Dear reviewers,

We thank two reviewers for their careful review of our paper, and their suggestions. We appreciate their useful comments and believe their input has improved the paper. Below, we respond to the comments in blue and the revised texts in the manuscript in green.

All the best, on behalf of all co-authors,
Jinhwa Shin

1) **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 CO_2 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.

The Holocene period (11.7–0 ka) was previously considered as a stable and warm period. Recent studies revealed climate variations on centennial to millennial timescale covering the Holocene thanks to high-resolution proxy records. Bond et al., (2001) first revealed millennial-timescale variations of ice-rafted debris (IRD) in North Atlantic with local minima at ~11.1, 10.3, 9.4, 8.1, 5.9, 4.2, 2.8, 1.4 and 0.4 ka, which is called the Bond cycle (Bond et al., 2001). Likewise, proxy records from Ocean sediments, tree ring and stalagmite in some regions also show similar variations of climate on millennial time scales (Bond et al., 2001; Liu et al., 2020; Marchitto et al., 2010; Nielsen et al., 2004; Reimer et al., 2004; Varma et al., 2011; Vonmoos et al., 2006). These variations of climate proxies resemble solar variations on millennial time scales, implying that millennial variations of climate were thought to be influenced by variation of solar activity (Bond et al., 2001).

Although millennial variations of climate covering the Holocene are observed, there are similarities and differences between the early Holocene and the late Holocene (Liu et al., 2020). During the early Holocene (11.7–7 ka), the solar activities on millennial time scales are prominent. IRD data and solar forcing show a periodicity at 1.0 kyr. These observations indicate that solar forcing (^{14}C) may have an important role in climate change on millennial time scales (Liu et al., 2020).

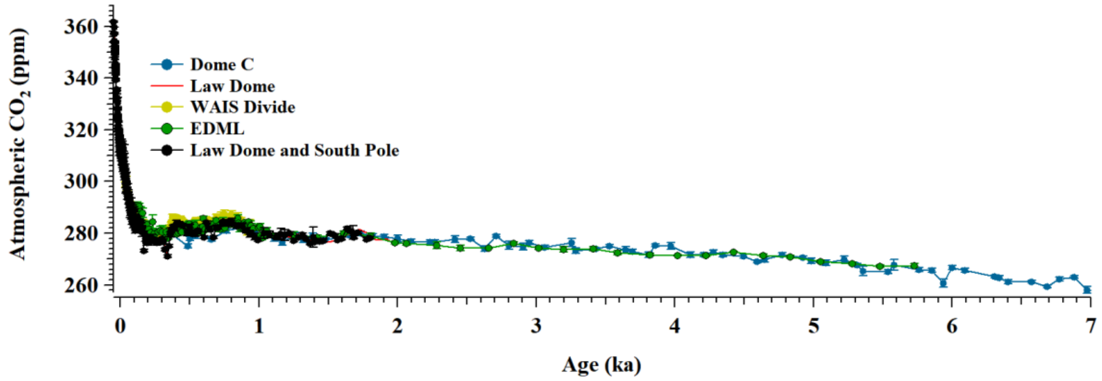

Figure 1. Atmospheric CO_2 from Antarctic ice cores during 7-0 ka. Blue dots: atmospheric CO_2 from Dome C (Monnin et al., 2001, 2004). Red line: atmospheric CO_2 from Law Dome (MacFarling Meure et al., 2006). Yellow dots: atmospheric CO_2 from WAIS Divide (Ahn et al., 2012). Green dots: atmospheric CO_2 from EDML (Monnin et al., 2004; Siegenthaler et al., 2005). Black dots: atmospheric CO_2 from Law Dome and South Pole (Rubino et al., 2013).

During the late Holocene (7–1 ka), the solar forcing on millennial time scales is moderate. The dominant periodicity of IRD and solar forcing proxy is 1.27 kyr and 0.71 kyr respectively. Given these observations, solar forcing during the late Holocene may not play a prominent part in millennial variabilities of climate (Liu et al., 2020). Thus, comparing CO_2 variations during the early Holocene and the late Holocene would be great to understand a link between atmospheric CO_2 and solar forcing. However due to the lack of high-resolution records of atmospheric CO_2 during the late Holocene, observing millennial variations of atmospheric CO_2 was limited (Figure 1).

