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Atmospheric Measurement Techniques Discussions

Interactive comment on "The Fossil Fuel Emissions of Tokyo estimated directly from measurements of the Tsukuba TCCON site" *by* Arne Babenhauserheide et al.

Anonymous Referee #1

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—->General Comments<—-

This paper discusses a first order estimate of carbon dioxide (CO2) emissions from the greater Tokyo area using total column measurements of CO2 at one location, namely the Tsukuba Total Carbon Column Observing Network (TCCON) site. The authors derive this flux estimate by first assuming typical annual and diurnal trends are what would be observed without local/regional anthropogenic emissions. Then they use radiosondes to get an average wind speed and direction of the layer of atmosphere enhanced by local emissions. Next they assumed an emissions area defining Tokyo as an arc of a circle with the center at the Tsukuba site, and the angles were based

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on wind direction and typical enhancements. Finally, they assumed uniform emissions then used this assumption with wind speed to infer a flux.

Studies like this are very important, as there are few such studies estimating fluxes using total column measurements to fully estimate a CO2 flux. It is well suited for AMT. The authors made good progress on a difficult problem – namely, how to estimate fluxes when few data are available. However, I have concerns with the stated and unstated assumptions, as well as with the general methodology. Further, the flux estimate is 5x that from the Bureau of the Environment Tokyo which suggests the uncertainties are very large. It is important to improve accuracy in these studies as much as is reasonably possible because inaccuracies could lead to false interpretation and perhaps improper carbon pricing by policymakers. Hence even though I think with major revisions this study could be useful to the community I have many comments.

—->Specific Comments<—-

1. Title: Clarify that the estimates are 1) of net CO2 (not all GHG and not just FF), 2) from the *greater* Tokyo area. Also 3) these are not “direct” as wind measurements were required.

2. Abstract: Needs more detail. Specify the FTIR is not in situ. Define TCCON. Briefly describe methodology (2-4 sentences). Compare with literature estimates.

3. p 1, line 13-16: Only some of these references appear relevant, and there are some the authors should consider including that include a CO2 flux estimate. I suggest omitting the following: Bovensmann (only an OSSE), Hakkarainen (includes enhancements only, not a flux), Hammerling (simulation), Hakkarainen (duplicate), Butz (did not use satellite data), Frey (only an instrumental study), Chen (estimated CH4, not CO2 flux). Consider including: Liu et al, 2017 (estimates of CO2 fluxes from different regions using OCO-2 data, doi: 10.1126/science.aam5690), Nassar et al 2017 (CO2 estimates from power plants using OCO-2 data, doi: 10.1002/2017GL074702), Ye et al 2017 (constraining urban CO2 emissions with OCO-2 data, doi: 10.5194/acp-2017-

1022), Irana et al 2018 (CO2 fluxes from a peatland using column measurements, doi: 10.1038/s41598-018-26477-3), Hedelius et al 2018 (CO2 fluxes from a California region using column measurements, doi: 10.5194/acp-2018-517), Vogel et al 2018 (CO2 fluxes from Paris using column measurements, doi: 10.5194/acp-2018-595), Wu et al 2018 (made simulation of CO2 columns for OCO-2 using a new modelling tool, did not compute flux but still may be useful, doi: 10.5194/gmd-2018-123).

4. p 2, line 1-2: provide a little more detail on TCCON here. How accurate and precise is it? Where do the data come from?

5. p 2, line 6: When I think "inexpensive," I think of BEACON (Shusterman et al, 2016 doi: 10.5194/acp-16-13449-2016), with a price of 5500 USD per sensor. Surely a TCCON site costs more than this otherwise there would be more around Tokyo? I would also expect the cost of one-time radiosondes to add up to more than this. Same comment for p 14, line 6 – are the "affordable" mobile spectrometers really less than 5500 USD?

6. p 2, line 10: Better to cite the original reference where the stated instrument-to-instrument bias is 0.3 ppm (Messerschmidt et al, 2010, doi: 10.1111/j.1600-0889.2010.00491.x).

7. p 2, line 13 – 0.2% is 0.8 ppm of CO2

8. p2, line 16-17 – This sentence can be omitted, as the dates are already listed on line 6

9. p2, line 18-19 – I checked this website, and the information does not seem "essential" to this paper. It is mostly interesting photos. This sentence can be omitted or should be reworded.

10. Throughout – please incorporate the footnotes into the main text (see https://www.atmospheric-measurement-techniques.net/for_authors/manuscript_preparation.html). For example, move data

references to the “Code and data availability.”

11. Section 3/4 – Put the discussion of mean wind speed/direction first because it seems important to the discussion of removing trends, especially the discussion of air origins.

12. Remove parenthetical comments about granularity. From the trendline in Fig 1 it is clear that the polynomial fits finer and coarser time periods.

