Report #2

Submitted on 04 Jun 2026

Re-review of “Evaluation of plume rise parameterizations in GEM-MACHv2 with analysis of image data using a deep convolutional neural network” by Axelrod et al.

The manuscript has undergone significant improvements to its clarity and in limiting repetition. There are, however, a couple of outstanding issues (detailed below) which are incorrect and, in my view, inappropriate. These make it difficult for me to recommend this paper is accepted for publication in its current form. I do believe, however, that addressing these issues would be sufficient – there is much worthy of publication in the paper (the observational dataset, the observational analysis, etc.)

1) As indicated in the original review, the addition of the plume rise (due to momentum only) to the plume rise (due to buoyancy only) is not appropriate and incorrect. Acknowledging that the effects do not combine linearly does not overcome this error and does not enable the inclusion of momentum effects to be assessed accurately, nor any conclusions of the performance of the Briggs formulae (which are for buoyancy dominated or momentum dominated plumes only) to be robust. An analytical solution for the maximum rise height of a forced plume can be easily derived from the bent-over plume equations in a uniform wind (there are numerous references, e.g. Middleton (1986) https://link.springer.com/article/10.1007/BF00117467 or Contini et al. (2011) https://www.sciencedirect.com/science/article/abs/pii/S0167610511000389).

We thank the reviewer for this comment as this has identified an inconsistency in our previous methodology. Since the point of the comparison was to assess the plume rise parameterization in GEM-MACHv2, which assumes buoyancy only, it was confusing in our original analysis to first investigate buoyancy and momentum effects combined and then investigate buoyancy only as a secondary analysis. It is more consistent to compare our image-based measurements to the parameterization as it appears in GEM-MACHv2 (for buoyancy only). We have restructured the manuscript to focus on a buoyancy only formulation.

However, since there is some interest in investigating the effects of momentum, we provide a separate test for the combined momentum and buoyancy plume rise. Briggs (1975) gives a combined plume rise as a function of downwind distance and a distance to final plume rise for neutral conditions only. Other examples in Briggs and elsewhere in the literature, which the reviewer mentions, give plume height as a function of downwind distance or time, but not final plume rise. We believe that solving these equations for final plume rise with combined momentum and buoyancy effects (which differs from the formulation in GEM-MACHv2, the model assessed here) would add unnecessary length to the manuscript and is beyond the scope of this study. For this reason, we limit our comparison of momentum effects to neutral conditions only. We modified Tables 2 and 3 and reduced Sections 3.2.3 and 3.3.3.

Although momentum effects are not a part of the GEM-MACHv2 parameterization (and are hence beyond the scope of this work), it is shown that the inclusion of momentum in the parameterization (for neutral conditions) has very little (< 2%) effect on the average plume rise for the conditions investigate here.

2) The above references (as well as Weil (1988) and Briggs (1984)) give appropriate values for the entrainment parameters. These entrainment parameters do not take values of the order of 39 and 1.2, as quoted in the paper. The relationship between the coefficients mentioned in the paper and the entrainment coefficients should be determined. This can be done by analysing the governing equations and solving them to give the Briggs formulae.

After equation 11 (Section 2.3.5), the text is modified to: "In equations 9-11, the numerical parameters include entrainment parameters based on assumed entrainment rates. Briggs (1984) assumes an entrainment rate of $\beta = 0.6$. The constants in the equations are proportional to $\beta^{-2/3}$." (Red text added or modified.)

In Section 3.6, "Reducing the dimensionless constants of 39 and 1.2 in the equations to 57% of their given values (to 22 and 0.7 respectively) results in an average observed (DCNN) within 1% of the parameterized plume rise. This corresponds to a factor of 1.45 increase in the assumed entrainment parameter (from the $\beta^{-2/3}$ relationship), giving $\beta = 0.87$." and "The factor of 3 applied to the dimensionless constant corresponds to a factor of 2.1 increase in the assumed entrainment parameter (from the $\beta^{-2/3}$ relationship), giving $\beta = 1.25$."

Similar corrections are made in the Conclusions section (Section 4) as "… corrected for by increasing the entrainment parameters in the plume rise equation (for neutral conditions) by a factor of 1.45 (to $\beta = 0.87$ from 0.6). In the case of unstable conditions, results are improved if the entrainment parameters in the equations are increased by a factor of 2.1 (to $\beta = 1.25$ from 0.6)… This suggests that the original entrainment rates suggested in the Briggs formulation maybe be underestimated when applied to the plumes studied at this location."

Finally, in the abstract, the text is modified to "The parameterizations could be improved by increasing the assumed entrainment parameters in the Briggs equations by factors of 1.45 and 2.1 in neutral and unstable conditions, respectively."

There are a few minor typos:

1. DPRNet is defined in the abstract, but it is not defined on first appearance in the text (line 98) – it is defined instead on line 155.

The definition is added at line 98.

2. Line 119: Should 'north' be 'south'?

Fort McMurray is 40 km to the south of the Syncrude complex, so it is correct to note that Syncrude is "approximately 40 km north of Fort McMurray".

3. Line 129: There is no longer a 'green star'

This text is deleted.

