

Response to the comments of anonymous referee #1

The comments of the referee have been presented *in italic* below, and our response as plain text.

Review of gmd-2013-188, Applicability of an integrated plume rise model for the dispersion from wild-land fires

Initial remarks

This manuscript presents a brief description of attempts to validate a plume-rise model with data from two field experiments. The presentation is poor in places, and the model comparisons are not particularly thorough, conclusive or well-executed.

There seems to be a misunderstanding of the aims and scope of this manuscript. The manuscript has two main aims, as stated in the introduction: (i) to present the plume rise model called BUOYANT, and (ii) to evaluate its applicability to simulate major wild-land fires in two prescribed burning experiments. The aim is not only to validate/evaluate the model. Actually, it is not even possible to thoroughly validate a model, using only data from two specific source and meteorological cases.

Due to the numerous challenges in organising major field experiments, especially in case of wild-land fires, it is also very difficult or impossible to derive conclusive evidence on model validity. It was not the aim of the manuscript to obtain conclusive evidence on e.g. whether the model structure is optimal or not. The underlying reason for this is the difficulty in measuring or determining the detailed source properties and the meteorological conditions in such field experiments.

However, comparing the model predictions using the best available input data with observations may still be useful for a range of reasons. The results illustrate the challenges and uncertainties in estimating the source terms and the atmospheric conditions for estimating the plume rise, which is expected to be useful for planning of new prescribed burning experiments. We have also discussed both the advantages of the model and the challenges in modelling plumes from major fires; this is expected to be valuable information in developing and refining such models internationally. The comparison of modelling with the measured data of the Finnish (Hyytiälä) experiment is also presented in this article for the first time.

It would be straightforward to revise the manuscript to be more thorough, as we have suggested in this response text below. For instance, the experiments could be described in more detail, more extensive sensitivity analyses on the importance of the range of input data values could be added,

and the discussion of the model and the results could be elaborated and improved at some places (details below).

Little attempt is made to set this work in context. I have confined detailed criticism of the paper to the substantive sections 2 and 3, as set out below.

It is not totally clear to us, why the reviewer states that the study has not been placed in context. The evaluation of plume rise models for major fires has been very scarce in published literature, especially against well-reported prescribed wild-land fire experiments, such as those presented in this paper. We have therefore made references to the limited number of previous studies in this area (in the introduction). It would be helpful, if the evaluator would specify, in which respect he/she feels that the study has not been placed sufficiently in context.

Section 2, Materials and methods

Section 2.1 describes the fire experiments. There is insufficient description of which quantities were measured, where and how.

The detailed descriptions of measurements in both the Quinault and Hyytiälä experiments have already been published in previous articles. We have stated this in the manuscript and made references to these articles. For instance, an article was recently accepted to be published in ACP journal that includes a detailed description of measurements in the Hyytiälä fire (Virkkula et al., 2014a; detailed references given at the end of this response). There is also a new submitted manuscript that focuses only on the airborne measurements in the Hyytiälä study (Virkkula et al., 2014b).

In summary, there were a lot of measurements in both experiments, and their complete description would require a lot of space. We therefore felt that it would be sufficient to give an overview of these measurements, especially as the detailed descriptions have already been published. However, we would be happy to add more detailed descriptions to this manuscript regarding the key measurements in view of this modelling study. However, we do not feel that the above would be a fatal error that would justify a rejection of the paper.

The text highlights the difficulty in estimating the source strength and evolution of large fires. For the burning experiment in Finland, if the area density of organic material is known to only one significant figure, then the total amount of organic material burned should be quoted to the same accuracy (p468, lines 24–25).

This point reads at the moment (p488): “The amount of burned organic material was approximately 46.8 tons (i.e., 60 tons ha⁻¹).” The area of the burned site was 0.806 ha. This should read: “The amount of burned organic material was approximately 46.8 tons (i.e., 58.1 tons ha⁻¹).”

Section 2.2 describes the model to be validated.

Subsection 2.2.1 indicates that the presentation is restricted to near and intermediate field dispersion, and subsection 2.2.2 describes a small amount of previous model evaluation.

Subsection 2.2.3 presents a rather simplistic conversion between source mass flux and source convective heat flux. This does not contain any discussion of source entrainment of ambient air, nor of the acceleration of the plume gases from rest by the action of the buoyancy force.