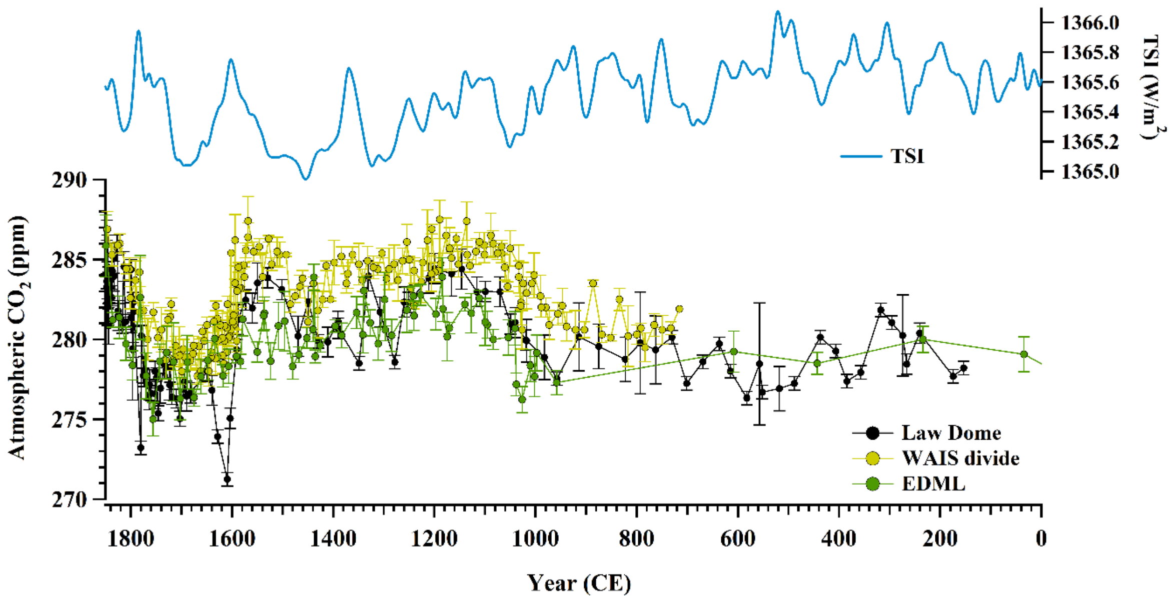

Figure 2. Atmospheric CO_2 from Antarctic ice cores during the last 2,000 years. Blue line: total solar irradiance (TSI) (Roth and Joos, 2013). Yellow dots: atmospheric CO_2 from WAIS Divide ice core (Ahn et al., 2012). Green dots: atmospheric CO_2 from EDML (Monnin et al., 2004; Siegenthaler et al., 2005). Black dots: atmospheric CO_2 from Law dome (Rubino et al., 2019).

Alternatively, we investigate a possible link between atmospheric CO_2 variations and solar activities during the last 1,000 years. Several studies suggested that the variation in total solar irradiance over the 11-year sunspot cycle accounts for less than 0.1% (Lean. 2000), the greater relative variation of solar ultraviolet output caused by sunspot cycle may affect stratospheric ozone concentrations, therefore impacting on the stratospheric and tropospheric circulation patterns (Meehl et al., 2009).

Figure 2 shows Total Solar Irradiance (TSI) and atmospheric CO_2 during the last 2,000 yrs. There are two periods in which sunspots were exceedingly rare. During the Maunder sunspot minimum (1647–1715 CE), TSI was reduced by 0.85 ± 0.16 W m^{-2}. Atmospheric CO_2 records from Antarctic ice cores commonly show a decrease trend during this period (Ahn et al., 2012; Monnin et al., 2004; Siegenthaler et al., 2005; Rubino et al., 2019). During the Spörer Minimum (1450–1550 CE), TSI record during this period also shows a decrease trend. However, atmospheric CO_2 decrease is not significant in Law and EDML records (Monnin et al., 2004; Siegenthaler et al., 2005; Rubino et al., 2019) while WAIS divide ice shows a decrease during this period (Ahn et al., 2012) (Figure 2). Thus, using CO_2 records during the late Holocene, it is limited to understand how atmospheric CO_2 is related with solar activity. To improve our understanding of a possible link between atmospheric CO_2 variations and climate variations caused by solar activity, improving the resolution of atmospheric CO_2 records covering the late Holocene would be helpful.

2) 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.

Figure 3 is revied (Please see below). Figure 2 is removed from the manuscript.

I could only a 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.

