Response to Short Comment by Ian Falooni

We would like to thank the commenter for his interest in our manuscript. We believe the comments are generally based on a misunderstanding of the technique and calculations and we regret not being clear enough in our original submission.

In addition to addressing the comments and suggestions of the three reviewers, we have further revised the manuscript to attempt to make the error analysis clearer for the reader. This includes a substantial rewrite of the uncertainty section (4.1), the replacement of Tables 7 and 8 with a new table (7), and expansion of the Appendix to help explain the calculation.

The error analysis is a sensitivity analysis. Each potential source of error is incorporated into the algorithm independently, and the effect on the final calculated emission rate is determined. Some of our initial language was misleading and implied that we are calculating the error on each term, which is not the case. The errors are fractions of the final emission rate (E_C) in all cases. To make this clearer, we include the errors in native units in the manuscript (as the commenter suggests) as well as relative percentages of E_C. But it is noted that this does not change the analysis or results.

1) Regarding the extrapolation of CH_4: The ~100 ppb enhancements that are mentioned are downwind and are due to the emission plume from the facility. These are the major downwind plumes that the commenter mentions. These enhancement are not from the forest. The blended approach is chosen for comparative purposes. Since we don't know which approach is correct, all different kinds of approaches are used and the effect that the assumptions have on the final emission rates are compared to give an estimate of the uncertainty.

2) Regarding the summation of the difference of large terms: All uncertainties are presented (in both the old manuscript and the revised version) as fractions of the final emission rate (not fractions of the terms themselves). The inclusion of raw units should clarify this and we are thankful to the commenter for that idea. So although the terms may be large, the uncertainties are very small compared to these large terms, but on the order of 20% of the final emission rate. This is surprising and counter-intuitive, but it is due to the fact the ($E_{C,H}$ and $E_{C,V}$) terms can be expanded (from the integrals of Table 2) and combined in Eq. 1 to give the following equation (which is now included in the Appendix):

$$E_C = M_R \iint_{Sides} \left(\chi_C - \chi_{C,Top}\right)\rho_{air} U_\perp ds\, dz + E_{C,HT} + E_{C,VT} + E_{C,VD} - M_R \iiint_{Volume} \left(\chi_C - \chi_{C,Top}\right)\frac{d\rho_{air}}{dt}\, dx\, dy\, dz\,. \quad \text{(A5)}$$

The in and out terms ($E_{C,H,in}$ And $E_{C,H,out}$) are provided only for interest – they are not independent terms to be analyzed separately. They are derived by separating the surface integral according to positive or negative wind ($U_\perp$<0 or $U_\perp$>0). Hence the three large terms that the commenter mentions, are actually all part of the same integral (the first term on the RHS of the above equation, which is similar to a background-subtracted term). All three of the terms that the commenter mentions could effectively be combined into one term to give an identical answer (and an identical uncertainty), but we feel that keeping the mass-balance terms separate (instead of using the background-subtracted term) makes the algorithm easier to follow. Hence there is no reason why the errors listed in the manuscript cannot be considered independent, which means that it is correct to add them in quadrature.

3) Regarding Convergence:

To be clear, the average vertical velocity (*w*) is not measured directly with the aircraft probe. It is inferred based on the integrated air flux through the box (Equation 2). Whatever the cause of the divergence (orographic lifting, thermal updraft, instrument error), the actual term is an artifact of the separation of the integral, as is shown in the equation above. Vertical advection is effectively contained in the background-subtracted surface integral shown above. Hence any error associated with the vertical velocity is not relevant.

Assuming a wind speed of 6 m s^{-1} and a 4 km distance between the SO_2 emissions source (the stack) and the box wall, a convergence giving an average vertical speed of 0.1 m s^{-1} would result in a plume rise of <70 m over this time. It is not clear if the commenter is suggesting that the plume will rise through the box-top (at > 1 km height) before reaching the box wall, but this seems highly unlikely.

The topography as represented by the perimeter may not represent the complex three-dimensional terrain within the box. The oil sands facilities are complex terrain, with mine faces, access roads, enormous coke piles, and a river valley. We do not believe it is possible to rule out orographic lifting based only on the small sample provided by the perimeter slope.

4) Regarding the wind speed extrapolation: We chose not to include a more in-depth discussion of the wind speed surface extrapolation (including fit statistics) because the effect of the fit is very small. The error analysis (See new Table 7 and Section 4.1.3) demonstrates that using a constant wind speed between the lowest flight level and surface (which is an extreme example) changes the calculated emission rate by less than 1% in all cases. Hence it is unlikely any improved wind extrapolation would have much effect on the final emission rate.