egusphere-2025-4582: Plume Rise/DCNN comparison manuscript reviews, round 1

We thank all the reviewers for their valuable comments and suggestions. We believe this feedback has improved the manuscript considerably. Our responses to each comment are below (in blue text). We have refrained from quoting line numbers at this point since the revised manuscript will be submitted and the numbering may change.

Reviewer 1

The paper provides a comparison of the Briggs plume rise parameterisations against a deep convolutional neural network for determining plume rise from imagery (DPRNet).

I felt that there was a lack of understanding of the Briggs formulae. The underlying basis of the Briggs plume rise equations are the governing conservation equations of mass, momentum and heat. Solving these governing equations under specific conditions (buoyancy or momentum dominated, or particular meteorological conditions e.g., zero crosswind, constant buoyancy frequency) leads to the Briggs formulations. The 'dimensionless constants' are entrainment parameters. The Briggs formulae are commonly used for predicting plume rise but there are reasons why they may not work well, for example if the assumptions made in deriving the Briggs formulae from the underlying conservation equations do not hold true (e.g., the true atmospheric meteorological profiles may differ from that assumed). Indeed, the authors do make use of a modification for the interaction with the boundary layer top. Other authors have made direct use of the underlying conservation equations, considering the true atmospheric and release conditions (Webster and Thomson, 2002) and more elaborate models do also account for latent heat release from moisture in the rising plume / entrained atmospheric air (Fathi et al., 2025).

We thank the reviewer for this clarification. Our misunderstanding was due to a misunderstanding of De Visscher (2013, Air Dispersion Modelling, Chapter 7), which demonstrates that equations for plume rise can be developed by dimensional analysis. Although the Briggs (1984) formulations are derived from conservation equations and assumed entrainment parameters, the formulation is very similar to the equations derived through dimensional analysis. To correct our misinterpretation of the formulation and to provide a more accurate discussion, we have modified the text as follows:

1) The reference to dimensionless constants and analysis in the introduction is removed.

2) In section 2.3.5 the following line is added: "In equations 10-12, the numerical parameters represent entrainment parameters based on assumed entrainment rates, which are chosen to give the best fit to observations (Briggs, 1984)."'
3) In Section 3.6 (and the abstract and conclusions), the term "dimensionless constants" is replaced by "the constant values which represent the entrainment parameters", "the entrainment parameter constants", or "the constants based on assumed entrainment parameters" (depending on the context).

Further, we add the text to the Introduction "Weber and Thomson (2002) developed a new plume rise scheme, based upon conservation equations, which was shown to improve predictions." And we note that this is followed by the text "More recently, Fathi et al (2025) modified a layer-based approach suggested in Briggs (1984) to incorporate the exchange of latent heat between the rising air parcels and the ambient atmosphere."

I found the text a bit vague in places, a little repetitive and verbose in other places and with many references to supplementary information and over-/under-predictions. It is worth the authors considering what key points / results they would like to present in the main paper and being both concise and precise. In addition, there are quite a lot of typographical errors which, with more care and attention, could have been avoided. All in all, this makes the paper quite hard to follow. Some further details are given in the detailed points below. The conclusion section was, however, well written and provided a good summary.

Much of the repetition (which is also noted by the other reviewers) stems from our decision to analyze two different data sets (Manual Image Selection and Ensemble Images) each with two different methods (Final Visible Height and Exponential Curve Fitting), while also accounting for both buoyancy only and buoyancy and momentum combined. We have tried to make this discussion more succinct by combining figures (7 and 8), consolidating tables (2, 3, 5, and 5, and 6, 7, 8, and 9), and deleting unnecessary repetitive discussion, especially concerning over- and under-prediction.

We have reviewed the manuscript for typographic errors and have addressed many of these through the detailed comments provided by all the reviewers.

How are the Briggs formulae for momentum and buoyancy applied to calculate plume rise for plumes with both momentum and buoyancy? Are they just added together and, if so, is this appropriate?

To investigate the effect of initial momentum (which isn't included in the GEM-MACH model), we add together the rise due to momentum and the rise due to buoyancy. To add more clarity around this (and the implied assumptions), we add the following text in Section 2.3.5 (before Equation 14):

“Briggs (1984) investigates momentum-dominated plumes separately from buoyancy-dominated plumes, and the effects are not combined. In this study, we will present plume rise results with plume rise due to momentum added to plume rise due to buoyancy.”

And at the end of Section 2.3.5:

“While we recognize that the rise due to momentum and the rise due to buoyancy may not combine linearly, we compare plume rise due to buoyancy effects only with plume rise due to momentum added to assess the potential difference due to the addition of momentum, although we recognize that the combined effect may be less that the linear addition of the two terms.”

The consolidation of the tables, which now show “Buoyancy + Momentum” and “Buoyancy only” should also help make this clear.

The calculation of the proportion of the plume rise due to momentum in section 3.2.3 assumes this simple addition but I’m not sure this is true in reality. Understanding the underlying conservation equations and the derivation of the Briggs equations (and the assumptions made) may shed some light on this.

See text above.

Presumably DPRNet has been trained on an earlier dataset? What dataset was this and will the trained model be applicable to the images processed here?

The DPRNet was trained using the same dataset used here. To clarify we added the text (2nd last paragraph in the Introduction) “Koushafar et al. (2023) trained the DPRNet through manual segmentation of 3600 randomly selected images.”

What about uncertainties / errors in the observations? Line of sight and plume direction are mentioned. Indeed, a sensitivity analysis of the wind direction on the determined observed plume height is conducted. Two methods for obtaining the plume rise from the observations are used, but conclusions are drawn on the performance of the Briggs formulae to these observations without consideration of uncertainties in the observations. Indeed, the authors state under- or overpredictions which are small compared to the uncertainties, say, in the observed plume rise height due to the wind direction presented in section 3.5.1.

