**Manuscript title**: Local air pollution from oil rig emissions observed during the airborne DACCIWA campaign by Brocchi et al.

**RESPONSES TO ANONYMOUS REFEREE #2**

**We thank the reviewer for his relevant comments that were helpful in improving the manuscript. Changes have been made in response to his specific comments listed below (in black). Our responses appear in red, and changes in the revised manuscript are highlighted in yellow.**

General points:

Like reviewer 1, I am a little concerned about the way the model to measurement comparison has been done. It does seem that the peaks in the model are wider than the measured peaks and therefore comparing the maximum mixing ratio enhancement of the two could give misleading results. The authors should try comparing the integrated area under the peaks and see if this gives a different result. The effect of this should at least be discussed in the paper.

→ We agree on this point. An error in the dispersion modelling has been mentioned by Referee #1 and leads to simulated peaks wider than the measured ones. We used the approach that consists in comparing the integrated areas and modified the text (2nd paragraph of section 4.2) as follows:

"Concerning the second and the fourth peak (Fig. 2a), the measurements show two close peaks that FLEXPART cannot simulate individually, leading to a single and broader simulated peak. This is probably due to an error in the dispersion modelling induced by the horizontal and vertical wind field resolution that prevents us from comparing peak-to-peak concentrations. Even with a finer wind field grid mesh of 0.125°×0.125° (simulation not shown) such close peaks cannot be distinguished, suggesting a still insufficient spatial resolution. Instead, the integrated area under each of the measured and simulated plume transects will be compared and presented in Figure 3 with the percentages representing the relative differences with respect to SPIRIT measurements."

According to this new approach, sensitivity tests with new fluxes were performed. They are summarized in Table 1. All the results of the simulations given now correspond to the integrated area under each peak (measured and simulated). We decided to summarize the results of all these new sensitivity tests by a figure instead of a table. This is illustrated in Figure 3 and results are discussed from the 2nd paragraph of section 4.2 onwards.