Equations (1) and (2) essentially define input parameters of the source, for the plume rise model. The equations are very simple, as these describe only the relations of model input values to each other, not the consequent plume evolution. Equations (1) and (2) can be used for estimating the initial vertical velocity (or mass flux) of the effluents, once the convective heat flux has been evaluated.

The input values regarding the source have been clarified in detail on p. 500, lines 25-30: “Information on the source term 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.” In short, the mass flux from the source is defined to be an input value for the model. We suggest to revise the manuscript to be more easily understandable, by starting with these basic rationale and statements on the model input values, before presenting any equations.

We have of course allowed for the entrainment of ambient air to the plume and its acceleration in the equations that describe the plume evolution, i.e., equations (9) and (10) in case of entrainment, and equations (11) and (12) in case of momentum fluxes.

The mass flux and/or vertical velocity in this context is a scaling quantity at best. Consideration of e.g. the vertical lengthscale over which the source fluid accelerates would be relevant to the later attempted determination of the source vertical velocity using a point measurement (p507, lines 9–10).

It is difficult to accurately measure the temporal variation of the mass flux or vertical velocity values in a field experiment. We agree that in this sense it is a scaling quantity.

The substance of the model is presented quantitatively in subsection 2.2.4.

The first part is a statement of the formulae to derive atmospheric profiles, which are different to those in an earlier version of the model. The motivation for this change (p490, line 21) is to use more up-to-date results, although the citations for the new profiles are all at least twenty years old.

The motivation for presenting the structure of the model is that we wish to make the model more widely known, and publish it in a reviewed journal. It also makes sense to present the latest version of the model, and its main constituents, one of which is the treatment of the atmospheric structure. Clearly, it is not the only motivation to present these atmospheric profiles. The model actually contains many other parts that are more innovative, such as, e.g., the treatment for the penetration of inversion layers.

It is correct that the references of this section are not new. However, these references are still valid; their older publishing years do not make them invalid by any means. If the reviewer will know of better references on this topic, we would be very interested to know, which ones those are.

Our practical suggestion is to add some concrete motivation on why these exact profile equations were selected. E.g., we could add after eqs.(7) and (8): “The above equations ((7) and (8)) take into account the different efficiencies between the exchange of heat and momentum in the intermittent regime, while avoiding the total vanishing of turbulence in very stable conditions (Blümel, 2000).”

The model does not appear to have the capability to use a more detailed atmospheric profile e.g. from a sounding or weather model.

This is correct, and it would probably be useful to state this in a revised manuscript. However, in operational cases as well as often in diagnostic evaluation of past cases, it often happens that there is no well-representative sounding data available. In our view, the presented method provides a scientifically justifiable method to estimate atmospheric profiles, which is at the same time simple in terms of input data.

The second part presents an “overview” of the plume model “for readability” (p494, line 21). For derivation and justification of the model, the reader is referred to a 277-page internal report dating from 1997 rather than a publication in a peer-reviewed journal.

This manuscript is supposed to be the first reviewed reference on this model in the future. However, it is seldom possible to present all the model equations and details in a journal article, due to, e.g., space limitations. It is therefore useful to have a more extensive reference that presents more comprehensively the model equations and details, background material and discusses various modelling options; this is the Martin et al report. It is common practise to publish a more extensive report first, and a more concise journal article later on.

We of course do not expect that the model would be justified only by publishing the report of Martin et al.

The implication is that the formulation of this model (as opposed to its output) has not been critically evaluated, nor is critical evaluation invited here despite the fact that there is at least some controversy (p496, line 19) about its components.

It is correct that the model has not previously been published in a reviewed journal. Its critical evaluation has been invited here.

It is correct that there is some so-called controversy (this refers here to uncertainties regarding its optimal values) about the added mass term (k_v), as we noted on p496, line 19. Added mass term is defined by analogy to the behaviour of a line thermal (p496, lines 19-21). We have therefore allowed for all the theoretically possible values for this term in the numerical computations.

However, the handling of this specific small detail (k_v) does in no way invalidate the model. First, we can numerically evaluate the effect of this uncertainty, and it has been shown (later in the article) to cause only a minor inaccuracy of the model performance. Second, such an uncertainty is present in all the models that treat momentum fluxes in the manner described in equations (11). It is normal that a complex model like the present one contains some input values that are not known with absolute certainty.

It is arguable that neither of the available presentations of this model, here or elsewhere, is adequate for a scientific readership.