This will be written in the revised manuscript in 3.3 Atmospheric CO2 variations on the millennial time scale during the early Holocene:

250-year running means were calculated to eliminate high-frequency variability. Small variations of atmospheric CO_2 are usually smoothed by the gas trapping process in the firn. It is likely that decadal- to centennial-scale variabilities of atmospheric CO_2 record can be attributed to the noise of the atmospheric CO_2 record. Before calculating a 250-year running mean, we made a 1-year interpolation, because sample spacing between data points covering the early Holocene is not constant. Then, to eliminate this long-term drift of CO_2 record, the data was high pass filtered at 1/1800 yr, following previous studies by Bond et al. (2001) and Marchitto et al., (2010).

We evaluated uncertainties of CO_2 record from Siple Dome by using a Monte Carlo simulation. For the simulation, we produced 1000 different sets of CO_2 concentrations which vary randomly with Gaussian propagation in their uncertainties.

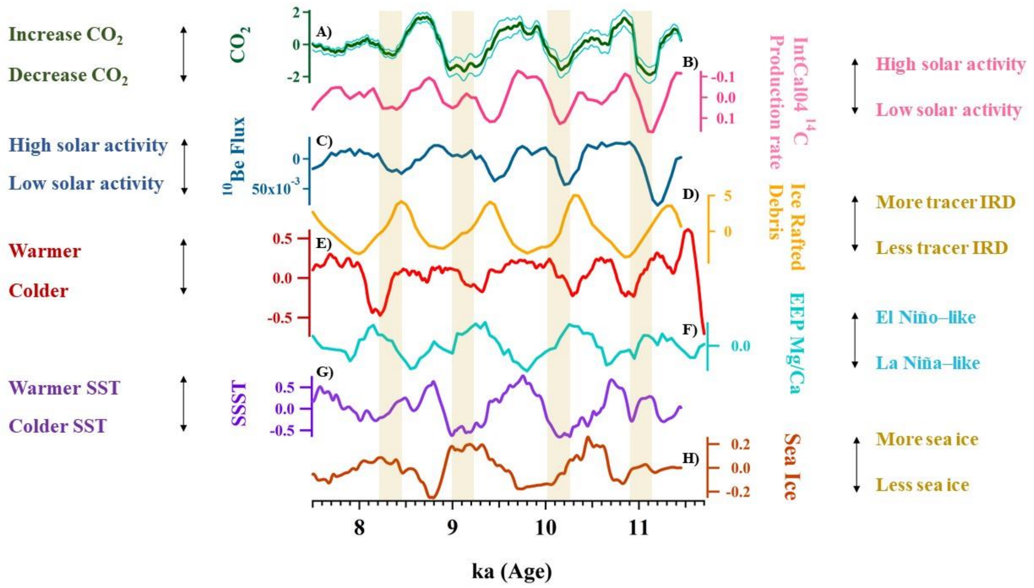

Figure 3 in the revised manuscript. Comparison of atmospheric CO_2 with climatic proxy records over the early Holocene. The records were smoothed at ~250 years and high-pass filtered at 1/1800year^{-1}. A) Atmospheric CO_2 record from Siple Dome (in this study). Dotted lines, 2σ uncertainties calculated from Monte Carlo simulation. B) ^{14}C production rate from IntCal04 $\Delta^{14}C$ data (Marchitto et al., 2010; Reimer et al., 2004). C) ^{10}Be flux record from ice core on the GICC05 timescale (Finkel and Nishiizumi, 1997; Marchitto et al., 2010; Rasmussen et al., 2006; Vonmoos et al., 2006). D). IRD stacked records from the North Atlantic regions on untuned calibrated ^{14}C age model (Bond et al., 2001; Marchitto et al.,2010). E) North Greenland Ice Core Project (NGRIP) ice core isotope ratio on the GICC05 timescale (Rasmussen et al., 2006). F) Sea surface temperature from the eastern equatorial Pacific indicating El Niño–like or La Niña–like conditions (Marchitto et al., 2010). The data was radiocarbon dated by accelerator mass spectrometry (AMS), which was recalibrated by the Marine09 calibration curve (Reimer et al., 2009). G) Sea surface temperature from the Polar Front of the Southern Ocean on the chronology of Mortyn et al. (2003) (Nielsen et al., 2004). H) Sea ice presence from the Polar Front of the Southern Ocean on the chronology of Mortyn et al. (2003) (Nielsen et al., 2004).