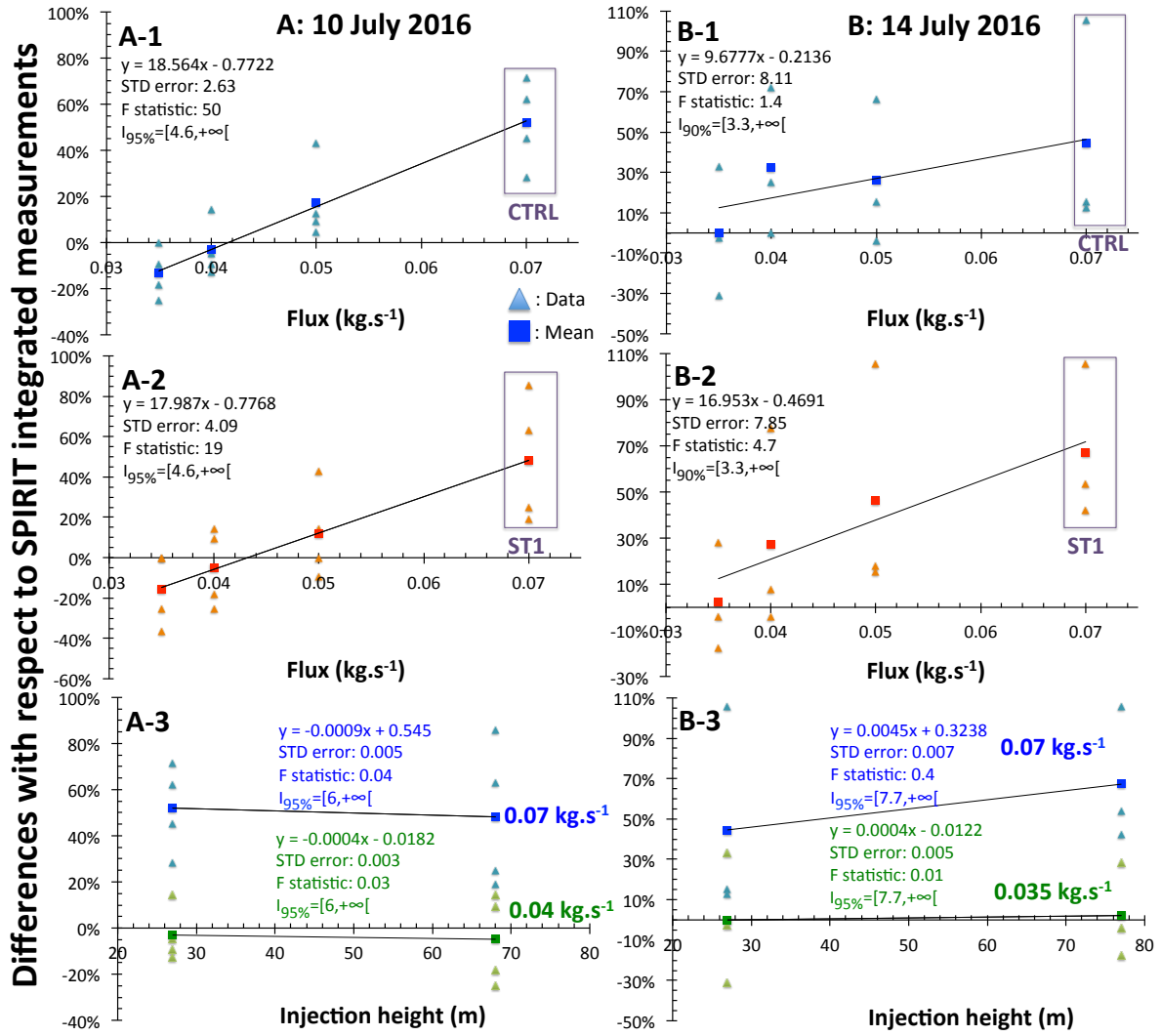


**Figure 3: Differences (in %) between SPIRIT integrated measurements and FLEXPART simulations depending on flux or injection height used as input in the model for A: the flight on July 10 and B: the flight on July 14 . Panels A-1 and B-1 represent the change in the percentage with the flux by using the injection height from Briggs' algorithm (1965; blue data; i.e. 27 m) and Panels A-2 and B-2 with injection height from VDI 3782 (1985; orange data; i.e. 68 m (A-2) or 77 m (B-2)). Panels A-3 and B-3 represent the change in the percentage with the injection height for the flux from Deetz and Vogel (2017; blue data; 0.07 kg.s$^{-1}$) and for the flux used in the sensitivity tests (green data, 0.04 kg.s$^{-1}$ for July 10 (A-3) and 0.035 kg.s$^{-1}$ for July 14(B-3)). For all panels, triangles represent the data for all the peaks measured and squares represent the mean from these data. The slope, standard error values for the slope coefficients and the F statistic are added for all the plots.**

The authors also need to expand on how NO / $NO_2$ chemistry is treated in the model. It is not clear to me whether they are changing the NO and $NO_2$ emissions in the model to reproduce the $NO_2$ measurements or just $NO_2$. I would have thought most of the emission from the rig would occur as NO, with subsequent conversion to $NO_2$ before the measurements is made. The text needs to be clearer on what chemistry is used in the model.

→ FLEXPART model uses a very simple chemistry: the particles are released with chemical properties like $NO_2$ using constant emissions (mass release during time). During the simulation, the $NO_2$ like particles mass is lost by wet and dry deposition and by OH reaction, only.

The emissions used to initialize FLEXPART come from Deetz and Vogel (2017) inventory and are expressed in terms of $NO_2$. They considered a rapid conversion for a part of the NO emissions into $NO_2$ very close to the source. This is confirmed with some quick simulations with a box model

(AtChem/MCM) showing conversion in the first 5 minutes of the simulation. Most of the atmospheric models consider $NO_2$ as primary emissions while it is in fact a secondary emission. A sentence has been included in Section 3.2:

"Fourth, for such a temperature, $NO_2$ is considered as a primary pollutant coming from the rapid conversion of NO close to the source, and the inventory does not include any later transformation of the species"

Does the emission from the rig include non-flaring combustion (e.g. power generation)?

I would have thought that this would also be a significant source of NOx from a collocated but different source? Could this have been picked up in the measured plume but not included in the emission inventory?

