

Response to the comments of anonymous referee #2

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

Review of Anonymous Referee #2, Interactive comment on “Applicability of an integrated plume rise model for the dispersion from wild-land fires” by J. Kukkonen et al.

General comments: The paper describes an application of the dispersion model called “Buoyant” to evaluate the plume rise consequent to wild fires. The paper is well structured and clear. By the way the two experimental data-set presented don’t allow for a complete validation of the model. The comparisons are only qualitative and the authors should clearly state that the validation of their model is not complete. In my opinion the paper doesn’t provide for a conclusive answer about the accuracy of the model “Buoyant” for wild fire applications. This fact must be clearly stated by the authors.

After re-examining the results, we agree with the referee. These results do not present a conclusive validation of the model. However, we would like to point out that this is mainly caused by the uncertainties of model input data in these two prescribed burning experiments, instead of deficiencies of the model.

The manuscript should be revised accordingly, e.g., we will need to revise the final sentence of the abstract, some text in the discussion of results, and the statement about model vs data agreement in the conclusions.

Specific comments

1. Pag 493 eq.3 and 4: It’s not clear if the model “Buoyant” is able to deal with an arbitrary wind and temperature profile inside the ABL (provided by observations or a meteorological model) or only equation 3 and 4 must be used?

It is correct that by default the profiles of Eqs. (3) and (4) are applied within the ABL. The profiles above the ABL are assumed to be as described on p. 494, lines 9-19. However, the model allows also the use of other forms of these ABL profiles, including more general ones or measured ones.

The only limitation for these profiles set by the model structure is that the length scales of characteristic changes in the vertical atmospheric profiles are not smaller than the extent of the plume in the vertical direction (this has been discussed in Martin et al., 1997). In other words, the atmospheric profiles cannot contain too abrupt changes vertically (this would mean that in a given cross-section of the plume, one set of meteorological quantities would not be representative). It is therefore possible to use either measured profiles or those predicted e.g. by numerical weather prediction models.

We would suggest that the use of various atmospheric profiles should be clearly stated in the revised manuscript.

...This would explain why at pag.507 the real meteorological measurements cannot be reproduced by the model. This aspect should be clearly stated by the authors as an evident weakness of "Buoyant" model. One should be able to take into account for any measured wind and temperature profile in order to accurately simulate the plume rise.

It is correct that for the Finnish experiment, there are some differences between the measured meteorological profiles and the predicted ones (presented in Fig. 4 of the manuscript). The differences in Fig. 4 are mainly caused by the fact that the theoretical form of the profiles that would optimally fit the observations cannot be known accurately. We have therefore used well-known, theoretically justifiable forms of these profiles (Eqs. (3) and (4)). It is very difficult to say whether some more complex forms of these profiles would have fit the observations better or worse.

Another important factor is the spatial representativity of the vertical profiles. The observatory of Jokioinen is located approximately 120 km south-southwest of the burn area (cf. p. 506). However, this is the closest location, at which such observations are available.

We would therefore state that these differences are not entirely caused by deficiencies of the model. The inaccuracies of the predicted profiles are caused also by the challenges (i) in measuring such profiles, so that these would be spatially representative, and (ii) the understanding the atmospheric state, so that optimal functional forms of these profiles could be selected for a given situation.

2. The authors should have reconstructed the local meteorology making an horizontal spatial interpolation between the 4 adjacent ERA40 meteorological profiles, not just by using the closest one (that is 120 km far away). This fact could partly explain why the observations differ significantly from ERA40 data (especially for higher height).

This is a good question, and we have examined in detail, how the selection of the ERA40 locations influences the predicted results. We have computed separately the temperature and wind speed profiles based on the data at all the four closest ERA40 grid points.

The Quinault prescribed fire was located on the Pacific coast of Washington State (US); the location has been presented in Fig. R1 (below). All figures in this ‘response’ file have been marked with ‘R’, and the figures in the manuscript e.g. as Fig. 1.

The lengths of the sides of the ERA-40 grid square surrounding the Quinault fire are about 280 km and 190 km, in the north-south and the east-west directions, respectively. Two of the ERA-40 points are situated in the westerly direction from the fire, at distances of about 60 km and 260 km offshore in the Pacific Ocean; referred here simply as marine-north and marine-south. The other two are located in the easterly direction inland, at distances of about 120 km and 290 km from the fire, referred as continental-north and continental-south.