4. Line 371: Should 'without' be 'with'?

Given that either choice might lead to confusion, we reword this as "we assess the effect of this limitation on the parameterization in Section 3.6."

5. Labels a, c, e and g are missing from Figure 7.

This is fixed.

Report #1

Submitted on 22 May 2026

Referee #3: Riccardo Simionato, riccardo.simionato@unige.ch

The authors have adequately addressed the concerns raised during the previous review round. The manuscript has improved in clarity and methodological transparency and can now be accepted subject to minor revisions. The remaining comments mainly concern clarification of the geometric calibration methodology and a few minor technical corrections.

The authors addressed the concerns raised in the previous review round and the manuscript can now be accepted subject to minor revisions.

In response to specific comments:

1. The authors state that the linear angular relationships (α=$i\Delta\theta h$ and χ=$j\Delta\theta v$) in Snee et al. (2023) are only correct for a curved lens. However, Snee et al. (2023) explicitly designed their framework for standard flat-sensor cameras operating under a pinhole camera model, but utilized these linear relationships as a simplified small angle approximation (tanθ≈θ) because the spatial distortion is typically minor at long focal distances. On what basis was this 2% error calculated? Is it the comparison between the method they used and the method of Snee et al. (2023)?

The 2% difference is based on comparison between plume rise calculated using both methods for a sample of tests from our image data.

To look at the effects of this spatial distortion, considering just the horizontal for simplicity, the Snee et al. approach assumes $\alpha = \frac{i}{M}\theta$ (were θ is the horizontal field of view). At $i = 0$, $i = \frac{1}{2}M$, and $i = M$ (corresponding to the left, centre, and right sides), this relation is clearly true, giving $\alpha = 0$, $\frac{1}{2}\theta$, and θ, respectively. However, at $i = \frac{1}{4}M$ (to use an example like what is shown in Fig. 3A in Snee et al.), the correct formula for the angle is

$$\alpha = \frac{\theta}{2} - \operatorname{atan}\left(\frac{W/4}{d}\right),$$

where W is the real horizontal width of the image plane (as M in Snee et al, but in metres not pixels) and d is the distance between the camera and focal plane (D in our manuscript). For the geometry used in our study, this gives an angle of 13.09° versus the angle of 13.97° derived from $\alpha = \frac{1}{4}\theta$. It is difficult to say how this difference would present itself for the plume rise images in our study, but it seems very likely that this accounts for the less than 2% difference between the test cases we compared. It is important to note that this difference is small and relatively insignificant given all the large uncertainties involved in the analysis.

Moreover, what the authors have added around line 102 should also be added at lines 209-211. In particular, the field of view (demonstrated to be one of the main sources of error and uncertainty in the geometric calibration, e.g., https://doi.org/10.3390/rs14071766) and camera inclination (even if in their specific case is zero) are not listed here, but mentioned only afterwards (lines 253 and 265). Consider citing these parameters also at the beginning (lines 209-211) for clarity.

This paragraph is rewritten as follows (added text in italics):

"The geometric transformation, the concept of which is used in other works (Koushafar et al. 2023, Luhmann et al. 2006, Snee et al. 2023), is a mathematical method that transforms locations of objects on an image plane (expressed by horizontal and vertical pixel position) into physical locations and distances. The geometric transformation from the image to actual plume position uses known properties of the camera and geometry, which include the camera field of view, the distance and orientation from camera to the stack (measured from satellite imagery), and the tilt/inclination of the camera relative to the ground. Here, the camera was mounted horizontal so that the tilt was zero. The distance per pixel (δ) at the image plane can be calculated from horizontal field of view of the camera, the distance from the camera to the stack, and the number of pixels in the image. The position of the plume into our out of the image plane is determined using the measured wind direction, assuming that the plumes travels from the stack exit downwind following this horizontal direction." (red text added)

Apart from these details, the proposed revisions are satisfactory and contribute to improving the clarity of the manuscript.

Technical corrections:

Line 34: as a previous sentence was removed, the authors may want to place the (Briggs 1969, 1975) citation here

The sentence is expanded as "The development of plume rise theory following Briggs (1969, 1975) has made use of this data collected over 50 years ago". The Briggs (1969) reference is added back to the citation list.

If ERA5 data are directly downloaded from cds.climate.copernicus.eu, a citation and attribution section is available in the right-hand panel when opening the dataset page (e.g., https://doi.org/10.24381/cds.adbb2d47)

ERA5 data were not directly downloaded for this work. The text here was unclear. We have changed it at line 324 (Section 2.3.4 after Eq. 5) to state "Here we used a global data repository (Guo et al., 2022)… which used a machine learning model with ERA5 reanalysis and the Global Land Data Assimilation System (GLDAS) product (Guo et al., 2024)." We hope this makes it clear that the Guo et al. study downloaded and used the ERA5 data in their analysis.

Supplementary Section S3 is repeated twice in the Supplementary Materials (the section with geometric transformations should be S4)

Due to the reorganization of the manuscript (see Reviewer 1 response), the first Section S3 "Comparison of plume rise with buoyancy only" is no longer needed here and it has been removed. Section S3 now refers to "Geometric Transformation of Plume Rise".