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Interactive comment on "Rapid growth in nitrogen dioxide pollution over Western China, 2005–2013" *by* Y.-Z. Cui et al.

Anonymous Referee #1

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Review of "Rapid growth in nitrogen dioxide pollution over Western China, 2005–2013" by Cui et al.

In this paper, a detailed look is taken on NO2 growth rates in Western China based on OMI NO2 data. A wavelet analysis is performed on the background corrected gridded time series, linear trends are computed on the long-term trend component and compared to a nested Geos-chem run using scaled emissions, and the results are discussed in the context of economic and legislative development in China.

The paper is clearly structured, well written, and contains many interesting results and discussions. The topic of NOx emission trends and their impact on the atmosphere is of large interest in particular for rapidly developing regions such as China, the Western part of China not having received much attention in the past, and this study fits well

into the scope of ACP. However, I do have concerns about several aspects of the study as listed below. I therefore recommend this paper for publication in ACP only if these comments have been addressed in a satisfactory way.

Major comments

1. I'm confused by the description of the background correction:

   - The authors claim that they use the seasonality of NO2 to remove the natural contributions and provide a map of seasonality in Fig, 2.a. However, I cannot see where the seasonality information is then used with the possible exception of motivating the choice of the 1E15 molec cm-2 threshold applied to identify polluted pixels. It's also not clear where the 1E15 threshold is actually being used – I have the impression that the removal of non-anthropogenic contributions is done by simply subtracting monthly averages derived over certain background regions.

   - In this context it is not clear to me if all the values in the background regions shown in Fig. 1 are used or if they have been further filtered by the NO2 threshold value.

   - The amount of NO2 in the background regions is stated to be small. However, from Table 1 it is clear that the background values are in several cases on the order of 30

   - It is also not obvious that it makes sense to subtract the NO2 columns from background regions, as soil and lightning emissions have specific regional patterns and cannot simply be assumed to be homogeneous over the large areas discussed here. In particular I would expect lower NOx soil emissions in urban areas than in the rural background regions used.

2. I'm not convinced by the usefulness of the wavelet analysis applied to the data prior to the trend determination. First of all, there seems to be a subjective ele-

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ment in the choice of "The approximate signal A5" used as representation of the long-term trend. I think the authors need to

- show how their results derived using a wavelet analysis compare to results from standard trend models used in previous work and explain why their approach is to be preferred
- explain in more detail how the wavelet analysis was performed and why they think A5 is a good representation of the long-term trend, how they can identify D3 as seasonality and why they can be sure that details of the wavelet analysis do not impact on the trend determination

3. In several places, the argument is made that the good agreement between model and data indicates that the trends observed are anthropogenic. While I'm convinced that the trends are anthropogenic, I don't see how the approach taken can prove that. As the emissions used are only available for one year, the authors scale the inventory by using the relative change of the OMI columns. To me it appears evident that such a procedure will lead to broadly consistent model and satellite trends (ignoring non-linearity effects) and I wonder what really can be learned from this exercise. In this context it is not clear

- what the spatial resolution of the MEIC inventory is
- how the scaling with OMI data was done – was this on a 0.25 x 0.25 degrees grid?

Minor comments:

Page 34916, line 12: to evaluating pollution => evaluating pollution

Page 34918, line 10: are referred to => are described in

Page 34918, line 15: As the AVK is given on satellite pixel basis, it is not clear how they are transferred to the model grid – please give more detail

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Page 34918, line 22: inventory => inventories

Page 34921, line 24: DTW => DWT

Page 34924, line 10: of other => other

Page 34925, line 8: in all days => on all days

Page 34925, line 21: but with a reduction in summer => but reduces it in summer

Page 34926, line 22: Fig. 7 => Fig. 6

Page 34928, line 22: growth rate of what?

Figure 2: NO2 columns, not concentrations

Figure 2: grid cell is sorted => grid cells are sorted

Figure 4: As the topic of this paper is Western China, please add a scatter plot for the points of the study region

Figure 4 and Figure 5: Colour scale difficult to read for colour blind readers

Figure 5: subtracted by its => subtracted by their

Figure 7: As stated above, I'm not convinced that this is the seasonality in the sense that for a given year, it reflects the seasonal change in NO2 column. For example, the amplitude for Shaanxi increases by more than a factor of 2 during 2005 which appears unrealistic to me.

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 34913, 2015.

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