The prevailing wind was easterly (Kaufman et al., 1996; Trentmann et al., 2002) during the fire. In the original manuscript, we used the ERA-40 profiles from the continental-north grid point, as we considered the inland meteorology to represent the burning site better than the marine points.

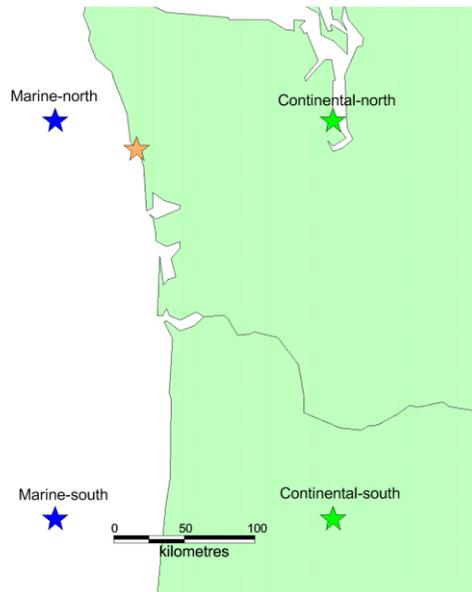
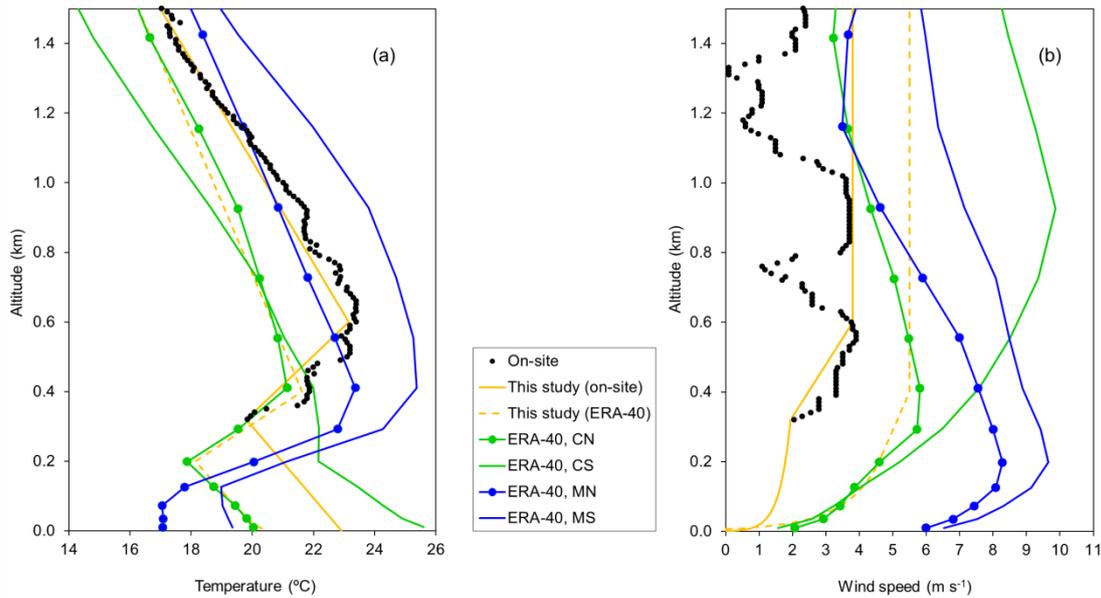


Figure R1. Location of the Quinault fire (orange star) on the Pacific coast of Washington State and the four ERA-40 data points that are located closest to the fire (blue and green stars).

The temperature and wind speed profiles corresponding to these four closest ERA-40 points have been presented in Figs. R2a-b. The on-site measured data and the previously predicted on-site profiles have also been presented in the figure.

The profiles at the two marine points show similar characteristics both in terms of their overall shape and numerical values. The temperatures at the two continental points differ about 6 degrees in the lower part of the atmosphere while the lapse rates are almost equal. At higher altitudes (above 400 m) the continental profiles are quite similar. The temperature inversions for the marine points are stronger and locates lower compared to the inversions for the continental profiles. Interpolating a temperature profile from all four ERA-40 profiles would most likely produce a profile closer (than the applied CN-point profile) to the on-site measured profile.



