

Interactive comment on “New approach to evaluate satellite derived XCO₂ over oceans by integrating ship and aircraft observations” by Astrid Müller et al.

Anonymous Referee #2

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General comments This manuscript details a new approach for evaluating variations of XCO₂ over the Oceans by integrating ship, aircraft and model data to create an “in-situ based XCO₂” dataset. This dataset is compared with GOSAT and OCO-2 satellite data to evaluate its capabilities. The paper does have value in its contribution to scientific progress and the scientific quality of the work is good, however at points I think the paper needs to go into more detail of how and why specific parts of this method were done as I was left with several questions concerning this (as mentioned in my specific comments). The paper discusses biases between their in-situ XCO₂ and the satellites, concluding that these can be attributed to measurement uncertainties of the satellite observations. I am left unconvinced by this argument and would like to see more

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analysis of other possible uncertainties in their in-situ profile to strengthen this claim (I explain in more detail in my specific comments no.11). The study goes on to look at differences in the seasonality of the satellite data vs the in-situ data, which again I am sceptical about because I was unconvinced that these differences aren't due to inaccuracies in the in-situ based columns. Specific comments 1. Page 3, line 71: I think it is misleading to say that TCCON has a very limited number of sites observing open oceans given that there are multiple sites on small islands in the oceans and multiple coastal sites. Again on line 80 you mention comparison to TCCON data in the tropical Pacific region. I think it would help to be more specific about these TCCON sites which are used and where they are. 2. Page 3, line 72: Can you please clarify if this is an ocean bias or if this bias is for land and ocean combined at these sites. 3. Page 4, lines 102-104: The last part of this sentence doesn't make sense to me “if the standard deviation does not exceed 3 ppm”. 4. Page 4, line 108: Please could you explain how you determined the tropopause height that you use as a cut off for the aircraft data. Did you use a static 11 km for all measurements or did you calculate it for each time and location? 5. Page 4, methodology: Why did you settle on monthly resolution? I am interested if instead of comparing with monthly averages it would have been possible to compare any of the in-situ data more directly to satellite overpasses on the same day for example. Were there any cases where you were able to do this or were the ship, aircraft and satellite data never on the same day? 6. Page 6, comments on section 3.2 and Figure 2: Naming this constructed profile “in-situ” when it consists of both in-situ and model data is misleading. 7. Page 6, comments on section 3.2 and Figure 2: Please could you comment in more detail why you chose to extrapolate the ship concentrations up to 850 hpa. 8. Page 6, comments on section 3.2 and Figure 2: You say that you extrapolate the aircraft data down to 380 or 400 hpa; and then say that the tropopause pressure is used as upper limit for the extrapolation. So are you also extrapolating the data upwards to the tropopause or is this only the case when the aircraft data are above the tropopause? 9. Page 6, comments on section 3.2 and Figure 2: The vertical grid which you are interpolating onto is not discussed. It is

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clear from Figure 2 that your red interpolation line does not have the vertical resolution of the different components which you are using to make the “in-situ” profile. I know that you have done this to match the profile of the satellite retrievals, but you don’t explain this. ACOS GOSAT and OCO-2 soundings have a different pressure profile for every sounding because they are built from the surface pressure which does change between soundings. Did you calculate the “in-situ” XCO₂ for each satellite sounding separately and then average the monthly/spatial results. Or did you assume one fixed (or perhaps make your own average) profile for pressure, a priori CO₂ and the averaging kernel? 10. Page 8, line 215: You say that the values you get are similar to Matsueda et al. 2008. Can you be more specific on how similar they are? You do this afterwards with the Conway et al. 1994 numbers so it would paint a more complete picture if you could also give numbers in this instance. 11. I disagree with your assessment that the mid troposphere is the only uncertainty in the in-situ profile which you need to consider. You also should consider the tropopause height as well. We can see from Figure A3 that the interpolated profile deviates from the model profile at the tropopause in panels a and c. I think your concentrations at the tropopause can’t be overlooked as an uncertainty because the ACTM and your in-situ profile are clearly in disagreement over the location of the tropopause in some of these figures. It may be that the satellite data and your in-situ XCO₂ are different because your tropopause height is incorrect, which would lead to seasonally dependent biases. How much does your tropopause height vary over all of the measurement times and locations in a month? By how much will this change your XCO₂ values and can this account for the differences you see to the satellite data? 12. I would be interested to see how the ACTM XCO₂ would look if you interpolated it onto the satellite grid in the same way you have done for your in-situ data, and how this would compare with both the satellite and in-situ XCO₂. 13. Page 15, line 354: It is unclear to me how satellite XCO₂ can show a delayed response to CO₂ changes since it is measuring in real time. Please could you elaborate on this. 14. Page 16, line 359: Please add the uncertainty and be more accurate in your “less than 2ppm” number since you are being very precise with the number you are

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comparing it to in the next sentence. 15. Page 16, line 360: 3.84 ± 0.65 and 3.39 ± 0.03 overlap so you can’t draw conclusions about their difference. 16. You conclude by saying that datasets such as yours can augment TCCON data. Did you consider applying your method to other aircraft and ground data at TCCON sites to compare with TCCON itself? Technical corrections Page 2, line 45: You say that most sites are in North America and Europe, and some are in East Asia and Oceania. This reads as though there are no sites in the other continents, which I don’t think was your intention to say. Please reword this sentence. Page 2, line 55: “improves” should be “improve”. Page 3, line 74: “Aircrafts” should be “aircraft”. Page 3, line 79: Negative symbol isn’t the same type of symbol used in previous cases. Page 3, line 80: Remove journal and doi from citation. Page 8, line 210: Missing “N” in “20° –30° N” whilst in other instances you have used “20° N–30° N”. Figure A2: Add an x-label.

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