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Atmospheric Measurement Techniques Discussions

Interactive comment on "Airborne remote sensing and in-situ measurements of atmospheric CO_2 to quantify point source emissions" *by* Thomas Krings et al.

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The manuscript "Airborne remote sensing and in-situ measurements of atmospheric CO2 to quantify point source emissions" by Krings et al presents results from an airborne campaign, inferring point source fluxes of CO2 using both mass balance approaches as well as a Gaussian plume modeling of remotely sensed total column averages. Even though the data presented here is indeed interesting, I tend to agree with reviewer 1 that it often reads much like a report and would need restructuring and more concise (and precise!) language. At times, the authors get caught up in details that are not entirely relevant to the study at hand, e.g. Figs 9-12 are too detailed or not necessary (9-10) or misplaced in the respective section. I would suggest putting a

general description of the domain as well as the data right at the beginning (e.g. showing MAMAP footprints as well as in-situ ground projections on the map in Figure 1. It would greatly help setting the stage for the discussion.

Some more specific comments: Abstract last sentence: this is a sudden topic break and needs some rephrasing

Page 3, line 30: wheras (there are many small things like this or "straight forward", which is one word. I won't go into more details, the copy-editor should catch those at a later stage but some sentences are too literal translations from german.

Section 4: I think this is poorly described and justified and I urge the authors to consider revisiting the differences between their approach and the paper Levi Golston mentioned.

E.g.

I) you mention "Kriging" is not necessarily the best suitable approach. If you provide critic, you have to back it up either with an analysis or a citation. There is also no real explanation what kind of interpolation schemes you are using (apart from the boundary voxels).

II) You mention that you include turbulent fluxes. This is very interesting and I was excited but then I didn't see any further analysis. Did you compute the differences with or without turbulent components? What is the relative error in your case? Can you plot c-mean(c) vs v-mean(v) for some voxels to show the correlations as expected for turbulent fluxes?

III) Wind speed seems to be a dominant error, do the others actually matter? You will need to provide realistic estimates regarding kriging and turbulent fluxes, otherwise the reader won't be able to judge the importance (even though this specific case study might not lend itself to extrapolation to a general case). Page 5, line 9: As above, please show what error you incur by doing mean(v)*mean(c) vs. mean(v*c)

Figure 4: Please add color-bar and make this a realistic example based on real data. How many data points to average do you typically have per voxel?

Page 9, line 8: Conditio sine qua non: Even though I have a "Grosses Latinum", I had to look it up again. Please rephrase in plain english, esp. as it is here used in a rather trivial way, not warranting the grandiose latin phrase ;-). One might argue though that precision "could" be important if it is really bad while accuracy won't matter. This could be a factor when flying very cheap instruments on small unmanned aerial vehicles near the plume. So I would keep the discussions as general as possible.

Page 9, line 17: Chimneys: It would be good to discuss how well you could measure the fluxes if the emissions were to happen at the surface. What would this imply for the in-situ based approach and potential flight-paths.

Figure 5: This figure confuses me. I "assume" the dots are actual measurement locations. Given what you wrote, there is a constant extrapolation to the surface. However, it doesn't look that way for the second little intrusion at x=0. Also, there are a couple of local maxima in between dots. You need to explain the interpolation scheme and this would a good place to compare against kriging or other interpolation schemes. Also, if the dots are measurements, please color-code them with the actual measurement values at that x-y position. This will help evaluate the interpolated fields better. A last point: Why is this continuous on x and y? Wouldn't it make sense to sketch out the actual grid boxes here as well? Page 12, line 3: Please add citation for proxy method (this is not common knowledge).

Figure 6: I think the figure itself doesn't tell more than the text, could be skipped. Same with Fig. 7

Page 14, line 17: So in essence, you don't really need the wind speeds in this case as the error is rather small?! Ideally, you won't always need both aircraft. If there is confidence in modeled winds, it would be a good sign for future remote-sensing only campaigns.

Figure 10: Weird x-spacing (value 493?). also better to use same x-scale for both subplots.

Page 21, line 10 +/-: Wouldn't you ideally fit a Gaussian model with a vertical wind-speed profile? This would rather directly model the total column AND the wind-profile. How high is your Gaussian profile extending to the vertical? That might be a plot to add (or is it just 2D in x and y?).

Page 23, line 19: "This is because they to a good extent cancel out..." "largely" cancel out?

Page 26, line 20: I think they gase don't need to be inert, just have lifetimes much longer than the time between emission and measurement. I would guess even NOx emissions could work with an "inert" assumptions on this very local scale.

As a last general point: Please try to re-structure somewhat to bring out the key messages in a more concise way (and illustrate better how your in-situ inversions differ from others). At the end, provide a more generic overview of both flux estimates and its pro/cons and path forward. This could extend to a discussion using high-resolution mapping like the cited AVIRIS-NG papers (which should be cited at page 26, line 9).

Last but not least my sincere apologies for the late review.

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