We have reworded or redeleted text in Sections 3.2 and 3.3 to remove much of the discussion around under-prediction and over-prediction. In the context of the uncertainties discussed in Section 3.5, the summary of results shown in Figure 8 (previously 9) places these results in context and it isn’t necessary to focus on the detailed comparisons.

Is Figure 3 in the horizontal plane? Is this a valid assumption? I can imagine that the plume height may not be at the same height as the camera. The caption mentions a similar transformation in the vertical plane to determine the plume rise height. A reference is given but it would be helpful to give more detail here on the calculation, what is measured (presumably SP'), what is calculated (presumably SP) and how.

We have added a vertical view to Figure 3 and the following text (last paragraph Section 2.2): "As discussed in Koushafar et al. (2023), the camera is mounted level, so the image plane is vertical (parallel to gravity)."

Minor points:

Some minor points on the text are listed here:

- Abstract: Adding a couple more words in places will make this more readable and standalone, e.g., 'facility' is vague – what type of facility? GEM-MACHv2 – what type of model is this?

We have added the qualifiers "oil extraction" and "three-dimensional multi-scale model" to facility and GEM-MACHv2, respectively.

- The Webster and Thomson (2002) paper compared ground level concentrations, not plume heights.

This was an error. Webster and Thomson tested their own Lagrangian model and Briggs parameterizations, where Briggs showed a tendency to underestimate downwind particle concentrations. Mention of this has been removed from this paragraph.

- Line 106: Does 'averaging' mean an average of the maximum (or minimum) daily temperatures or a daily average? Without clarification, I would assume daily averages (and if that is the case, I do not think any changes are required). What is "ECCC"?

As correctly assumed by the reviewer, this temperature is a daily average. ECCC stands for Environment and Climate Change Canada (now expanded in the text).

- Line 216 "(highlighted by the green star in Fig. 1)" – this reads as though it applies to "the largest smokestack" rather than the Buffalo Viewpoint.

The phrase "(highlighted by the green star in Fig .1)" has been moved to earlier in the same sentence to clearly indicate that it refers to the Buffalo Viewpoint location.

- Line 131: Repetition of 'in total'.

This phrase has been removed to avoid repetitive phrasing.

- The yellow 'plume rise' text on Figure 2 is hard to read on screen (and impossible to read on paper).

Following the reviewer's suggestion to make figure 2 more legible, all text in this figure is black font.

- The notation for plume rise is confusing (lines 168 & 175, caption Figure 2). Can you use the same notation for the figure and text? Furthermore, what does the subscript "e" refer to (Figure 2 caption)? In Figure 2's caption, the notation used for plume rise height and plume rise distance seem too similar and will easily cause confusion. Furthermore, the text adopts different notation for the plume rise obtained from each method, but Figure 2 does not. Yet on line 185, there is mention of two sets of results for Delta h_{obs}, rather than Delta h_{obs} and Delta h_{pixel}. Is one Delta h_{obs} = 0.99 Delta h_{pixel}? Would it be helpful to add notation to figures 2d and 2f? That said, I think the 'plume rise' in the text and figure 2 are not the same thing (lines 182 onwards) so perhaps different notation is required. In this case, I won't deny that this might be difficult, but can this be less confusing / better explained?

There was an error in figure caption. Also considering Reviewer 3's comments, we modified the text in the Figure 2 caption to: "The plume rise (Δh_{pixel}, which is the asymptote coefficient in Equation 1)…" and "d_(e,obs)" is deleted (this variable is not used in the manuscript). Within the figure, Δh_{pixel} is added to identify the plume height (measured in pixels). Throughout the revised manuscript, we have deleted any use of the subscript "obs". Hence, Δh_{pixel} refers to the plume's rise on the image (in pixels) as used in Equation 1, whereas Δh refers to the physical plume rise (in metres).

- Line 217: "Using this model" – what is 'this model'? GEM-MACHv2? (In which case there is a jump from Briggs parameterisations.) Or the 'plume rise calculations'? (In which case, 'model' doesn't seem the right word.)

The phrasing has been changed to "Using these parameterizations" to make it clear that we are referring to the Briggs parameterizations in this sentence.

- Heights (e.g., line 237) are presumably above ground level (this should be stated). What is the elevation difference between the monitoring stations and the stack location? Is this of relevance?

The following text is added: While the Lower Camp tower is at a lower elevation (238 m above mean sea level) that the main stack (304 m amsl), we assume that the wind and temperature profiles are terrain following and this 66 m difference has minimal effect on the temperature and wind profiles.

- Should GEM-MACH (lines 264 and 296 and other places) be GEM-MACHv2?

We have replaced all instances of GEM-MACH with GEM-MACHv2.

- Equation 7: if T_s < T_a then the plume would be negatively buoyant (i.e., dense).

While this is technically true, it is not accounted for in the model because the plumes being modelled are related to combustive industrial processes. The plume temperature (at the stack exit) being near or less than the air temperature can only happen if there are no emissions. The GEM-MACHv2 model does not account for "cold" plumes.

- Equation 9: H_{ast} is not a convective *velocity* – check the units.

The text is modified to "H_* is defined by Briggs (1985) as a convective velocity (although the units are m^2/s^3) given by…"

- Line 324: 'vertical extent' – this only makes sense for a 'bent-over plume' rather than a vertically rising plume. Indeed, the entire study probably only makes sense for such situations. I did not, however, think this was clear early in the paper. In fact, "bent-over" is not mentioned until line 366. I don't follow either why the vertical extent is assumed to equal the plume rise height (Delta h). Where does 2/3 H come from on line 325? Also, the discussion of removal from the analysis of vertically rising plumes occurs on line 513. Similarly line 335 mentions momentum. It would be clearer to mention the focus regarding bent-over / vertically rising and buoyancy- / momentum-driven(or both) early in the paper.

