A review of "A Comparison of Plume Rise Algorithms to Stack Plume Measurements in the Athabasca Oil Sands" by Mark Gordon1, Paul A. Makar2, Ralf M. Staebler3, Junhua Zhang2, Ayodeji Akingunola2, Wanmin Gong2, Shao-Meng Li3

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General Comments

Authors find that the Briggs formulations underestimates plume rise for the sources investigated.

Useful alert to potential inaccuracies in the Briggs plume rise formulae which is also used in local scale regulatory air pollution models (e.g., AERMOD). I would recommend this for publication.

If further investigation confirms this result the implication is that this will cause dispersion models using these formulations to over estimate ground level concentrations for sources with tall stacks in flat terrain, for example.

Specific Comments

1. In section 2.1 - Authors should provide more explanation on the limits of L used to define stability classes. For example, why the specific lower limits (-0.25h_s) applied to L to define unstable conditions?

2. In section 2.3 – Authors should provide more comment/investigation of uncertainties introduced by assuming NPRI inventory values for effluent velocity and exit temperature for the flaring stack.

3. In section 2.3 – Authors should make it clear that stack characteristic values shown in Table 1 are for information purposes only and not used in the actual calculations (presumably hourly stack data was used [emission rates, exit temperatures, exit flow velocities]).

4. In section 2.7 – the flights (e.g., box flights) seem to take up to 2 hours and so some measurements during the flight will be time-displaced from other measurements during the flight. Although this is expected, the authors should at least comment that results, such as shown in figure 3(a) actually represent different time periods and that significant evolution of the plume could have occurred during the flight. Are there correction methods for this? How does this affect the measurements?

5. In section 2.7, paragraph starting at line #441 – Authors explain attempts to match calculated plumes to observed; was this matching substantiated by say, video-recording of the plume event? Also final sentence of that paragraph states assumption of multiple plumes having merged; was this actually observed in the field as happening? If so, I'd suggest it be noted.

6. In section 4.1, paragraph starting at line 570 – Authors observe a significant difference between Obukhov length-based stability and temperature lapse rate-based stability. It would be useful to have comment on which is the better method to use.

7. In section 4.2, 1st paragraph – Authors indicate that they change input variables by an "arbitrary fraction" but it would be better to change them to the reasonable limits of their range; this would then provide a more useful ranking of model sensitivity. Some of the variations they have used do, in fact, seem like reasonable limits so are they truly arbitrary?

8. In section 5, second paragraph – Authors quote the Webster and Thomas (2002) study implies an underestimation of plume rise based (seemingly) only on an overestimation of surface concentrations. I have not reviewed that study but there can be multiple reasons that a model overestimates ground-level concentrations (for example, overly conservative emission rates). It would be useful for the authors to provide comment on how they discerned that it was only the (presumed) plume rise underestimation that led to those results.

Technical Corrections

1. Some references made in text that are not provided in the reference list:

    Holmes 2006 is the Holmes and Morawska ref?

    Bringfelt (1968) missing in ref list

    Rittmann (1982) missing in ref list

    England et al (1976) missing in ref list

    Hamilton (1967) missing in ref list

    Moore (1974) missing in ref list

2. Some references in ref list seemingly not used:

    Carson and Moores (1969) – or is this the Moses and Carson ref in the text?

    Beychok (2005)

    Makar et al refs

    Menard et al (2014)

    Pregger and Friedrich (2009)