We thank both reviewers for their insightful comments. The manuscript is revised based on the reviewers' comments.

Reviewer 1:

The paper by Gu et al. reconciles the GOME2- and OMI-based emission estimates by using a 'local derivative' approach. Such reconciliation is important for use of either satellite instrument. The paper is easy to read and appropriate for publication after some revisions.

We thank the reviewer for the constructive comments. The manuscript is revised based on the reviewers' comments.

The local derivative approach or its variants have been proposed by many previous studies. It appears that the real strength of the current paper is to demonstrate the success of the approach to reconcile emission estimates based on different satellite instruments. The paper may benefit from further clarification of its key contribution to the literature (title, abstract, introduction, etc.)

We thank the reviewer's suggestion. Due to the unusual high emission rates over China, the nonlinearity in photochemistry can lead to significant biases on emission estimation. Therefore we investigated the feasibility of applying the local derivative approach on emission estimation over eastern China. The nonlinearity problem is quite unique for NO_x inversion studies. Otherwise, the original approach by Martin et al. (2003) should have worked well. In this aspect, the conciliation of two satellite based inversion results is a reflection of the nonlinearity problem. Given that the mathematical derivation of the local derivative method is straightforward and its applications in previous studies are not related to the nonlinearity problem we have there, this study is first of its kind.

The wording in the introduction appears that alpha in Eq2 is applied to Eq1, which is not appropriate, as Eq2 assumes local mass balance and is not derived from Eq1. Please clarify.

Plugging Eq. (2) into Eq. (1), we obtain the top-down emission estimate formulation by Martin et al. (2003). The original text referred the derived emission as "a posteriori", which is inappropriate. We corrected the mistake. Thank you.

The inverse modeling process is not clear. Is the averaging kernel applied? How are model results sampled according to the satellite products? What is the spatial resolution of inverse emissions and how is the regridding of satellite and/or model data done? Is it possible to briefly elaborate on the model errors and its implications on the emission inversion? Previous studies have revealed various model uncertainties for emission inversion (e.g., Lin et al., 2012; Stavrakou et al., 2013).

Generally, we use the same framework by Martin et al. (2003) for the inverse modeling process for the bulk ratio approach (line 132). We excluded the data flagged with row anomalies from the OMI we only use measured NO_2 column data when cloud fraction is < 20% for both measurements to reduce the cloud interference (line 98). The spatial resolution is 36×36 km^2 (line 121). The uncertainties were given at line 164. The details of the model errors and impacts on emission uncertainties were discussed in Gu et al. (2014) and Zhao and Wang (2009). We now refer readers to the previous studies.

The description of the differential approach could be further clarified. First, satellite data contain both systematic and random biases (Boersma et al., 2004), and currently it is not clear which portion of satellite biases is systematic. The key merit of the differential approach is to reduce the effect of systematic (and common) biases in the satellite products at different times of day. Secondly, please clarify that the Eq4 here is an approximation of the original Lin et al. (2010) formulation, and please discuss the implication of this simplification for the inversion results. Thirdly, Lin and McElroy (2010) already shows that because the differential approach is based on the weighted difference between NO2 columns at different times of day, it may lead to emissions lower or higher than single-instrument-based emissions retrieved from both satellite products.

We agree that the differences between the two satellite products are not well characterized (line 231). We added that the Eq 4 is simplified from the original equation now (line 213), and discussed the results in the Appendix A. In this study, we further analyzed that the implication of the differential approach, and show the mathematical analysis demonstrating the reasons for the bias consistency in the a posteriori emissions, which is a new result. It is not in conflict with Lin et al. (2010), but we believe that the local derivative method is more robust relative to the bulk ratio and satellite differential methods. We now cite Lin and McElroy (2010) in the paper.

The interpretation of Fig. 1 should be cautioned. That more emissions may be associated with less NO_x, shown in the figure, reflects the spatial dependence of NO_x lifetime and NO_2/NO_x ratio. This spatial dependence may not be simply interpreted as the nonlinear relation between NO_2 column and NO_x emissions IN A GIVEN GRID CELL. This is because the spatial dependence may be a result of differences in other factors such as VOC emissions, meteorology, etc. For example, it would not be realistic that increasing emissions in a given grid cell leads to a reduction in NO_2 column in that grid cell. Please clarify.

We agree that the emission to column ratio in Fig. 1 can be impacted by other factors. But we believe that the shape of the dependence function is mainly a reflection of the nonlinearity in chemistry. In the modeling analysis, we used the local perturbation approach (Line 156) to isolate the effects of in situ nonlinear chemistry. We stated in the paper that the Fig 1. reflects in part the difference in the relative importance between transport and chemistry at different time of a day (line 74).

Reviewer 2:

The manuscript by Gu et al. (2016) focuses on inverse modeling of NOx emissions over China using GOME-2 and OMI satellite measurements. Inversion is done with previously used "local derivative" and "bulk ratio" approaches. The emission estimates from the two approaches are compared, and limitations of the "bulk ratio" approach applied to both single and multi-satellite measurements are discussed. Main strength of this paper lies on the application of the single-cell based emission perturbation scheme in the model to compute the sensitivity factor. The paper is well-written, and should be of interest to the AMT readers. However, a few issues listed below need to be addressed before the paper can be recommended for publication.