It has not been specified by the reviewer, what exactly would be such a fatal error in the model structure that would justify a rejection of this manuscript.

Another significant omission is that the plume equations are not described in the context of the large body of published plume research (and especially other integral models of this type) during the last fifty-odd years. Morton et al. (1956) and Ricou and Spalding (1961) are the only other references cited in this part of the paper.

We have presented numerous references on plume research and other integral models, and discussed these in the introduction (this covers most of page 485). Recently, also comprehensive overview articles on these topics have been presented; we have referenced the reviews of Devenish et al. (2010) and Jirka (2004). Both of these discuss in detail the history, development and application of integral plume rise models. We felt it somewhat unnecessary to repeat those discussions here.

It would of course be possible to add more discussion on these models also to the later sections.

The model as described appears to have some limitations in terms of its application to open-air fire modelling. It assumes a steady state (p495, line 4), it does not allow for directional wind shear (p494, line 17), and it does not account for the effects of latent heating (p494, lines 19 and 24; also p491, line 26).

It is correct that the model has these limitations, and we have felt it fair to present and discuss these openly in the manuscript.

Subsection 2.2.5 discusses the criteria for the termination of plume rise. The text exemplifies the tendency throughout the manuscript to concentrate on plume rise through stable air. There is no discussion of the plume-lofting effect of large-scale eddies in the convective boundary layer, despite the fact that at least one of the two experimental fires takes place in a moderately unstable surface layer (p506, line 22).

It is correct that one of the experiments (Hyytiälä) occurred in slightly unstable conditions. However, the model can handle both stable and unstable atmospheric conditions (as noted on p494). Stable conditions and inversion layers are interesting, as e.g. a failure to penetrate an inversion may result in high near-ground level concentrations.

Subsection 2.2.6 indicates that the model requires input information on the source temperature and the source mass flux. These data are difficult to estimate even for the controlled fires described later, and presumably the difficulty and uncertainty is magnified for accidental fires.

It is correct that the determination of the source term for prescribed burning experiments is a challenging task (we have specifically discussed this matter on p. 507, line 24- until page 508, line 5), and this is even more difficult for accidental fires. However, the basic starting point of plume rise models is that some source properties are given as input; this cannot be avoided for this category of models.

There is no discussion of the expected sensitivity of the model to uncertainty in these input parameters.

This is not correct. We have defined several scenarios using various input parameters, see p. 508, lines 6-14, and Table 1. The results of these computations were presented in Figures 6a-b and 7. These analyses are a crucial part of the analysis of the results.

In addition, we have determined the sensitivity to meteorological (model input) conditions using two alternative methods, and evaluated the resulting differences.

The statement of input requirements given in subsection 3.1.2 (p502, lines 7–11) lists the convective energy release from the fire, rather than the source mass flux, as the required parameter, and states that it is “the most important source parameter in terms of final plume rise”. This is presumably a reference to model sensitivity tests, the results of which are not presented.

Yes, we have performed a substantially more extensive collection of sensitivity tests, than the ones currently presented (and referred to in our comment above). However, we decided not to present all of these, to avoid writing an excessively long manuscript. However, we would be happy to include at least the most important of these to the revised manuscript.

Section 3, Results and discussion

Section 3.1 discusses the SCAR-C experiment.

Subsection 3.1.1 and figure 2(b) indicate that there are approximations made in the creation of simple profiles for model input.

The vertical profiles of wind speed and temperature required by the model were determined both by applying the on-site measurements and the ERA-40 meteorological re-analysis data (Uppala et al., 2005). This approach will provide for an estimate on the uncertainty associated with the determination of meteorological input data for the models.

The methods for determining vertical meteorological profiles always include some inaccuracies, depending on the input meteorological observations and the computational method. It is not possible to determine such profiles without some approximations.

Subsection 3.1.2 presents an estimate of the plume source strength. Regarding the value for the convective fraction of heat release, it is perhaps odd that this is chosen as 0.55 for the reason that is in the middle of the accepted range of 0.4–0.8 (p502, lines 26–28).

It is correct that the value of this fraction is uncertain. As there is no evidence for favouring the relatively higher or lower values, we have simply selected the average of the accepted range (0.55). The value of 0.55 is also equal to the value chosen and applied by Trentmann et al. (2002) for their

simulation of the very same SCAR-C experiment. In our view, it would be more odd to select e.g. the highest possible value, without any physical reason for doing so.

