

Clim. Past Discuss., referee comment RC1
<https://doi.org/10.5194/cp-2021-113-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on cp-2021-113

Anonymous Referee #1

Referee comment on "Millennial variations in atmospheric CO₂ during the early Holocene (11.7–7.4 ka)" by Jinhwa Shin et al., *Clim. Past Discuss.*,
<https://doi.org/10.5194/cp-2021-113-RC1>, 2021

Review of

Millennial variations of atmospheric CO₂ during the early Holocene (11.7-7.4 ka)

by J. Shin et al.

The authors present a new high-resolution CO₂ record, measured on samples from the Siple Dome ice core, from 11.7 to 9 ka and complement an earlier data set from 9 to 7.4 ka from the same core. This results in a high-resolution CO₂ record covering the beginning of the Holocene. Interpreting the combined data set they identify small millennial-scale variations of a few ppm and correlate them to various paleoclimate records. The authors speculate that solar irradiation may be responsible for the CO₂ variations. The new data, although covering only 2,700 years and thus very short, are important as they close the gap of the early Holocene in the CO₂ data of this ice core.

In the present version the authors do not make a sufficiently convincing case for their hypothesis of an influence of solar fluctuations in causing CO₂ changes. This is due to (i) questionable data processing that results in very small sigma uncertainties, (ii) essentially correlation-based arguments, (iii) a relatively short discussion of mechanisms, (iv) a nearly inexistent critical reflection on leads and lags that are identified in the data, and (v) a missing credible causal chain from solar fluctuations to purported CO₂ variations. Overall, this manuscript requires substantial revisions to reach the maturity of a CP article.

Comments:

1) The interpretation rests on the relatively small CO₂ fluctuations that are visible in the filtered data presented in Fig 1. The authors report 2 sigma uncertainties based on Monte Carlo simulations. 2 sigma uncertainties typically contain more than 95% of the data points based on the assumption of normal distribution. The dashed lines in Fig 1, however, are extremely close to the running mean. How can this be? I would have expected a much wider uncertainty band based on the scatter of the data points. Such a wider band would put serious question marks on the significance and robustness of the small fluctuations (few ppm) that are reported in this paper and that are the basis for the claimed sun-CO₂ relationship. The authors need to critically revisit the determination and depiction of this 2 sigma uncertainty.

2) The authors use a data processing that is not sufficiently explained. They mention a 1-yr interpolation (line 193) and a 250-yr smoothing, followed by a high-pass filtering at 1/1800 yr⁻¹ and a resampling every 10 years. This sounds like very heavy machinery, and I wonder how robust the results are in light of these interventions. In particular, the 1-year interpolation may add some information to the time series that is simply not inferable from the limited resolution of the measurements and their individual uncertainties. I am very sceptical of this statistical treatment of the data. Furthermore, all other paleoclimate data are treated with the same method, and without showing the original data points of these records the authors do not make a convincing case for the significance of such small variations. In short, the data treatment is insufficiently described, and a robustness analysis is missing.

3) Panel B of Fig 1 shows the high-pass filtered signal. Peak-to-peak amplitudes are max 4 ppm with some fluctuations of less than 1 ppm relative to the mean. So far, such small variations in CO₂ measured from ice core samples, have not been interpretable, given the typical measurement uncertainties that are known from the literature. The authors have the burden of making a convincing case that such fluctuations here can indeed be interpreted as variations in atmospheric CO₂ concentrations.

4) Comments 1 to 3 also apply to Fig. 2. For the CO₂ data from EDC (Monnin et al) and WAIS (Marcott et al), the curves are misleading. Inspecting the original data in these papers, I am not convinced that the fluctuations that are shown in the processed data exhibit a robust signal that would represent atmospheric variations. Here a much more careful analysis and statistical assessment (see comment 2) would have to be carried out to see whether such small CO₂ fluctuations can be identified in all three ice cores. It appears on the basis of the presented information that the authors go too far in their interpretation for this relatively short record.

5) Fig 3 suggests evident leads and lags, but they are not discussed and explained in the text. If it turns out that the fluctuations are robust, then these leads and lags need to be considered and discussed in the context of mechanisms. They may be helpful in constraining the causal chain, if such does indeed exist, from solar variations to the CO₂ fluctuations. The authors end their discussion at a correlation analysis among the different paleoclimatic records of Fig 3. Correlation is not causation, and thus the arguments for a solar connection to CO₂ fluctuations is rather weak, if it is active at all.

6) Further to Fig 3, age scale uncertainties between the different records seem to be ignored. These would represent an additional significant uncertainty regarding leads and lags. It is evidently difficult to come up with a common age scale, but the minimum expected would be an assessment the consequences for the conclusions.

7) ENSO is offered as one of the possible mechanisms for CO₂ fluctuations (lines 227ff). The discussion is rather superficial and incomplete. While Feely et al (1999) identify a decrease of CO₂ during the 1991-94 El Nino, Chatterjee et al (2017) provide a more detailed, satellite-based analysis of the effects of the 2015-16 El Nino. After an initial decrease, consistent with Feely et al, they observe a stronger increase in the later stages of this El Nino, with the overall result of a CO₂ increase. The two El Nino episodes are quite different with the former persisting for 3 years, while the later lasts for only one year but is stronger. Therefore, it seems not robust to assign a clear correlation between small CO₂ fluctuations and ENSO.

8) Lines 256ff on a possible role of the AO are speculative and do not add substance to the paper. Either this connection should be explored more in depth or deleted.

<end of review>