Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2022-251-RC2, 2022

Comment on acp-2022-251

Anonymous Referee #2

Referee comment on "Inverse modelling of Chinese NO_x emissions using deep learning: integrating in situ observations with a satellite-based chemical reanalysis" by Tai-Long He et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-251-RC2, 2022

The authors developed a deep learning model to integrate satellite data and in situ observations of surface NO2 to estimate NOx emissions in China. They illustrate the potential utility of the DL model as a complementary tool for air quality applications. The manuscript is well written. The introduction is very informative. The method looks sound, and the results are comparable with existing inventories. I recommend publication after minor revision.

conventional data assimilation approaches for air quality applications investigate the NO2 changes over India during COVID-19 lock-down period using both satellite and in-situ measurements. The authors investigated the differences between rural and urban areas. The contributions from natural sources are also considered. The manuscript is easy to follow and the primary conclusions are sound. I recommend publication after revisions.

General comments:

Section 3.2. validation using C-index. I would suggest clarifying the driver of the C-index change before the analysis. It’s easy to understand the relationship between C-index and NOx emissions is not linear since they are potentially driven by different activities. As far as my understanding, the change of C-index is closely related to mobile emissions. However, the urban NOx emissions have multiple sources. Can we assume the differences in drivers are the reason for different recovery time? If so, do the differences suggest the mobile emissions recover quicker than other sources? Additionally, the comparison among regions shows very diverse patterns. It seems the consistency for JJJ is significantly worse than other regions. Any insight about this? The similar questions are applied to section 3.3. I believe it will be worth to investigate the driver for the differences, since the authors try to us C-index to validate the method.

Specific comments:

- Page 1, line 21. NOx also has natural sources from the surface.
- Page 3, line 58. Please try to define “two-stage transfer learning strategy” before using them.
- section 2.1. Please clarify which OMI and TROPOMI products and data version are used.
- Page 6. Line 119. What is the magnitude of the correlation? It is good to use a few sentences to explain the reason behind the correlation here.