

## ***Interactive comment on “Spatial and temporal variation of CO over Alberta using measurements from satellite, aircrafts, and ground stations” by H. S. Marey et al.***

**H. S. Marey et al.**

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Author's response letter Thank you for your careful reviews and comments to improve the quality of the manuscript. We have provided point to point responses to all comments and questions.

Anonymous Referee #2 Page 31770, line 18: Change “no researches” to “no studies” Done and it is shown in line 76.

2. Page 31770, line 27: Use a comma when you have a number with four or more digits, e.g. “428 692.5” should be 428,692.5” Done and it is shown in line 85. 3. Page

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31771, line 7: Remove “it can” so that the sentence reads “CO can also be produced...” Done and it is shown in line 93.

4. Page 31771, line 11: CO is not a significant source of ozone. What are its “significant” impacts on air quality? I would suggest removing the word “significant” here. Done and it is shown in line 96.

5. Page 31771, line 12: Should be “significant” Done and it is shown in line 97.

6. Page 31774, lines 5-9: What is the range of altitudes for which you have aircraft data? The range of altitudes of aircraft data is from surface till about 300-250 hPa.

7. Page 31775, line 13: It should be “especially in central and southern Alberta.” Done and it is shown in line 200.

8. Page 31778, line 11: Should this be “fresh air from the mountains”? Done and it is shown in line 276.

9. Page 31778, line 12: Can you substantiate this claim that Calgary has less development than Edmonton? Are there bottom-up inventories for emissions from Calgary and Edmonton that you can compare? Deleted and it is shown in line 271.

10. Page 31778, lines 14-16: I don't understand this sentence “The seasonality in the Fort McMurray area is progressively less pronounced...” Please clarify or rephrase. Done and it is shown in line 279.

11. Page 31778, line 17: Change “in summer than the springtime” to “in summer than in spring” Done and it is shown in line 281.

12. Page 31778, line 22: Change “on a clean air area” to “to a clean air area”. Done and it is shown in line 280.

13. Page 31778, line 24: Change “transported at the aircraft's” to “transported to the aircraft's” Done and it is shown in line 287.

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14. Page 31779, line 4: Please see comment #2 above about numbers of four or more digits. Done and it is shown in line 296.
15. Page 31779, line 7-9: It is not clear if the authors are referring to “deep convection” over North America or elsewhere. I would be surprised if deep convection is important over North America in spring. Changed to “vertical motion or mixing” and it is shown in line 300.
16. Page 31779, line 16: Change “where cities such as” to “whereas cities such as” Done and it is shown in line 307.
17. Page 31779, line 18: Add “the” between “over” and “Edmonton area” Done and it is shown in line 309.
18. Page 31779, lines 25-26: I don’t understand what the authors mean by “the degree of repeatable seasonal variability varies by year and region” in the context of Fig 7. Changed to: “the magnitude of seasonal variability is not the same for all years and regions” as shown line 316.
19. Page 31780, lines 1-3: The claim that emissions are confined to the PBL due to subsidence is unsubstantiated. The plots of the vertical motions in Fig 4 do not prove that the emissions are confined to the PBL. For example, the surface CO peaks in April over Calgary (Fig 7), but the mean omega in the region is negative in April (Fig 6), suggesting ascent. Anyway, it is not clear to me why the authors are concerned about confinement here. The vertical gradient in CO in Fig 7 is expected if there are emissions at the surface. We agree that emissions at the surface affect vertical gradient, as stated in line 319-320 of the original manuscript. However we analyzed omega variations at 700 hPa to understand the strength of vertical mixing. For example, the mean omega values are negative in April which means there is an ascent, and this is seen in Fig. 7 (CO profile) as high CO values are extended to higher altitudes especially over Calgary.

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20. Page 31780, lines 7-9: This statement about transport emissions relative to industrial emissions is redundant. It was already stated on page 31779, lines 1-3. It is deleted.
21. Page 31780, lines 15-17: Why remove the annual mean for each year instead of the 12-year mean? If there is a long-term trend, removing the annual means should remove the trend, but that is not the case here. Actually this was incorrectly stated in the original manuscript. The calculation is based on the 12-year mean. This issue is fixed as shown in line 332.
22. Page 31780, line 20: I assume that this is -1%/year? Actually it is -1 % for the whole period. This is clarified in the revised manuscript.
23. Page 31781, line 8: “the” should be “The”. Done and it is shown in line 353.
24. Page 31781, lines 14-19: I assume that the aircraft profiles do not extend much above the upper troposphere. In applying Eq (1), what do the authors do for the upper part of the profiles? For example, Worden et al. (2010) used a MOZART climatology at altitudes above 250 hPa when they transformed the MOZAIC data. The missing profile part above the highest altitude where MOZAIC/IAGOS measures is estimated from the MOPITT a priori profile and it is added in line 361.
25. Page 31781, lines 23-25: Deeter et al. (2013), which is cited in the manuscript, showed that there is a 14% high bias in MOPITT V5J data at 200 hPa. This should be mentioned here. Done and it is shown in line 371.
26. Page 31782, line 5: I don’t understand what is meant by the statement that “winter surface emissions are more entrained in the boundary layer”. Do the authors mean confined to the boundary layer? What is the evidence that this is the case? Yes we meant confined in the boundary layer as this is indicated by the omega monthly variations and the seasonal spatial variations of omega (positive values). The sentence was revised accordingly, as shown in Line 380.

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27. Page 31782, lines 8-11: The statement that there is more lofting of the emissions in spring is unsubstantiated. Either demonstrate this or remove this statement. This shift in the seasonality could be due to the sensitivity of the surface level retrievals, which should increase in spring and summer as the thermal contrast increases. However, because the CO burden is decreasing from winter to spring, the surface maximum will be shifted into spring. I would recommend that the authors look at the seasonal variation of the surface layer degrees of freedom for signal (DFS), as shown in Fig 3 of Worden et al. (2010). That might provide greater insight into what the MOPITT data are showing. We agree that there is a seasonal variation of degrees of freedom for signal (DFS) and it affects CO monthly variations, but also Fig.6 shows negative omega for all regions in April and May which implies better emissions lofting as mentioned in the manuscript. We believe this demonstrates that there is more lofting of the emissions in spring. Additionally the highest MOPITT sensitivity in general is still to CO in the middle of troposphere (as shown by averaging kernel), so when there is better vertical mixing (negative omega), this will help MOPITT to capture the surface emissions.

28. Page 31782, line 28: Remove "cities" after Calgary. Done and it is shown in line 402.

29. Page 31783, line 1: Declining rate on what time scale? Are these annual decreases? The declining rate is over the whole time. This is clarified in the revised manuscript.

30. Page 31783, line 14: Remove "the" between "where" and "central" Done and it is shown in line 415.

31. Page 31783, line 21: "MOPPIT" should be "MOPITT". Done and it is shown in line 421.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 31767, 2014.