We add the following text to the Introduction (second last paragraph): "Here we limit our analysis to "bent-over" plumes, where the horizontal wind speed is strong enough to deflect the plume into the wind. While parameterizations for vertical plumes do exist, there are not used in GEM-MACHv2 and they are difficult to assess using image analysis."

We also add to Section 2.2 (second last paragraph): "It is worth noting that when searching for the neutral buoyancy point of a plume, it is generally easiest visually to determine the neutral buoyancy point when the plume is in a "bent over" shape, i.e. there is lateral wind speed significant enough for the plume to bend sideways before it visually dissipates, giving it a bent over shape and allowing for its neutral buoyancy height to be more easily extrapolated."

In the Introduction, we add: "Since the main stack emissions range from 120°C to 163°C, we assume a plume rise dominated by buoyancy (as opposed to momentum), but we compare plume rise due to buoyancy and momentum combined with plume rise due to buoyancy only."

The assumption of a vertical extent equal to the plume rise is from Briggs (1975) and is repeated in Briggs (1984). We add the text "… following Briggs (1975)." To our knowledge, there is no verification of this assumption (although Beiser et al., 2011 investigate the vertical emissions profiles of model output). Preliminary analysis of the images in our dataset demonstrates the average vertical plume extent does approximately equal the plume rise; however it is probably beyond the scope of this manuscript to discuss this and we plan to investigate this in future work.

The 2/3H solution was in error as this only referred to a particular solution and isn't generalized for all results. We have removed this line.

- Is the limitation of the plume top by H realistic? Depending on the buoyancy of the plume and the strength of the inversion at the top of the boundary layer, it is possible that the plume could punch through into the stable atmosphere above the boundary layer.

We add the following text to the end of this paragraph: "Given that this bumping scenario may not be realistic in all situations, as the plume may punch through the boundary layer height and the assumed vertical extent of the plume may be incorrect, we assess the parameterization without penetration of the boundary layer in Section 3.6.".

- I do not think the use of the word 'fumigation' is correct. Fumigation occurs when material is trapped in a stable region (for example in the stable atmosphere above the boundary layer) and is then subsequently mixed down to ground when it is incorporated into the boundary layer, say, as the boundary layer grows in depth due to the diurnal cycle.

"Fumigation" has been replaced with "downward movement" to avoid confusion.

- Line 331: 'and the other models' – this is vague. What other models?

This phrase was included unintentionally and has been removed.

- Line 332: Can you say what the layer-based approach is? What are the layers? Or what is the purpose of it? Is it an approach to account for the true atmospheric meteorological profile?

We have added text in the introduction (see Reviewer 2's second comment): "This approach is useful within air-quality models where the stability and meteorological parameters in each model layer are available. The layer-based model subtracts plume buoyancy at each model layer until the plume is neutrally buoyant."

- Line 336: 'This is the parameterization' – what is 'this' here?

The first sentence of this paragraph is rewritten as "The parameterization that is used in GEM-MACHv2 only predicts plume rise due to buoyancy…"

- Line 338: 'both with momentum' – what does 'both' refer to here? 'both with momentum and buoyancy'?

The word "both" is deleted and this section has been modified following the Reviewer's comment about momentum and buoyancy above.

- Line 388: '50 pixels'. Is this in any direction (e.g., could it be vertically) or is it a horizontal distance?

This is modified to: "…within 50 pixels *(in both the horizontal and vertical directions)*…". (Italics show added text.)

- Could the authors give a summary in the main text of the conclusions of the sensitivity analysis in the supplementary sections (e.g., to the choice of the pixel box or of the wind direction cutoff). Furthermore, how does this fit in with the uncertainty analysis in section 3.5?

Section 3.5.4 is renamed to "Other uncertainties" (from "Potential for Selection Bias"). In that section we have added a summary of the supplementary sensitivity analysis. We also add "…and is summarized in Section 3.5.4." where the pixel box size and wind speed and direction cutoffs are introduced.

The added text in Section 3.5.4 is as follows:

"Sensitivity to the choice of selection parameters (start point restriction, wind speed and direction) is analysed in the supplementary materials (Figs. S1, S2, and S4). We applied a wind direction cutoff angle of 45° from the perpendicular. Lowering (or raising) the cutoff angle increases (or decreases) the estimated plume rise by approximately 1.0 m per degree change (m/°) for the DCNN analysis and 0.8 m/° for the parameterized plume rise (Fig. S1). The change is approximately linear over a range of ±10°. The start point restriction was applied to restrict analysis to plumes that originate from the smokestack exit. The restriction used in the analysis is that the start point of the plume much be within a box ±50 px (vertically and horizontally) from the stack exit. Reducing this to values below 42 px (Fig. S2) significantly reduces the number of images available for analysis (by > 1000 images for a 25 px reduction) and changes the resulting plume rise significantly. However, between a box size range of 42 to 75 px, the plume rise determined by DCNN analysis or the parameterization does not change significantly (< 1 m), suggesting the analysis is not overly sensitive to the proximity of the plume start point to the stack exit for a moderate allowance of > 42 px. Finally, a wind speed threshold of 3 m/s is used to separate bent-over plumes from vertical plumes. Visual inspection of images demonstrates that 83% of plumes identified as bent-over occur during wind speeds > 3 m/s, while 82% of plumes identified as vertical occur during wind speeds < 3 m/s (Fig. S3). The plume rise determined with DCNN is not sensitive to the choice of threshold wind speed, changing less than 4 m over the 2 m/s to 4 m/s range. However, the parameterized plume rise does show some sensitivity to the choice of threshold wind speed, changing (approximately linearly) from an average of 196 m for a threshold of 2 m/s to 182 m for a threshold of 4 m/s. Hence, we do not expect our results to change significantly (>10%) for a reasonable range of criteria values..”

