Reply to Reviewer Comment 1 (Anon. Ref. #3)

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We thank the anonymous reviewer #3 for the valuable suggestions how to improve our manuscript about NO_2 trends over India as observed from satellite and make its focus more clear to the reader.

> Hilboll et al. presents an analysis of NO2 pollution changes over different regions of India and their socioeconomic drivers, by combining multiple satellite products and official socioeconomic data. I have a few suggestions as follows. Multiple satellite products are used. Although some consistency is found in trend results (Fig. 1 and Fig. S1), there are clear quantitative differences among these products, especially after 2012. For example, the trend of OMI NO2 is clearly different from those in GOME2-a and GOME2-b (for all regions in Fig. 1 except North Indian Plain). For North Indian Plain, the OMI trend (Fig.1) is different from the DOMINO NO2 trend in Fig. S1. The large uncertainties in these satellite datasets make it difficult to conduct further linkage to socioeconomics. Is it possible to focus the analysis on regions that multiple satellite products show quantitatively consistent trends? What are the uncertainties in trends from individual products considering retrieval and representative errors?

We agree with the reviewer that the temporal variation is not always consistent between sensors and retrievals for all regions; this is indeed one of the major challenges in direct attribution of NO_2 changes to socio-economic factors. That being said, this manuscript's main goal is to report on these changes, most notably the surprising slow-down of observed NO_2 columns in spite of a growing economy and no sufficiently noteworthy changes in technology.

A robust, quantitative analysis of the potential *causal* relationship between socio-economic factors and observed NO_2 columns is, while admittedly both interesting and important, however far beyond the scope of the present study. Therefore, we prefer to report on observed NO_2 and socio-economic changes for all Indian states, as the available satellite data have not been reported in such a manner before. This focus is reflected in the manuscript's title, which does not refer to socio-economic data at all.

Regarding the trend estimates, whenever we give quantitative trend estimates (as is the case in Tab. 1), trends have been marked as statistically significant if the 95% confidence interval for the slope parameter does not contain zero, as described in Sect. 2.7. The trend estimates are rather robust against retrieval errors, as trends from the different instruments agree well quantitatively. Regarding representative errors, we obviously rely on the assumption that over the course of the multi-year study period the spatial sampling of polluted and non-polluted areas inside one state averages out.

> The NO2 growth rates are quantitatively significantly different from those in socioeconomic data, often by a factor of 10 (Table 1). It appears that chemistry, meteorology and/or other factors play major roles here. Can these factors be better accounted for in linking NO2 trends to emission trends? How are the roles of chemistry/meteorology in NO2 trends over India compared to the roles over other countries? A statistical model is used to calculate the NO2 trends. Please discuss the model here briefly. Also, the model does not account for shift in seasonality when the pollution grew, which is important for fast changing pollution regions. Please discuss the caveat of this model.

The attribution of the observed NO_2 signal to individual socio-economic factors is indeed difficult. A quantitative analysis of this relationship is a whole study of its own and therefore outside the scope of the present manuscript, which aims to simply report on the temporal changes in NO_2 over the Indian states as observed from satellite.

The quantitative differences between change rates of NO_2 and socio-economic factors can come from a variety of sources. For example, the number of registered motor vehicles can only be a rough indicator for the incurring NOx emissions, as it does not take into account vehicle technology and fuel type. The same is true for the relationship between the generated electrical power and the incurring NOx emissions, which are not necessarily following a linear relationship, e.g., due to enhanced efficiency and advances in technology and flue gas cleaning. Similarly, GSDP in itself encompasses all sectors of the economy, including services which only have minor impact on NOx emissions.

All these reasons make a direct attribution and quantitative comparison of observed NO_2 columns to state-wide socio-economic factors difficult. Therefore, in this manuscript we have chosen to lay the focus on reporting on the temporal evolution of satellite-observed NO_2 over India, and to our knowledge, our study is the first to quantitatively describe its change at state level.

A detailed analysis of this relationship, maybe focused on one or two states, would be a very interesting follow-up to the present study. Lacking a good, up-to-date NOx emission inventory with reliable source attribution, such a study would involve the setup of a chemistry transport model for detailed sensitivity runs of a new, to-be-constructed emission model linking socio-economic activities to NOx emissions.

In the revised manuscript, we will try to give a better explanation of these relationships and make sure to make the focus of the present study clear to the reader, suggesting possible follow-on studies.

