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Atmospheric Chemistry and Physics
Discussions

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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Dayalu et al. 2019 compare observations of atmospheric CO2 mole fractions from the Miyun stations from 2005-2009 with a modelling framework based on the WRF-driven STILT model, biospheric CO2 fluxes from VPRM and three different spatially explicit inventories for anthropogenic CO2 emissions. The study investigates the model-observation mismatches at different temporal scales for a domain limited by the sensitivity of observations at the measurement site according to WRF-STILT. The scaled inventories are then used to assess of regional changes in carbon intensity can are visible in the 2005 to 2009 time period. The paper is overall well-written despite a sometimes complicated structure. It seems that a bigger focus should be given to the key results (annual CO2 emission increments and regional carbon intensity trends).

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Discussion paper

The authors address a lot of the caveats of a study that are to be expected due to the limitation of a single observational site. However, not all issues are fully addressed at this stage. Significant changes to both content and structure seem required before this manuscript could be considered for ACP.

General comments:

The title implies that actual flux estimates for Northern China are the central point of this paper. However, this paper is very technical and focuses much more on the comparison of existing inventories with atmospheric observations and also the regional emission intensity. The title should be updated to better reflect this core content.

The authors present the L_90 footprint, which usually reflects the theoretical sensitivity of the observations to a unit of flux. Two issues arise with this. First and foremost, the actual area influencing the receptor observations is not reflected by this, as CO2 sources span multiple orders of magnitude. Therefore, a major source e.g. coal-fired power plants just outside the L_90 footprint will have more influence on Miyun CO2 mole fractions than a deserted patch of land without CO2 flux inside the L_90 footprint (e.g. in inner Mongolia). The second problem is that the authors use a plethora of different terms and all seem to refer to nearly(?) the same thing: “L_90 footprint”, “influence region”, “L_90 region”, “L_90 evaluation region”, “90th percentile of multi-year mean annual STILT footprint influences”, “surface influence maps”.

A further limitation is that only one biosphere model is used. The authors seem to ignore this limitation, while nicely highlighting that having 3 anthropogenic prior is very helpful to better understand general results of modelled atmospheric CO2. The fact that biospheric fluxes might be even more uncertain than anthropogenic CO2 fluxes seems to unrecognized. A straightforward analysis to investigate the relevance of natural versus anthropogenic fluxes would be to investigate if the biggest model-observation mismatches systematically occurr during times of high contributions of anthropogenic or natural fluxes to the modeled CO2.

Furthermore, multiple papers that address anthropogenic CO2 emissions in China and specifically the Beijing region are ignored, e.g. the PKU-CO2 inventory (see comment line 35f) or the isotope studies by Niu et al. 2016 (see comment line 364f). A comparison to their results would be an important addition to this study. Lastly, one important result of this study is the trend in regional carbon intensity. Unfortunately, the calculation of GRP and its trend as well as their uncertainty is not clear enough. To really assess the importance of reducing the uncertainty in CO2 emission estimates by using atmospheric observations strongly depends on how well GRP and GRP trends can be calculated and also scaled to GDP and GDP trends. The explanation, data sources and methodologies for the GRP calculation should be expanded.

Specific comments:

Line 35f: When did this research begin? The PKU-CO2 emissions inventory which is China-specific was published in 2013, but is not considered or even mentioned in this manuscript (Wang et al. 2013; doi:10.5194/acp-13-5189-2013, available through e.g. http://inventory.pku.edu.cn/download/download.html)

Line 56f: The author's should expand more on the nature of the differences of different inventories. Atmospheric measurements will only be able to consider scope 1 emissions and do always include all sources, while national inventory reporting and provincial reporting might use different methodologies and also different emission category definitions and reporting thresholds. Therefore, it is unclear if the mere fact that there is a discrepancy between provincial and national estimates really means that there is a difference that an atmospheric approach could detect, help to decrease.

Line 79: see comment line 35f

Line 118: A key element that needs further explaination is the notion of "surface influence map". This seems to be used to describe the footprint, i.e. the sensitivity of the observations to a unit of flux (emission) from a given area. However, in line 645 the "L_90 footprint" is apparently something separate from the "influence region". See general comments.

L130: Figure 10 from Dayalu et al. 2018 does indeed show that natural and anthropogenic fluxes are the same order of magnitude in the growing season. But given the very significant variability (1-sigma is near 100%) it seems unclear why the authors assume that this is only in the peak growing season and not also in other months e.g. May 2006 seems to have high uncertainties in relative importance of natural versus anthropogenic CO2 fluxes.

L146f: Please elaborate why transport errors (which can be systematic in nature) could not cause a bias when comparing inventories with distinctly different spatial distributions.

L160f: Wang et al. 2010 provide information on instrumental precision and that a calibration strategy was in place to monitor long-term drifts. This seems like an important addition here.

Line201f: Given that a short tower is used for observations it seems useful to know what the height of the lower/lowest WRF levels used are. 41 vertical levels are mentioned, but without additional information this seems difficult to interpret.