We thank the reviewer for the constructive comments. The manuscript is revised based on the reviewers' comments.

Major Comments:
Satellite measurements: A careful comparison and characterization of the two satellite retrievals is necessary for a credible emission estimates. Retrieval algorithms for OMI and GOME-2 differ on few aspects (fitting, surface reflectivity, cloud), not just a- priori NO2 profiles as discussed in the manuscript. In fact, a-priori NO2 profiles are less of an issue since they can be replaced with user-supplied model profiles using the auxiliary information (averaging kernel) contained in the data file. For consistency between retrievals and emission estimates, this is a necessary step, but it is unclear if that is indeed done.

We thank the reviewer for comments. We agree that using different retrieval algorithms can introduce systematic biases. However, we used the satellite retrieval products from KNMI for both GOME-2 and OMI instruments in this study (line 94). The retrieval algorithms are described by Boersma et al., (2004, 2007, 2011). While we are trying to reconcile the difference of emission estimates from inverse modeling using two products, we are using the standard KNMI retrieval products and not focusing on the retrieval process.

Uncertainty in satellite measurements and emission estimates: The manuscript should expand this aspect – how are they calculated? Discussion of uncertainty calculation for tropospheric AMF is unclear. Reported uncertainty in stratospheric SCD sounds too large. If uncertainties in tropospheric NO2 are indeed calculated, I recommend including uncertainty figures for both OMI and GOME-2 data.

In this study, we are using the standard KNMI retrieval products for the inverse modeling. The stratospheric SCD values in OMI and GOME-2 retrievals are from the same global model (TM4). The uncertainty in the satellite products was described by previous studies (Boersma et al. 2004, 2011, 2007; Martin et al., 2003; Zhao and Wang, 2009). We now refer readers to these studies.

Simulated NO2 column vs NOx emissions: This is very important part of the manuscript, but I have difficulties in understanding and interpreting it. First, the scenario presented in Figure 1 does not fully represent eastern China as NO2 columns vary only up to 3×10^{15} molec cm^{-2}, not typical of eastern China. Second, I have problem to interpret OMI curve that indicates saturation at NOx emission of $\sim 2.6 \times 10^{15}$ molec cm^{-2} hr^{-1}. Does that mean a decrease in NOx emission would still increase NO2 column by up to 50%? Would a 25% increase in emission result in a factor of two increase in column? Why would these happen? Third, please consider reversing the axes for clarity.

We thank the reviewer for pointing out an error in the paper. The unit of x-axis in Figure 1 was a typo, and we corrected it to 10^{15} molec. cm^{-2}. Figure 1 shows the results from the regional model simulations. It does not represent the local response of NO_2 column to NO_x emission but includes the effects of various factors (i.e. influxes). However, the figure shows that tropospheric NO_2 column is not linearly proportional to NOx emission, which was assumed in Eq. (2). We also suggest it may reflect in part the difference in the relative importance between transport and chemistry at different time of a day (line 74). The per-cell sensitivity of column NO_2 to NOx emissions must be calculated separately as we did in this study. An illustration of the sensitivity correction can be found in Figure 1 of Gu et al. (2013). The work by Gu et al. (2013) was cited in the relevant discussion in this manuscript.

Minor Comments:
Page 2, line 28: “There” => “They”.

We corrected the typo.

Introduction: This section should acknowledge similar work by other groups (e.g. Boersma et al., 2008; few papers using adjoint modeling).

We added the references.

Page 4, line 81 (and method section): How was source speciation done?

We followed the Zhao and Wang (2009) and Gu et al. (2014) in the study.

Page 5, line 93: Two GOME-2 instruments are in operation currently. Please, be specific that you are using GOME-2A measurements.

We specified the instrument is onboard MetOp-A satellite now.

Page 5, line 109: Please, include appropriate reference for this statement on ~10% error from a-priori profile.

We added the references.

Page 6, line 125: Does not the mode include soil NOx emissions? Please include information about this source.

We added the information and references in the content.

Page 7, line 143: What does "a correction of profile" mean, and how is it done?

Here is a general reference of previous study, and we correct the sentence due to the relativity to this study.

Page 7, line 156: "inversed" => "inverted" .

We corrected the typo.

Page 7, line 156: Why "either OMI or GOME-2 observations" and why not both?

We actually computed the results from both observations and corrected the sentence now.

Page 9, line 196: "inversed" => "inverted".

We corrected the typo.

Page 10, lines 211-212: Please, use appropriate symbols for T and tau.

We are using the same symbols that used in previous studies (i.e. Lin et al, 2010).

Page 11: line 238: "inversing" => "inverting".

We corrected the typo.

Page 23, Table 1: How was the total emission for East China (last column) calculated? You mentioned that you analyzed the data for August, 2007 only. Was the analysis expanded other months as well?

We clarified in the table title that the estimates are for August 2007.