

Clim. Past Discuss., referee comment RC2  
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## Comment on cp-2021-28

Maria Elena Popa (Referee)

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Referee comment on "Northern Hemisphere atmospheric history of carbon monoxide since preindustrial times reconstructed from multiple Greenland ice cores" by Xavier Faïn et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-28-RC2>, 2021

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Review to:

**Northern Hemisphere atmospheric history of carbon monoxide since preindustrial times reconstructed from multiple Greenland ice cores**, by Faïn et al.

The paper presents a composite record of atmospheric CO based on several icecores from Greenland. The high resolution measurements reveal CO variations that have a too high frequency to have originated in the atmosphere. Production of CO in-situ, in the ice itself is assumed as the main cause for the CO spikes, after other potential explanations are tested and rejected. Considering this, the baseline of the measured CO is used to reconstruct the history of the maximum CO level in the Northern hemisphere over the past 300 years.

This is a very nice paper, showing a thorough, impressive work. The paper is well written and clear. The work described was extensive and complex, and the main results seem trustworthy. I have a few comments below, mainly on text clarifications, and I recommend the paper for publication.

**General comments**

- It is somewhat confusing that a long records are mentioned, but the data are not shown; the paper refers sometimes to longer pieces shown only in the supplement. It would be good to have an overview, somewhere in the beginning of the results: what time period is presented in the paper for CO (and if not the whole record, explain why) and what time periods or ice depth intervals are used for various tests or secondary measurements.

- The main results of this paper are in Sect. 3.5. This comes quite late in a complicated paper, and the reader may have lost some energy by then. Consider moving this section to the beginning of the results, and have the more technical discussion later. (it is also a bit difficult to go back from here to Fig. 1).

- A more detailed discussion would be interesting on the link between the observed CO in ice and the known/ assumed history of atmospheric CO sources. A very short mention is included now in the Conclusion at line 545, but this seems to me insufficient. Are the trends observed consistent with what is known (or believed) about atmospheric CO? In particular, what caused the steady increase starting in 1970s?

- uncertainties: sometimes 1-sigma and sometimes 2-sigma uncertainties are reported. This is somewhat confusing, would it be possible to stick with one of them? Also, please check that they are not mixed when propagating errors.

- If possible provide a link to the data or include the data as a supplement.

### **Specific comments**

Line 32: Reference "Myrhe et al. 2013": the correct spelling is "Myhre"; the reference does not appear in the reference list at the end.

Line 71: CH - 4 typo?

Line 93: Table 1 shows 6 entries, please clarify if two of these refer to the same ice core

Lines 133, 134: spell out DRI and IGE at the first use

Line 176: I did not understand how exactly the external precision was calculated: what is "pooled standard deviation calculated on the differences", is this the standard deviation of the differences? Or the standard deviation of the normal distribution that would result in the measured differences, when sampling randomly two data points? Or something else? Please clarify.

Line 187: How is the link to the WMO-CO X2014A calibration scale established? Is it via the same cylinders described in 2.3? If yes, better to mention them already here, and also send to the more detailed description in SI.

Line 253: I suggest removing the drilling information from here and including it all in Table 1

Line 288: "Median Average Deviation" - should this be "Median Absolute Deviation"?

Lines 288-296: MAD is discussed here, but it is not shown anywhere - consider showing it in the supplement

Line 291: are these really integrated values? then the unit should include a time component. Otherwise remove the word "integrated"

Line 320 and Fig. S12: it actually looks like the minima in the initial data are systematically lower than in the replicate, by 5-10 ppb. Considering that the baseline is the main subject of this paper, it seems relevant.

Line 368 and Figs. 2, 4: since TOC and NH4 are only discussed in 3.3.1, is it possible to let Fig. 2 focused on the resolution comparison between DRI and IGE, and move the PLACE TOC and NH4 data to Fig. 4?

Lines 455-458: the description of the differences between Eurocore discrete data (blue dots) and PLACE baseline does not seem consistent with the figure, please check the years. (e.g. the two datasets seem to diverge around 1830, there is no Eurocore datapoint around 1905, the increase rate in Eurocore baseline is approx. constant starting from ~ 1870, etc)

Lines 518-519: more details may be needed here. What is ACCMIP? Does the "[CO] output" refer to atmospheric CO simulated by a model? What time scales are included here?

Fig. 7: the green and blue point markers are difficult to distinguish in the figure - consider changing the color of the green markers

Table 1: I suggest including the drilling year

Sect. 3.3 title can be misunderstood: this section is not about extracting the CO from icecores, but the atmospheric CO history from the icecore data.