Line 234f and Figure 2: It seems important to expand on how your "L_90 region" was calculated. It is referred "90% of the surface influencing measurements". So this would mean that it is NOT the footprint or the 90th percentile of the surface sensitivity. The surface sensitivity/footprint reflects how a unit of flux will alter the observed mole fraction. However, even regions with very low sensitivity can still have a noticeable influence on the observed concentrations. Figure S8a clearly shows that some regions within the "90th percentile (Northern part of China) have emission rates that are at least 3 orders of magnitude lower than areas just South of the 90th percentile footprint (Nanjing-Shanghai region). It seems very likely that atmospheric CO2 mole fractions at Miyun would be more affected by these Southern Emissions than from some remote Northern regions that. A true influence/contribution map could be calculated very quickly with the existing data.

Line 238f: Are really 40% influence/contribution coming from outside of L_50 or rather 40% of footprint sensitivity lies outside L_50?

Line 256f: The justification of the interpolation seems to rely on the fact that only a few regions show large differences. However, a more straightforward method would be to convolute the 2005 footprints with 2005 emissions and then with 2009 emissions (using the same 2005 footprints). This way we can directly assess if the flux changes are theoretically noticeable in the atmospheric record used later or if this just adds "random noise" to the observations.

Line 274/275: citations needed

Line 287f: see comment line 35f

Line 332f: Given that only one simple biosphere model is used in this study a discussion of its performance and uncertainty would be very useful here. How well does VPRM-China compare to the local/regional flux towers sites within the L_90 footprint?

Line 351 – eq 1? The equation seems to imply that CT2015 was used for all years – maybe add clarification that CT fluxes for the appropriate years was used and not a climatology based on CT2015. CO2(t) = CO2,obs(t)-CO2,CT2015(t-7d) Also, the equation is not numbered/labelled.

Line 364f and S5: The authors suggest that the anthropogenic fluxes dominate the annual total in the main text and then go even further in the supplement and suggest that natural fluxes are negligible. Quote from S5: "... correction at annual scales is therefore applied only to the anthropogenic emissions inventories" This a very strong assumption and seems to require further explanation. During the growing season fluxes seem comparable and VPRM underestimates respiration fluxes in the non-growing season (see line 504). In the absence of other biosphere models in this study to cement this notion it seems necessary to refer to other studies in China to rationalize this.

For example, Niu et al. 2016 [https://pubs.acs.org/doi/abs/10.1021/acs.est.5b02591] found that even in Beijing (Haidan district) CO2 from fossil burning only contributes 75% to the annual average CO2 offset. So, it seems unlikely the natural contribution to the CO2 mole fractions at Miyun can be ignored, even at annual average scale.

Line 373f: Why is VPRM now classified/labelled as an inventory and not as a biosphere model anymore?

Line 402f: Please clarify the distinction you make between "footprint extent" and "influence region"

Line 415 - 417: Please clarify - on one hand, during winter the receptor is predominantly influenced from low emitting regions northwest (Inner Mongolia), but also subject to CO2 from inefficient district heating? Is that district heating in Mongolia? Or do the more local CO2 sources (e.g. Beijing) dominate the atmospheric CO2 mole fractions at Miyun during this season?

Line 457f: Suggests that section 4.2.1 implies that better performance of EDGAR and CDIAC is due to an artifact of their lower emissions. However, section 4.2.1 does not explain why EDGAR+VPRM and CDIAC+VPRM being too low has to be due to EDGAR and CDIAC and not a feature of VPRM. One could also easily argue that ZHAO+VPRM matching at hourly scale is an artifact due to too high anthropogenic fluxes. A more detailed discussion why matching hourly data is correct and matching the seasonal data is likely an artifact would be helpful here. One point raised later (line 504) is that VPRM underestimates non-growing season CO2 respiration. Wouldn't this even further improve the fit of EDGAR/CDIAC+VPRM in Figure 4? And maybe partially explain the underestimation at hourly timescale?

Line 505-510: see comment line 457f.

Line 550: More detailed needed on the calculation of GRP and a proper assessment of its uncertainty is crucial, see general comments.

Line 551-556: A list of potential reasons for changes in GRP and CO_2 emissions is given here, but it is unclear if this is linked to table 3 or just a list of events happening in the discussed time window. For example, the financial crisis of 2008 is mentioned, but no decrease in GRP is visible in Figure 9a.

Line 556: Should maybe refer to Figure 9a not 6a?

Line 573: Previous section was already 4.4

Line 590: Figure 8 seems not to support an evident increase in CO_2 emissions strongly correlated with GRP. Maybe a scatter plot of GRP versus CO_2 emissions would help to highlight a possible correlation.

Line 596: Please elaborate why doubling of GRP suggests enlarged production capacity as driver for emission reductions? Would a shift towards more service-oriented businesses or production of higher value goods have the same effect?

Line 597: The reported decrease of regional carbon intensity by 47% (28%, 65%) is based on which inventory?

Figure 9: This is a core result of the study and could be discussed in more detail. Are the regional carbon intensity trends of the 3 inventories significantly different after applying the correction when uncertainties in GRP are accounted for?

Line 608: A longer discussion of the limitations introduced by only using one biosphere model could be added.

Line 645f: see comment line 402f

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