Dear Hubertus,

We are thankful for the constructive comments you provided on the revised version of our manuscript. The two main issues you have raised (about signal smoothing, and about charcoal records interpretation) are fully addressed in the point-to-point response below. We have also considered all the editorial suggestions you made on the manuscript, thanks for such a thorough review. Please note that the page and line numbering included in this response refers to the last version you annotated.

Xavier Faïn on behalf of all co-authors

Point-to-point responses to editor comments

Page 1 line 37 :

Comment from the editor:

Here and throughout the record. Do you really mean ppbv??? Usually standards are provided as molar mixing ratios (ppb), which is only the same as ppbv assuming ideal gases. In case you mean ppb then change this throughout the text.

Authors response:

We have modified the text (including SI) to use ppb instead of ppbv. All figures have been updated to report ppb instead of ppbv.

Page 3 line 83.

Comment from the editor:

The CH4 oxidation may be constant but still makes a sizable contribution to the overall CO level. I would reformulate: "Thus variations in the PI CO budget are expected ..."

Authors response:

The sentence “Thus, the PI CO budget is expected to be driven principally by biomass burning and oxidation of biogenic VOCs (BVOCs), thereby providing an opportunity to use PI atmospheric CO to reconstruct past biomass burning.” has been modified as follows : “Thus variations in the PI CO budget are expected ...”

Page 8 line 252:

Comment from the editor:

I understood what you meant by synthetic data points only after reading the supplement. Please reformulate to make this clearer.

Authors response:

The paragraph has been reworded and additional explanations added.

Page 12 line 393:

Comment from the editor:

The abbreviation SC has not been introduced at this point, please spell it out here

Authors response:

The acronym “SC” has been replaced by “solubility correction”.

Page 15 line 511:
Comment from the editor:
there is something missing in this sentence, Please reformulate.
Authors response:
The sentence “The high resolution datasets represent the CFA output calibrated (WMO-X2014 scale, Sect. 2.6.3. and SI), filtered to remove lab air infiltrations (Sect. 2.6.5), with an integration time of 10 s.” has been reformulated as follows : “The high resolution datasets represent the CFA output calibrated (WMO-X2014 scale, Sect. 2.6.3. and SI), filtered to eliminate lab air infiltrations (Sect. 2.6.5), and integrated over 10 s intervals”.

Page 19 line 578:
Comment from the editor:
I think most of the times you use the British spelling with s, please be consistent throughout the manuscript
Authors response:
We actually tried to use the American spelling: thus we have corrected one occurrence of “regularisation” into “regularization”, and few occurrences of “modelling” into “modeling”. We have also corrected “analysed” into “analyzed”, “analyses” into “analyzes”, etc...

Figure 3:
Comment from the editor:
please explain the inset in the caption
Authors response:
The caption has been modified to better explain the inset.

Page 21 line 627:
Comment from the editor:
not sure about the wording here. Do you mean contiguous or overlapping??? or in line???
Authors response:
“are contiguous” has been replaced by “link”.

Page 22 line 653:
Comment from the editor:
This formulation is misleading. There is definitely a significant smoothing of the atmospheric record by the trapping process taking place. Even for WAIS this has a half-width of 20 years, for TD it is more. Essentially you cannot resolve interannual and likely also not decadal variability using the ice core record. The Mawson record shows clear interannual variability, which is not resolved in the firn or ice record. Please reformulate. See also my comment in the supplement.
Authors response:
We agree that this formulation was misleading. We now explain that we focus specifically on the long term (i.e., multi-centennial variability) variations in atmospheric [CO], as the editor is correct

when mentioning that there is a significant smoothing of the short-term (i.e., decadal variability) atmospheric record by the trapping process taking place.
This section has been reformulated, also considering that the SI section 2.8 was removed (see below).
The main point of that paragraph was to investigate if the difference in the amplitude of the decrease in [CO] during the LIA, between our record and the Wang et al. (2010) record, could be related to differences in firn and gas trapping smoothing. We consider that the comparison between the South Pole (Wang et al., 2010) and ABN (this study) glaciological specifications (e.g., accumulation rate) still included in the manuscript represents a strong argument, and that the methodology originally reported in section 2.8 was not indispensable and could be removed.

