

***Interactive comment on* “Spatial distribution of gaseous pollutants (NO₂, SO₂, NH₃, HNO₃ and O₃) in Abidjan, Cote d’Ivoire” by Julien Bahino et al.**

Anonymous Referee #1

Received and published: 26 September 2017

This paper presents a relatively high resolution spatial distribution for inorganic gaseous species measured at Abidjan in Cote d’Ivoire. Since very little data exist for this part of Africa, I welcome any data from this region. Scientists conduct measurements under difficult conditions and results presented should be viewed within the logistical constraints associated with measurements conducted in this part of the world. However, my main concern with this paper is that results for a very short period are presented, i.e. 8 weeks during which passive sampling measurements were conducted for two-week periods. That is a very small data set, although it is considered an intensive campaign within the scope of the DACCIWA FP7 project. It would have been good if there were 8 weeks of continuous measurements, but then again the scientists would not have been able to cover the spatial distribution (21 sites). It is mentioned that one

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of the 21 sites is a "supersite". However, I presume that this so-called "supersite" does not contain any continuous active measurements that could have been used to present e.g. diurnal patterns.

Therefore, my general comments on the paper to the authors are:

1) I would like to suggest that the title, introduction and structure of this paper is changed.

It should indicate that the main focus of this paper is to present and contextualise concentrations of inorganic gaseous species for African cities. Begin the paper with the spatial distributions measured for Abidjan, i.e. inorganic gases concentrations associated with specific source sectors. Thereafter the average concentrations of inorganic gases (maybe also include more than just NO₂ and SO₂) measured in Abidjan is compared to other African cities with the discussion and contextualization presented. Then you end of with comparing specific source sectors in Abidjan with other similar source sectors in other African countries.

To summarize, the introduction should be re-written in order to indicate that his paper deals with concentrations of inorganic gases in African cities. You have a high spatial resolution for Abidjan where you have measured and then contextualise with other African countries.

As mentioned, my main concern with this paper is the short measurement period, i.e. 8 weeks (4 samples for each gas). I do not think that it is sufficient for publication in ACP if the paper is only presented primarily from the view of a very short sampling campaign. If the paper is pitched from a perspective to review/contextualise African cities then the impact of the paper can improve.

2) The paper can be significantly shortened. In its current format, the paper is of a repetitive nature with many sentences/arguments/results repeated throughout the document. In addition, many obvious general statements are given throughout the

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manuscript.

Restructuring the paper will also eliminate the repetitive nature of the paper.

3) There is a lack of depth in the explanation of the observed spatial distributions. Incorporating meteorology or air mass movements, for instance, would improve the understanding and interpretations.

4) The paper also require significant language and text editing.

Detailed comments: I have included detailed specific comments relating to the above general comments in the attached PDF version of the submitted manuscript. Many language and text edits/suggestions are indicated throughout the reviewed PDF version of the manuscript.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2017-724/acp-2017-724-RC1-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-724>, 2017.

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