

Interactive comment on “A Comparison of Plume Rise Algorithms to Stack Plume Measurements in the Athabasca Oil Sands” by Mark Gordon et al.

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General comments

Plume rise calculations are an important aspect of air dispersion modeling. Without reliable plume rise calculations, the ambient concentrations predicted by air dispersion models will not be reliable either. This paper presents uses of some of the most commonly used plume rise calculations, known as the Briggs parameterization, and tests them against plume rise values based on aircraft measurements. They find that the Briggs parameterization systematically underestimates the plume rise, and hence overestimates the ambient concentration. The conclusions are generally supported by the results, but the authors could do more to fine tune their conclusions, and provide

C1

some context by testing some alternatives to Briggs (see next section). In particular, it would be useful to find out which of the Briggs equations is responsible for the lack of agreement. It is unfortunate that the authors chose an area with such complex geography for their validation. A test in an area with simpler geography would have been able to use more reliable meteorology. As it stands, the lack of agreement between the measured data and the predictions is at least partly due to the lack of representative meteorological parameters due to the complex terrain. The paper is clear and well-written. Overall, this is a useful contribution, and can potentially be a very good paper if a number of modifications are made. Detailed comments are given in the next sections.

Specific comments

Note: Many comments here are essentially criticisms of the Briggs parameterization. These are not meant as criticisms directed at the authors.

- p. 4 lines 133-134: The authors used the temperature gradient between the surface and the stack tip as a proxy for the temperature gradient above the stack tip. This will cause the atmospheric stability to appear less neutral than it actually is (i.e., more stable, as s is meant to be used in stable atmosphere).

- p. 4 line 135: when the “maximum” temperature gradient is set at -5 K/km, do you mean -5 is the least negative gradient (i.e., the most stable gradient)? Please provide a reason for this choice, as more stable atmospheres are quite common. Also, it would be useful to test the effect of this restriction on the plume rise predictions.

- eq. 4 p. 4: I realize that most air dispersion models define a final plume rise for unstable atmosphere, but I find this a fundamentally flawed notion: in an unstable atmosphere the plume will continue rising until it either approaches the top of the mixing layer, or gets trapped in a downdraft stronger than the plume’s rise. Given this, it is not surprising that the Briggs parameterization tends to underestimate plume rise. I would argue that the parameterization was designed to underestimate plume rise.

C2

Given this, it is surprising that earlier studies indicated that the Briggs parameterization overestimated plume rise. It would be useful to gain insight as to why earlier studies found plume rises less than predicted by Briggs. Were these also final plume rise calculations, or transitional plume rise?

- eq. 4 seems to predict unrealistically small plume rise when the wind speed is high. Also, depending on what friction velocities are used in unstable vs neutral plume rise, the neutral plume rise equation (eq. 5) often predicts larger plume rise than the unstable plume rise equation. That seems unrealistic to me.

- Eq. 5 p. 5 contains an error. u^* in denominator of the last term should be squared. Please check that the calculations were carried out correctly.

- Eqs. 4 p. 4 and 5 p. 5: the second function of eq. 4 and the first function of eq. 5 are dimensionally not homogeneous, which means they are not supported by similarity considerations, and they will not have a broad validity. Please bear this in mind.

- For both eq. 4 and eq. 5, the second function of the minimum seems more realistic to me. It would be useful to check if these second functions provide better predictions than the first functions.

- An equation that is sometimes used for final plume rise in a neutral atmosphere is $400 F_b/U^3$. It might be useful to check if that equation gives any better predictions than eq. 5.

- eq. 7 only makes sense in a stable atmosphere, because s only has physical meaning in a stable atmosphere. Was it used for stable atmosphere only, or for all types of atmosphere?

- p. 6 lines 208-209: For some emissions, it was assumed that the emission profile in 2013 was the same as in 2010. That puts the calculation on shaky ground. Do you really need these data?

- Table 1 p. 7: Please indicate which data were collected with CEMS, and which

C3

weren't. The CEMS data will be a lot more reliable, and should be treated as such.

- Table 1 p. 7: If I understood correctly, you mention 19 emissions here, but you only used 8 of them. Unless I misunderstood or unless you have a compelling reason to keep all the data in the table, please remove the data that were not used in the test.