3) Comparison to other CO_2 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.

This will be mentioned in the revised manuscript in section 3.2 :

The WAIS Divide CO_2 data was reconstructed from the ice just below the bubble clathrate transition zone (BTCZ). Previous studies raised an issue about the possibility of high frequency noise of atmospheric CO_2 record in the ice just below the bubble clathrate transition zone (Lüthi et al., 2010; Shackleton et al, 2019). This phenomenon might be related to gas fractionation effect at the bubble-clathrate transformation depth zone. Gas content starts to be fractionated in the BCTZ because of the differential permeation of gas when bubbles have transformed to clathrates. CO_2 concentration in the first layer of clathrates is more enriched with higher bubble-to-clathrate permeation rates. Below the BCTZ, gas content slowly homogenizes again through molecular diffusion (Bereiter et al., 2009), which can cause high frequency noise to the ice below the BCTZ. Thus, part of the WAIS Divide CO_2 data may not be sufficient to discuss millennial variabilities of the early Holocene.

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.

To verify the levels of agreement, we calculate the Pearson correlation coefficient between Siple Dome CO_2 and existing CO_2 records (Figure 3). Before calculating the correlation coefficient between Siple Dome CO_2 record and other CO_2 records, all CO_2 records were smoothed by 250-running means to eliminate high frequency noise. The offsets between existing CO_2 records and our data are also calculated (Figure 3).

We observe that CO_2 data sets from Siple dome and Dome C share similar trends in CO_2 variations despite the CO_2 offset in longer term means of 3~8 ppm. The CO_2 record from the Siple Dome is highly correlated with the Dome C record ($r = 0.89$, $p < 0.001$). The CO_2 offset between Dome C record and Siple Dome record decreases continuously from 11.7 ka to 7 ka with small variations at around 9.3 and 8.3 ka (Figure 3B). These small variations of CO_2 offset might be caused by the low sampling resolution and a stronger damping effect on the CO_2 concentration change due to the slower gas trapping process at Dome C (Spahni et al., 2003). The correlation coefficient between Siple Dome CO_2 and WAIS divide CO_2 is 0.7 ($p < 0.001$). However, the CO_2 offset between WAIS Divide and Siple Dome records is quite random (Figure 3B) because of scattering the WAIS Divide CO_2 record during the early Holocene period. Although the existing low resolution CO_2 records are not sufficient for comparison with our Siple Dome record, we cannot exclude the possibility of the millennial variations in the Siple Dome record due to the significant positive correlations.

