Review of Revised Submission:

Quantification of CO2 and CH4 emissions over Sacramento, California based on divergence theorem using aircraft measurements: Ju-Mee Ryoo et al.

General Comments:

After the revision this manuscript has improved tremendously. The structure is much clearer and results are presented more uniformly. Figures and tables are well presented and include all the necessary information for the conclusions. Furthermore, a lot of issues due to language use have been resolved. Still, I have two minor revision points I would like to be addressed clearer before publication:

First, I believe that your treatment of interpolation/extrapolation methods is not complete. You show different methods and compare them, but never show how they influence the final emission estimate. A small section on "Sensitivity of the calculated flux to interpolation/extrapolation method" would be great at the beginning of chapter 3.2.

Second, the chapter on uncertainties needs restructuring and clarification. Several factors of uncertainty are named at several locations in the text, e.g. wind (l. 415, l. 423, ll. 441-446) and kriging (ll.411-414 and ll. 425-429). Please consolidate these sections for better readability. Your method (ll. 430-432) is mentioned after some factors of some uncertainty, but afterwards (l. 432 ff) you start listing additional factors (PBLH, vertical fluxes) again. Please list all factors first, or present your method first, but don't mix. How do you get to the overall estimate of 10% uncertainty? What is the variance and standard deviation of the kriged mixing ratios? Grid resolution (4%), variogram model (5%), wind measurement (? %), mixing ratios (? %), PBLH (10 %), vertical fluxes (1%), does not sum up to 10%. Please explain.

Additionally, how do your sensitivity studies influence your uncertainty estimate? You do all these sensitivity studies, receiving differences in the emission estimates of up to 80 %. Is that no uncertainty?

Specific Comments and Technical Corrections:

l. 49: "The uncertainty is also impacted by meteorological conditions and distance from the emission sources". These are two factors that you don't discuss in Chapter 3.3.

l. 50: "The largest CH4 mixing ratio was found over a local landfill." Is this a key finding of your study? Should I be in the abstract at this position?

l. 58: "modeling" might confuse here. You did not do any real modeling.

l. 61: "identifying emission sources". Is that what you focus on in this paper?

l. 66: Is air quality the main concern of this paper? GHG are generally not considered as air pollutants.

l.143: Here some sentences are different from the marked-up version of your manuscript.

l. 149ff: Here a whole section is not in your marked-up version. Which will be the final version?

l. 315: You should investigate this huge difference in you emission estimates due to wind treatment a little more. Could the cause be a certain meteorological condition? Maybe an accumulation of methane and CO2 in the outflow region due to low wind speeds. If you then average the wind over the entire loop, it might be extremely overestimated in the outflow region and the flux is too high. What is the difference between raw wind and mass-balanced wind in relation to GHG mixing ratio along the lowest and only flight track within the boundary layer?

l. 331: As visible from Fig. S5 only your lowest ellipse lies within the PBL. Clearly the wind is quite different within the PBL from above. Does whole-column-averaged wind (as in Fig. 6c) make sense in this case?

l. 339: "Flux estimates using raw wind are more sensitive to the choice of background…" Do you have any idea why it is this way?

l. 388: Table 3 states 13.4 Mt CO2 for Turnbull.

l. 395ff: Which areas do the Turnbull et al (2011), Vulcan, and CEPAM inventory values given here cover?

l. 396: Please give a reference for the 1.1% annual increase in CO2 fluxes.

l. 401: Are there any CH4 emission inventories you could compare your emission estimates with? How about EDGAR or the U.S. Environmental Protection Agency's inventory?

ll. 467-479: Here you emphasize how the ellipses flight pattern reduces the sensitivity towards the choice of background. But, using a curtain flight the choice of background only induces 50 % difference, while for an ellipses flight the difference between wind-treatment techniques is 80%. How does this relate? How sensitive are curtain flights to wind treatment? Please add in Table 3. Why would you still recommend ellipses flights?

l. 493: "Second, the seasonality …" This sentence does not make sense.

Figures:

Figure 3: What does the gray dashed line in (c)-(f) depict?

Figure 6: It is hard to believe that between case (a) and (b) the difference is 86 %, but between case (b) and (c) only 3.9 %. Case (c) looks as if the flux estimate should be much larger than (b). Do you have an explanation?

Figure 7: Again it is hard to understand from the figures how (d) and (e) differ by 25 % but (e) and (f) only 2.8 %. The flux in (d) and (e) look almost the same, while (f) looks much different.

Tables:

Table 1 and 2: Please indicate negative differences from the base case with a minus in front of the given percentage.

Table 3: Please add a row of results for curtain flights using raw wind.