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Interactive comment on "Long-term trends in air quality in major cities in the UK and India: A view from space" *by* Karn Vohra et al.

Anonymous Referee #3

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This manuscript presents an analysis of the variability and trends of 4 important air quality indicators (NO2, NH3, PM2.5 and HCHO) measured from the ground and from space in 4 major cities, two in UK (London and Birmingham) and two in India (Dehli and Kanpur). In a first part of the study, the ability of space-based column observations to capture the monthly variability in surface concentration of the target species is investigated. In a second step, times-series of satellite data are analysed for long-term trends at the different sites and for the 4 species. Results indicate that satellite data reproduce well the monthly variability in surface NO2 and NH3 at the different sites, but AOD and PM2.5 do not show the same relation. The long-term trend analysis show a good consistency between satellite and in-situ data, and is also consistent with results known from the literature. Although the scope of this study is limited, the approaches

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Discussion paper

used are robust and well described. To my opinion, this is an interesting case study illustrating how surface in-situ and satellite data sets can be combined to derive useful information on air quality in cities at different stages of development. The manuscript is well written, concise and well organized. Figures are clear and there is adequate credit to existing literature. I therefore recommend publication in ACP after attention to the comments and remarks below.

Detailed comments

Pg. 1, first sentences of the abstract: The focus on the deficiencies of the air quality in-situ networks (costly, inconsistent. . .) is very strong and does not make justice to efforts being done in many countries to deliver accurate and reliable surface measurements. Although there are certainly issues with in-situ data, I would rather say that satellite and in-situ measurement system are complementary and can reinforce each other. I strongly recommend that you reformulate this part of the abstract to make it more balanced.

Pg. 5, l. 2: please clarify what you mean by a ‘dynamic range’ of air pollutants

Pg. 5, l. 103: MODIS AOD measurements have indeed been used in many studies as a proxy for PM2.5, however it is fair to say that the relationship between these two quantities is not direct and studies generally use a number of additional proxies in addition to AOD to establish a complex relationship, generally with help of Machine Learning techniques. It is therefore not unexpected that, in a straight comparison, AOD and PM2.5 show a smaller degree of correlation than e.g. NO2 columns and surface concentrations.

Pg. 8, l. 182: To the list of uncertainties on satellite UV-Vis retrievals (NO2 and HCHO), you may also add clouds and aerosols, which have a strong impact on the radiative transfer and are usually not well characterised.

Pg. 10, third paragraph: the separation between winter and other months in the NO2 comparisons at the two UK sites is justified by the existence of a seasonality in the relationship between tropospheric columns and surface concentrations, due to seasonal differences in the NO2 lifetime and mixing layer height. Although I roughly see the reasoning here, I think it would useful to elaborate a bit more on the reasons explaining these relationships.

Pg. 11, l. 245: the large difference in the slope of the regressions of satellite NO2 columns against surface concentrations in UK and India is striking and deserves more discussion. Why is it so? I suppose that there might be several reasons, but one I can see is the large difference in aerosol content in India and UK (obvious from Fig. 6). If at immediate proximity of the surface, a thick aerosol layer would act as a screen for the solar light leading to a reduction of the sensitivity of satellite measurements to the surface NO2. Likewise, why is the slope larger in winter than in other months in Birmingham? Can this be related to the seasonal differences in NO2 lifetime or mixing layer heights discussed above? Why is the behavior different in London?

Pg. 12, l. 12, Fig. 5: please briefly explain the meaning of the p-values and how to interpret it in the context of this study.

Pg. 13, l. 298: in addition to uncertainties in surface reflectivity, could residual cloud contamination be responsible for the observed overestimation of MODIS against AERONET? (UK is notoriously cloudy)

Pg. 14, l. 331, Fig. 7: why not showing the trend analysis applied on surface concentrations, in addition to the satellite data analysis. This could be added in the form of two additional panels on top of Fig. 7.

Pg. 14, l. 339: the less steep decline in NOx emission in London (in comparison to outer London and national) is to some extent explained by a weakening effect due to an increase in the contribution due to the free tropospheric NO2 background. Is there any evidence for this effect? What would be the source of this background in London?

Pg. 15, l. 348: again the much smaller difference between the NO_2 columns in Dehli and London in comparison to surface concentrations could possibly be related to the large aerosol content in Dehli leading to a systematic underestimation of the column.

Pg. 17, l. 400: in addition to the given explanation (increase in the frequency of extremes, e.g. fires), it might be that the increased HCHO spread after 2009 is to some extent related to the OMI row anomaly, which developed after 2008. This anomaly strongly affected the sampling and data coverage, with a possible impact on monthly-averaged values.

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