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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric transport simulations in support of the Carbon in Arctic Reservoirs Vulnerability Experiment (CARVE)" *by* J. M. Henderson et al.

Anonymous Referee #2

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This manuscript presents the atmospheric transport modeling component underlying the CARVE mission. The main focus is on the evaluation of high-resolution WRF simulations and (subsequently) STILT-based footprints that have been generated. The ultimate goal of this exercise is not only its applicability for flux inversions (as part of the CARVE science analysis) but also the availability of a robust set of WRF-STILT footprints for Alaska. Ultimately the novelty of this work lies in generating and making these footprints available for the wider research community. As such the manuscript is acceptable for publication. The authors may want to consider the following comments, however, to make the manuscript more interesting and scientifically relevant

to the wider atmospheric transport and modeling community. Additionally, the authors may want to distill the results they have presented to improve the readability of the manuscript.

(1) It is necessary to highlight the bigger scientific relevance of this work, i.e., high resolution transport modeling, nested WRF domains, impact of high-resolution input to a transport model, etc. At this stage, the manuscript is very much geared for the CARVE community and reads more like a technical note rather than a scientific paper. In Pg. 27267, Lines 15-17, the authors state that this manuscript is intended to demonstrate the benefit of high-resolution transport modeling on simulation of GHG concentrations. But only Section 5.3 presents brief results for ozone concentrations, which does not justify the claim of the authors in Section 1. What about other trace gases, for e.g., CO2, CH4, etc.? Another paper by the same team (Chang et al. 2014, PNAS) discusses the CH4 simulations. Maybe the authors can consider adding a CO2 component here.

(2) What are the main reasons for presenting the WRF v3.5.1 simulations? In the latter parts of Section 4 and Section 5 the authors persist with the v3.4.1 results; hence, the initial switch between WRF v3.4.1 and v3.5.1 is not apparent. I would suggest the authors to stick to the results with the WRF v3.4.1, and maybe add the WRF v3.5.1 results in the Supplement. Until and unless the authors have compelling evidence to show that WRF v3.5.1 works significantly better than WRF v3.4.1. That does not seem to be the case, however. A few additional comments/questions along those lines: (a) Section 4.1.1 - Switching to WRF v3.5.1 seems to impact the bias in the 10 m wind speed analysis (a consistent impact of -0.1 m/s across all months). The authors justify this by saying that the decreased vertical mixing in v3.5.1 results in lower surface wind speed (Page 27275, Lines 27-30). But Tables 11 and 12 indicate that the bias remains same or decreases at all pressure levels. Can the authors clarify the differences between v3.4.1 and v3.5.1 that potentially impact the wind speed? How does this impact the STILT footprint calculations? Have the authors run the STILT footprints with the

WRF v3.5.1 simulations? In that case, it would be interesting to see a version of Figure 16 but showing the differences between the two footprint calculations. (b) Section 5.1 - It is unclear why the authors suddenly switched to a non-polar WRF v3.5.1? Can the authors clarify? (c) Section 5.2 - What do the authors mean by "CARVE production v3.4.1 two-way nested runs"? (Page 27292, Line 4).

(3) Finally, can the authors comment on the computational time required to run WRF v3.4.1 at the highest resolution and then the time required to generate the STILT footprints? The authors make a strong argument against Eulerian models based on computational time and complexity (Page 27268, Lines 5-9). The authors also need to state the number of particles that were released. 500, or 100?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 27263, 2014.