- There is repetition of the selection criteria (lines 366-, lines 420-, lines 449-, lines 571- (repeating 528- note also the numbers after removing images for which the start point is too far from the stack should agree but do not (lines 529 (14431) vs 572 (11431))).

We have simplified the text in these instances (in Sections 3.2.1, 3.2.2., and 3.3.1) to:

“After applying the criteria discussed in Section 3.1 (plume starting location and wind direction), there were 60 images remaining for analysis.”

“The 208 images underwent the same filters as in section 3.2.1, after which a total of 60 image remained.”

“After applying the criteria discussed in Section 3.1 (plume starting location, wind speed and direction, and statistical outliers), there were 3785 images remaining for analysis.”

This removes the error.

- There is much mention of over- or underprediction but often the values agree within the quoted uncertainty. The authors are comparing the mean values (without considering the uncertainty) but this isn’t always clear. Furthermore, in some places the text states an underestimation when the mean is greater (i.e., if anything it should be an overestimation) (see lines 456, 459-460 (stable conditions) and 550 (stable conditions).

We have removed much of this text in response to other reviewer comments. Generally, sections 3.2 and 3.3 are now more focused on the larger differences between the methods and less on the over/under-estimations that are within the uncertainties.

- Line 454: The R^2 value of 0.93 +/- 0.04 (coefficient of determination) refers to the fit between the plume midpoint and the exponential fitted curve, but this isn’t clear here. Furthermore, using the same notation (R^2) as ‘correlation’ between the DCNN and Briggs plume rise adds to the confusion.

We have added the text "The average coefficient of determination *(between the plume midpoint line and the fitted curves)*…", and we use r^2 (small r) for coefficient of determination (which also occurs in Section 3.3.2).

We also add "R^2 is the Pearson correlation coefficient." to the caption of Table 2 and add "The correlation is strongest (*Pearson correlation coefficient*, $R^2 = 0.35$)…" at the first instance in the text where it is introduced.

- Line 486: 'likely to be flatter'. On the other hand, for bent-over plumes, the maximum height of the plume is likely to be some distance downwind and therefore possibly more likely to be rejected by the distance constraint.

This text has been deleted in response to another comment.

- The mention of with and without momentum applies just to the Briggs formulation. This is not clear (e.g., line 480 or line 507), and one could alternatively assume that some knowledge of whether the plume was buoyancy driven or had a significant momentum component was used to select examples. In particular, the captions for tables 4 & 5 (and 8 & 9) are poorly worded: 'Average plume rise for manually selected images without plume rise due to momentum…'.

We add "plume rise due to both buoyancy and momentum were included *in the parameterization*".

Those tables are combined with others and the text has been deleted.

- Line 516: 'investigated' should be 'investigate', line 518: 'given Section S1' should be 'given in Section S1', line 547: 'overpredict predict plume rise' should be 'overpredict plume rise'.

These corrections have been made.

- The uncertainty (95% confidence interval) in the plume rise is much less in the ensemble case (section 3.3) than in manual image selection study (section 3.2). Presumably this is related to the number of data points. Is this worth a comment?

We add the sentence (Section 3.3.1) "The 95% confidence intervals are much smaller in these results compared with Section 3.2 as there are a larger number of samples.".

- Line 543: Would not the consideration of latent heat lead to an increase in plume rise (with condensation of plume moisture leading to a release of latent heat)? This seems contrary to the text here.

While it is true that the immediate condensation of plume moisture at the stack exit would lead to release of latent heat and thereby more buoyancy, conversely, evaporation of water droplets in the cloud as it rises and mixes with surrounding air would lead to a reduction in heat, limiting plume rise. The reviewer is referred to Fathi et al. 2025 for further explanation.

- Lines 581-582: The % values are not in agreement with Figure 8a. Similarly Figure 8b lines 594-595 % value and text.

This text is deleted (see general comments above).

- Line 624: 'better agreement' – in neutral conditions Table 6 shows better agreement.

In the interest of brevity, we have removed that sentence. The following sentence is then rewritten as "For all images *when the visible plume rise height is used,* Briggs *with buoyancy only* predicts…".

- Line 640: 'a larger underprediction' – it's an overprediction in unstable conditions

For brevity, we have removed this sentence.

- Line 657: new sentence starts mid-way through a sentence 'largest overall discrepancy'

This sentence is corrected to "The largest overall discrepancy is, in the ensemble image set using the final visible height method."

- Line 710: 'underprediction' should be 'overprediction'

This is corrected.

- Line 719: 'in Section 4' – is this correct? Section 4 has not appeared yet and, in fact, is the conclusions.

This is corrected to "3.5.1".

- Is the data in Figure 10 comparable? In other words, are the same image datasets used for both the exponential fit and visible?

The selection of images for the exponential fitting and for the visible plume height are described in sections 3.2.1, 3.2.2, 3.3.1, and 3.3.2. While they all draw from the same dataset, there are differences in images used for the data portrayed in Figure 9 (formerly 10) due to the selection criterion in which plume rise (in pixels) on the image had to be within a half-image-width of the plume start point, which was a criteria only applied to the exponential fitted results to remove plumes that did not have an asymptotic shape. As such, Figure 9 compares the difference in the results due to the method used, including the different selection criteria.

- Line 779: ‘dawn at dusk’ should this be ‘dawn or dusk’?

This has been corrected.

- Line 780: ‘hour.,’ should be ‘hour,’.

This has been corrected.

- Line 781: ‘Tables and 6 and 8’ should be ‘Tables 6 and 8’.