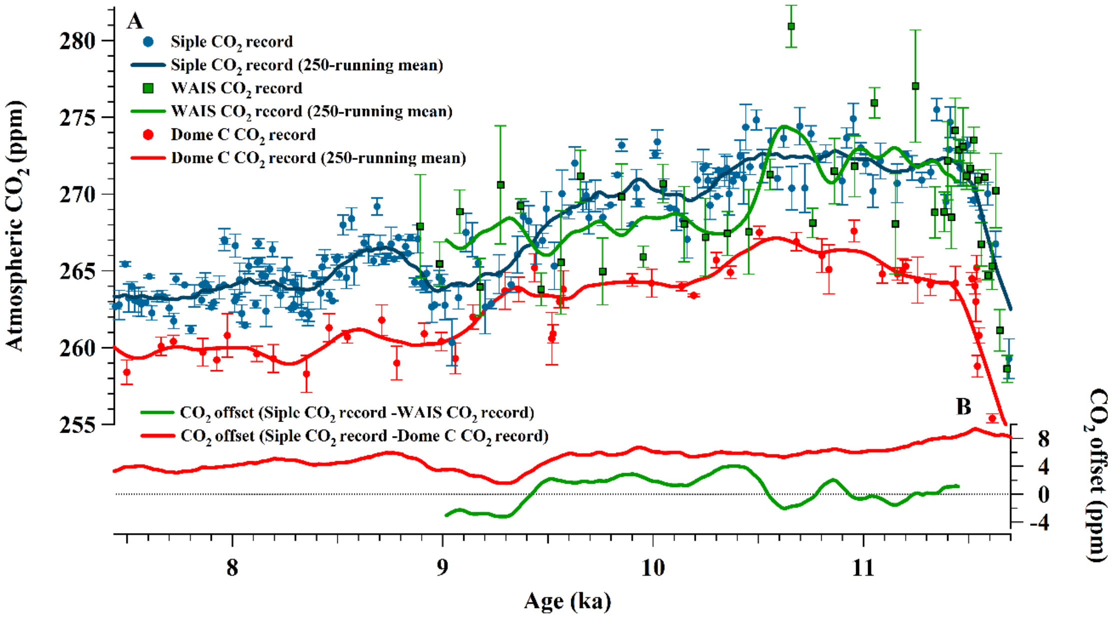

Figure 3. A: Atmospheric CO_2 records. Red dots: Atmospheric CO_2 record from Dome C ice core. Red line**:** 250-running mean of atmospheric CO_2 record from Dome C ice core. Blue dots: Atmospheric CO_2 record from Siple Dome ice core. Blue line**:** 250-running mean of atmospheric CO_2 record from Siple Dome ice core. Green dots: Atmospheric CO_2 record from WAIS Divide ice core. Green line**:** 250-running mean of atmospheric CO_2 record from WAIS Divide ice core. B: CO_2 offset between Siple Dome CO_2 record and other published CO_2 records. Red line: CO_2 offset between Siple Dome CO_2 record and Dome C CO_2 record. Green line: CO_2 offset between Siple Dome CO_2 record and WAIS divide CO_2 record.

The discussion of the CO2 offsets relies heavily on the findings in Nehrbass-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.

We mentioned Nehrbass-Ahles's PhD thesis to mention the largest CO_2 offset between Dome C and Law Dome records during the early Holocene. As the reviewer mentioned, the Law Dome record during the early Holocene is not published yet. Because it is not easy to discuss the CO_2 offset between Dome C and Law Dome records without showing the unpublished Law dome CO_2 record, we will delete the relevant words.

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).

The driver of climate variations during the early Holocene is still unknown. However, many studies suggested periodicity of proxies and solar activities during the Early Holocene (11.5–7 ka) is similar (Liu et al., 2020). This implies that solar forcing (external forcing) might cause climate variabilities on millennial time scales during this period. We also observed maximum correlation coefficients between CO_2 and proxies with CO_2 time lags (Figure 4 and Table 2 in the manuscript). We assumed that high (lower) solar activity cause increased (decreased) atmospheric CO_2.

We found maximum correlation coefficients between CO_2 and proxies with CO_2 time lags. Thus, we assumed that atmospheric CO_2 might be affected by solar activities via climate (Figure 4 and Table 2). How solar variability causes these changes in atmospheric CO_2 concentrations/climate on millennial time scales are still the subject of debate. Changes of solar activities may impact on stratospheric ozone concentrations, which can change stratospheric and tropospheric circulation patterns (Meehl et al., 2009). Higher solar activity enhanced precipitation in the Intertropical Convergence Zone (ITCZ) and South Pacific Convergence Zone (SPCZ) (Meehl et al.,2009; van Loon et al., 2007). The intensified moisture at those areas increased trade wind strength and upwelling in the East Equatorial Pacific region. These conditions lead to Na Niña like climate states on millennial time scale (Marchitto et al.,2010). This ocean condition change in the East Equatorial Pacific has affected the North Atlantic (Darby et al., 2012).

Adde to section 4.1:

We found maximum correlation coefficients between CO_2 and proxies with CO_2 time lags. Thus, we assumed that atmospheric CO_2 might be affected by solar activities via climate (Figure 4 and Table 2). How solar variability causes these changes in atmospheric CO_2 concentrations/climate on millennial time scales are still the subject of debate. Changes of solar activities may impact on stratospheric ozone concentrations, which can change stratospheric and tropospheric circulation patterns (Meehl et al., 2009). Higher solar activity enhanced precipitation in the Intertropical Convergence Zone (ITCZ) and South Pacific Convergence Zone (SPCZ) (Meehl et al.,2009; van Loon et al., 2007). The intensified moisture at those areas increased trade wind strength and upwelling in the East Equatorial Pacific region. These conditions lead to Na Niña like climate states on millennial time scale (Marchitto et al.,2010). This ocean condition change in the East Equatorial Pacific has affected the North Atlantic (Darby et al., 2012).

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.

