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Interactive comment on "Air Quality Predictions using Measurement-Derived Organic Gaseous and Particle Emissions in a Petrochemical-Dominated Region" *by* Craig A. Stroud et al.

Anonymous Referee #2

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This study compares hydrocarbons and organic aerosol measured downwind from oil sands operations in Alberta with the output from the Canadian chemistry-transport model GEM-MACH. The measurements and model are state-of-the-art; the model uses nested domains for North America and Alberta/Saskatchewan at quite high resolution (10 vs. 2.5 km). It is important research as the environmental impact of the production of crude oil from oil sands needs to be carefully documented to allow public opinion and policy decisions about this energy source to be based on solid science. Nevertheless, the paper has some weaknesses that I believe can and should be addressed prior to publication:

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Discussion paper

A. The study relies heavily on an earlier submission to Atmospheric Chemistry and Physics (Zhang et al., 2018), which describes the emissions used in the model. Publication of this earlier paper seems essential for acceptance of the present manuscript.

B. In Sections 3.1 through 3.3, the model is compared with the observations of various lumped hydrocarbon species. I would like to see a much clearer discussion of what is actually being evaluated here. Briefly, the aircraft measurements were used to calculate emissions, which are then used as inputs for the model. The output from the model is compared with the measurements again. Not surprisingly, the model with revised emissions, i.e. those driven by the measurements, agrees better with the measurements. The argument can thus be perceived as being circular, but I do believe it is still a useful exercise and also lays the groundwork for Section 3.4 where the organic aerosol is compared between model and measurements. Nevertheless, the paper should describe much more clearly what is being evaluated in this study (for example more detailed atmospheric transport, model resolution, temporal variability in emissions, etc.). Were the data shown in Figures 3-7 used to calculate the emissions? If so, what is learned from this study about the accuracy of the revised emissions? Are the box flights adequate to quantify emissions or is the transport more complex leading to inaccurate emissions estimates? Another option might be to use part of the measurement data to derive emissions and test the model output with these emissions versus another part of the data set. As it is, the paper gives a fairly dry comparison between the measurements and two different models, and does not describe the above subtleties in any detail.

C. I found the analysis in Section 3.4 to be quite confusing. Earlier work from this group had shown that low-volatility organic compounds are important to explain the strong SOA formation downwind from the oil sands (Liggio et al., 2016). Therefore, my expectation reading this part of the paper was for the Authors to show better model performance using the improved emissions including for low-volatility organic compounds. However, emissions of these low-volatility organic compounds were not explicitly in-

cluded in the model and only mentioned as an afterthought in Section 3.4. The conclusion that is conveyed to the reader is that the observed SOA can be better explained using the revised emissions of hydrocarbons and the Authors recommend a better treatment of SOA from monoterpenes and, perhaps, including SOA from low-volatility organic compounds. I find these conclusions to be almost orthogonal to the earlier work published in Nature.

Detailed comments:

The Abstract is quite long and even contains two paragraphs. While I do not have specific suggestions, one has to wonder if the content of the paper can be summarized more succinctly.

Along similar lines, I wonder if all five Figures 3-7 are needed to present the comparisons between model and measurements.

References

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