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Comment on cp-2021-113

Thomas Bauska (Referee)

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.*,
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Review: Shin, J., Ahn, J., Chowdhry Beeman, J., Lee, H.-G., and Brook, E. J.: Millennial variations of atmospheric CO₂ during the early Holocene (11.7–7.4 ka), *Clim. Past Discuss.*

Summary:

Shin et al., present a novel, high-quality dataset of atmospheric CO₂ during the early Holocene. Using the relatively high-resolution Siple Dome ice core, they provide an atmospheric CO₂ record with the best resolution possible in this underexplored time interval. They identify previously un-resolved millennial-scale variations in atmospheric CO₂. Relying on statistical inferences, they suggest that solar variations may be the primary forcing of the small changes in atmospheric CO₂ (2-6 ppm). Various mechanisms to link the solar variability with atmospheric CO₂ level operating in the EEP, Southern Ocean and the Northern Hemisphere terrestrial biosphere are discussed.

Shin et al., provide an excellent new ice core CO₂ dataset that will be of wide interest to the palaeoclimate and carbon cycle community and should certainly be published. Crucially, the record extends our knowledge about fine-scale changes in CO₂ into the early Holocene. The early Holocene is an important interval to study as it gives us clues about the sensitivity and stability of the carbon cycle during past warm intervals. In the early Holocene, CO₂ and global temperature have reached interglacial levels and NH temperature may have risen to peak Holocene levels (Marcott et al., 2013, albeit this is likely limited to marine margins e.g Marisiek et al., 2018; Kaufmann et al., 2020), the large ice sheets of the NH were still in retreat, and the NH terrestrial biosphere was undergoing a substantial amount of regrowth (Elsig et al., 2009).

Overall, I felt the paper makes a thought-provoking observation that small, high-frequency changes in CO₂ may be correlated to solar variability. However, the causal links explored in the paper are not very convincing. Because the timescales of solar variability are wide-ranging (~decades to millennia) yet highly uncertain, they can easily be misattributed

other processes in the climate system (e.g. internal climate variability) or noise in a proxy system. Thus the bar should be set very high for hypotheses invoking solar forcing based purely on statistical methods.

I also had some questions about how the raw data were transformed into robust time series for analysis. Finally, although the paper is quite brief, the organization could use some improvement as I found myself having to jump around looking for key information.

I look forward to reading a revised manuscript on this exciting new dataset.

Testing the solar hypothesis

In parts of the paper, the authors compare and contrast the variability they observe in the early Holocene to that relatively well-known centennial-scale variability of the pre-Industrial late Holocene. The premise of the paper is in part setup as a test of whether Anthropogenic emissions are the main driver of high-frequency CO₂ variability. But this test is never followed up on. The reader is left with some questions:

- Given the proposed link between solar and CO₂ in the early Holocene, what is the predicted influence of solar variability in the late Holocene (when solar forcing and climate variability are much better constrained)?
- Do the mechanisms proposed agree with the late-Holocene CO₂ data and thus support the author's hypothesis?
- If not, does it none-the-less challenge our understanding CO₂ in the late Holocene? Is there another way to test this hypothesis?
- Is there something different in the carbon cycle boundary conditions that make the comparison between the early and late Holocene difficult?
- Finally, it would worthwhile pointing out that any changes in CO₂ due to solar variability are very small and, even if present, would be swamped by changes in anthropogenic emissions during the Industrial Period.

Error propagation in high-pass filtering and subsequent analysis

It was not clear to me if the analytical precision of the CO₂ data was included in the high-pass filtering and also the lag correlations. This could be crucial in demonstrating the robustness of the results. For example, no uncertainty bands are provided in Figures 2 and 3. I could only find a reference to the uncertainty in the measurements being included in caption for Figure 1. Please provide more detail in the methods section.

Also, the exact the method used to obtain the high-pass filtered time series is not mentioned. Similarly to the above, the cut-off frequency for the CO₂ curve was only mentioned in the figure captions.

Comparison to other CO₂ records

The WAIS Divide data in this interval lies just below the bubble clathrate transition zone. As first discussed in Lüthi et al. 2010 and shown specifically for WAIS Divide in Shackleton et al, 2019, ice core CO₂ data become very noisy in this interval. Although the exact mechanism remains open for debate, this is an important caveat when discussing the agreement or lack thereof with WAIS Divide.

I agree that both records capture the YD-PB jump in CO₂ very nicely. But beyond this, it is difficult to say what variability is common to both records. For example, there is essentially one major oscillation in the WAIS Divide data to compare with in the Holocene (~10.6ka) that appears seems to be heavily biased by one very high CO₂ value.

On the other hand, comparison to the Dome C seems exceptionally difficult given the resolution of the data and the small amplitude. For example, Figure 2 implies that Dome C resolves variations, on the order of ~1 ppm, but the precision of the measurement at the time the data was produced is quoted as 1ppm (Monnin et al., 2001).