Thank you for your suggestions. However, TSI record reconstructed by Roth and Joos (2013) covers only 10 ka. In this study, we used ^{14}C records from tree rings and ^{10}Be records from the ice core. ^{14}C and ^{10}Be concentrations are affected by Earth's geomagnetic field change on long-term scales. Marchitto et al. (2010) discussed that

Earth's geomagnetic field change can be filtered by a high-pass filtering. Thus, it is likely that the high pass filtered ^{14}C and ^{10}Be records shown in the manuscript represent variations of solar irradiance.

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?

Revised: data series

"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?

Added

Table 2. Correlation between Siple Dome CO_2 record and climate proxy records. Column A shows correlation coefficients between CO_2 and proxies with CO_2 time lags. Column B shows correlation coefficients between CO_2 and proxies without CO_2 time lag. "With MC" are mean values from the simulations taking age uncertainties into account. "Without MC" is the classic calculation of correlation, without taking age uncertainty into account. Significance of the lag correlations was assessed against 1,000 repetitions of the lag correlation calculation using synthetic data stochastically generated to have the same red noise characteristics as the original series.

Proxy records (Reference)	A: Correlation between CO_2 and proxies with CO_2 time lag (yrs)				B: Correlation between CO_2 and proxies without CO_2 time lag	
	With MC		Without MC		With MC	Without MC
	r (p-value)	Time lag	r (p-value)	Time lag	r (p-value)	r (p-value)
CO_2 - ^{14}C production rate Marchitto et al.(2010); Reimer et al.(2004)	-0.49± 0.12 (0.3192)	-20±148	- 0.76 (0.0003)	50	-0.48 (0.007)	-0.70 (< 0.001)
CO_2 - ^{10}Be flux from Greenland ice core Finkel and Nishiizumi (1997); Marchitto et al. (2010); Vonmoos et al. (2006)	-0.52± 0.08 (0.2847)	110±63	- 0.61 (0.0087)	110	-0.29 (0.05)	-0.32 (< 0.001)
CO_2 - IRD from the North Atlantic region Bond et al. (2001); Marchitto et al. (2010)	-0.49± 0.1 (0.3084)	120±155	- 0.73 (0.0009)	170	-0.33 (0.05)	-0.21 (< 0.001)
CO_2 - SST from eastern equatorial Pacific Marchitto et al. (2010)	-0.40± 0.13 (0.337)	50±219	- 0.61 (0.009)	80	-0.38 (0.04)	-0.55 (< 0.001)
CO_2 - Sea ice in the Southern Ocean Nielsen et al. (2004)	-0.35± 0.17 (0.2899)	190±228	- 0.57 (0.0151)	100	-0.24 (0.17)	-0.48 (< 0.001)
CO_2 - SST in the Southern Ocean Nielsen et al. (2004)	0.35± 0.17 (0.3070)	52±228	0.57 (0.0144)	30	0.35 (0.06)	0.56 (< 0.001)
CO_2 - NGRIP $\delta^{18}O$ Rasmussen et al. (2006)	0.21± 0.07 (0.2684)	-130±63	0.11 (0.3411)	270	0.09 (0.5)	0.06 (0.2)

Section "3.1 The new high-resolution CO2 record during the early Holocene". Would this section read better if it followed immediately on the 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.

The section 2.3 is moved to supplement information.

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.

The sentence is deleted.

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.

Thank you for this, as you mentioned Law dome CO_2 record during the early Holocene is not published yet. As, it is difficult to show the unpublished Law Dome CO_2 record, it would be better to delete this paragraph.

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.

Revised to: Carbonate-acid reactions can be related with the millennial time scale CO_2 variability. There is no direct proxy to examine carbonate-acid reactions in the ice. Concentration of nssCa is mainly involved in dust delivery but it also can be produced partially by the carbonate-acid reaction. We use the concentration of non-sea-salt Ca (nssCa) ion as an indirect indicator of carbonate-acid reactions in the ice. Thus, we examined the concentration of non-sea-salt Ca (nssCa) ion in the Siple Dome and Dome C ice. The nssCa records do not correlate well with the filtered millennial CO_2 variations in both Siple Dome ($r = -0.33$) and Dome C ($r = 0.15$) records during the early Holocene (Figures S4 and Figure S5 in SI). The nssCa trends in Dome C and Siple Dome ice do not agree (Figure S4 and Figure S5 in SI), but millennial CO_2 variations do. Thus, the millennial CO_2 variations are not likely artifacts caused by the carbonate-acid chemical reaction in the Siple ice.

