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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric NO_2 concentrations over West Africa are influenced by climate zone and soil moisture variability" *by* Ajoke R. Onojeghuo et al.

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"This paper is does not make a persuasive case that it has learned something new or that it has demonstrated any sort of useful link between soil moisture and NO2 emissions. "

We have done a more extensive search of literature and can confirm that our findings are relevant to what is known of the relationship between soil moisture and NO2 over West Africa. Our discussion section has now been revised citing Zörner et al. (2016) to give credibility to the results presented in this paper.

"If the authors choose to revise I recommend a revised manuscript have 2-3 figures

and no more. The figures should more directly address the authors claim of showing a causal and mechanistic relationship between soil moisture and NO2."

We have revised the number of figures and merged Figures 3 and 4. We will consider merging a few more figures.

"In addition, a revised manuscript should pay careful attention to time scales for rainfall and subsequent emissions, to separating seasonal cycles in transport and OH from other factors that affect NO2 columns, to removing the effects of biomass burning on NO2 columns"

We indicated in the paper that biomass burning cycles play a vital role in the NO2 levels observed in the West African dry season. The dry season was determined relative to published literature and our observation from the annual soil moisture cycles. A reference to Roberts et al. (2009) has now been added to describe the months of active fires over the unique West African climate zones. For the timescales of emissions after rainfall, reference has been made to Zörner et al. (2016) who considered precipitation, water vapour, soil moisture and their impacts on emissions before and after rain events in the Sahel.

"Perhaps a convincing case about soil moisture could be made if the paper started with a single climate zone and it illustrated how the soil moisture argument affects the NO2 column in a way that controls for these and other well known important variables."

This is a good idea that could be explored further with future research themes.

"Another way to make a convincing case would be to show that the same methods of analysis applied to a 3-d model with and without soil NOx emissions produces meaningful differences."

This is also a very good idea but deviates from the theme of the research presented in this paper which was the use of existing open source satellite data to analyse the relationships between soil moisture and NO2.

“In addition, a revised paper should carefully summarize current understanding of soil NOx emissions in the region so the reader has a clear understanding of what is new about the analysis and what aspects confirm prior results.” A section, which discussed soil emissions of NOx over West Africa, was included in the manuscript (Page 2 line 20 -). Some literature consulted for this section include Hudman et al. (2012) and Jaegle et al. (2005) who have carried out NOx source partitioning globally. Zörner et al. (2016) has also carried out some valid research on soil NOx emissions from the West African Sahel and has been analysed in the reviewed manuscript. The “soil emissions of NOx” paragraph in the research has also been discussed and restructured as recommended by the first reviewer.

References

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