

## ***Interactive comment on “Uplifting of carbon monoxide from biomass burning and anthropogenic sources to the free troposphere in East Asia” by K. Ding et al.***

**Anonymous Referee #2**

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Review of Ding et al. Uplifting of carbon monoxide from biomass burning and anthropogenic sources to the free troposphere in East Asia

This paper studies elevated CO in mid- to upper-troposphere, its origins, vertical transport and implications for long range transport. While there have been a lot of studies focused on CO, including those employing satellite data and studying East Asia, this study is unique in that it examines vertical transport of CO, still a very uncertain topic. Given the wealth of CO data, this is a very appropriate way to study vertical transport and free troposphere pollution enhancements. The authors chose 3 case studies, not many, but examine them very closely, learning about important aspects of topography

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influencing vertical and horizontal transport of pollution. It's a bit of a shame that despite improvements in vertical information of MOPITT data, still the authors count on deriving only qualitative information about vertical transport.

One thing that wasn't clear to me is how frequent are these episodes of enhanced vertical transport. The authors identify just 3 cases across several years. Are these episodes hard to spot in the sparse data? Very infrequent? If the latter, do they add up to an important contribution regionally? world-wide? Does the model see more of them? Or are they a case study to more easily learn about vertical transport? It would be very useful to read more background.

In general, I think the sections should be shortened and made more concise, especially given the lack of quantitative information. The authors are strongly encouraged to add quantitative information.

p28025, l 5-10. It's a bit confusing to read about demonstrating MOPITT's vertical sensitivity, since I thought it was MOPITT's vertical sensitivity that was being used to evaluate the vertical transport, not the other way around. It would be great to be clear about that here already.

p28027, l 17-19. It would be great to put the list of parameters at the beginning of this subsection, otherwise, it's a bit unclear what "analyses" include Section 2.5 Are the met data described in 2.3 used only to drive FLEXPART? Perhaps it's better to combine those two sections then, similarly to how GMAO met fields are in the same section as GEOS-Chem

Section 2.6 Is the full chemistry version of GEOS-Chem being used as implied or is it just tagged CO/single tracer simulation? p.28030, l20-21. I don't understand the concept of "difference between averaging kernels", is it its diagonals?

Section 4.1 It's an interesting reconstruction of the history of a pollution plume, but I'm not sure how much of this was not known before. It's not clear what's new here. Is it

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that pollution is transported via WCB? That it is lofted 9km? That it reaches Canada? That MOPITT and models agree? None of this sounds new, so it would be helpful if the authors stated more explicitly what new insights they have gained here. I'm also curious what does GEOS-Chem say about this case study.

p28036, l2. "This source is confirmed in the GEOS-Chem simulation". Are the authors really relying more on GEOS-Chem than MOPITT to confirm cases of vertical transport of pollution? Shouldn't it be the other way around? especially at 700hPa. Naturally, it would be good to have independent data set here, but I guess MOZAIC was not available? It would be good if the sections were a bit more parallel (each commenting on the skill of both models and availability and quality of both data sets) Is Figure 12 necessary?

p28038, l 1-3 How was this somewhat random time frame chosen? I'm sure there was a good reason, but it's not clear from the text what it was.

p28038, l 21. It's a bit difficult to believe that different altitude of this plume as identified by MOPITT is an indication of MOPITT's ability to resolve vertical structure. While this could be true given how good MOPITT instrument is in general, the earlier section relied more on GEOS-Chem than MOPITT to even identify the plume, so here I am wondering if we should take MOPITT at face value or wonder what GEOS-Chem is showing in this case. Section 5 Discussion. It's hard to tell what is the exciting findings. Is it that there was high CO documented? Is it that topography affects vertical CO transport? Is either new and/or surprising? Please tell the reader so. It all seems intuitive and the section is purely qualitative, so it's not clear that anything new is being reported. It would also be helpful to have quantitative information. For example, how does topography affect CO transport, does it have to be a mountain region? only in the north/east/west/south? does any of this or could any of this vary with seasons? It's hard to draw conclusions from individual case studies.

p28044, l28-29 It's not clear why the statistical analysis couldn't be done here already.

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Authors should consider doing at least preliminary work on that.

p28045-28046. These whole pages (and the remainder of that section) can be deleted, especially the first paragraph. It's just repeating background information.

p28048, l17 replace "interplaying" with "interacting"?

Figures: figure captions and labels are a bit too small to be readable, especially on figures 1-7, figure 11

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 28019, 2014.

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