A comparison of CO_2 data reconstructed by a dry and a wet extraction measurements can be helpful to calculate the carbonate content of the ice (Anklin et al. 1995). Additional CO_2 record reconstructed by a wet extraction technique may help to estimate the CO_2 production by the carbonate-acid reaction.

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

Deleted

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.

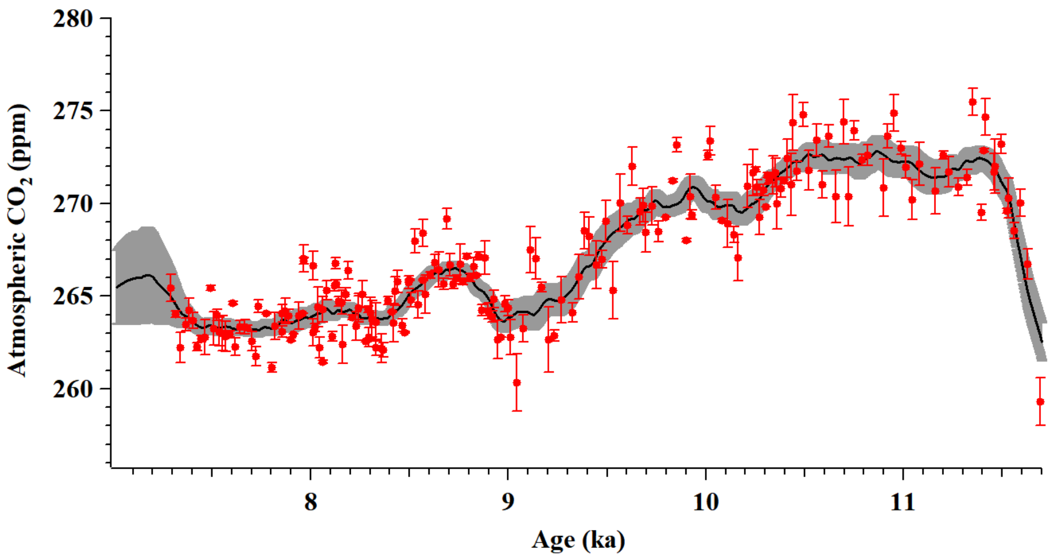

Figure 4. Red circles are Siple Dome ice core records during the early Holocene (11.7-7.4 ka). The black line indicates the average of 1,0000 times modified akima simulations showing an error-weighted average of the CO_2 record. The dark shaded indicates 2σ uncertainties calculated from modified akima simulations.

To assess variability at the millennial time scale, we evaluated 250-year running means and their uncertainties by using a Monte Carlo approach. Random sampling was made from a probability distribution for each measured value and its standard deviation. If a standard deviation was smaller than the mean external reproducibility of the measurement (0.87 ppm of 1σ), we used 0.87 ppm as the uncertainty of a measured value. Then, interpolation and resampling (1 yr) were applied to generate an evenly-spaced time series and to calculate the 250-year running means. We repeated this series of simulations 10,000 times and evaluated the mean of 250-year running means and its uncertainty (shown as 2σ in Figure 1). We used a modified Akima method using the built-in makima function in Matlab for the interpolation and changing the interpolation (or smoothing) method resulted in insignificant differences in the 250-year running means.

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.

Revised:

The rapid CO_2 increase at 11.7-11.3 ka might be associated with abrupt warming in Greenland and parts of the NH at the end of the last glacial termination (Marcott et al., 2014; Monnin et al., 2001)

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

Revised. Please see Table 2 in this document.

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.

This paragraph is deleted.

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?

A local SST record in the South East Atlantic polar front (Nielsen et al., 2004) shows ~1°C changes during the early Holocene on millennial time scales. However, it is hard to quantify the solubility pump with a local record. Mean ocean temperature record is needed to estimate the solubility pump effect in detail.

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.

Revised. We inserted "CO_2 lags" and "CO_2 leads" in the graphs so that the readers may recognize the sign of the CO_2lags.

