Authors’ comments to Referees no 1 and 2

The comments of the referee have been presented in blue font below, and our response as plain black text. We have considered at this point mainly the major comments of the reviewers.

Authors’ comments to Referee no 1

The paper presents recent developments implemented within an existing plume dispersion model for forest and pool fires, which aim to improve how the emissions source is parameterized within the modelling system. Given the sensitivity of plume rise and subsequent dispersion to source input parameters, estimation of buoyancy, mass and momentum fluxes is key to improving model prediction accuracy. Unlorturnaly, while this paper provides the means to estimate these parameters, it is missing key model evaluation to support the presented approach.

General Comments

Moving away from static inputs towards a more physical model for fire source parameters is incredibly valuable. The authors present an approach for estimating various source properties derived from the classic MTT model, which in my view constitutes the main contribution of the paper. However, no attempt is made to actually evaluate this “source term model”.

The inter-comparison study with RxCADRE data presented in the paper specifically excludes the source term model, focusing instead on the two previously-studied components of BUOYANT (plume rise and dispersion).

While such results are still valuable, they do not substitute for proper evaluation of the derivations presented in Section 2 and, in the current state, provide no supporting evidence for the main contribution of the paper.

Fortunately, RxCADRE dataset is incredibly detailed and can be used to extend the evaluation to include the source term model. My recommendation for the publication of this paper would, hence, be contingent on the authors demonstrating the results for the following:

Comparison of RxCADRE observations to:

BUOYANT model with **observations** as source inputs (this is essentially what’s currently included in the paper), with more details included on methodology (as per comments below)

BUOYANT model with **old** source term parameterization (fixed parameters)

BUOYANT model with **new** source term model included

Operational version of BUOYANT (if different from above)

We totally agree that it is unfortunate that the source term module could not be convincingly evaluated using the RxCADRE data. The reasons for this should be discussed in the revised manuscript. The main reason is that in the RxCADRE experiment, the actual trunks of the trees were not burnt; whereas the operational BUOYANT model assumes that this is the case.

According to the referee's advice, we have nevertheless also done a comparison of the source term model predictions with the RxCADRE data and presented those together with the results obtained using the original model (using measured input data, without the source term module). We suggest including also this comparison to the revised manuscript. This comparison could be useful to illustrate the potential use of the operational model in cases, in which the forest fire does not burn the actual main tree structures, i.e., the trunks of the trees.

We have also conducted a sensitivity study related to the model evaluation; we propose that this will be included to the revised manuscript. The sensitivity study evaluates the impact of the changes of input data to the predictions, especially regarding the influence of such input data quantities, the values of which were uncertain in the original RxCADRE data archive.

Unfortunately, at the time of writing the manuscript we did not find any suitable datasets in the publicly available literature for evaluating separately the source term model directly against experimental data (including burnt tree trunks). We therefore did what is possible in practise; i.e., we evaluated the whole model (although not specifically the source term module) against the best available dataset.

We also would like to mention that the main aim of developing the source term module for the buoyant plume dispersion model was actually to make the use of the overall model easier for the rescue personnel. The required input data for the refined model is indeed substantially simpler and easier to estimate, compared with those of the original model (i.e., the model without the source term module). The main aim was not to obtain more accurate predictions for all the conceivable forest fire cases with the revised modelling system. In major forest fires, which are the most important for rescue operations, also the tree trunks, or at least part of the tree trunks will be burnt. However, in all openly available prescribed burning forest fire experiments, that is unfortunately not the case. We suggest to describe these arguments more clearly in the revised manuscript.

Regarding the evaluation against data of large and complex models, it is fairly common that not exactly each and every module included in the overall modelling system will be separately tested. There might be several reasons for this, one of which is simply the lack of sufficient quality experimental data on that specific aspect of the model. In some cases, one has to satisfy to simply evaluate the whole model, including all the separate modules – although this process of course may not critically test all the modules included.

We totally agree with the reviewer that RxCADRE is a useful dataset. It has also been well documented.

Lastly, Section 4 of the paper is dedicated entirely to an overview of an operational modelling system. It is my understanding that the system is supposed to be accessible online, however no links are provided in the paper (aside from those pointing to an offline archive of the Fortran source code for the BUOYANT model). My current review of Section 4 is, hence, fairly superficial. If the authors are unable to provide access to the model for peer-evaluation, my recommendation would be to exclude this section from the manuscript.

