Dear editor and reviewers,

We thank two reviewers and the editor for their careful review of our paper, and their suggestions. Our detailed responses to the comments are shown in blue, and the resulting changes in the manuscript are shown in green.

On behalf of all co-authors,

Jinhwa Shin

Anonymous Referee #1

The authors present revisions to the original manuscript and address comments by two reviewers. I appreciate the detailed response to the comments. Again, the new high-resolution data of the Siple Dome record is a very valuable data set for the period 7.4 to 11.7 kyrBP. This data set should eventually be available to the scientific community. However, the revised version still suffers from the two major issues: (i) statistical treatment of the data to isolate millennial-scale variability, (ii) interpretation of the purported cyclic variations found based on (i).

Many minor points were addressed by the authors in the revised version, but the two major issues are still of concern and are not convincingly addressed. This prevents me from recommending publication.

Major Comments:

1) The uncertainty band of the CO2 data remains unrealistically narrow as Figure 1 evidences. a 2*sigma uncertainty of only 0.87 ppm may result from a statistical analysis but does not withstand common-sense analysis of the original data as depicted in Fig. 1.

The uncertainty value, 0.87 ppm, is obtained by Monte Carlo simulation. Due to a 250-running mean, those small variations were removed, thus, 2 σ uncertainties calculated from Monte Carlo simulations is 0.87 ppm. According to the individual CO_2 data points, the standard error of the mean of replicates from the same depth interval was 0.8 and 0.5 ppm on average for SNU and OSU data, respectively. The range of the standard error is from 0.01 to 1.75 ppm (1σ). To make it clear, a sentence and the caption are added in section 3.1 and on Figure 1 respectively.

A sentence added to section 3.1: ranging from 0.01 to 1.75 ppm.
The caption added on Figure 1: “2 sigma uncertainties of the 250-year mean value, and cannot be used to interpret variations on shorter timescales”.

A 2*sigma uncertainty band should contain about 95% of the values, when assuming Gaussian distribution of the uncertainty. The number of data points outside the grey band in Fig. 1 is clearly exceeding 5% of the total number of data points.

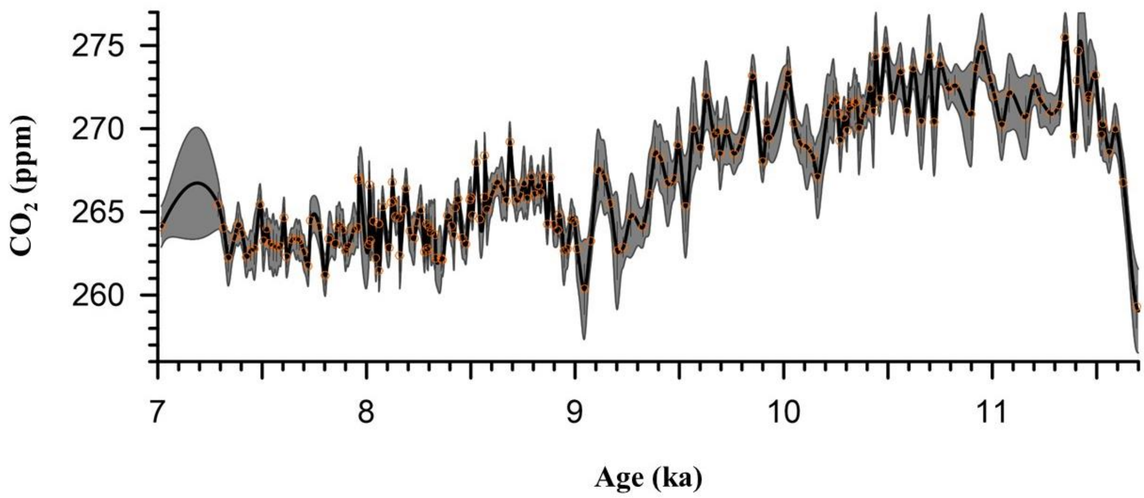

Figure R1. Red dots indicate atmospheric CO_2 records obtained from Siple Dome ice core. Grey band indicates 1σ envelop by using Monte Carlo average