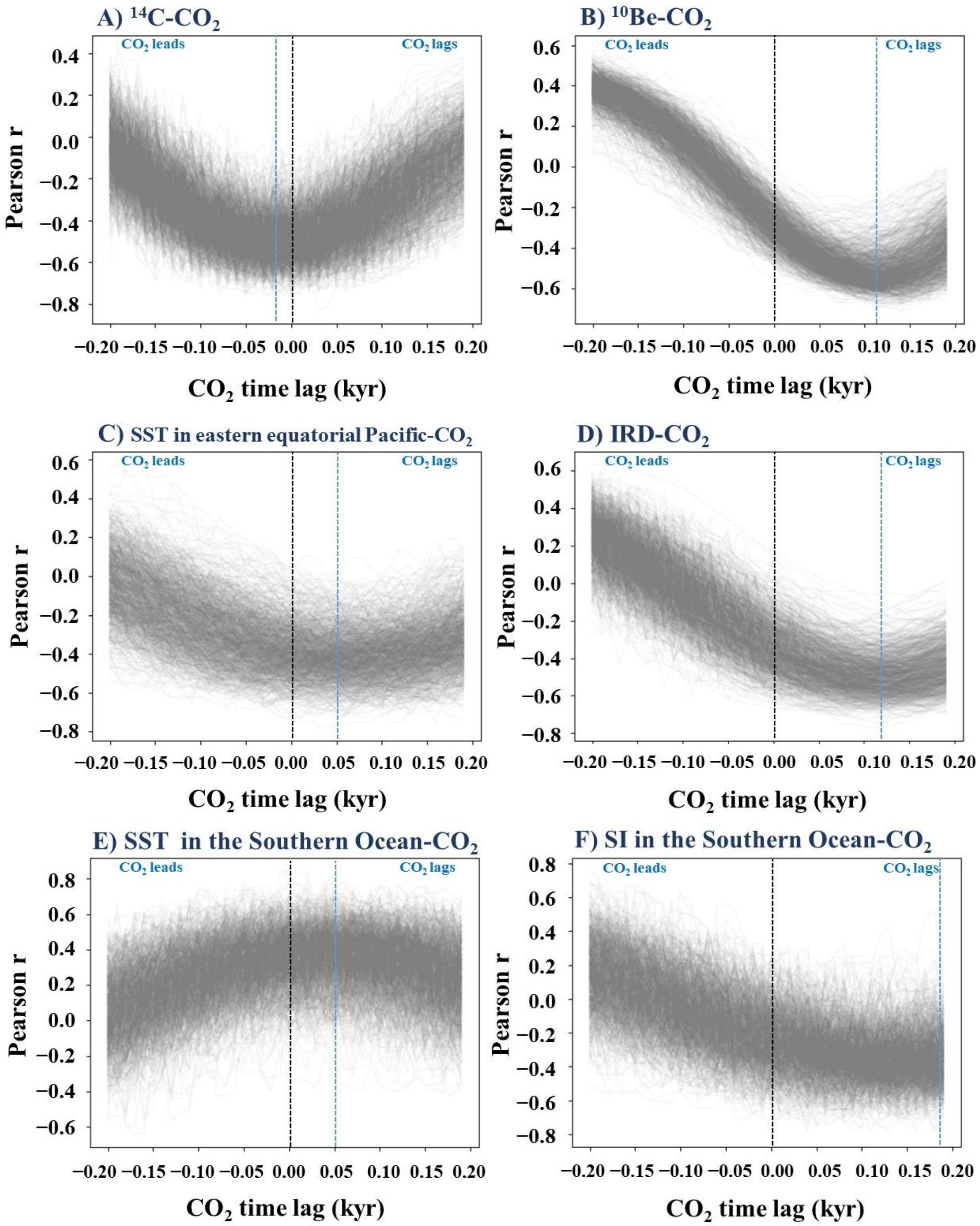

Figure 4 in the manuscript. Correlation coefficients between CO_2 and proxies with CO_2 time lag calculated from Monte Carlo simulation. Vertical lines in black indicate zero time lag. Vertical lines in blue indicate maximum correlation coefficients between CO_2 and proxies with CO_2 time lag. A) ^{14}C production rate and atmospheric CO_2. B) ^{10}Be flux and atmospheric CO_2. C) SST in the eastern equatorial Pacific and atmospheric CO_2. D) IRD from the North Atlantic and atmospheric CO2. E) SST in the East Equatorial Pacific indicating El Niño–like or La Niña–like conditions and atmospheric CO2. F) SI in the East Equatorial Pacific and atmospheric CO_2.

Sincerely,
Thomas Bauska

References:

Martin Anklin, Jean-Marc Barnola, Jakob Schwander, Bernhard Stauffer & Dominique Raynaud(1995) Processes affecting the CO2 concentrations measured in Greenland ice, Tellus B: Chemical and Physical Meteorology, 47:4, 461-470

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