→ The emission inventory (Deetz and Vogel) from the rig includes only flaring emissions. Emissions from power generation for the facility could be also a source of NOx. However, considering that the aircraft was flying at an altitude of about 300 m, only the emissions that have been heated up at very high temperature could reach rapidly and directly this altitude and induce a very localized spike in the $NO_2$ signal. This is why we think that we have only measured flaring emissions.

It would also be good to have a short discussion as to what actual effect the oil rig emissions have on air pollution in West Africa. For instance, if the emissions are doubled in the inventory, what effect does this have on NO2 and O3 levels at the coast? I realize a full study like this is beyond the scope of this paper but some short statement should be made as to the potential impact of underestimated emissions from oil rigs in the area.

→ The impact of the FPSO emissions on the air quality on the coast is not mentioned in the paper. In fact, the platform is situated 70 km downwind of the coast, which supposes that there is no impact of direct emissions of $NO_2$. However, we agree that $O_3$ and other pollutants such as $CO_2$, $CH_4$, BC and CO with relatively long lifetime should impact the air quality on the coast. As you mentioned, "a full study like this is beyond the scope of this paper" but as suggested a short statement is included in the paper with more information directly included in the text (end of section 4.2) and in the conclusion:

"We can use the best simulation on each day to estimate the percentage of pollutants transported inside and above the MBL. In both cases, about 90% of the pollutants stay inside the MBL and are susceptible to impact the population living along the coastline. Measurements made along the coastline have shown that $NO_2$ concentrations are generally greater than 2 ppb for suburban sites and greater than 20 ppb near industrial sites (Bahino et al. 2018). Given the wind velocity (from 6.6 to 9.4 m $s^{-1}$), the air masses attain the coast in 2 to 3 hours, which does not allow to bring significant $NO_2$ concentrations to impact air quality in this area. This is confirmed looking at FLEXPART simulations in Fig. 2b and 2e. They show that $NO_2$ concentrations are already very low (< 1 ppbv) from 40 km from the source on July 10, and even closer (from 20 km from the source) on July 14. The distance between the coast and the emission source following the wind direction being 70 km, only pollutants with a relatively long lifetime or secondary pollutant as $O_3$ can impact the air quality of the coast."

"An estimation of the pollutant distribution above or inside the MBL shows that the pollutants stay mainly inside the MBL, limiting the transport to the coastline located 70 km downwind of the FPSO.

Were there measurements of CH4 made on the aircraft? If so it would have been good to see this included in the study as the rigs could also be an important CH4 source.

→ The SPIRIT instrument measured $CH_4$ during the campaign. Unfortunately, the flight conditions for these specific days were not ideal with very high temperatures inside the aircraft cabin (low altitude make less efficient the air conditioning) that did not allow the $CH_4$ laser to work properly.

Specific points:

P4 L27: Can the authors confirm if this is an NO2 flux or a NOx flux?

→ We confirm that this is an $NO_2$ flux given in the inventory. A NO flux is also given but no direct $NO_x$ flux is provided.

P6 L15: Is this really true. Can it really be said that because no SO2 was measured (on a relatively insensitive instrument) that no H2S was present. The authors should at least put a lower limit on the H2S that could be present.

→ It is well known the oxidation of $H_2S$ fully leads to $SO_2$ formation. See any general book about Atmospheric Chemistry; we added the reference (Sonibare and Akeredolu, 2004) already quoted in the present paper. Sentence is added in the paper section 4.1:

"Knowing that SO2 comes from H2S combustion (Sonibare and Akeredolu, 2004), these results suggest that a gas composition of 0.03% of H2S induces an emission of SO2 concentration lower than the detection limit of the instrument from 3 km of our measurements or the natural gas composition given by Deetz and Vogel (2017) for the Niger Delta is different from that in Ghana for those two flights."

P8 L16: this needs expanding, it is not clear what 'disturbed weather conditions' means and how this could effect the CO concentrations in the plume.

We agree this expression was rather confusing so we replaced it by a more specific one which should clarify the reason why CO formation is increased: "the wind direction was not clearly established, as can be seen from the much more dispersed plume in Fig. 1b, resulting in incomplete combustion pockets favoring CO formation."

P8 L19: How will the results of the campaign improve computational flare fluid dynamics modelling?

→ This sentence was deleted