

## ***Interactive comment on “Estimating daily surface NO<sub>2</sub> concentrations from satellite data - A case study over Hong Kong using land use regression models” by Jasdeep S Anand and Paul S Monks***

**Anonymous Referee #2**

Received and published: 28 March 2017

In this manuscript, Anand and Monks present the development of a land use regression model to predict daily surface NO<sub>2</sub> concentrations over Hong Kong by combining satellite observations of NO<sub>2</sub> tropospheric columns with finer scale land use parameters. This approach infers high-resolution spatial features in NO<sub>2</sub> surface concentration using land use parameters while incorporating the long-term synoptic temporal coverage captured by satellite measurements. They show that their land use regression model predicts surface NO<sub>2</sub> concentrations more successfully than estimates obtained by combining satellite observations with modeled NO<sub>2</sub> vertical profiles. The lack of a trend in predicted surface NO<sub>2</sub> concentrations contradicts expectations from a bottom-up NO<sub>x</sub> inventory for the region which shows a significant declining trend.

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My main concern with this manuscript is one of scope. This manuscript is primarily of local and technical interest. While air quality in this region is certainly of interest to the community, I'm not convinced that the authors have developed the land use regression approach sufficiently for it to have novel general implications. The authors acknowledge their model is similar to a previously developed mixed-effects model, and thus the advancement here is largely in its application to a different region. If the authors were presenting a radical new approach to land use regression, then the regional nature of the study would be less of an issue. As it stands, I wonder whether ACP is the appropriate journal for publication. I urge the authors to present a clear argument for how this paper makes a more general contribution to atmospheric science.

Having said that, the study is interesting, and the manuscript is written clearly. If publication in ACP is to be pursued, I have the following general and specific comments to take into consideration.

General comments:

I am not yet satisfied that 11 monitoring stations are sufficient to build a national-scale LUR model. As they admit, the coverage is predominantly urban. We have no sense of what "dynamic range" exists in the predictor variables across these urban sites, and whether there is enough to build a robust model. My problem is that while there are many individual daily observations, parameters like road length, urban area coverage, population density, vegetation area coverage, and elevation will not vary day-to-day. Thus, they only have 11 unique data points for each variable. This strikes me as an extremely small sample. One transparent way they could address this is by including regression plot results for each predictor in a supplementary section.

Another test would be to try building the model(s) by holding back specific ground-station locations (and not just a percentage of the available data from each location, since this does not test the sensitivity to the loss of a ground station location). Was this the purpose of Section 3.4? I was a bit confused about what this section was

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presenting. In general, I am not satisfied that a cross-validation approach involving removing 20% of all the data is a good enough test. It strikes me that a better test would be to remove entire stations instead. Keeping any observations from every station basically means that most of the predictor variables aren't actually getting tested (since these are not changing day-to-day).

Finally, I think the introduction to Section 2.3 about mixed effects LUR could use more detail. What would the formulation of a "fixed"/"random" effects model look like in comparison? What is the advance in the mixed effects model? As someone who is not overly familiar with the LUR approach, it is not really clear to me what the mixed effects model actually captures and how it is calculated (despite the inclusion and explanation of Equation 1). The authors should assume that some of the ACP audience will not be familiar with LUR.

Specific comments:

Section 2.1: Can you describe in a more detail the network of DOAS vs. in-situ measurements? How many of each? Do they have the same length of coverage? Are the data for both available online? Can you show which ones are which on the map? Are the chemiluminescence measurements via catalyst or LED conversion?

Equation 1: Should the omega symbol have both a subscript i and subscript j?

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1101, 2016.