We apologise for not specifying a link for the operational model version. The link is http://ilmanet.fi/order/147135, user name: GMD, password: FLARE21!. A slight problem is that the FLARE user interface has been currently coded only in Finnish. However, we will provide the translations of the relevant texts to English as a separate document. We believe that this would give the reviewers a sufficient picture on the functioning of the software.

Short user guide in English (attachment).

Page 1: Login, user name: GMD, password: FLARE21!

Page 2: General information concerning the location of the burning and time of forest or pool fire. E.g., select 'Forest fire'.

Page 3: Editing of weather and forest parameters. Weather parameters are produced automatically by the by NWP model HARMONIE.

Page 4: Same as 'Page 3', but now ‘Extended weather parameters' selected.

Page 5: 'Pool fire' option selected.

… If the agreement was great, why would BUOYANT need improvement? What were the limitations? (specific comment)

The main aim of developing the source term module for the buoyant plume dispersion model was practical, to make the use of the overall model easier for the rescue personnel. This has been elaborated above.

The limitations of the BUOYANT model include: (i) The model assumes a steady state in terms of emissions and meteorology. However, the user can easily conduct multiple runs with various values of the emissions and meteorological parameters, to evaluate the impacts of changing emission and ambient conditions. (ii) The current model version does not treat the impacts of phase changes of water in the plume (in particular, condensation and evaporation). (iii) The model adopts some values of model parameters according to the best available previous experimental and modelling studies. (iv) The chemical reactions of pollutants during the source term and plume rise stages are not addressed.

We suggest that these limitations will be better described in a revised manuscript.

Authors’ preliminary comments to Referee no 2

General Comment:

The paper presents the development of a source term model that evaluates the fire plume properties just above the flame as an extension to the previously published BUOYANT model for the dispersion of buoyant plumes from wildfires and liquid pool fires under varying atmospheric conditions. The refined BUOYANT model v4.20 is then evaluated against observational data of CO2 concentrations from aircraft measurements during a wildfire experiment from the RxCADRE campaign. The model captured well the vertical profiles of CO_2, while the highest concentrations were moderately overpredicted. The authors also state that the widths of the plumes are slightly underestimated, without giving an explanation for this behavior.

We will expand the discussion on the differences of model predictions and measured data in the revised manuscript, including especially the widths of the plumes.

Further, an operational version of the BUOYANT model, called FLARE, is briefly presented, although it remains somewhat unclear how it is related to the research version of the model.

This is a good comment and deserves clarification in the manuscript. These model versions are closely connected with each other. Both the operational version of the model (named as FLARE) and the original research model (named as BUOYANT) use an identical code for the dispersion and transport of a buoyant plume. The operational model includes the research code in full, and all the core physics computations in the operational model are done using the research code. However, there is a dedicated user interface in the operational model, to facilitate an easier operational use. These connections will be explained in more detail in the revised manuscript.

In more detail, the differences of FLARE and BUOYANT are:

(a) FLARE uses as default the presented source term model; it does not therefore allow the user to specify the source related input in the more complex format (as in the alternative (ii) in the above paragraph).
(b) The specification of the meteorological conditions in FLARE is determined by the used numerical weather prediction model, in BUOYANT, this can be done also in various other ways.
(c) Output of BUOYANT is more versatile and can be adjusted by the user, compared to FLARE.
(d) BUOYANT allows the user to post-process the model results at will. FLARE includes a standard format post-processing.
(e) FLARE can be used with a restricted set of web browsers, while BUOYANT is designed to be as platform independent as possible.

These differences should be presented more clearly in a revised manuscript.

Currently, the evaluation against the experimental data from the wildfire plume is performed with the refined BUOYANT model, but not including the extension with the new source term model. It is a bit unfortunate that the validity of this essential new module has not been demonstrated. My recommendation is to carry out a dedicated sensitivity study of the possible input value ranges in the source term model and after interfacing it with the BUOYANT model, comparing to the experimental data.

We totally agree that it is unfortunate that the source term module could not be convincingly evaluated using the RxCADRE data. The reasons for this will be discussed in the revised manuscript. The main reason is that in the RxCADRE experiment, the actual trunks of the trees were not burnt; whereas the operational BUOYANT model assumes that this is the case.

However, the reviewer's suggestion of a sensitivity study is very useful. We have already conducted such a sensitivity study regarding the impact of the changes of input data to the predictions. Such results are useful and illustrative, especially regarding the influence of those input data quantities, the values of which were uncertain in the original RxCADRE data archive.

