you do nudge, so perhaps line 22 should read: "For all other simulations : : :."

We will apply this change in our revised manuscript.

2) Page 6651 – lines 12-15 – why did the authors choose 4km? Was this an arbitrary height?

The upper limit of 4 km is an arbitrary value, but it represents the standard plume height implementation of ECHAM6-HAM2 described by Zhang et al., 2012a. In order to ensure comparability of our results to previous studies, we apply this standard implementation for one reference simulation, whereas improved plume height parametrizations are used for all other simulations.

We will insert this statement on page 6651, after line 17.

3) Page 6653 – As mentioned in "General Comments" – It is not explicitly clear that FRP is a reliable predictor of plume height. The literature supports this. This caveat should probably be suggested.

See general comment #2. In the revised manuscript, we will explicitly mention this caveat also on page 6653 at the end of subsection 2.2 as follows:

‘Although FRP is strongly correlated with the heat flux of a fire and thus with fire-induced atmospheric convection, the reliance of plume heights on FRP measured by remote sensing techniques is much more uncertain than the theoretical relationship between FRP and heat fluxes might suppose.’

4) Page 6653-6654 - It should probably be mentioned that the MISR plume height data you used was all digitized using the red band only. While no conclusive validation study exists (yet), there is evidence that the latest version of the MISR plume height digitization tool (MINX), which includes retrievals from the blue band, is significantly more accurate for thin plumes. This could be mentioned. The global data from 2008(which were not published prior to April 2012) include the blue band. It would be interesting to see a comparison of that new data with your ECHAM6-HAM2 simulations.

Thank you very much for this helpful suggestion. We will add the following statement after page 6653, line 21:

‘The MPHP data set used in this study is based on red band retrievals only as no blue band data was available. For future studies, an explicit validation of red and blue band retrievals is highly desirable, because for thin plumes blue band retrievals are expected to provide more accurate plume height estimations than red band retrievals.’

5a) Section 6 is more of a “summary” and less of a “conclusion.” For example – on page 6668, lines 1-10, the authors mention that the use of FRP and meteorological conditions improve the distribution of plume heights, but there is no mention of “why” this happens.

On page 6668, line 5-6, in sentence ‘A statistical-empirical correction for the Fire Radiative Power (FRP) of high plumes turned out to significantly improve the uppermost 10 % of the plume height.’, we will add the explanation ‘because this correction compensates the smoke opacity effects which reduce the detectability of FRP for intense fires.’ in the revised manuscript.

5b) Additionally, in lines 20-25, the authors mention that introduction of a diurnal cycle and a doubling of FRP did not substantially increase or modify plume height distributions. Do the authors have any insight on why this is the case? This conjecture would be particularly useful for the community. Certainly the model simulations offer some insight as to why a doubling of FRP does not significantly increase plume height?

We admit that our conclusions on page 6668, lines 20-26 are slightly imprecise. In order to account for the required clarifications, we will replace lines 20-26 on page 6668 in the revised manuscript by the following statement:

'As a result of the strong damping in the FRP impact on plume heights described by the Sofiev plume height parametrization, a hypothetical doubling in future fire intensity as well as the implementation of a diurnal cycle in FRP only marginally increase the vast majority of emission heights. Basic global plume height patterns are rarely affected by these changes in FRP, but for the uppermost 10 percent of all plumes, an average increase in plume heights by 300-500 m is simulated.

5c) Also, going back to my initial comments in "General Comments", if a doubling of FRP does not significantly increase plume heights in the simulations, then why is FRP used to "modify" the SP in the first place? It would seem like FRP has less of a connection to plume heights than the authors seem to suggest? Perhaps a sentence or three of discussion on this in the Conclusions would be helpful.

We implemented the FRP correction for plume heights >1500 m to compensate for the smoke opacity effect of deep injections which has been demonstrated by various authors, see page 6657, lines 14-26. The introduced increase in FRP is of limited importance for plumes below 3 km, but it has a significant impact on the very high plumes as we show in Figure 4, see page 6661, lines 1-6. Global plume height statistics of the highest percentiles of plumes get substantially more realistic if the FRP correction is applied. For the related changes, which we apply in the revised manuscript, see our general comment #2 and comment 5a) and 5b).