This has been corrected.

- Lines 809-810: ‘account friction velocity’ maybe ‘account for friction velocity’?

This has been corrected.

- Lines 811 and 825: What is the basis (other than fitting to your observations) for changing the constants in equations 10 and 12? Where do the original constants come from?

This refers back to the reviewer’s first point, which we have addressed separately above.

Reviewer 2

This study uses a deep convolutional neural network framework developed by Koushafar et al. (2023) to analyse images of smoke plumes from an industrial site, comparing the plume rise estimates against theoretical predictions using Briggs parameterisation. The authors concluded that Briggs consistently overpredicts plume rise during unstable conditions with high wind speeds, and that neural network-based plume rise estimates show no diurnal variability compared to those from Briggs parameterisation.

The application of deep convolutional neural networks to plume image analysis represents a methodological advancement for estimating plume height over extended time series compared to previous studies relying on in-situ or flight measurements. Based on their results, the authors proposed revised values for the Briggs parameterization to improve plume height estimation accuracy. Overall, this is an interesting study that advances the methodology for estimating plume rise through the application of deep convolutional neural network. However, the manuscript would benefit from improvements in language clarity and revision of several figures to enhance readability and accessibility.

Major Comments:

1. The paper would benefit from more precise language to improve readability and flow. The text contains repetitive wording in several sections. Additionally, the citation style is inconsistent throughout the manuscript. I have identified some of the specific citation issues in the minor comments below, but the authors should conduct a thorough review of all in-text citations.

We have edited certain sections of the paper to improve clarity when needed (please also see the response to Reviewer #1, second comment). Figures 7 and 8 have been combined, the 8 tables are consolidated into 2 tables, and we have made an effort to remove unnecessary and redundant wording, explanation, and comparison of results.

We have addressed the citation issues listed below and done a thorough review to check in-text citations.

2. In the introduction, the authors reference several previous studies that estimated plume rise and compared results to Briggs parameterization (e.g., Gordon et al., 2015; Akingunola et al., 2018; Gordon et al., 2018; Fathi et al., 2025). However, the distinctions between these approaches are not clearly articulated. What are the potential reasons for the over- or under-estimation of plume heights compared to observations? Furthermore, the paper lacks substantive comparison with these previous studies. The limited comparisons provided (e.g., Lines 542-545) would be strengthened by incorporating more quantitative analysis.

We have expanded and rewritten the introductory paragraph as follows:

"In 2013, an aerial measurement study was done in the Athabasca region of Alberta, Canada (Gordon et al., 2015). During this campaign, a Convair aircraft flew 84 flight hours in 21 flights while taking atmospheric pollutant measurements. These aircraft-based measurements were used to determine the smokestack plume locations (Akingunola et al., 2018; Gordon et al., 2018). In the Gordon et al. (2018) study, 82 plume heights were determined from the aircraft measurements. Back trajectories based on wind direction were then used to determine the smokestack source for each plume, and meteorological and stack emissions data were used to determine plume rise following the Briggs equations (as used in GEM-MACHv2 and described herein). This comparison demonstrated a strong underprediction of plume rise, with approximately 50% of the plume rise predictions below half the observed values. Although the authors could not determine the reason for this underprediction, the heterogeneity of the meteorological conditions was suggested as a possibility, since conditions at the stack locations were estimated using distant meteorological towers. Akingunola et al. (2018) compared 176 test cases, also based on the 2013 aircraft measurements. In addition to the comparison to the Briggs equations a layered method is proposed based on Briggs (1984). This approach is useful within air-quality models where the stability and meteorological parameters in each model layer are available. The layer-based model subtracts plume buoyancy at each model layer until the plume is neutrally buoyant. The layer-based model (using GEM-MACH model profiles) improved the predictions, resulting in 70% of predicted plume rise being within a factor of 2 of the observed heights. Weber and Thomson (2002) developed a new plume rise scheme, based upon conservation equations, which was shown to improve predictions. More recently, Fathi et al (2025) modified the layer-based approach from Akingunola et al. (2018) to incorporate the exchange of latent heat between the rising air parcels and the ambient atmosphere. Evaluation of both the new theoretical development and the approach of Akingunola et al (2018) against 2018 aircraft observations during both winter and summer conditions was carried out; the Akingunola layer-based parameterization tended to overestimate plume heights, while the revised parameterization provided a much better match to observations (Fathi et al., 2025)."

At the end of Section 3.6, we also add the following discussion:

"The suggested reduction of the entrainment parameters in neutral and unstable conditions is in contrast to the general underprediction of plume rise in Gordon et al. (2018) using the same model with aircraft measurements. Conditions in Gordon et al. (2018) were typically stable or neutral, although there was substantial variation depending on how and where the conditions were assessed (using either Obhukov length or Pasquill-Gifford to assess stability with measurements from one of two meteorological towers, RASS, or aircraft). The differences may also be due to the different observation methods. Aircraft measurements were between 3 km and a limit of 50 km downwind of the stack locations and it was assumed that the plume heights were terrain following (i.e. maintaining plume height above ground level with changes in ground level height above mean sea-level). Including distances of more than 60 km in the analysis resulted in lower correlation of predicted and observed plume rise. In this analysis using camera images, the camera field of view limits the plume distance viewed to less than 3.8 km (half the field of view with a wind direction offset 45^{o} from the image plane), although the use of the exponential fit may account for further plume rise. It is possible that the plumes may continue to rise or may not be terrain following over longer distances."

3. Section 2.2 would benefit from a brief summary of the DCNN-based framework to help readers better understand the methodology.

We added the following text to Section 2.2 "The DPRNet was trained by manually outlining (segmenting) plume boundaries (if they occur) in 3600 randomly selected images. This data set is then used to train the model to identify the plume boundaries in the remaining images.".