Page 22 line 660:
<u>Comment from the editor:</u>
I would reformulate: As the SP/D47 CO record from Wang et al., (2010) is apparently affected by additional, extraneous CO, it is expected that also its isotopic signature is affected by this process and in this case biased towards isotopically enriched values.
<u>Authors response:</u>
The sentence "The SP/D47 CO isotopic ratios published by Wang et al. (2010) could also be impacted by additional, extraneous CO and thus should be interpreted with caution." has been reformulated as follows: "As the SP/D47 CO record from Wang et al., (2010) is apparently affected by additional, extraneous CO, it is expected that also its isotopic signature is affected by this process and thus should be interpreted with caution". We have not commented on the expected isotopic bias (i.e., 'enriched isotopic ratios) as the mechanism involved in the higher CO values of the Wang et al (2010) dataset are not understood.

Page 24 line 726:
<u>Comment from the editor:</u>
This is highly dependent on the assumptions made. Kaplan reconstructs a much larger land area change. Maybe you want to mention that?
Kaplan, J. O., Krumhardt, K. M., Ellis, E. C., Ruddiman, W. F., Lemmen, C., and Klein Goldewijk, K.: Holocene carbon emissions as a result of anthropogenic land cover change, The Holocene, doi:10.1177/0959683610386983, in press, 2011.
<u>Authors response:</u>
We now cite the 2011 study from Kaplan et al., to highlight the large differences in early estimates of anthropogenically induced land cover.

Page 25 line 760:
<u>Comment from the editor:</u>
I reformulated this a bit to make clear that the Antarctic is not sensitive to northern hemisphere sources
<u>Authors response:</u>
The sentence has been reformulated as suggested by the editor.

Page 25 line 770:

Comment from the editor:

I think it is good that you use zonal averages now, but this needs a bit more qualification of the data. The charcoal data comes as z-scores for each individual site and from that, I assume, you calculate an average z-score for the entire area of the zonal average. The z-scores imply that the variance of each individual record is standardized to 1 irrespective of the true amount of biomass burned or the true CO emissions that come with it. Typically the largest amount of biomass burned today comes from the African Savannah every year and those regions should be weighted more heavily than other sources that contribute less to the total emissions in this zonal average. I don't ask you to try this in this publication, but you should mention this problem. In essence charcoal records are better suited to assess the frequency of burning than the emissions that come with it.

Authors response:

We agree with the editor and have completed the text accordingly to warn the reader of the limitations of the comparison. The following text was added to the manuscript: However, the charcoal indexes are based on z-scores for each charcoal record and an average z-score is calculated for the entire zonal area. The z-scores imply that the variance of each individual record is standardized to 1 irrespective of the true amount of biomass burned or the true CO emissions from it. Today, the largest amount of biomass burned comes from the African savannah every year and those regions are also expected to have strongly contributed to CO emissions in the past. In essence, charcoal records are better suited to assess the frequency of fires than the resulting emissions. They therefore provide a good overall estimate of fire dynamics over time, which cannot be directly interpreted as the volume of fire products. With those limitations in mind, we can note that our atmospheric [CO] record exhibits similarities with the tropical charcoal index (Fig. 5c), with stable burning during the MP, and a minimum in burning during the LIA followed by a sharp increase. ".

Page 30 line 1002:

Comment from the editor:

again, is contiguous the right word here?

Authors response:

"is contiguous" was replaced by "link".

SI page 4 line 79:

Comment from the editor:

do you mean 0.1-0.2 ppb over the 8 years?

Authors response:

The sentence "ranging 0.1 - 0.2 ppbv depending on cylinders" was reformulated as follows : "ranging 0.1 - 0.2 ppb per year depending on cylinders

SI page 8 line 135:

Comment from the editor:

I am still not sure exactly what you did. Are these 4.0 ppb (resp. 8.8 ppb) the standard deviations of the differences (residuals) of the two cuts for the same depth bins? Please clarify.

I also wonder in how far the different depth resolution of the bins (10 cm for DRI, 1 cm for IGE) affects this result. Pleae clarify.

Authors response:

We define the external precision as the pooled standard deviations calculated on the differences of CO concentrations from main (*M*) and replicate (*R*) analyzed ice sticks, averaging continuous CO data over few cm long intervals. We then obtain *n* duplicate measurements.

