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Comment on acp-2022-627

Anonymous Referee #2

Referee comment on "Ground-level gaseous pollutants (NO₂, SO₂, and CO) in China: daily seamless mapping and spatiotemporal variations" by Jing Wei et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-627-RC2>, 2022

The manuscript by Wei and colleagues titled "Ground-level gaseous pollutants across China: daily seamless mapping and long-term spatiotemporal variations" professes to generate seamless daily maps of three major pollutant gases, NO₂, SO₂, and CO, across China from 2013 to 2020 at a uniform spatial resolution of 10 km. While the topic is overall still quite interesting for the global air quality community, the manuscript has a number of serious scientific flaws which unfortunately led me to the recommendation of **rejection**. These issues are explained below, but are also clearly noted and commented upon in the annotated text that is included with this review.

The main premise of the generation of the daily maps of gaseous concentration is that the authors used artificial intelligence technologies and big data to produce these maps. The model used is not at all adequately described: it is simply named, Space-Time Extra-Tree, and a reference to a previous work that produced O₃ maps is given. This is not at all sufficient for the reader of this work to assess the model, its strengths, its limitations, nor to assess whether a model that functioned well for one gas would work for another gas. Section 2.2 is extremely poor in reproducible content in that respect. The input parameters used in the model are not at all adequately described: in section 2.1.2 a long list of satellite, reanalysis, and model datasets are more or less simply named, without the most pertinent details of provenance, usability, references, validation and quality assurance being provided. Exactly how these input parameters were used in the STET model are not explained at all. Furthermore, these datasets have obvious important differences, for e.g. the OMI/GOME2 VCDs and the CAMS reanalysis VCDs, there is no discussion how these were merged into a usable dataset. The meteorological ERA5 data are on a 3h level, how were these turned into daily means, and what does it actually mean that they did, etc., is an issue also not discussed. The main input parameters, both for the training of the model and the verification of the model, i.e. the ground-based measurements are not at all adequately described. In section 2.1.1 it is not at all clear what these "reference-grade ground-based monitoring" stations are, how they were chosen, if and how the data pass QA/QC protocols, what the *reference state* is, how these stations were split for the verification of the STET and the training of the STET, how the gaps in the datasets were dealt with, how the hourly observations were turned into daily, etc. The results are not sufficient to support the interpretations and conclusions. The section starts, not with the expected maps of the input parameters, maps of the output parameters and maps of the ground-based stations, but with model performance scatter plots which are not at all explained as to what is being compared to what. Absolute levels

are also provided for biases which have no meaning whatsoever if the actual levels of these gases around China are not provided to begin with. A section is also provided, 3.3, where this dataset is being compared, basically via Table 4, to numerous other related works. How the comparisons were made is unclear, how the statistics shown in the table were created is unclear, how so different datasets were homogenized before comparison is unclear, and the final statement that *our gaseous pollutant datasets are superior to those from the studies* is not at all shown in this work. **It is impossible to assess the interpretations and conclusions stated by the authors based on the information provided in the results section.**

Another premise that the authors mention numerous times, in the title even, is that the new dataset is *long-term* and that it will *benefit future (especially short-term) air pollution and environmental health-related studies*. They provide a section, 3.4, where they enumerate successful applications however it is unclear if these studies used their previous work on O₃, or other similar works. The benefits of this work should be clearly stated, to support this work, and not generalities.

Concluding, while it is possible that this work has potential for air quality-related studies, through the current manuscript the description of experiments and calculations is not sufficiently complete and precise to allow their reproduction by fellow scientists and provide traceability of results. I recommend to the authors to take the opportunity of this review to reconsider their strategy for their future publications.

Please also note the supplement to this comment:

<https://acp.copernicus.org/preprints/acp-2022-627/acp-2022-627-RC2-supplement.pdf>