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

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This paper presents a re-analysis of the tropospheric composition for the period 2005-2018. This re-analysis is constructed by assimilating a wide range of satellite observations in a high-resolution chemistry-transport model.

This is a great effort that should find its spin-off in the community. The paper is based on a large number of underlying studies that outline the method in more detail. This makes it difficult to read the paper as stand-alone, because many details are lacking. Still the paper is very long, with abundant figures and tables. Since this is a "data" paper, I can understand this approach. Nevertheless, it would be good to present clarifications on a number of points, or at least point the reader to the correct papers

for reference. This holds specifically for the following points:

1) The state vector. The paper mentions a state vector of emissions and concentrations. In more detail: NOx, CO emissions, lightning NOx, SO2 emissions and 35 chemical species. The latter surprises me, because the observations to not contain information on these 35 species. Probably this is outlined in an earlier paper, but this needs to be clarified/referenced to properly.

2) The weighting between emissions and concentration updates. This approach is unique in the sense that both emissions and concentration fields are in the state. However, with a assimilation window of only two hours there is a serious problem, specifically for CO (and, like the authors falsely claim, for O3). Emissions are only "seen" for two hours by the system. Still the authors present a detailed analysis of the emission increments and emission time series. But if also the concentrations in the model are adjusted, I wonder what happens to the mass balance? The concentration updates are not propagated to emissions. Likely these considerations are part of earlier papers, but need to be outlined here to some extend.

Further questions and remarks are given in the annotated pdf. These remarks also point to some obvious unit errors in figure 4. All in all, however, this paper presents the re-analysis in a fair and objective way, and points to further improvements needed. I hope my comments help to improve the paper further.

Please also note the supplement to this comment:
https://www.earth-syst-sci-data-discuss.net/essd-2020-30/essd-2020-30-RC2-supplement.pdf