Pooled standard deviation is then calculated as follows : $\sqrt{\frac{1}{2n}\sum\left([CO]_M - [CO]_R\right)^2}$.

The exact formula is now reported in the manuscript (SI Section 1.5).

The intervals were 1-cm long at IGE, and 10-cm long at DRI. We could not apply 10-cm long intervals on the IGE calculation, because the dataset was too limited which would have resulted in a n value too low for statistics.

The DRI depth scale uncertainty has been estimated to be ±1 cm (1σ) (Faïn et al., 2014), and we thus had to consider intervals larger than 1 cm for [CO] external precision calculation. We tried to calculate the DRI [CO] external precision using 1-cm long intervals but the result was not satisfactory. This is notably due to (i) a more complex CFA setup at DRI (compared to IGE) which included longer sampling lines and more pumps, (ii) a faster melting speed showing some temporal variability.

We noticed a typo on SI page 8 line 135, where "4.0 ppb" was replaced by "2.8 ppb" (2.8 ppb is the value re-estimated during the previous review process, and already reported elsewhere in our manuscript).

SI page 19 line 330:

Comment from the editor:

why is this?

Authors response:

The manuscript reads as follows: "In general, realisations of the ice reconstruction that are higher (on average) lead to firn reconstruction solutions that are lower, and vice versa, so that the convolution with the Green's function provides a similar match to the firn observations." The second half of the sentence explains why this is the case.

Comments on SI Section 2.8 (lines 448, 453, 469, 484):

Comment from the editor:

see my comment in the main text above. This formulation is misleading as there certainly is firn and bubble trapping smoothing taking place. Maybe you want to say something else?

Maybe what you want to say is that the longer-term trend displayed in your figures is significant and that variations on these time scales are not significantly damped anymore by the firn and bubble trapping. However, you should also say that the true variability in the atmopshere on higher frequencies is much higher (see for example the Mawson record). Your record is just the remnant variation that survives the damping by firn transport and bubble trapping.

I am not sure where the argument goes here? The signal that you put into the forward model is already the smoothed signal and thus no more significant smoothing is expected if you put it into *the model again, except for DC, where the smoothing filter is stronger than the initial smoothing filter of BKN and ABN*

Again, this argumentation is somewhat circular and does not exclude that true high-frequency CO variability in the atmosphere did exist (see the Mawson record which clearly shows that there is significant interannual variability). The experiment where you put in the spline of all three site into the inverse model, does not allow to recontruct the higher frequency as it has been so effectively filtered out in the spline. Without any remnant higher frequency signal (may it be analytical or atmospheric), your inverse model can not create higher frequency variability in the deconvolved atmospheric record.
The case where you put in the DC record in 4 cm resolution is more suprising as deconvolution of noise usually leads to spurious peaks. Maybe your 4 cm record is so dominated by very high frequency analytical noise (which is not autocorrelated!), that the effect of this noise in the inverse model compensates each other and does not lead to decadal let alone interannual variability
In any case your tests do not exclude that there is higher frequency variability in the atmosphere (Mawson record!) and you should rephrase that or just remiove this discussion, as it does not really affect the conclusions of your paper. In essence you cannot resolve the higher frequency using your record, but that is not really a problem. The long-term variability that you see after 1500 is real and significant (and also seen in the ethane record)

<u>Authors response:</u>
This section was confusing : we investigated the significance of our long-term [CO] trend (e.g., the decrease in [CO] during the LIA), and how the smoothing effect from gas transport in firn and from progressive trapping in ice would affect such a long term trend. But we agree that there is a significant smoothing of the atmospheric record (such as observed at Mawson station) by the trapping process taking place. Our text did not make a clear distinction between these two aspects.
However, we also understand that the methodology used here raises many questions, as pointed out by the editor comments (lines 448, 453, 469 and 484). Improving both this methodology itself and its description would be required.
We decided to follow the editor's advice, i.e. to remove these statements and the related discussion in the main text and the supplement, as it does not affect the conclusions made based on your record. We also made this choice to improve the overall clarity of the manuscript. Specifically, SI section 2.8 was removed, and the manuscript adjusted consequently.