Additionally, we have added more information about the geometric transformation in Section 2.2 and changed Figure 3 (see the response to Reviewer 3's first point below).

Several figures are not colorblind-friendly, including Figures 1, 2, 9, and 10. The authors should revise the color schemes to avoid using red and green simultaneously in the same plot. The resolution in Figure 2 is very low and the annotations are not easy to read because of the colour scheme chosen.

We have modified all the figures to use a Blue, Orange, Yellow, and Black colour template (for the stars in Fig 1 and the markers/lines in figures 9 and 10, now 8 and 9). In figure 2, text is changed to black.

4. In Figures 9 and 10, why are the data for Manual/Visible/Unstable and Manual/Exp Fit/Unstable not shown?

Text is added to the figure captions as "Note that there were no instances of unstable conditions in the Manual dataset, hence it does not appear in this figure."

5. The authors can highlight the newly fitted dimensionless constant values in the conclusion, as these values could be valuable for future studies applying this parameterisation.

These values (now referred to as entrainment parameters following Reviewer 1's comments) are now included in the conclusions section, giving the before and after values.

Minor comments:

- Line 33, be more specific about the dimensionless constants

This has been removed in response to a comment by Reviewer 1.

- Line 34 and 37, repetitive about the observations made

One of the sentences referencing 50 years has been deleted.

- Line 40, missing citation for Moore

This is modified to "Moore (1974) used data…".

- Line 48, what kind of surface concentrations measurements?

Following the comments of Reviewer 1, this sentence has been removed from the revised manuscript.

- Line 52-55 used very similar wordings to Koushafar et al. (2023). The authors should consider paraphrasing the sentences.

The sentence has been reworded.

- Line 56, missing comma in citation

This has been corrected here and in all other instances where this citation style occurs.

- Line 60, check citation style

As above.

- Line 65, the authors commented that the studies referenced are relatively short in duration. Is it on the scale of hours to days? The authors can be more specific about "long term data collection" here, and mention seasonal variations in atmospheric conditions that may affect plume rise theory.

The text is modified to read "Furthermore, the studies referenced above, spanning 50 years, were each relatively short in duration, *mostly repeated measurements spanning minutes or hours each (typically in the daytime), the longest duration being 22 daytime flights over the span of 18 months (Sharf et a., 1993)*. Long term data collection *with consistent measurements through the hours of the day and through changing seasons* would clearly aid in further improvement and development of plume rise theory, which this work addresses." (added text italicized).

- Line 68, since the main stack is already defined here, suggest keeping the terminology throughout the manuscript

We now use the "main stack" terminology throughout the manuscript.

- Line 73, check citation style

As above, commas have been added to citations when necessary.

- Line 73-74, subscripts for CO2, and also elsewhere throughout the paper (e.g., SO2)

This has been corrected.

- Line 77-78, "most of the emission" is a bit vague. What's the percentage of emission from the main stack?

We were directed to a website referenced in the Zhang et al. paper (which will be now included in the references). The value of 67% (average of the 2018 to 2020 period) is added to the text. The web reference (https://open.alberta.ca/opendata/aeirairemissionrates) is also added to the text.

- Line 78, it is unclear what is ordinary operating condition

Replaced with “runs continuously, unless there is a plant shutdown,…”.

- Line 80, this paragraph is disconnected from the previous paragraph.

We have moved the line “In this work…” to the start of the previous paragraph to improve the flow.

- Line 83, what are traditional methods?

This is replaced with “other conventional shallow methods”.

- Line 83, check citation style

As above, commas have been added to citations when necessary.

- Line 90, I find the wordings “determine plume rise” not very clear in this sentence.

The line “determine” has been changed to “measure” in the revised manuscript to improve clarity.

- Line 91, the authors mentioned that Koushafar et al. (2023) used the same initial dataset to evaluate DPRNet. Is the image data used in this paper part of the training dataset or validating dataset?

We add the line “All images from the dataset (including training and validation images) are used in this study.” Put another way, the training dataset and validation dataset are subsets of the entire dataset used in the study.

- Line 99, to compare with the parametrisation -> to compare with the Briggs’ parametrization

This line has been changed as suggested by the reviewer.

- Line 103-108, a lot of the description of the research site is not relevant to the analysis. The authors should consider revising this paragraph.

We have removed the following text: “This river valley is situated within another, wider valley, that gently slopes north-south, measuring 10 to 50 kilometres in width. Boreal forest dominates the landscape.” And “These stations provide helpful data for evaluating the effect of industrial activities on air quality and understanding atmospheric processes. Types of data collected by these stations include meteorological variables (pressure, temperature, humidity, horizontal and vertical wind speed, and wind direction) and air quality parameters (e.g. SO2, NO2, CH4, CO, PM2.5, PM10, and total hydrocarbons). The data collected from WBEA stations are often used to assess atmospheric dispersion models like GEM-MACHv2 (e.g. Russell et al., 2019), in which meteorological and air quality information is essential for accurate modelling, reliable simulations, and validity of air quality assessments and environmental impacts."

- Line 108, missing fullstops.

Punctuation has been corrected in the revised manuscript.

- Line 132, main stack already defined earlier

The information on the main stack, which is phrased in parenthesis "(designated #12908, also referred to as the Main Stack, highlighted by the red star in Fig.1)" has been moved to the first mention of the main stack in the manuscript to provide more complete information upon its introduction.

- Line 133, not necessary to mention 600 feet

This has been removed following the reviewer's suggestion.

- Line 151, move the sentence out of the bracket.

Parenthesis/brackets have been removed from this sentence following the reviewer's suggestion.

- Line 168, missing comma

Punctuation has been corrected according to the reviewer's suggestion.