13. Section 3: I am concerned with these fits in general. Why are they polynomials instead of the typical sines and cosines (e.g., Thoning 1989, doi: 10.1029/JD094iD06p08549). There is even a Python package to do this: ccgfilt (ftp://ftp.cmdl.noaa.gov/user/thoning/ccgcrv/). Why are these fits done in 2 parts (overall linear then annual polynomial), rather than one unified fit? Why were these degrees chosen (the higher order, the better the fit should be)? How are you sure the diurnal fits are really background and not a measurement artifact (the R2 is very low)? How are you sure there is not a persistent enhancement in column CO2 due to Tokyo emissions that would get fit out and hence bias your enhancements? How do these “background measurements” or fits compare to similar measurements (TCCON or satellite) away from urban areas nearby?

14. p 4, line 12: State which supplemental figure number.

15. p 5, line 4: This looks like a 2D heatmap of averages values per bin, rather than counts per bin (i.e., it does not look like a histogram. . .)

16. p 5, line 7-8: These last 2 sentences are confusing, please clarify.

17. p 5, Fig 3: It would be useful to include a variant of the statement from p 14, line 22-24 here.

18. Section 4.1: General convention is to only number subsections if there is more than one. Also, please clarify throughout what the winds data source was. It seems like there were 3: ground-based from the measurement site (?), HYSPLIT, and ra-

diosondes.

19. p 6, line 10: A few sentences to a paragraph should be written to describe how HYSPLIT was run. Were these forward or backward trajectories? What heights were they released from? What gridded model was used with them? How long were these runs? How many days were these runs for? Consider adding a figure to show the results. Those in the SM are a start, but are in my opinion over too short a time period and with too coarse a model.

20. p 6, line 10: Please have a separate comment to indicate Ready was also used for the Rolph 2017 reference.

21. p 6, line 11: Include Tateno on a map and/or include coordinates.

22. p 6, line 16: Is “average wind speed in the profile” defined as that from the surface to 1 km, 2 km, or the highest sonde point?

23. p 6, line 22: This statement does not appear to be in this reference.

24. Fig 4: This figure is slightly deceptive as a ratio is plotted, so 1/10 is equivalent to as much scaling as 10x. This makes it look like there are more high outliers than low ones. Consider plotting on a logarithmic scale instead.

25. Section 5: I personally had difficulty understanding this section and hence the validity. It should be rewritten in a stepwise, building fashion with explanations and assumptions stated throughout. Specifically

a. Watch units, and keep them consistent with names. P6L2 should units be gCO2/m2? P6L3 area is in m2, so use a different letter besides A for m2/s. P6L6 “area” (mˆ2) should be “distance” (m) here. P7L8 “t” usually refers to time. It would also be helpful to include units for everything.

b. Try to be consistent with past literature notation. P7L9 I confused barDelta_CO2 as just the average column CO2 enhancement, but here it refers to the average column

enhancement multiplied by wind speed.

c. Eliminate subscripts where possible. E.g., v_wind -> v, E_m -> DeltaC, S_T -> F or E, v_alpha -> v(alpha), g_a_t,l -> g, p_t -> p (or p(t) if time is important). These are just examples, choose notation that suits you and is in line with the literature. In some cases the authors could argue that one or two letter subscripts are useful, but to me subscripts have been used in excess.

d. Proceed in a stepwise fashion, and state assumptions along the way. It seems Eq. 4 should be first, and should probably be split up. It would also be helpful to list limits of the integral, and not recycle alpha (this is one case where I think alpha_0 and alpha_1 would be acceptable).

e. Consider placing P7L17-27 and the footnote in a Table. Units could be included in this table.

f. Equation 5 – is water not important? Also, this term is duplicated on line 24.

g. Equation 7 belongs in Section 3. Pick units and stick with them for this fit. Here the "m" terms are in g/m2, but Figure 1 is in ppm. As currently described in P9L11-16 it seems circular (Eq 7 requires 5 & 6, but 5 & 6 are not applied until after 7).

h. p 9, line 26 – This is mislabeled as "gravity," but is actually acceleration due to gravity (m/sˆ2). Gravity is a force (kg m/sˆ2).

26. p 8, line 9: What is the importance of the "perpendicular spread"? Generally, I would think of estimating fluxes using the wind speed divided by the transect across the emitting region to get a residence time (e.g, Eq. 2 in Viatte et al, 2017 doi: 10.5194/acp-17-7509-2017) multiplied by the total region area assuming constant emissions. How do you reconcile this difference?

27. p 8, Fig 5: What are the bin sizes for the mean values?

28. p 9, line 5: What are the coordinate of the palace? Why was this location chosen?

29. p 10: I think equations 10-12 can be omitted, and the result of Eq. 12 simply appended to Eq. 9.

30. p 10 Equations 13-15 and p 11 Equation 17: these seem to be using multiple definitions of "degrees." Gridded emission inventories (e.g., ODIAC) are expressed on grids of global latitude and longitude degrees. Here the degrees refer to the angles around the TCCON site out to an unspecified distance. These should be omitted or properly converted to emissions per latitude/longitude degree box instead.