Figures R2a-b. Vertical profiles of temperature and wind speed in the Quinault fire, measured on-site on board the Convair aeroplane (black dots), at the four ERA-40 grid points closest to the fire (solid green lines for inland points and solid blue lines for marine points) and the two modelled profiles (solid and dashed orange lines). Notation: CN = continental north, CS = continental south, MN = marine north, MS = marine south. For the modelled profiles based on the ERA-40 analysed data (dashed orange line) we have applied the data at the continental north point.

There is a substantial variation in both the temperature and wind speed profiles at the four considered ERA-40 points. However, the ERA-40 wind speed profiles (Fig. R2b) for the two marine points (solid blue lines) show similar characteristics with each other.

In case of the wind speed profile, the values at the ERA-40 CN-point are closest to the on-site measured meteorology. In case of temperature, the values at the ERA-40 points CN and MN seem to approximate best the measured meteorology. In conclusion, an interpolation of meteorological profiles based on all these four points would not provide estimates that would be closer to the on-site measurements, compared with the profiles at the CN-point.

In summary, it was appropriate to check the effect of using values at the various ERA-40 points. The spatial representativity can partly explain why the observations differ from ERA-

40 data. However, a spatial interpolation based on the four closest ERA-40 points would not improve this representativity. These aspects should be discussed in a revised manuscript.

... Even the choice of an adiabatic temperature profile until the observation closer to the ground is too arbitrary. As a matter of fact this assumption is too influent on the plume rise and should be taken with more care.

The adiabatic temperature profile was chosen to be the same as assumed by Trentmann et al. (2002) for their Quinault fire simulations (as noted in the manuscript on p. 500, lines 16-20). The physical reason for this choice is the value of the estimated inverse Monin-Obukhov length ($L^{-1} = -0.0015 \text{ m}^{-1}$) derived from the ERA-40 analysed meteorology; this value indicates a near neutral, very slightly unstable condition. The choice is therefore not arbitrary (and of course should not be), but instead based on the ERA-40 data. However, this reasoning should be clearly stated in a revised manuscript.

Why the authors have not merged ERA40 data at lower level and observations at higher levels?

This would result in a discontinuous wind speed (as can be seen based on the values in Fig. 2).

3. Figure 3 Plume rise simulated with on site meteorology must be extended at the same down wind distance (300 km) as for ERA40 meteorology. It's not clear if the ascending motion is going to stop, or not, at a height of 600 m where the observed temperature profile exhibits an inversion layer. This is important because the ascending motion should stop or at least clearly decelerate inside an inversion layer.

This is a good comment, and we suggest a revised Fig. R3 below. We have increased the downwind distance scale to 1.0 km, to show clearly all the maximum height values (there is a misprint in reviewer's comment, he/she must refer to 300 m, instead of 300 km). For increased clarity, we have also added the upper and lower boundaries of the plumes. We also moved the LIDAR and ATHAM results outside the distance scale, as these do not correspond to any specific distance value, according to the original references.

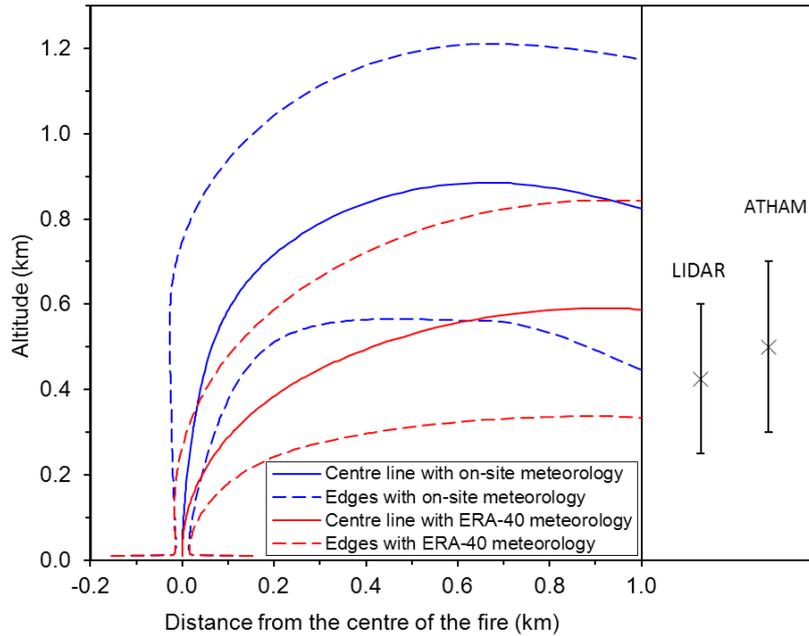


Figure R3. Simulated altitude of the centre line, and the lower and upper edges of the plume for the Quinault prescribed burn as a function of the downwind distance. The results are shown both for using (i) the meteorological measurements made on site (blue solid and dashed lines, denoted by “on-site meteorology”) and (ii) the re-analysis of the meteorological observations (red solid and dashed lines, denoted by “ERA 40 meteorology”). The vertical ranges of the previous results obtained using on-site LIDAR measurements (“LIDAR”; Trentmann et al., 2002) and computations using the ATHAM model (Trentmann et al., 2002) have also been presented.