We have also included a comparison of the source term model predictions with the RxCADRE data. Although the amount of wood burnt cannot be accurately evaluated with the new source term model in that case, this comparison could be useful to illustrate the potential use of the operational model in cases, in which the forest fire does not burn the actual main tree structures, i.e., the trunks of the trees.

Nevertheless, I think that the development of a physical model that can deal with the early evolution of the fire plume, the plume rise, and the local atmospheric dispersion is of great value for achieving advances in the prediction of impacts from major natural and man-made fires. Overall, the paper deserves publication after my general comment and the specific comments below are sufficiently addressed.

Thank you for the pertinent comments.

Specific Comments:

1.) P. 2, lines 48-51: It is mentioned that hundreds of chemical compounds are emitted into the atmosphere during wildland fires. Table C1 also lists a number of different fuels for which the convective heat flux and mass fluxes during a liquid pool fire can be calculated. How much of the combustion chemistry and oxidation is BOUYANT capable of simulating?

The mass burning rates (i.e., rate of mass burned per time) from a liquid pool or forest fire are estimated with semi-empirical correlations (as described in detail in Appendix C of the manuscript). Estimates of substance-specific fire products (e.g. mass flux of CO_2) are obtained from the modelled mass burning rate and experimentally derived emission factors. Currently applied substance-specific emission factors for liquid pool fires are shown in Table C1 of the manuscript. For forest fires we have applied the substance-specific emission factors presented by Kaiser et al. (2012).

However, after the emissions have been modelled for a range of chemical compounds, the model does not address chemical reactions within the source term and the plume rise regimes. The model can of course predict the plume properties after those regimes and then be coupled with dispersion models that address chemistry. For instance, the BUOYANT model has previously been used in that way in combination with the chemical transport model SILAM (which contains a chemistry sub-model including more than hundred chemical compounds).

As far as we know, none of the publicly available plume rise models treat explicitly the chemical reactions during the plume rise regime.

2.) I suggest to revise the paragraph on CFD models in the Introduction (P. 3, lines 70-80) to address the treatment of plumes from the two different types of fires: wildfires and liquid pool fires. Currently, only the dispersion models for treating liquid pool fires are described. Further, the respective description should deal with above-fire (source term), plume rise and large-scale dispersion for the two fire types.

We will revise this paragraph to be more clear and to address also forest fires. The CFD models have been used for estimating liquid pools fires, as described in the manuscript. However, we are not aware of studies, in which these would have been used for analyzing forest fires. However, the physics of such models is the same irrespective of the source of the buoyant plume. That is the reason for using only one computer module for analyzing the plume rise regime (after the source term regime), also in the BUOYANT model. The source term model evidently has to be different for the forest fire and pool fire cases.

At which point, i.e. distance from source and vertical layer, does a large scale atmospheric model take over?

The criteria for the termination of the plume rise stage have been described in detail in our previous article, Kukkonen et al. (2014) (section ”2.1.6 Criterion for the termination of plume rise”). The criterion used in the BUOYANT model is the following: ”In the current model version, we have chosen simply to use the height, at which buoyancy force first vanishes as the final rise height of the plume.” In short, when there is no buoyancy left in the plume, the model makes the transition from the plume rise regime to the dispersion modelling regime.

This distance (and the corresponding height) clearly depends a lot on the intensity and area of the fire, and on the meteorological conditions. In the RxCADRE model simulation presented in the article the plume rise stage comes to an end at the distance of 6.2 km from the source, at a height of 510 m (the height at the plume centerline).

3.) Section 2.2: it is not really clear from the descriptions in this section, how the new source term model for evaluating the properties of the fire plume above the flame tips is interfaced with the plume dispersion in the BUOYANT model.

This is a good point. This transition has been described in the section 2.2.4 (from the source term to the plume rise regime). However, the description is probably too brief and general. We will add a more specific description to that section. This has actually been described in our previous article Kukkonen et al. (2014), quote: “Information on the source term (to be input to the plume rise module) includes the following: the source radius, the source height above the ground, the temperature of the released mixture of contaminant gas and particles, and air, the mass flux of this mixture, the mass fraction of the released gas, and the molecular weight and heat capacity of the released gas.”.