31. p 10, line 20-22: The TIMES product provided by Nassar et al (2013) is on a 0.25-degree grid. What grid area was selected here to represent Tokyo?

32. p 11, line 23: It seems like this estimate would be more representative of actual emissions, and is better aligned with other estimates anyways. Consider listing it in the abstract and placing it in the Conclusions section instead (or in addition to).

33. p 11: The "background" could also be biased (see my previous comment), and this uncertainty should be included. What is the accuracy of the winds? Why are the uncertainties added directly instead of summed in quadrature as is standard for Gaussian uncertainties? Are there uncertainties from the unstated assumption of uniform column sensitivity (i.e., averaging kernels equal to unity)? This last one is a common oversight, but needs to be discussed in remote sensing studies.

34. p 12, Fig 6: Draw enclosed boundaries representing 1) the extent of the Tokyo area using your circle arc definition, for 2) the ODIAC area summed, and for 3) the Bureau of the Environment Tokyo definition if available.

35. p 13, line 2: which version of ODIAC was used? Also, ODIAC is an abbreviation, so it should be defined and capitalized.

36. p 13, Equation 18: Why is the full area summed, and then the background subtracted? Why isn't just the area from non-background (i.e., source) directions added?

37. p 13, line 9-10: A wind direction difference by layer should be added as a subplot

to Figure 4 to support this claim.

38. p 13, line 17: I do not understand the last part of this sentence, please rephrase.

39. p 14, line 7: Please change to a reference that includes a decade of CO2 column measurements from the mobile spectrometers instead.

40. p 14, line 11-16: It seems like this includes contradicting statements. First it is stated that uncertainties could be reduced, but at the end it is implied that the simpler evaluation cannot reduce uncertainties. If the uncertainty can be reduced, it should be. If it can (likely) only be reduced by using a more complete and hence complex model, this should be stated instead of the first sentence.

41. p 14, line 18: Turner et al (2016, doi: 10.5194/acp-16-13465-2016) would be an appropriate reference for increasing network density to improve spatial understanding of emissions. It seems Hase et al (2015) also made some progress towards this.

42. p 14, line 20: I do not understand this claim. Please rephrase and/or provide a reference.

43. Consider including an Author Contribution section (strangely stated as optional in the *.tex template, stated as required online).

44. References – In several cases a discussions paper is cited, when a peer-reviewed version is available. Of the articles I would not exclude these are Massart 2014, van der Laan-Luijkx 2017, 2014b.

45. p 18, line 35: Is more information available for this reference? A doi? A url?

—->Technical Comments<—-

p 1, line 17,20: Should “i.e.” (in other words) be “e.g.” (for example) here?

p 1, line 19: *historically* short mission times. (Some satellites like GOSAT have been in orbit nearly 10 years! Though GOSAT-2 has been in orbit less than 1 week)

p 4, line 4: there's -> there is

p 4, line 14: over *data from* all TCCON

p 6, line 2: source *angles* of Tokyo

p 6, line 10: *HYSPLIT* (define on first use also)

p 8, line 2: the source of Tokyo -> the CO2 flux from Tokyo

p 11, line 2: distribution *of distances* of

p 11, line 5-6: I see no reason why "from Tokyo area" should be italicized (same with p 13, lines 3-4)

p 14, line 3: megatons carbon per year -> MtC/yr (or megatonnes carbon per year)

p 14, line 19: lower -> finer (?)

—->Supplemental Comments<—-

I did not give this a full review, but I am including some comments here.

S1. The SM includes two parts, (1) scripts to reproduce work, and (2) figures to support the main text as needed. Because most readers will only be interested in (2), the material besides the most relevant *.pdf (emissions-tokyo-auxilliary.pdf) should be moved into a subdirectory.

S2. SM Fig. 1: I actually disagree that there is only a weak correlation between windspeed and time of day. In the morning the direction appears to predominately be from $\sim$300, and then it moves towards $\sim$120 in the afternoon. Also, where did these data come from? A meteorological station near the TCCON site? What is the quality? Is the banding at 100, 200, and 300 real or an artifact? There also appears to be a lot of null data at 0 degrees, likely from when the sensor was not moving.

S3. Section 2: This appears to be unfinished.

S4. Section 3: availble -> available

S5. Section 4: More detail is needed in the text on how HYSPLIT was run. Also, the current model (GDAS1) is 1 degree, which seems too coarse to support claims of understanding vertical transport within 65 km. I cannot see the concentric circle labels, but it appears this plot only goes out to 20 km.

S6. Figure 3: I agree that scientific presentations could use more humour, especially in talks and posters. However, I think for this more formal scientific article the figure should be recreated without the doodles though indeed the resemblance is there. Besides, such references can lead to bizarre dreams, obscure fevers, and such knowledge was dangerous to Professor Angell. The caption is also unnecessarily verbose (same for Fig. 2, 4).

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