

## ***Interactive comment on “An updated tropospheric chemistry reanalysis and emission estimates, TCR-2, for 2005–2018” by Kazuyuki Miyazaki et al.***

### **Anonymous Referee #1**

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This paper presents the results from the TCR-2 for the period 2005-2018 obtained from the assimilation of multiple updated satellite measurements. The derived emissions are validated against independent aircraft, satellite, and ozonesonde observations. The results are sound. I recommend the publication after minor revision.

General comments: 1. Section 2.2. The covariance matrices of observation and background error are very important for the accuracy of forecasts. Please clarify how the uncertainties are determined in the text. 2. Section 3.1. The super-observation approach is applied to NO<sub>2</sub> and CO, but not other species. Is there any specific reason for this? 3. Page 12, line 340, the authors tabulate the bias of modeled NO<sub>2</sub> compared to OMI, SCIAMACHY, and GOME-2 observations. I notice that the consistency is significantly better for results relying on single species (e.g., DECSO in Ding et al.).

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What could be the driver for the bias? Is the uncertainty of the assimilated system or the conflict caused by satellite observations of different species used for assimilation? Line 357, Over India, the model negative bias increased with year because of the lack of the emission increases in the a priori emissions. I suggest adding some analysis about the uncertainty associated with the a priori emissions in the text as well.

4. Section 7.2. As mentioned by the authors VOCs have a pronounced influence on the tropospheric chemistry. Any specific reason for excluding satellite-observed VOC into the assimilated system?

Specific comments: 1. Page 3, line 58, the description of IFS and the following discussion about reanalysis pop up here. What is the relationship between TCR and those models? 2. Page 21, line 625, the negative increments over India seems to be inconsistent with the recent report. please try to clarify the reason for such inconsistency. 3. Fig 8 & 9, the legend is too small to see. 4. Fig 11, I recommend adding the r and RMSE in the figures. 5. The authors performed comprehensive evaluations for multiple species using multiple measurements. It is very impressive, but easy for readers to get lost. I recommend a table to summarize the validation results by species and sources of measurements. It would be easier for readers to follow the improvement by listing the global average difference between aprior and post simulations against measurements.

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