Earth Syst. Sci. Data Discuss.,
https://doi.org/10.5194/essd-2018-146-RC2, 2019

Open Access Earth System Science Data Discussions

Interactive comment on "Revised records of atmospheric trace gases CO_2, CH_4, N_2O and δ^{13}C-O_2 over the last 2000 years from Law Dome, Antarctica" *by* Mauro Rubino et al.

Anonymous Referee #2

Received and published: 25 January 2019

This paper by Rubino et al. compiles revised GHG records (CO2, CH4, N2O, d13CO2) from Law Dome climatic archives. Law Dome is located in East Antarctica and is a unique high accumulation site which allows to study in details the recent atmospheric history. Revising and synthesizing gas data from Law Dome ice cores is thus a really valuable effort. This paper can certainly contribute to enhance the use of such data. For these reasons, the paper should be published.

The paper mostly includes a review of previous interpretations, as a base to identify where future Law Dome studies should focus, and where more lab intercomparison studies should be conducted. These advice for future work are nicely identified. In-

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terpretations reported in the manuscript where published earlier, thus I focus here on minor comments and technical comments (related to Supplement). I note that the paper is well written and was easy to read.

Specific comments :

P2, line 15- 16 : I suggest to rephrase "it is extremely difficult to separate the impacts of anthropogenic increases in CO2 on carbon sinks from the impacts of global warming or increased CO2 concentration on these sinks.". This statement was unclear for me.

P3, line 7: Rhodes et al., 2016 (Climate of the Past) include more CH4 data from Greenland.

P5 sect. 2.1: use the same unit for all accumulation data, for homogeneity.

P6 line 23: typo "I think".

P11 Line 20: I understand that the atmospheric mixing between Northern hemisphere and Southern hemisphere is fast enough so [CH4] would exhibit almost simultaneous trend in both hemispheres. Here the shift in in LIA CH4 decrease seems to be about 40 yrs (Fig. 3). Similar shift seems to exist at the onset of the industrial period CH4 increase. Can we explain such shift with Age Scale uncertainty? Maybe discuss this shift by providing more quantitative estimation.

Figures :

I find the figures difficult to read: I would advise to increase size for labels and titles.

Figure 4 : this figure highlight two important past findings (Rubino et al., 2016 ; Ferreti et al., 2005). It also includes data that can potentially be relevant for further studies and interpretations. I am not sure these data (b, d, and e) need to be plotted; likely description in the manuscript is enough. If the authors want to keep these data as part of plot 4, I would recommend to clarify the figure, e.g. the panel b and c can be shifted, so the figure reports first ice core data, and second complementary climatic data.

Technical comments related to Supplement:

My main concern is about the way uncertainties are calculated by multiplying the blank uncertainty with a factor u.f. . I find this process complex, not fully understandable for data users, and maybe operator-dependent.

- To me, the blank uncertainty is independent from other sources of uncertainties (e.g., dispersion of results observed for replicated measurements on of the same sample). Independent uncertainties when propagated do not multiply each others.

- We can observe here that for a sample where u.f. = 1 (i.e., qf and mq = fair or good), the data uncertainty is reduced to only the blank uncertainty, ignoring for example that different replicated measurements of the same sample will likely not be exactly all the same.

- The u.f. factor includes many parameters (flags, and criteria associated to weights), and some of them are not related to uncertainty. As an example, the first parameter is "melt layer". A melt layer can, e.g., results in high methane concentration (due to in situ production), but the measurement uncertainty of such high concentration should not be different from a regular sample. Just higher concentration will be measured. A melt layer sample does not have the quality required for reconstructing past CH4, but its measurement could be of great quality! Overall, I would advise that the authors identify more clearly what causes uncertainties, instead of considering everything.

- Some of the flags and criteria associated to weights seem subjective, and maybe operator-dependent in their evaluation (at least this is what I feel when reading the Supplement).

- When all criteria reports "reject" (i.e., u.f. = 4), the data is not rejected, but the uncertainty is increased more. It seems to me that these data should be excluded (as suggested by the wording "fatal problem").

- It would be great, if this approach is kept, to provide the full list of flags and weights

Others technical comments:

- What are the typical blanks observed, and typical blank uncertainties observed?

- I am not convinced that CO concentration is a good tool to evaluate the quality of a measurement (similarly to "melt layer", see before), or the quality of a sample. CO can be produced by chemical processes (the authors mention biological production of CO, citation is missing for that), but to my knowledge no collocated productions of CO and, e.g. CO2, CH4 or N2O have been reported so far in ice cores. The processes involved could be different, and a sample compromised for CO can be of good quality for others analyses. Ambient CO is often higher than what is in ice core bubbles, but this is also clear with CH4.

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