Response to Reviewer's Comment

We thank the reviewer for reading our paper again and appreciate his/her comment. We provide a detailed response to the comment below.

**Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)**

I really appreciate the authors' effort on addressing my comments. I only have one more question.

It seems that MOPITT CO is lower than modeled CO in both March and December (Fig. 10 and 11). The authors attributed the model high bias in March to the FINN fire emissions, and provided no explanation for December when fire emissions are unimportant. If we believe MOPITT CO is correct, then anthropogenic emissions in the model are probably too high in December.

On the other hand, the authors showed in Table 4 that modeled CO is too low compared to ground measurements in December (except one site CBR). Can the authors elaborate on the inconsistency here? Could it be possible that there is some bias in the MOPITT surface CO? Or is there any other explanation for this consistency?

We thank the reviewer for pointing out the discrepancy between what Figs. 10 and 11 show (MOPITT CO is lower than WRF-Chem CO) and Table 4 (WRF-Chem CO is less than CO from ground sites). It forced us to re-examine our script that created the plots for Figures 10 and 11. We found that we did not apply the MOPITT averaging kernel to the WRF-Chem output. The previous figures were not a fair comparison between MOPITT and WRF-Chem. We corrected the script and are now submitting new versions of the figures. While WRF-Chem is still higher than MOPITT for March, its mixing ratios for December are now quite similar to MOPITT retrievals of daytime surface CO. Thus, the inconsistency no longer exists. Again, we are grateful to the reviewer for pointing this out and apologize for not submitting the correct plot initially.

After reviewing the text on page 18 of the manuscript, we do not think there is a need for any changes.