We calculate 1σ envelop by using Monte Carlo average. There are two outliers at 11.08 and 10.83 ka. Thus, we conducted a high-pass filtering at 1/1800 year^{-1} without two single outliers at 11.08 and 10.83 ka. The trend of CO_2 data filtered by high pass filtering without 2 points at 11.8 ka and 10.83 ka is similar to the trend of original data filtered by high pass filtering at 1/1800 year^{-1} (Figure R2). In addition, the magnitude of CO_2 variation at around 11 ka becomes smaller but these two records show the almost same local minima.

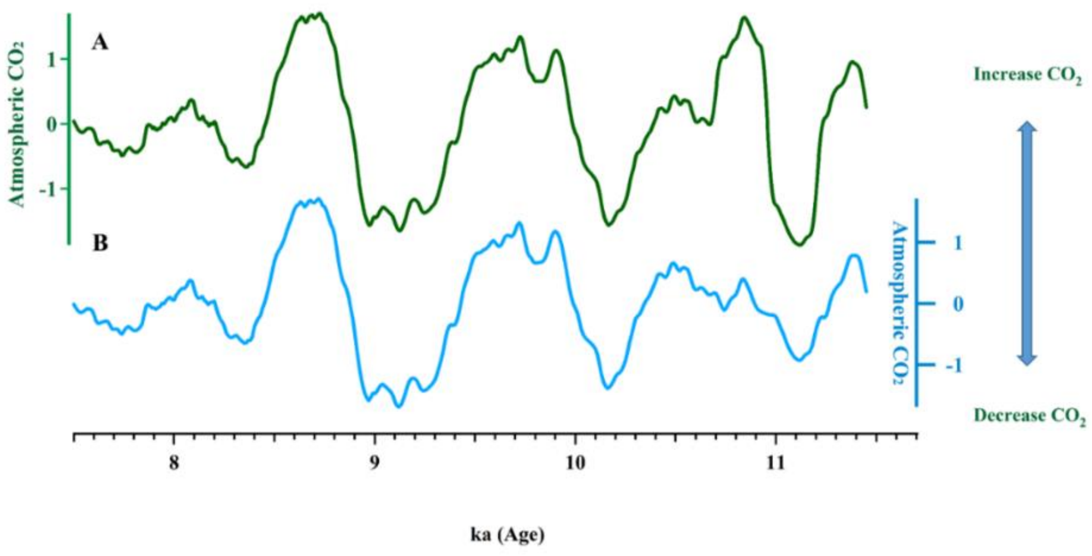

Figure R2. A. Green line indicates CO_2 original data which was filtered by high pass filtering. B. Blue line indicates CO_2 data filtered by high pass filtering without 2 points at 11.8 ka and 10.825 ka.

We also calculate the correlation coefficient between CO_2 without two outliers at 10.8 ka and 11.1 ka and climate proxies (Table R1). These results are almost similar to the results calculated with the original CO_2 record. In my opinion, these two outliers at 11.08 and 10.83 ka may not highly impact our interpretation. Figure R2 and Table R1 were added in the Supplement.

Table R1. Correlation between Siple Dome CO_2 record without single outliers at 10.8 ka and 11.1 ka 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	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)	-0.44±0.10 (0.010)	0±148	-0.76 (<0.001)	40	-0.43 (0.005)	-0.62 (<0.001)
CO_2 - ^{10}Be flux from Greenland ice core Finkel and Nishiizumi (1997); Marchitto et al. (2010); Vonmoos et al. (2006)	-0.30±0.06 (0.101)	130±63	-0.58 (<0.001)	120	-0.30 (0.021)	-0.36 (<0.001)
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CO_2 - NGRIP $\delta^{18}O$ Rasmussen et al. (2006)	0.18±0.06 (0.180)	-140±63	0.20 (0.080)	-110	0.17 (0.180)	0.16 (0.001)

A paragraph added to section 3.3: There are two outliers at ~11.08 and 10.83 ka, which are far from the 250-running mean. The Siple Dome CO_2 record except for the two data points at ~11.08 and 10.83 ka was smoothed and high pass filtered at 1/1800 yr. With this processed data, we calculated correlation coefficients between the filtered CO_2 and climate proxy series again (Figure S6 and Table S3). The correlation coefficients between climate proxies and CO_2 data except for two outliers at ~11.08 and 10.83 ka are similar to those between the original CO_2 record and the climate proxies, showing that the two outliers may not highly impact our interpretation.