- eq. 8 p. 7: Why not use an equation based on the momentum flux parameter F_m for plume rise due to momentum? (see p. 22)

- Table 2: Some correlations seem quite low. To what extent is the lack of agreement between the predictions and the measured plume heights due to wind and temperature uncertainties?

- eq. 9 p. 10: Please check if this is correct. Richardson numbers are normally based on the potential temperature, not the actual temperature. Please also check the other variables in the equation and make sure they were interpreted correctly.

- eq. 10 p. 10: what value of z is used here?

- eq. 10 p. 10: Estimating L without a sensible heat flux measurement or estimation is very difficult. Expect substantial inaccuracies with this equation. This may explain why the values of z_0 vary so strongly by location. A value of 10.1 m, for instance, is suspiciously high even for a forest.

- eq. 11 p. 10: Also expect substantial inaccuracies for this equation. At the verge of a temperature inversion, this equation predicts infinite boundary-layer height. In an unstable atmosphere, the boundary layer height is mainly influenced by the accumulated sensible heat deposited into the atmosphere during the current day, so parameterizations such as eq. 11 are questionable in unstable atmospheres.

- Figure 4 shows a distribution of the calculated plume rise values, for the different calculation schemes and input data. How do these distributions compare with measured plume rise values?

C4

- p. 16: Comparing the average ratio between predictions and measurements of the plume rise will tend to be biased, because a small number of data points with very low measured plume rise (small denominator) can skew the results upwards. To complement this information, it would be best to also calculate the average calculated plume rise, and compare it with the average measured plume rise. This will tend to give the instances of high plume rise the largest weight, so it is also an imperfect measure. Reporting both average ratio and ratio of the averages will give the reader a good sense of how the measurements compare with the calculations.

- Figure 5: It could be coincidence, but I have the impression that there is some clustering of the data points, particularly near the x axis (very low predictions irrespective of the actual plume rise). This gives me the impression that some equations within the Briggs parameterization are far less accurate than others. It would be useful to see the performance of each equation separately (even distinguishing between the two equations where a minimum is calculated). Also, if some of these data are based on CEMS and some are based on emission inventory data, it would be useful to know which is which, because the CEMS data will be much more reliable. I realize that I'm asking for a lot of disaggregation here. Perhaps a supplementary document could be prepared alongside the paper.

- Table 4 p. 19: there is a huge discrepancy between the stabilities evaluated from the data of the different sources. This confirms the poor reliability of the calculation scheme for L. If the Pasquill stability classes are known for these measurements, then it might be possible to determine which data set is most reliable.

- Table 5 p. 20: This sensitivity analysis is very useful, but I find the result suspect. The surface temperature is found to have almost no influence on the plume rise, but the value of H has a large effect. The surface temperature affects H quite strongly, so I don't see how this is possible. Please check.

- p. 22, top: The authors claim that final plume rise is reached within 2 km in all cases.

C5

I find that hard to believe when some plumes rise by 600-800 m. If the Briggs parameterization greatly underestimates plume rise, it will also underestimate the distance to final plume rise. Hence, I would suggest that the authors use the maximum measured plume rise as a guide for estimating the maximum distance to plume rise.

- p. 22 line 668: Please capitalize letter D in my name and sort my book under D in the reference list, not under V. Eq. 17 on line 670 is useful, but I suggest checking out eq. (15.69) on p. 533 of my book as well (after correcting the typos: the factor x^2 should not be there, and the factor u_s in both denominators should not be squared). This equation, as used by CALPUFF, gives predictions of final plume rise when both momentum and buoyancy affect plume rise.

Technical corrections

- p. 6 line 184: delete "from"

- Figure 1 (b) p. 8: the scale on this figure is off by about a factor 2. The scale on Figure 1 (a) seems OK.

- Table 2 caption states that AMS03 measurement height is 90 m, whereas the text (line 286) states it's 167 m.

- p. 23 line 718: Beychok and Milton, 2005 should read Beychok, 2005.

- Please check reference lists of other papers for the correct abbreviations of journal titles. For instance, Atm. Env. should read Atmos. Environ.

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C6