Regarding our statistical trend model: The model is presented in Sect. 2.7. and described in the referenced publication (Hilboll et al., 2013). Explicitly accounting for a changing amplitude of the seasonality component in the trend model usually leads to only marginally lower estimates of the linear trend component (see Sect 4.7.4 in Hilboll, 2014). In the revised manuscript, we will give more details about the trend model, including the caveat about changing seasonality.

> Many regions are discussed here. A map defining all these regions will be extremely helpful for a general reader to understand the analyses.

The revised manuscript will include a map detailing the locations discussed in the analysis.

> The explanations in the last three paragraphs of Sect. 3.1 could be further improved. North Indian Plain also have large emissions from non-traffic sources such as power generation and industry. Can other factors be ruled out? The explanation for Chhattisgarh, Jharkand and

> Odisha is focused on power generation, how about other factors?

According to the Ministry of Power's monthly reports, the source mix for electricity generation in the North Indian plain has not significantly changed in the last years. To our knowledge, there have been no changes in emission regulation and exhaust cleaning installation in power generation facilities in the North Indian Plain, which makes the electricity sector unlikely to be the cause of decreasing NO_2 concentrations. While GSDP increases in that area have lost some momentum compared to earlier years, they are still significant, giving no indication of declining emissions from industry sources.

In absence of effective regulation of coal-fired power generation emissions, and given the continuing commissioning of new power stations in Chhattisgarh, Jharkand and Odisha, it seems unlikely that the power sector would not be the driving force behind continuing NO_2 increases observed in that region. Of course, other factors cannot completely be ruled out, but lacking detailed and reliable NOx emission inventories at least at state level, it is impossible to be more confident.

In the revised manuscript, we will improve the description given in the paragraphs mentioned by the reviewer and give better account of our reasoning.

> Also, it is not clear why and how the monsoon signal is clear for South India but not for other regions. Can the changes over these regions be also found in OMI NO2 data? Overall, a region-specific analysis of major socioeconomic factors before discussing the causes of NO2 trends in these regions will much help the causation analysis. The OMI NO2 data should be analyzed more intensively (e.g., in Figs 3,5,6 and Table 1), given its long temporal coverage (2004-present), different time of day (to help discuss the role of chemistry), and a higher resolution (to help reveal the hotspots). Comparing OMI with morning-time instruments will also help reveal the satellite uncertainty.

For the revised manuscript, we will follow the reviewer's suggestion and include OMI NO_2 data in the analysis.

> Fig.1 and S1 – starting the y-axis from a higher value (e.g., 15) than zero will help visualize the NO2 changes.

We strongly believe that bar plots should always start at zero, in order not to mislead the viewer's reasoning. Therefore we will not change the y-axis in Figs. 1 and S1 (see, e.g., `https://flowingdata.com/2015/08/31/bar-chart-baselines-start-at-zero/` or `http://www.storytellingwithdata.com/blog/2012/09/bar-charts-must-have-zero-baseline`)

> Fig. 3 – can you show results from OMI and quantitatively compare to GOME-2 results?

The revised manuscript will contain an OMI version of Fig. 3, and the differences will be discussed.

> Fig. 6 – NO2 was flat (or even declined) from 2008 to 2011 while electricity and GSDP grew clearly. Why? How about the OMI NO2 data?

OMI and GOME-2 show very consistent results in the Tamil Nadu case, so that including OMI data in the plot does not give any new insight. However, for sake of completeness, we will include OMI data in Fig. 6 for the revised manuscript.

According to the Ministry of Power's monthly reports, the capacity of fossil fuel power generation in Tamil Nadu did not significantly increase in 2008-2011 (yellow line in Fig. 6). The total electricity generation was still strongly increasing, the bulk of the additional capacity coming from renewable sources. We describe in the manuscript how we believe that therefore, the NO_2 burden over Tamil Nadu could stay flat in spite of increasing GSDP and total electricity generation (renewable energy doesn't have NOx emissions).

> Table 1 – how about the NO2 trends from OMI?

In the revised manuscript, Table 1 will also include NO_2 trend results from OMI.

> Sect. 3.4 – where are the numbers (3e14 – 24e14 molec cm-2) from?

The numbers are from the Jena et al. (2015) article which is cited in the previous sentence. In the revised manuscript, we have rephrased the sentence to make this more clear.

> Conclusion – "This may imply that changes in meteorology or up to now not understood changes in tropospheric chemistry are also of significance." – given the uncertainty (especially after 2012), similar sentences addressing the roles of non-emission factors should be highlighted in the abstract.

We will add an according sentence to the abstract of the revised manuscript.

References

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