2) The authors quote 0.87 ppm as "the uncertainty of a measured value". This cannot be true as many of the data points in Fig. 1 indicate an individual uncertainty of 3 ppm, a much more realistic value of the current analytical approaches to measure CO2 on ice samples. This point is a fundamental one for it determines to which extent variations in the time series can be identified.

The uncertainty value, 0.87 ppm is obtained by Monte Carlo simulation, which means that this value is not individual uncertainty. The main reason of the small uncertainty is attributed to the removal of the high frequency signal by a 250-running mean.

3) The successive filtering – 250-year running mean, then 1/(1800 year high-pass filtering – with a 1-year interpolation in between these steps is still applied. The authors essentially employ a band-pass filter to isolate variability on time scales from 250 to 1800 years. This treatment will necessarily result in some millennial variability even if applied to a white noise time series. I am therefore not convinced that this statistical analysis provides an unbiased view of the CO2 data. Therefore, I remain very skeptical of the treatment of the data.

This statistic is wildly used in paleoclimatology (Marchitto et al., 2010; Schmidt et al., 2012; Yang et al., 2017.). As you concerned, this statistic tool can create artificial variations or delete small variations. However, as mentioned in the previous response letter, all the original records as well as 250-year running means were presented. The original proxy and CO_2 records clearly show the millennial variations. Thus, the treatment may not affect our conclusions. Additional CO_2 concentration using better-quality ice cores and carbon cycle models will be very helpful to confirm the observation.

4) The absence of a mechanistic model, or at least causal chain, to relate solar variability to CO2 changes makes the statements very speculative. Unless a stronger case is built I am seriously doubting the conclusions of this paper as formulated in the last sentence of the abstract.

Abstract revised to:

We present a new high-resolution record of atmospheric CO_2 from the Siple Dome ice core, Antarctica over the early Holocene (11.7–7.4 ka) that quantifies natural CO_2 variability on millennial timescales under interglacial climate conditions. Atmospheric CO_2 decreased by ~10 ppm between 11.3 and 7.3 ka. The decrease was punctuated by local minima at 11.1, 10.1, 9.1 and 8.3 ka with amplitude of 2–4 ppm. Although the linkage between atmospheric CO_2 and the climate change remains uncertain due to insufficient paleoclimate records and model simulations, these variations correlate with proxies for solar forcing and local climate in the South East Atlantic polar front, East Equatorial Pacific and North Atlantic. Additional CO_2 measurements from a higher accumulation site and carbon cycle models are needed.

<end of review>

Referee #2

First and foremost, I profusely apologize for being so late with this review. In hindsight, a part time work schedule meant that I should have managed my time differently with this paper.

Overall, the authors have addressed many the technical concerns of both reviewers regarding the data quality and processing. They have improved the description of the signal smoothing techniques and the question of offsets between CO2 records is now treated more thoroughly. To the question of how accurate the reconstruction is presented by the authors, I am now of the opinion that the only true test of the data quality and data processing techniques will be done through replication studies using more precise methods and perhaps better-quality ice cores. It remains my opinion that the data set itself will be of wide interest to the paleoclimate community and is thus highly deserving of publication.

Regarding the mechanisms proposed in the study, both reviewers questioned the links to solar variability in the initial round. Ultimately, little has changed in the terms of the conclusions of the paper (note the abstract and conclusions remained relatively untouched). This is of course the prerogative of the authors, but I would have appreciated seeing a more in-depth rebuttal in the authors' responses. I personally would have thought the link to TSI would have featured less prominently in lieu of an expanded discussion on the direct carbon cycle mechanisms (terrestrial biosphere, Southern Ocean controls).