Freitas et al. (2010) applied the same fraction (0.55) for two deforestation fires with sizes of 10 and 50 ha in the Amazon basin. They reason the choice as: “The fraction of the total energy that is effectively available to the plume convection depends on the ambient and fuel conditions and is highly uncertain. Here we use a value in the middle of the commonly accepted range of 0.4–0.8 as described in Trentmann et al. (2002).”

Trentmann et al. (2006) wrote: ”There is significant uncertainty in the literature on how much of the energy, released by combustion, contributes to local heating of the atmosphere (sensible heat flux) and is available for convection, and how much of the energy is lost due to radiative processes. Commonly found estimates for the radiative energy are between nearly zero percent (Wooster, 2002; Wooster et al., 2005) and 50 % (McCarter and Broido, 1965; Packham, 1969). These estimates are based on laboratory studies or small scale fires and their application to large scale crown fires resulting in pyroCb convection remains highly uncertain.”

The actual fire strength and extent is not steady, and the model inputs are chosen to be the maximum convective heat flux and source area. There is no discussion of the merit of these choices compared to mean values, for example.

It is correct that a prescribed burning (or accidental burn) can strictly speaking never be in a steady state. We agree that it would be reasonable to add argumentation on why the reported maximum values were used. One reason for this choice is that the maximum (heat flux and source area) values would be expected to correspond to the highest detected altitudes of the plume. We would also suggest to add some additional simulations also for the Quinault case in a revised manuscript, using other probable source input values (e.g., mean input values). We have already included two possible sets of meteorological input values.

(In several places the manuscript emphasises the lack of model tuning, however subjective choices such as these are arguably a form of tuning of the input parameters.)

It is therefore generally recommendable to use a range of input values, corresponding to the uncertainties of their determination, instead of only a single set of input values.

Model plume-rise comparisons are discussed in subsections 3.1.3 and 3.1.4. Presentation of the results is limited to one figure (3), which indicates very little other than that there is large uncertainty in both the measurements and model predictions.

Yes, it is evident from the simulations in Fig. 3 show there is a major uncertainty in the modelling, due to differing meteorological data. However, it should be noted that the vertical bar denoted by ‘LIDAR’ (the observations) shows the measured range of the plume (it is not a measurement uncertainty). The vertical bar denoted by ATHAM is the extent of the plume predicted by that model. We could improve the wording here to state this more clearly.

There is one important lesson to be learned from Fig. 3: the applied method for determining meteorology has a major role. The fig. illustrates the differences in the injection height when two different sources of met. data are applied: one measured ‘on-site’ and the other obtained from a re-analysis of meteorological data. On-site met data will commonly not be available in an emergency situation, and some other source of data has therefore to be utilized.

Section 3.2 discusses the Finnish experiment.

Subsection 3.2.1 details the background atmospheric profile. Subsection 3.2.2 provides some details of ground measurement which ought to be in section 2. The lack of representativity of the point measurement of fire temperature and vertical velocity is discussed briefly.

The second sentence above: The basic idea in the manuscript was to present first overviews of both prescribed burning experiments in section 2, and then description of the determination of detailed model inputs in section 3. This treatment has been written consistently both for the Quinault and Hyytiälä experiments. However, it would be OK to move some parts of this section to section 2.

In subsection 3.2.2 the text refers to the “substantial temporal variability” of the computed convective heat flux (p507, lines 15–16) and figure 5 indicates that 1-minute averaging substantially reduces the peak value of convective heat flux density by approximately an order of magnitude. From equations (1) and (2), figure 5 and table 1, it appears that the peak value from the raw 10 Hz data, approximately 550 kWm⁻² is used to obtain the value of the convective heat flux

Q_c in cases 1 and 2, and the the peak value from the 1-minute averaged data, approximately 60 kWm^{-2} , is used in cases 3 and 4. (The caption of table 1 refers to “maximum during one minute”).

Yes, this is correct. ‘Maximum during one minute’ refers to average values during one minute integration time, as presented in Fig. 5. The convective heat flux values for these cases are presented in Table 1.

For a steady-state plume model, the use of the maximum instantaneous value is surprising, particularly so given that even one-minute averaging substantially reduces this value. In the context of the model presented this is equivalent to assuming, among other things, a sustained updraught of 7.3 ms^{-1} at a height of 10m over an area of 0.4 ha. In reality the parcel of buoyant air produced by this peak value, if correct, may behave more as an isolated thermal than part of the main body of the steady plume. Given the model results presented in figure 6, there is a temptation to conclude that unrealistically large values have been used in an attempt to improve the comparison with the particle number-concentration data.