The revised Fig. shows also the lower and upper boundaries of the plume. These were defined to be equal to the distances from the plume centre line, at which the concentration is 37% of the maximum concentration at the centre line of a Gaussian distribution (these correspond to a distance σ , defined in Appendix A of the original manuscript, equation A3).

4. pag. 504 I don't agree with this statement and it should be changed. "The predictions of the BUOYANT model can therefore be considered to be in a fairly good agreement with the observations, taking into account the substantial uncertainties especially in the model input data." Given the high level of uncertainty it's not possible to state that there is a good agreement or not.

After re-examining the results, we agree with the reviewer. This needs to be revised.

5. Figure 6a. The comparison presented is really qualitative. The model (case 1) with $k_v=1$ (k_v must be chosen “ a priori” and not trying to fit the observations!) under estimate the plume elevation after 1 km from the centre of the fire.

The value of the added mass term, $k_v = 1$, was not chosen to fit the Quinault or Finnish (Hyytiälä) observations. Instead, the assumed value ($k_v = 1$) is based on a previously conducted comparison of model predictions and wind tunnel experiments of the University of Hamburg (this was stated on p. 498, lines 5-8, but the wording was probably not sufficiently clear). This should be stated more clearly in the revised manuscript.

The k_v parameter has a fixed value in the model (1.0), and it is not a free parameter, the values of which could be adjusted to fit each case. The aim of using two alternative values for k_v (in Fig. 6) was only to study the sensitivity of the model to this selection.

... Again, in my opinion this experiment doesn't allow for any conclusion about the “Buoyant” model accuracy.

Yes, we agree.

The authors should clearly state which is the best method for computing plume rise initial data (Table 1) from wild fires after that, they should try to explain their model behaviour.

OK, we agree. We need to argument more clearly, which case would be the best estimate. There was some discussion on pp. 507-508, but this should be improved.

... In my opinion it's not correct making attempts with 4 different initial plume conditions that are clearly too important for the final plume rise. It's evident even without applying a plume rise model that a difference as those between case 1 and 4 will cause a very different final elevation. As a consequence, any conclusions about the model accuracy cannot be based on the best simulation (case 1 that, by the way, is underestimating the plume rise observations) between 4 attempts.

Yes, we agree.

6. Line 14 pag 510. The statement seems to contradict line 24 pag 511. Is kv a model parameter? If yes the model has free parameters.

No, it is not a free parameter. Please see our response to comment number 5 above. These statements should be revised to say this more clearly.

7. All the statement regarding “fairly good agreement” of the model should be changed accordingly to what indicated in previous points.

Yes, agreed.

8. It would be interesting to introduce a general discussion on the best method to estimate initial plume conditions (temperature and vertical velocity) in wild fires for practical application? How the authors would apply their model in case of an emergency situation, for example?

Yes, this would probably be an interesting extension to the manuscript. In general, the initial data needed by plume rise models could be determined in at least the following ways:

- (1) by using the measured or estimated fire temperatures, vertical velocities and area of fire,
- (2) based on estimates of the total amount of the burned material,
- (3) based on remote sensing (commonly satellite) data and
- (4) utilizing a fire emission model.

Each of these methods has advantages and limitations. These could be discussed, if the submission of a revised manuscript would be accepted. In this study, we used the method (1) for the Finnish (Hyytiälä) case, and (4) for the Quinault case (the predictions of the EPM model).

In an emergency situation, the main issue is the availability of data or estimated values regarding the fire. The method to be used would be simply selected based on which method is

applicable in view of the existing data (which is commonly scarce and uncertain in an emergency). Most likely, the methods (1) and (2) would be most quickly applicable.

Technical corrections

Pag 508 Raw 21 pag 509 raw 10 case number 1 -> case 1

Corrected.