- Line 223, main stack

This reference to the main stack has been changed to avoid repetitive information given earlier in the manuscript.

- Line 236, Figure 1 instead of Figure 2

This figure reference has been corrected in the revised manuscript.

- Line 455, The numbers look comparable between DCNN-analysed and Briggs-predicted plume rise for the average values and the numbers are also within uncertainty. Why does it indicate Briggs underestimate plume rise on average?

This line is deleted.

- Line 458-459, From Table 3, Briggs under predicts the plume rise for neutral condition and over predicts the plume height for stable conditions. It is unclear why the authors concluded that Briggs generally under predicted plume rise

We have replaced this with "…the results vary based on stability conditions".

Reviewer 3

This study highlights that the Briggs plume rise equations struggle with high-resolution imagery and parameterisation. The authors use a Deep Convolutional Neural Network to generate masks from visible-wavelengths images and compare the obtained geometrical parameters with theoretical calculations from Briggs equations. Their contribution represents a good advancement in the study of this kind of plumes which are usually analysed through more classical in industrial applications. Their manuscript is to be considered close to publication standard once a couple of concerns are addressed.

Specific comments:

1. The authors should clearly state at the beginning that they are correcting the heights they measure for the wind direction. Moreover, they only refer to Koushafar et al. (2023) for the wind correction, without explaining the precess with any equations. Also in that paper, if used as reference for the wind correction, there is a lack of information. I would suggest to check that the equations used by the authors correspond to those published in Snee et al. (2023; 10.30909/vol.06.02.447458), a paper in which the wind correction is applied to correct height and width measurements of volcanic plumes. For example, in lines 200-201 the authors stated that only the wind direction and the camera-smokestack distance are necessary for the wind correction, but normally also field of view, inclination and camera orientation are normally required to perform a complete wind correction. Also, at lines 686-688, a bent-over plume with wind blowing towards the camera normally gives a plume height calculated in the wind-corrected plane that is smaller than that calculated in the image plane (see Snee et al., 2023, for a description of the wind correction applied to plume-shaped objects and for some examples). Also, I am not sure to understand the purpose of lines 682-686.

We thank the reviewer for directing us to Snee et al. (2023), as we were not aware of this very interesting and highly relevant work. We note that we have done substantial calibrations of the geometric transformation with test images and known geometries to confirm the equations are correct. We have also run our calculations against Snee et al. and we get the same results. We do note a slight difference since the Snee et al. equations

$\alpha = i\delta\theta_h$ and $\alpha = j\delta\theta_v$ are only correct for a curved lens and our image plane is flat, but the error is less than 2% for our geometry and camera.

In the 2nd last paragraph of the Introduction we modify the text as: "a geometric transformation from the image to actual plume position *using known properties of the camera and geometry (field of view, cameral level, distance from stack) and the measured wind direction.*" (added text italicized). We also add "using geometric transformation calculations that convert plume position in the image to plume position *in a plane corrected for wind direction.*"

We have added more information about the geometric transformation in Section 2.2 and changed Figure 3. The change is too substantial to copy here, so we refer the reviewer to the modified manuscript. The equations from Koushafar et al. have been added to a new supplementary information section.

Winds blowing towards the camera can result in an overestimation of height depending on the camera angle. An important difference between our camera setup and that of Snee et al. is that our camera is mounted level (on a ridge pointing towards the stack) and this can result in some plumes being below the image centreline, which is reversed from the setup described in Snee et al., where the camera points upward at an inclination.

To clarify the purpose of lines 682-686, we delete the line "Of the 60 images…" (as this may cause confusion), and add the text "This test will demonstrate how much potential error there is if a plume that is actually moving parallel to the image plane (252°) were erroneously associated with a wind direction of +/- 25° (i.e. 277° or 227°)."

2. I would not use the wording "real-life" referred to the position (lines 199,). It is more a corrected projection on a plane corresponding to the wind direction than its actual position (as the wind direction can have uncertainties and the reconstruction is still in 2D). Moreover, I think Figure 3 might be misleading in this sense, as the plume is represented in 2D in birds-eye view, but this is not a real information obtained from the camera.

Figure 3 has been modified to include a vertical side-view of the configuration in addition to the top-down view.

The reference in the text is rewritten as "Figure 3 is a simplified illustration of how the *physical* position of the plume *in horizontal and vertical space* can be *estimated* with knowledge of the wind direction and the distance between the camera and smokestack.".

The figure 3 caption is modified as "The point P' on the image plane is projected to an *estimated physical* coordinate…".

The text in Section 2.3.1 is modified as “Wind direction is used in the calculation of plume rise as part of the geometric transformation from image location (in pixels) to *estimate physical* distance (m)…”

3. Lines 463-464: If my understanding is correct, you are not comparing the same dataset (60 plumes vs only 26 plumes). I guess this might also play a role on this lower heights determined with exponential curve fitting. This needs at least to be stated, or, better, the comparison should be made between the same dataset.

This paragraph is rewritten as follows “We can compare the plume rise determined with exponential curve fitting to the plume rise determined by the final plume midpoint curve height (Table 2), although we note that the data sets are different due to the removal of images where the plume has not reached the full height (given by the exponential fit) within the image. Although the differences are within the uncertainties shown in Tables 2, the curve fitting results in plumes that are generally lower. This is discussed further in section 3.5.3, where comparisons are made using the same number of images. The correlation between the DCNN plume rise and the Briggs plume rise is stronger using the exponential fit relative to the correlation using the final midpoint curve height.”

4. Lines 811-812 and 825-830: Dimensionless constants usually represent physical relationships. Reducing them by a factor of 2 or 6 implies that there is an important change in the dynamics. Also, this parameterisation is site-specific. All this might be at least stated in the manuscript, but has strong physical implications.

