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. 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.

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 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).

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. 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).

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

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

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.

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).

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).

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

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.

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). 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. (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.)

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.

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.

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 Qc 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”).

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.

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

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 1×10^6 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.

Recommendation

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