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Interactive comment on "NO_2 pollution over India observed from space – the impact of rapid economic growth, and a recent decline" *by* Andreas Hilboll et al.

Anonymous Referee #1

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The study by Hilboll and co-authors focuses on changes in tropospheric NO2 pollution over India over the last 15 years. Using retrievals from 4 different satellite sensors, they present indications that NO2 pollution has been increasing between 2003 and 2013 over India, and some hints for a reduction 2014-2015. The authors relate the changes in the satellite NO2 columns directly to changes in presumed socio-economic drivers.

Major comments

1. There is insufficient detail on the satellite products used. It is not clear whether the SCIAMACHY, GOME-2, and OMI data used here have been validated. Neither it is clear whether the data from the SCIAMACHY and GOME-2 (MetOp-A and MetOP-B)

have been intercompared to check that they measure consistent columns over India. The paper should show for instance that GOME-2(A) and GOME-2(B) measure highly similar NO2 columns on the same day over India. Furthermore there is no information given on how the OMI NO2 product was generated, i.e. with a similar algorithm as GOME-2?

2. The direct attribution of trends to socio-economic drivers is questionable. There are many factors influencing the relationship between economic activities, subsequent emissions, and the measured NO2 columns. To name the most important ones: (a) sampling – measurements taken during the monsoon period (cloudy) are not suitable to detect the influence of emissions on NO2 (why not reject the monsoon period from the analysis?), (b) atmospheric chemistry – it is well known that the relationship between NOx emissions and the NO2 column depends on chemical and meteorological circumstances, and there may be differences between years that influence the relationship, especially when NOx emissions are changing, (c) errors in the socio-economic and in the satellite data – if one or both data sets suffer from time-dependent errors, it becomes difficult to argue that similar trends in both data sets allow direct attribution.

The authors seem to be aware of at least some of these issues, but do not address any of them other than making some remarks. I think they should make a much more convincing case for taking the satellite and socio-economic data at face value to make us believe there is a strong correlation between the two. In any case a more thorough analysis of sampling issues, intra-instrument consistencies and uncertainties is required, and the impact of variable meteorology and chemistry on the NO2 columns should be addressed with a model or otherwise.

3. The claim that the economy may grow without increased NO2 pollution on page 12 is very difficult to follow. The figure 6 shows very similar NO2 levels between 2003 and 2015 over Tamil Nadu, but also that energy production from fossil fuel combustion has increased strongly between 2011 and 2015. I can understand that if fossil electricity generation is driving NO2 pollution, we expect SCIAMACHY and GOME-2 to follow the

yellow line from 2003 to 2012. But, elsewhere in the paper, we are led to believe that NO2 increases when coal-burning starts, so why would this then not be the case over Tamil Nadu after 2012? It would help if the NO2 column values were given , and also the NOx emission contributions from the various sources.

Specific comments

P2, line 12: the Burrows et al. 2011-reference is not included in the reference list.

P2, L14: the vertical integration limits used in the retrieval should be given, i.e. what defines the tropopause?

Page 3, Lines 33-34: please explain why anthropogenic emissions are lowest in August.

Page 4, an indication on the accuracy and reliability of Indian socio-economic data would be welcome.

P5, section 2.7: there is no discussion on how uncertainty in the monthly mean is taken into account in the trend analysis. This should be done especially in view of the sometimes sparse sampling of SCIAMACHY data (between 0-5 measurements per month).

P6, Figure 2: it is not clear if the trends in the NO2 columns in Figure 2 have been obtained for retrievals without clouds. If so, do the bars represent proper 'annual means'? Or rather monsoon-filtered annual means?

P7, Figure 3: please include estimates of the uncertainties of the monthly means in the Figure.

P7, L12-13: Figure 3 a really strong seasonal cycle over India with a factor of 2-4 differences between winter and summer NO2 columns. It seems implausible that these differences can be explained from the difference in NOx-lifetime alone. Have the authors checked other reasons for this seasonal variability, e.g. emission variability or the

influence of air mass factors on the variability? Are slant column densities normalized with a geometric AMF also varying this strongly between Summer and Winter?

Page 8, Line 3-4: it is unclear why a "reduced growth rate" (of traffic-related NOx emissions) would contribute to NO2 decreases. If emissions are still growing, I'd only expect a decrease in NO2 concentrations if the emissions increase pushes the photochemical regime into the titration phase.

P8, L11-18: this paragraph on the delayed monsoon and its possible influence is merely speculating. My suggestion would be to analyse whether the decrease in 2014/2015 is due to the later monsoon in a more quantitative way via model simulations or other supporting data.

Page 8, line 12: pai?

Page 8, Line 24: it is unclear how the relative annual change rate in Figure 4 was calculated.

P8, L26-29: please indicate the cities of Ballari etc. on the large map of India. Not all readers will be familiar with the names of cities and regions in India.

P9, L2: with a sudden increase in 2010, how can you trust the linear regression trend analysis? This should be better explained.

P13, L5-7: this part is rather vague. Please clarify why this needs to be in the paper.

P13, section 3.4 seems like stating the obvious, and rather belongs in an introduction section.

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