

## ***Interactive comment on “Local air pollution from oil rig emissions observed during the airborne DACCIWA campaign” by Vanessa Brocchi et al.***

**Anonymous Referee #2**

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This paper presents measurements from an aircraft of emissions of various air pollutants from oil rigs in the Gulf of Guinea, West Africa. The measurements are then used to quantify emissions from the rigs, which are in turn compared to calculated emissions from a new satellite derived gas-flaring emission inventory developed for the DACCIWA project. This is done using FLEXPART dispersion model simulations of the plume using the calculated emissions and then comparing the model output to the measurements. The main conclusion is that the emission rate in the inventory is too low to reproduce the measured plume concentrations using the FLEXPART model. Oil rig emissions are an important source of air pollution in this area and therefore a study like this is potentially crucial for understanding their magnitude and impact. The work is within scope of ACP however I feel there are some areas that need expanding and clarifying before it

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should be published.

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.

The authors also need to expand on how NO / NO<sub>2</sub> chemistry is treated in the model. It is not clear to me whether they are changing the NO and NO<sub>2</sub> emissions in the model to reproduce the NO<sub>2</sub> measurements or just NO<sub>2</sub>. I would have thought most of the emission from the rig would occur as NO, with subsequent conversion to NO<sub>2</sub> before the measurements is made. The text needs to be clearer on what chemistry is used in the model.

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 NO<sub>x</sub> from a co-located but different source? Could this have been picked up in the measured plume but not included in the emission inventory?

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 NO<sub>2</sub> and O<sub>3</sub> 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.

Were there measurements of CH<sub>4</sub> 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 CH<sub>4</sub> source.

Specific points: P4 L27: Can the authors confirm if this is an NO<sub>2</sub> flux or a NO<sub>x</sub> flux?

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P6 L15: Is this really true. Can it really be said that because no SO<sub>2</sub> was measured (on a relatively insensitive instrument) that no H<sub>2</sub>S was present. The authors should at least put a lower limit on the H<sub>2</sub>S that could be present.

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.

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

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