As a test, it would be useful to show the correlation between the Siple Dome and all the other CO₂ records to quantify the levels of agreement (alternatively, the differences could be plotted). At the moment, the discussion itself tends to be well balanced, but the conclusions reached seem to me to be too positive about the apparent agreement between the datasets. For example, it is stated quite a few times (using slightly different phrasing) "In conclusion, the existing ice core records support the millennial CO₂ changes in the Siple Dome record although their temporal resolutions are not sufficient." This statement contains a contradiction. If the records are of insufficient resolution how can they be used to test the reliability of Siple Dome? My take-home from that section was the comparisons are inconclusive.

The discussion of the CO₂ offsets relies heavily on the findings in Nehrass-Ahles's PhD thesis. As far as I know this thesis is not publically available. For me, some parts of the discussion seem to assume that the reader could easily verify various conclusions in the thesis.

Mechanistic links between solar forcing and CO₂

At the transition between the results and discussion there is missing logical step that links solar variability to the climate-driven mechanisms hypothesized to be responsible for carbon cycle changes. The authors have just highlighted in the results section that the most interesting finding is the correlation with solar proxies but then immediately jump into a discussion of various climate proxies without mentioning how solar variability could plausibly force these changes. Some of these links can be gleaned from information in the introduction but I would think it would better placed at the onset of the discussion.

I am sceptical that solar forcing could explain all this variability or, moreover, disentangled from all the forms of internal climate variability. I would welcome a clearer presentation of the chain of events/mechanisms that could link solar to CO₂ (e.g. a schematic).

In a future revision, I recommend using the TSI reconstructions. See Roth and Joos, 2013 and references therein for an overview of what I believe is state-of-the-art. Although the reconstructions come with their own host caveats and uncertainties it would be useful to consider if the variability in TSI is plausibly large enough to impact the climate and carbon cycle as the authors suggest.

Line by line comments:

Line 93: "We use a similar method to calculate the significance of this correlation against a random red-noise process. At each of the 1,000 steps, we use an AR(1) model (lag-1 auto regression) to fit the series."

What does "the series" refer to here?

"Then, we calculate the percentage of correlations between the randomized synthetic series that are lower than the correlation coefficients of the real series to assess the significance of the correlation."

Can you relate this to the more traditionally reported p-value? Alternatively, at what values is a test significant?

Section "3.1 The new high-resolution CO₂ record during the early Holocene". Would this section read better if it followed immediately on from the analytical methods section? The flow of the paper is a bit interrupted by the inclusions of the time series methods before the actual data description.

Line 134 "In-situ production of CO₂ cannot be ruled out but the effect should not greatly impact the offset between records from the different ice cores." This conclusion seems a bit premature, as you have not discussed organic production. Also, the remaining section seems to suggest that in-situ production is indeed the likely culprit for the offsets among cores.

Lines 143-153. This section was a little hard to follow as it mostly refers to a figure or set of figures in Nehrbass-Ahles that the reader cannot see. For example, the paper refers to a very large offset between EDC and Law Dome data in this interval which, to my knowledge, has never been published. While it is probably a good idea to reference this thesis as it seems has influenced the discussion, I would suggest keeping the references to specific data and conclusions therein to a minimum.

Line 163 "The nssCa can be produced in ice by the carbonate-acid reaction or transported as a dissolved form." This discussion could use some nuance about what we've actually measured in the ice when we report nssC. Elevated nssCa is not a sure sign you've had production by reaction with carbonate minerals, most the time it's increased dust delivery. What you'd really want to see is some sort of anomalous change in the ratio of soluble Ca (increase) to preserved carbonate minerals (decrease) - which at the moment we can't easily measure. One reasons being is that when we melt the cores, some of these Ca-rich minerals dissolve. The best resources to think through these issues are the original CFA papers by Anklin et al. 1995.

Line 165 "we pay attention to the observation" please consider a different phrasing.

Line "In summary, CO₂ data sets from different ice cores share similar trends in CO₂ change despite offsets in longer term means of a few ppm. These offsets between the Siple dome CO₂ record and others do not impact our conclusions."

If "trends" refers to the gradual decline in CO₂, I feel you have made a convincing case. However, if "trends" refers to millennial-scale changes that this needs more support.

Line 181 "The rapid CO₂ increase at 11.7-11.3 ka might be associated with abrupt warming at the end of the last glacial termination (Marcott et al., 2014; Monnin et al., 2001)". I would argue that this is clear now but please note that the "abrupt warming" is restricted to Greenland and parts of the NH.

Lines ~200. All r-values need supporting significance values (preferably p-value style)

Line 215. The comparison with the ATS seems like tangent as once I looked into the SI it seems the comparison focus on the CO₂ jump at the onset of the Holocene. If ATS is central to the discussion I would suggest showing it in Figure 3.

Line 220. On the topic of the solubility pump, note that ~5 ppm changes in CO₂ would require ~0.5 deg C changes in mean ocean temperature. Is this plausible given the regional changes in SST you show?

Figure 4. It would be helpful to have an arrow indicating which direction shows the proxy leading CO₂ and which directions shows the proxy lagging CO₂.

Sincerely,

Thomas Bauska

References:

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