

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.

Anonymous Referee #2

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Review of “Spatial and temporal variation of CO over Alberta using measurements from satellite, aircrafts, and ground stations” by Marey et al.

The authors have used MOPITT satellite and MOZAIC/IAGOS aircraft observations to study the variability of CO over Alberta, Canada. The new multispectral MOPITT data, which combines near infrared and thermal infrared measurements, provides greater sensitivity to the vertical distribution of CO throughout the troposphere than any other satellite measurement of CO. I believe that the authors are pushing the limits of what can be done with these data in an air quality context, given the relatively coarse horizontal and vertical resolution of the data. However, these are the best satellite data

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of CO that we have for this type of analysis. I would therefore recommend the paper for publication in ACP, after the authors have addressed my comments and requests for clarification listed below. In particular, there are a number of unsubstantiated statements in the manuscript that require further evidence or should be removed, since I do not believe that they are critical to the discussion.

1. Page 31770, line 18: Change “no researches” to “no studies”
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”
3. Page 31771, line 7: Remove “it can” so that the sentence reads “CO can also be produced. . .”
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.
5. Page 31771, line 12: Should be “sufficient”
6. Page 31774, lines 5-9: What is the range of altitudes for which you have aircraft data?
7. Page 31775, line 13: It should be “especially in central and southern Alberta.”
8. Page 31778, line 11: Should this be “fresh air from the mountains”?
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?
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.
11. Page 31778, line 17: Change “in summer than the springtime” to “in summer than in spring”

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12. Page 31778, line 22: Change “on a clean air area” to “to a clean air area”
13. Page 31778, line 24: Change “transported at the aircraft’s” to “transported to the aircraft’s”
14. Page 31779, line 4: Please see comment #2 above about numbers of four or more digits.
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.
16. Page 31779, line 16: Change “where cities such as” to “whereas cities such as”
17. Page 31779, line 18: Add “the” between “over” and “Edmonton area”
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.
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.
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.
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.
22. Page 31780, line 20: I assume that this is -1%/year?

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23. Page 31781, line 8: “the” should be “The”.
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.
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.
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?
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.
28. Page 31782, line 28: Remove “cities” after Calgary.
29. Page 31783, line 1: Declining rate on what time scale? Are these annual decreases?
30. Page 31783, line 14: Remove “the” between “where” and “central”
31. Page 31783, line 21: “MOPPIT” should be “MOPITT”

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32. Page 31783, lines 21-24: The surface layer retrieval is not an average from the surface to 900 hPa. It is actually an average over a much deeper layer of the atmosphere, as reflected by the averaging kernel. Examination of the averaging kernel should show the vertical sensitivity of the surface layer to CO throughout the lower troposphere (e.g. Fig 7 of Worden et al. (2010)).
33. Page 31784, line 26: Please see comment #2 above about numbers of four or more digits.
34. Page 31785, line 5: change "biomass burning of" to "biomass burning from"
35. Page 31786, line 8: Change "in comparison with" to "compared to"
36. Page 31787, line 7: This study did not "quantify" CO emissions!
37. Page 31787, lines 7-9: "MOPITT-based climatology and inter-annual variations were conducted. . ." This sentence needs to be rewritten.
38. Table 1: Why not restrict the latitudes and longitudes to 2 and 3 significant figures, respectively?
39. Table 2: How many significant figures are needed for the data here?
40. Figure 9b: The horizontal lines make it difficult to read the figure. I would suggest plotting the errors with dashed or thin solid lines, or with error bars on the actual symbols.

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