4.) Section 3.3: as it is now, the evaluation is done for the BOUYANT model without the source term model. This may appear reasonable at first since the fire properties for the selected wildfire case L2F are well characterized by the observations. However, in order to evaluate the source term model presented in this paper, it would make sense to conduct a sensitivity analysis, studying the probable value ranges of selected fire parameters, and comparing the outputs of the BOUYANT model - including the source term sub-model - against the measurements in L2F.

Yes, we agree. Our plans to remedy this have been described above.

5.) Section 3.3.1: which method was used to evaluate the meteorological parameters on P. 18, lines 458-464, for use in the BOUYANT model?

The methods to evaluate the Monin-Obukhov length, and the vertical profiles of wind speed and temperature are briefly described in the second paragraph of the section 3.3.1. We suggest to elaborate these in a revised manuscript.

Prevailing wind direction was assumed to be the arithmetic mean of the experimental wind directions at the heights 6.2 m and 990 m. Atmospheric pressure was determined utilizing the measured ambient temperature profile and the hydrostatic assumption. The measured vertical temperature profile indicates (by visual inspection) a boundary layer height of approximately 2.2 km.

6.) Section 3.3.2: a table should be provided that contains the required input parameters and the values used in the evaluation both for the BUOYANT model without the source term model (currently listed on P. 19, lines 483-484) and for the BOUYANT model when using the source term model.

This is a good idea, and will clarify the model application. We have compiled tables containing the input parameters of the model, as used with and without the source term model. We suggest to add these to the manuscript.

7.) P. 23, line 536: the temporal evaluation should also be shown in a plot, for example at 450 m height above ground at the plume centerline.

This is a reasonable suggestion. However, we suggest to remove the sentence on line 535: ”However, the temporal agreement of the measured and modelled highest concentrations was good.” The aircraft measurements represent centered moving average over 20 seconds, at certain average three-dimensional locations. The exactly corresponding spatial and temporal averages were computed with the model and compared with the measured values. Although both of these have been carefully performed and archived, it is therefore in our view an over-statement to write something about the temporal evolution of measurements vs. data (as both the time and location are changing simultaneously). However, we can compare the measured data and predictions for certain flight manoeuvres, such as the crosswind distributions in Fig. 4.), but not really versus the time.

8.) Section 4: I think the presentation of the operational version of the model should be placed before the evaluation chapter 3 to make it more visible to the readers.

Yes, this can be done.

The operational version FLARE needs to be better related to the research version of the model. Does it use the new source term module?

This has been described above in response to the reviewer’s general comments.

Information should be added about the stakeholder groups that are targeted as potential users. It would be nice to include a screenshot from a real-world example application.

Yes, these will be added to the manuscript. The stakeholders and users of the operational model include currently a wide range of emergency response personnel in Finland, the operational meteorologists at the Finnish Meteorological Institute (FMI), the Ministry of the Interior in Finland and researchers at the FMI. This user group could be expanded to other countries in the future.

9.) P.28, lines 689-690: statement “for most of the highest concentrations” seems to contradict with the finding of moderate overprediction of the highest concentrations.

Yes, quote: “the model captured well the observed vertical excess concentration distributions” is an overstatement and will be revised. The model predicted the heights for the ”parking garage” (PG) measurements, there were 11 measured or predicted peak values. Six of these were over-predictions and 5 were under-predictions by the model. In addition, the model predicted one peak that was not found in the measurements. The model under-predicted the measured data in PG#1:n and over-predicted those in PG#2 and PG#3.

The sensitivity analysis that we performed after submitting the manuscript actually improved our understanding of the reasons for these deviations of data and predictions.

Technical Corrections

Figure 4, middle plot: the green curves from modelling and measurements do not show well.

We have corrected Figure 4b.

References

Kaiser, J.W., Heil, A., Andreae, M.O., Benedetti, A., Chubarova, N., Jones, L., Morcrette, J.-J., Razinger, M., Schultz, M.G., Suttie, M., and van der Werf, G.R.: Biomass burning emissions estimated with a global fire assimilation system based on observed fire radiative power, Biogeosciences, 9, 527-554, doi:https://doi.org/10.5194/bg-9-527-2012, 2012.

Kukkonen, J., Nikmo, J., Sofiev, M., Riikonen, K., Petäjä, T., Virkkula, A., Levula, J., Schobesberger, S., and Webber, D.M.: Applicability of an integrated plume rise model for the dispersion from wild-land fires, Geosci. Model Dev., 7, 2663-2681, doi:https://doi.org/10.5194/gmd-7-2663-2014, 2014.