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Discussions

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**Interactive
comment**

Interactive comment on "Estimating daily surface NO$_2$ 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 #1

Received and published: 16 February 2017

This study presents development of a mixed-effect LUR model to estimate surface NO2 concentrations over the Hong Kong SAR. In-situ measurements from surface network and tropospheric NO2 column data from multiple satellite instruments are combined with fine-scale land use parameters to predict daily surface concentrations. Their analysis shows that models with satellite data and mixed-effect LUR show superior predictive performance as compared to the reference LUR model. Similar conclusions were drawn by few previous studies, albeit different regions. This study goes beyond other studies by exploring the models' predictive ability with data from multiple satellite instruments. The manuscript is well written. However, I have few concerns as listed below that need to be addressed before it can be published in ACP.

General comments

1) Some results may point to deficiency in method or errors in data analysis. First, the authors state in Section 3 that models combining OMI and SCIAMACHY data always failed to converge, which suggests a problem in their implementation. Second, the model (and their interpretation) seems to neglect some important predictive parameters such as NOx lifetime. Third, average NO2 concentrations presented in Figure 7 are not consistent with seasonal behavior of NO2 (peaking in winter time) especially over regions east of 114 deg longitude. Fourth, their estimated trend contradicts results from several other trend studies over Hong Kong and is not consistent with the trend in emissions.

2) The work is built on a poor foundation. The authors use satellite data obtained from different sources. As a result, retrievals are not consistent due to differences in all aspects of retrieval algorithm – from spectral fit to the use of various input parameters. The first task should have been checking consistency between different data set. Assuming each data product as truth is another major limitation. Therefore, it might be more helpful to focus on measurements from a single instrument and carry out a thorough investigation rather than presenting lengthy and speculative discussions.

Specific comments

Page 4, lines 2-3: This sentence is incomplete, please revise.

Page 4, line 14-15: Reverse the citation.

Page 5, lines 24: What do you mean by NO2-Omega relationship? Please, clarify.

Page 6, line 19: How does ocean deposition affect local NO2 concentration? Describe the mechanisms if that is indeed the case.

Page 7, lines: 5-6: Correct R2 here and in other places.

Page 9, lines 7-8: You state "This null result may be due to a lack of cloud-free days

when both instruments were coincident over Hong Kong." This may point to some deficiency in your implementation. Why is it necessary to have cloud-free observations for both instruments?

Page 10, line 3: I do not understand your statement "vertical mixing being dominated by emissions from mainland China." How would distant sources affect vertical mixing?

Page 11, Figure 2: What does the gray area represent? What does the data gap in the mean surface NO_2 map mean? What explains the large spatial gradient (box-to-box gradient) in the mean concentration map? Wouldn't wind transport pollution to neighboring areas?

Page 12, lines 3-4: The wintertime enhancement is also due to increase in NO_x lifetime in winter.

Page 13, line 1: Your statement "The spatial resolution of GOME-2 and SCIAMACHY are much larger than OMI" is not correct. OMI has higher spatial resolution than GOME-2 and SCIAMACHY.

Page 14, lines 11-14: It is pity that you are not recognizing the fact that there is large inconsistency in retrievals. Please, see my general comments.

Page 16, lines 9-12 and lines 14-17: Are there any studies that suggest effect of instrument degradation in satellite NO_2 retrievals? I would be surprised if DOAS-type retrievals from satellite can have significant impact from instrument degradation.

Page 16, line 13: I believe, the terminologies "row anomaly" and "instrument degradation" are not same. Data affected by row anomaly are not supposed to be used.

Page 16, lines 20-21: Wouldn't your statement "which suggests that coverage losses or instrument degradation are not significant influences on model accuracy or precision" here and in other places contradict your discussions regarding SCIAMACHY (less sampling due to global coverage in 6 days) and GOME-2 (more cloudy pixels)?

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Page 16, lines 25-27: Don't understand this, suggest revise the statement.

Page 17, lines 1-2: I wonder how temperature can be a proxy for photochemical dissociation of NO2. Shouldn't it be actinic flux?

Page 17, lines 10-11: What is your measure for your model accuracy? Why are improvement in R2 and decrease in RMSE not considered for model improvement?

Page 19, line 1: What is the logic behind applying daily average profiles instead of early-afternoon profiles that are more relevant for OMI? Could this be the reason for low correlation between OMI and in-situ observation?

Page 20, line 15: Seasonal variation is driven mostly by changes in NOx lifetime and emissions.

Page 21, Figure 8: Deviation of red curve (fitted line) considerably from data points may suggest that the term in Eqn 4 that accounts for seasonal variation over time may not have been properly applied. Visually, the area under the curve passing through the points seems decreasing over time, consistent with the trend in emissions. Please check your calculation of trend. Please show the trend in OMI column data as well.

Page 21, lines 10-11: Clarify this statement. How does the change in precision result in negative bias in surface concentration?

Page 22, line 20: Your statement "OMI has the smallest spatial resolution" is incorrect. It should have been either highest spatial resolution or smallest pixel size.

Page 22, line 23: Where is that positive bias - background regions, polluted areas, or everywhere?

Page 22, line 24: Why would multiple satellite data have improved sensitivity? Sensitivity to diurnal variation? It could provide information on diurnal changes, only if the measurements are self-consistent (instrument, algorithm, etc.).

Page 22, lines 28-29: I don't understand this. Why do you need to exclude clear-sky

OMI observations if SCIAMACHY observations are cloudy for a given day? This might point to deficiency in your approach.

Page 23, lines 13-15: Unclear. Revise this statement.

Page 24, line 6: Correction: Geostationary Environmental Monitoring Spectrometer.

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

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