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Geoscientific Model Development Discussions

Interactive comment on "Applicability of an integrated plume rise model for the dispersion from wild-land fires" *by* J. Kukkonen et al.

Anonymous Referee #2

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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.

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? 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.

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

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.

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 1good 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.

5. Figure 6a. The comparison presented is really qualitative. The model (case 1) with kv=1 (kv 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. Again, in my opinion this experiment doesn't allow for any conclusion about the "Buoyant" model accuracy. 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. 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.

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.

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

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?

Technical corrections

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

Interactive comment on Geosci. Model Dev. Discuss., 7, 483, 2014.