In response to Reviewer 1, we have corrected our interpretation of the dimensionless constants to be constants based on entrainment parameters. This is modified throughout the text and based on this suggestion we have added the line in the conclusions: “This suggests that the original entrainment rates suggested in the Briggs formulation maybe be overestimated when applied to the plumes studied at this location.”

Minor comments:

1. Line 79: I suggest to describe better the composition of these plumes. Are they only water vapour plumes? Do they contain other species?

We add “Other notable species emitted from facilities such as this one include SO_2, NO_x, CO, NH_3, particulate matter, and various VOCs (Zhang et al., 2018).”.

2. Lines 126-129: The sentence is maybe unnecessary, as it seems the authors are not correcting the plume rise measurements for the wind direction, which they are actually doing. I would consider erasing the sentence or already specify that the values are actually wind-corrected.

This section is rewritten as "One aspect of the analysis with the use of a single camera is that *the plume location may be outside of the image plane* when the plume direction is *not* horizontally perpendicular to the line-of-site between the camera and the stack**.** *This can be corrected for using the known wind direction (outlined in Section 2.2) if winds are approximately* easterly or westerly (discussed in Section 3.1); however the dominant wind directions in the valley are aligned nearly north-south, as opposed to the dominant synoptic scale SW trade winds. *These images with primarily north-south winds were not used in our analyses, rendering many images unusable, although images that were used were still wind direction-corrected.*"

3. Lines 183-184: I would use "plume position in the image plane to plume position in a wind-corrected plane", as this is still a planar correction (not a 3D reconstruction)

This line has been rephrased according to the reviewer's suggestion.

4. Figure 2: I suggest adding the notations for plume rise height and distance on the images, as well as a white/black buffer around words to improve readability. In the caption, the notation of plume rise height seems to be wrong, while that of the distance is not in math format.

The text is converted to black font to improve readability. Height and distance are noted on the Figure and the notations in the caption are corrected.

5. Line 221: I would use "physical units" instead of "real-life distances", as it is about the conversion from pixel units to physical units (i.e., meters)

This line is modified to "...to estimate physical distance (m)...".

6. Equation 4: Is dividing by L correct? Or is it just the notation that is misleading?

To avoid confusion, we have removed equation 4 and re-phrased the text as: "The Obukhov length can be obtained by setting $z = 100$ m (sensor height for the Lower Camp tower) in the stability parameter (z/L)."

7. Lines 287-288: Why data points are only available every three hours? What ERA5 product has been used? Also, ERA5 datasets normally have a citation.

To clarify, we expand this sentence as: "Here we used a global data repository (Guo et al., 2022) spanning our measurement period in which H is determined (at a resolution of 3 hours and 0.25°) using a machine learning model with ERA5 reanalysis and the Global Land Data Assimilation System (GLDAS) product (Guo et al., 2024)."

And we add the reference:

Guo et al.: A merged continental planetary boundary layer height dataset based on high-resolution radiosonde measurements, ERA5 reanalysis, and GLDAS, Earth Syst. Sci. Data, 16, 1–14, https://doi.org/10.5194/essd-16-1-2024, 2024.

8. Lines 306-317: Could the authors motivate why they chose these particular formulations? Do other formulations exist for smoke plume rise and why they were not chosen?

We rewrite the first line of Section 2.3.5 to read "Here we use the formulation of plume rise from the GEM-MACHv2 chemical transport model, which generally follows Briggs (1984)."

9. Line 357: Is it worth adding the variation for hour of day to the supplementary materials?

Since there is no significant variation, this does not seem necessary.

10. Figure 5: I would add the camera orientation value on the figure, maybe also with a small arrow pointing towards that direction to show it effectively

The red line in Figure 5 has been changed to a red arrow and we add to the figure caption "The red arrow on the red line indicates the direction that the camera faces (from wind angle of 162°).".

11. Line 395: I would change "plumes that are perpendicular to the camera orientation" for "having propagation axis that are close to the perpendicular to the camera orientation", as the plume has an extension and because also plumes twith axis hat are close to the perpendicular are used in the study.

The text is modified to "…to select plumes that *propagate at an axis close to* perpendicular to the camera orientation…"

12. My general feeling is that a lot of tables could be merged (for example tables 2-5 and 6-9) to improve readability.

Tables 2-5,and 6-9 have been merged into two new tables (2 and 3).

Technical corrections:

- Line 108: Missing point at the end of the sentence

Corrected.

- Lines 338-340: Maybe change the structure of the sentence for readability

The sentence is shortened to "In this study, we will present plume rise results with plume rise due to momentum added to plume rise due to buoyancy, since momentum is still expected to contribute to an observable plume rise measured by the DCNN.".

- Lines 341, 343, 345: I would move "plume rise becuase of momentum" before the bullet point for clarity

Corrected.

- Lines 448-451: This sentence is redundant, as it is the same processing applied in section 3.2.1, consider removing it

The sentence now reads: "The 208 images underwent the same filters as in section 3.2.1, after which a total of 60 image remained."

- Lines 572-576: This sentence is unnecessary, as you could just mention that with the applicaiton of the plume rise distance the remaining images are 1514 (from the 3785 obtained in section 3.3.1 with the other filters)

Rewritten as: "In the analysis where the plumes were fitted to asymptotic curves, of the 3830 images that remained following the filters applied in section 3.3.1 (with the exception of the statistical filter), 1514 had a plume rise distance of less than 1296 pixels (i.e. ½ image width), and 1501 of which passed the statistical filter in which data outside of 3 standard deviations from the mean were removed."

- Line 647: missing comma after "average"

Corrected.

- Line 779: Missing "and" between "dawn" and "dusk"?

Corrected.

- Line 780: There is an extra point after "hour"

Corrected.