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Interactive comment on "Estimating ground-level CO concentrations across China based on national monitoring network and MOPITT: Potentially overlooked CO hotspots in the Tibetan Plateau" *by* Dongren Liu et al.

Anonymous Referee #1

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Reviewer's Comments on 'Estimating ground-level CO concentrations across China based on national monitoring network and MOPITT: Potentially overlooked CO hotspots in the Tibetan Plateau' by Liu et al.

General Comments

This manuscript reports an analysis of ground-level CO concentrations over China based on both measurements from a network of in-situ CO monitoring stations and the MOPITT satellite CO dataset. The writing is generally clear and the figures are of

good quality. However, while the subject matter is generally consistent with the focus of ACP, there are a number of important issues which are inadequately addressed in the manuscript. Revisions would be likely to involve considerable additional effort. Below are the major issues that I see, listed from the most general to most specific.

1. The overall goal of this research is not really clear. Shouldn't this paper attempt to show whether MOPITT data are or are not useful (combined with the surface in-situ network) for estimating surface-level CO concentrations over China? That question does not really seem answered by this study. Or, if the focus is on the RF-STK model and surface monitoring network, are the MOPITT data really even necessary for this research?

2. The sensitivity of MOPITT TIR-NIR surface-level CO retrievals is highly variable and should be analyzed more deeply. For example, averaging kernels should be presented for different regions (and perhaps different seasons) in order to anticipate situations where MOPITT surface-level retrievals should be useful versus situations where the retrievals will be strongly weighted by the a priori. For example, it would not be surprising if MOPITT averaging kernels over the Tibetan Plateau were generally weak (which is often true for mountainous regions), implying a strong dependence on the a priori.

3. A useful study would be to compare results for two experiments: one where the actual MOPITT retrieved surface-level CO is used in the analysis and a second experiment where the MOPITT a priori surface-level CO is used instead. Comparisons of the results for these two experiments with data from the monitoring network should reveal whether the MOPITT retrievals include additional useful information beyond the a priori.

4. As described in the MOPITT V7 validation paper, bias drift is a known issue for the MOPITT V7 TIR-NIR product. At the surface, bias drift appears to be approximately -0.7% per year, which is significant compared to actual CO trends. This artifact of the MOPITT data should be recognized and somehow represented in the data analysis.

5. Although MOPITT surface-level CO concentrations are reported in terms of volume mixing ratio (ppb), the manuscript consistently analyzes CO concentrations in terms of density (mg/m3). This issue is not discussed at all, although it is implied in several places that a single conversion factor is used to convert from ppb to mg/m3. Since CO density will decrease as the pressure decreases, and surface pressure varies considerably over China, a single conversion factor from ppb to mg/m3 is not appropriate.

6. The methods used for filtering and 'gap-filling' the MOPITT data (mentioned in Section 2.1) should be discussed in more detail.

7. For readers who are not familiar with machine learning methods (including myself), a more basic description of the RF-STK model in Section 2.3 would be helpful.

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