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Interactive comment on "Quantifying variability, source, and transport of CO over the Himalayas and Tibetan Plateau" *by* Youwen Sun et al.

Anonymous Referee #2

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The authors analyzed the site measurements and modeling results of CO over the HTP in terms of its diurnal and seasonal cycle as well as interannual variability. They also used GEOS-Chem and HYSPLIT to identify the major long-range transport path of CO globally to the HTP. The analysis was mainly done for several urban regions in the HTP. In general, the manuscript structure is well laid out, but there are still a few places requiring further clarification and improvement. My suggestions and comments are as follows.

Major comments:

1. My major concern is the relatively low model spatial resolution for GEOS-Chem (2deg) and HYSPLIT (1deg) simulations, which could not capture the complex topography over the HTP. Particularly, this might miss some efficient valley-mountain trans-

port. How would the spatial resolution issue affect the transport and variation analysis in this study? Besides, it is also a problem when evaluating GEOS-Chem model by comparing 2deg grid value with point-scale site measurements.

2. Section 5.3: It's interesting that the authors combined GEOS-Chem simulation and HYSPLIT back trajectory analysis to identify the path. But note that the meteorological fields used in GEOS-Chem and HYSPLIT are different, which could lead to some inconsistency. Maybe a brief comparison of wind fields for these two would be useful.

3. One important thing that was not discussed by the authors is how the uncertainty in VOC emissions contributes to the uncertainty in the analysis here, given the non-trivial contribution from secondary CO production. Besides, how would the stratospheric intrusion of ozone which is important over the HTP affect the CO simulations here? Some discussions are needed.

Minor comments:

1. It seems that this study mainly focuses on urban areas over the HTP, so I suggest changing the title to reflect this aspect to avoid confusion, since conclusions here may not be applicable to remote areas in the HTP.

2. Page 3, Line 8: Note that BC is also one aerosol component, so the authors could be more specific about the aerosol here. Also, a few important recent studies can be included here, for example, Li et al. (2021): https://doi.org/10.1016/j.envint.2020.106281; Gul et al. (2021): https://doi.org/10.1016/j.envpol.2021.116544

3. Page 3, Lines 21-22: "... are still poorly understood". I think the community has made important advances in the past 10 years on this topic over the HTP. Many studies have investigated the sources and transport of atmospheric pollutants over the HTP, although many of them have used black carbon instead of CO as a tracer (e.g., Thind et al., 2021: https://doi.org/10.1016/j.atmosenv.2020.118173; He et al., 2014: https://doi.org/10.1002/2014GL062191; Zhang et

al., 2015: https://doi.org/10.5194/acp-15-6205-2015; Zhu et al., 2019: https://doi.org/10.5194/acp-19-14637-2019). I think these studies have also helped to improve our understanding of pollution transport in this region and could be mentioned in the introduction and compared with the transport results based on CO tracers in this study in the discussion section.

4. Page 3, Lines 38-44: One thing that was not mentioned by the authors is that what is the new aspect of this study to look at CO over the HTP compared with previous studies. It seems that not much has been described in the introduction section regarding what knowledge of CO transport in this region we already obtained from previous studies. Some descriptions are needed and the novelty of this study needs to be highlighted.

5. Page 12, Line 4: "Factors drive . . ." should be "Factors driving".

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