We have argued the selection of these input values starting on p. 507, line 24: “As input for the plume rise modelling, we would ideally need spatially representative measurements of the fire temperature and vertical flow speed. This implies that the measurement site for these quantities should ideally be situated in the middle of the formed fire plume at all times. Clearly, this was not possible in the present experimental set-up, as only one permanently positioned site was available in the middle of the burned area. A practical solution is to select as model input the maximum measured values of the fire temperature and vertical flow speed, either directly from the measured data, or using first a selected temporal averaging of the measured data. A similar approach has also been used in case of the Quinault fire in several previous studies (e.g., 5 Trentmann et al., 2002), and in this study.”

In short, the measured values do not correspond to the maximum heat fluxes most of the time, maybe even all of the time. Measured values were obtained from one fixed location that seldom coincides with the plume centre-line (the location of which varies constantly). However, for modelling, representative heat fluxes inside the main plume will be required. We therefore considered it reasonable to use the maxima of the measured values.

However, as a practical suggestion to address these uncertainties of the input data, we would suggest to include more scenarios to a revised manuscript, computed with various possible input values. The range of these scenarios would then better reflect the range of possible input values.

In subsection 3.2.3 the authors take the opposite position, that the under-prediction of the plume rise using the maximum 1-min average input parameters is caused by lack of representativity of the measured data (p509, lines 7–8).

This is the same position that we have presented previously in the manuscript text (p. 507, line 24). However, as there are also many other factors that affect the model vs. measurements comparison. After re-examining this section, we would suggest to expand and revise this discussion.

However, if the data used to validate the model are themselves inadequate, the model validation remains inadequate.

In our view, it cannot be concluded that the data is inadequate. A review of the measurement set-up and the main results has already been published at the ACP journal (Virkkula et al, 2014a); in other words, it has been evaluated to be adequate.

An alternative calculation of heat flux would seem to be available, since the total mass of burned material is stated as approximately 50 tons (p488, line 24). The paper by Wooster et al., cited in this manuscript, suggests a representative value of the burn yield of dry vegetation of around 20 MJ kg⁻¹ (p2 therein), hence in this case a total yield of around 1×10⁶ MJ. For a burn of 2h 15min (p488, line 22), this gives an average total heat flux of approximately 120 MW, or an average convective heat flux of approximately 70 MW using the 0.55 value from the manuscript. If, for the sake of argument, half the available material was consumed in a peak period of duration 15 minutes (e.g. figure 5, 0935–0950, say), this would give a sustained convective heat flux over this period of perhaps 300 MW. By this quick estimate, the assumed steady convective heat flux of case 1, perhaps also case 2, would again seem to be rather high. Since case 1 is the only case to be validated further, in figures 6(a) and 7, the remaining conclusions of the paper must be treated with some reserve.

We suggest to allow for the reviewer's suggestion by including also the cases 1 and 2 for further analysis. The convective heat fluxes for these cases are similar to the ones approximated by the

reviewer above. These curves could then be included to figures 6 and 7, to show better the effect of the possible uncertainties in evaluating the source input data.

Recommendation

In my opinion this manuscript is not suitable for publication and should be rejected.

References

Virkkula, A.V., J.L. Levula, T.P. Pohja, P.P.A. Aalto, P.K. Keronen, S.S. Schobesberger, C.B.C. Clements, L.P. Pirjola, A.-J.K. Kieloaho, L.K. Kulmala, H.A. Aaltonen, J.P. Patokoski, J.P. Pumpanen, J.R. Rinne, T.R. Ruuskanen, M.P. Pihlatie, H.E.M. Manninen, V.A. Aaltonen, H.J. Junninen, T.P. Petäjä, J.B. Backman, M.d.M dal Maso, T.M. Nieminen, T.G. Grönholm, V.-M.K. Kerminen, D.S. Schultz, J.K. Kukkonen, G.d.L. de Leeuw, J.B. Bäck, P.H. Hari, and M.K. Kulmala MS, 2014a. Overview of a prescribed burning experiment within a Boreal forest in Finland, No.: acp-2013-483, accepted for publication.

Virkkula et al., 2014b. Airborne measurements of aerosols and carbon dioxide during a prescribed fire experiment at a boreal site. *Boreal Env. Res.* (submitted).