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Interactive comment on "Carbon dioxide emissions in Northern China based on atmospheric observations from 2005 to 2009" *by* Archana Dayalu et al.

Anonymous Referee #2

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This manuscript compares 3 anthropogenic emission inventories over China's heavily industrialized and populated Northern region, which accounts for ~33-41% of national emissions over the 2005-2009 period. The inventories are evaluated against in situ measurements at a single site using WRF-STILT modeling. The objective of the research is to determine which inventories and their methodologies provide more accurate information about China's CO2 emissions, since China is the highest emitting country. This is a worthy objective, however the paper has a few serious flaws that stand in the way of their conclusions.

The authors state that evaluating all existing CO2 inventories is outside of the scope

of this paper, so they compare ZHAO to two global inventories – CDIAC and EDGAR, which primarily use population as a proxy data to distribute emissions. There is another key proxy that is ignored in this work, satellite observations of nighttime lights. ODIAC (Oda et al., 2108) and FFDAS (Asefi-Najafabady et al., 2014) primarily use this proxy (along with some other data). A strength of ODIAC is that it first uses a point source inventory (CARMA, which is no longer available) then distributes the remaining emissions according to the night lights. One can expect that population would not be a good proxy for a large country with regional variations in wealth, industry and climate. This has already been demonstrated in a comparison of CDIAC and ODIAC over Canada in Nassar et al. (2013) (which the authors have cited in another context), where CDIAC did not accurately represent the provincial distribution, yet ODIAC was much closer. By ignoring ODIAC and FFDAS, I don't think the authors have demonstrated that a regional inventory is generally better than a global inventory for this region of China, just that a regional inventory is better than a population-proxy-based global inventory. In fact, production of the CDIAC 1x1 gridded inventory based on a population proxy has been discontinued. It is my understanding that it has effectively merged with ODIAC such that CDIAC national emission totals are distributed spatially using the ODIAC method, hence the author composition of Oda et al. (2018).

Furthermore, CDIAC 1x1 gridded data have a seasonal cycle (monthly) but the authors state that emissions are invariant over the course of a year. Why was the seasonal cycle ignored? Gregg et al. (2008) discuss the seasonal cycle and show that the amplitude of the seasonal cycle is not negligible. In fact, China has a unique seasonal cycle with a peak in December and a minimum in January, which differs from the standard seasonal cycle of other countries in its latitude range. Most other countries peak during the cold months due to heating or more recently show two peaks due to heating and air-conditioning use in the coldest and warmest months of the year. Due to the use of a single observation station and the changing wind direction with season that the authors have demonstrated, I think the issue of seasonality becomes even more important for assessing inventory biases.

I am also not convinced that the authors have demonstrated that observation-model discrepancies cannot be attributed to their biospheric model fluxes, initial CO2 fields, or transport from outside of the regional model domain or transport errors. In fact, all of this is very difficult to do (maybe impossible) with a single observation station. Information ruling out some of these factors may actually be buried in the supplementary information (35 pages), which I should note is the most extensive that I have ever experienced in many years of reviewing.

It is difficult to predict the global climate policy implications of finding a large error in China's reported CO2 emissions. For this reason, any scientific studies that suggest our understanding of China's emissions may be incorrect, need to have very solid evidence to support the finding. In the present version of this manuscript, there are some weak spots in the evidence presented.

Specific points

P11, line 11-16: CDIAC national emission numbers are a benchmark as stated, but the gridded and spatially distributed data are really a separate dataset.

P17, line 13: Dayalu et al., 2018 is not listed in the references. Do they mean Dayalu et al. 2017, or a different manuscript?

P17, Fig 4: There is no discussion on why the VPRM signal relative to CarbonTracker is so small for June, when biospheric uptake is near its maximum.

References

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