The authors thank reviewer 2 for their efforts in reviewing our manuscript. Our replies are in blue.

This study applies EMG approach to estimating NOx emissions from oil sands in Alberta, Canada. The authors further derived NOx emissions from large off-road mining fleets. Overall, the manuscript is well structured, and the results are robust with clear discussions of the uncertainties of the methods. I think this manuscript is almost ready for publication. I only have a few minor comments.

1. Line 565: It's not clear to me how the EMG method applies to multiple sources. My understanding is that the x,y,s are specific to each plume, but it's unclear to me how the x,y,s are defined with respect to multiple source locations. As the authors mentioned, one source location may affect the other, especially when they are nearby. It's unclear to me how the proposed methods could separate the influence from nearby sources. Maybe it'd be better if the authors could present a figure of several example plumes to explain the methods.

   We have added a figure as requested, and overall expanded on the existing explanation. As part of this we explain how x,y,s are specific to each plume (or location). This material was placed in Appendix D, which describes the multi-source method, and which we invite the reviewer to read. Not that ee also adjusted equation D1 to explicitly show (using script-"R") that the EMG is rotated back into the original lat-lon co-ordinates before being used:

$$\mathrm{VCD}(x,y,s) = a_0 + \sum_{i=1}^{N} a_i \cdot \mathcal{R}[EMG(x,y,s)]$$

   As to the issue of separation, this was examined in detail in Figure 7 which summarizes how far apart two source need to be in order to be resolved (in section 3.2 and appendix F). The finding was that OMI is not generally capable of distinguishing between individual mines, and so this is why only the total (over the surface mining area) emissions are reported.

   Figure 2: Since the trend analysis is based annual emissions, I'd suggest the authors present a figure for a single year. Same for other figures. I think the novel part of this manuscript is the long-term trend, but the figures presented are mostly for multi-year average. It'd be useful if the authors could show the contrast between 2005 and 2022 to highlight the changes occurred.

   We have this figure (designated Figure C1) and supporting text to Appendix C which discusses additional detail of the method used. The text added read:

   Figure 2 shows the mean OMI VCD, OMI rotated VCD, fitted VCD, and reconstructed VCD plots considering all years of OMI data. Analogous plots are shown here but considering only a single 3-year period, 2005–2007, in Figure C1. Each three year period considered, even those in later years with reduced data, are comparable in fit quality. This time period was chosen as it predates much of the expansion of the northern mines, and thus better represents a point source as reflected by a smaller width parameter, σ = 17.5 km vs 20.6 km, and smaller total emissions, E = 55 vs 70 kt(NO_2)/yr, as compared to the all-year analysis.

2. Line 400: The emissions are reconstructed from NPRI emissions averaged from 2005 to 2020, but it seems that NPRI emissions vary yearly. How would this affect the derived trends?

First off, this was suppose to say 2005-2023 (the entire range of NPRI available that overlaps the OMI timeseries). This text discusses the initial proof of concept, and is meant to be illustrative. Later in the paper this procedure was repeated for each year (or 3 year running window) and here NPRI emissions - which do vary somewhat (as the reviewer points outs) – were used.

3. Figure 3: Lifetime should be hours, not years.

Corrected.