I suggested a test using the TSI and the late Holocene data that might allow the authors to test their solar link, which was partially completed. I was hoping that a nuanced discussion of how TSI can be highly variably in the last millennium, but CO2 varies more slowly (possibly related to anthropogenic land-use changes) might temper the conclusions about solar forcing of CO2. In the current manuscript, it seems that the test agreed with their hypothesis in one instance (Maunder) and not in the other considering all the cores and much more muted response in WAIS Divide (Sporer). Other oscillations in TSI were ignored (minima at ~1300 CE and ~1050CE) and there was no discussion of the lead/lag timing. For example, it seems strange of me that the sharpest drop in CO2 of the last millennium (~1600 CE) coincides with a maximum in TSI and not a minimum. With only two tests and a 50% success rate, the results are most likely down to random chance. In my opinion, the paper doesn't really follow through on these results into the discussion and conclusions and they are simply presented as a result. Again, in my opinion, there is a conclusion to be arrived from this, which is that whilst solar forcing might be important in some instances, it is clearly not the major driver of all sub-millennial CO2 variability. If the author's feel the test is inconclusive than it might be a better choice not to include it at all rather than include it but not tie it to any firm conclusions.

We fully agree with your suggestions. Discussing the relationship between atmospheric CO_2 and solar forcing on shorter time scales during the late Holocene would be great to understand the major driver of CO_2 variabilities during the early Holocene. However, it is not easy to compare atmospheric CO_2 during the early Holocene with atmospheric CO_2 during the Late Holocene due to different boundary conditions during the early Holocene and the late Holocene. For example, variations of solar forcing are large on a centennial time scale during the Early Holocene. Thus, the solar output effect might be enhanced since the climate system is not responded linearly (Mohtadi et al., 2016). However, due to a decrease in summer insolation and the small variation of solar forcing during the late Holocene (7–1 ka) (Berger, 1978), solar forcing might play a less important role during the late Holocene.

Atmospheric CO_2 can be controlled by CO_2 exchange between the ocean and the atmosphere, as well as changes of terrestrial carbon stocks. Each reservoir has different response time scales. The deep ocean inventory requires a few millennia to re-equilibrate to climate change (Schmittner and Galbraith, 2008). However, the response of the terrestrial biosphere is usually fast (decadal to centennial timescale) (Bouttes et al., 2012; Menviel et al., 2014; Schmittner and Galbraith, 2008). Thus, atmospheric CO_2 might be affected by changes in solar forcing via the terrestrial processes on the short time scales. However, it is difficult to investigate the relationship between CO_2 and solar forcing (or TSI) during 1900–1000 CE due to anthropogenic causes such as wars and pandemic diseases. Additional studies are needed.

Section 4 revised to: In this study, we observed that atmospheric CO_2 is highly anti-correlated with the ^{14}C production rate and ^{10}Be flux with CO_2 time lag during the early Holocene (Figure 3). However it is the case that large variations of solar forcing at ~11.1, 10.1 and 8.3 ka. The ^{14}C production rate and ^{10}Be flux are correlated with CO_2 at ~9.1 ka on submillennial time scales.

We also check the correlation of CO_2 with solar activity during the last 2000 years on centennial time scales. A positive correlation between solar forcing and atmospheric CO_2 is observed during the Little Ice Age (LIA). There are two periods in which sunspots were exceedingly rare. During the Maunder sunspot minimum (1647–1715 CE), total solar irradiance (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 Dome and EPICA Dronning Maud Land (EDML) records (Monnin et al., 2004; Siegenthaler et al., 2005; Rubino et al., 2019), while WAIS divide ice record shows a decrease during this period (Ahn et al., 2012) (Figure S7 in SI). However, atmospheric CO_2 decrease drastically at ~1600 CE when total solar irradiance (TSI) shows a local maximum, which is similar to the relationship between solar forcing and atmospheric CO_2 at ~9.1 ka. To conclude, it is vague how solar forcing is related with atmospheric CO_2 variations on millennial time scales. Comparing the early and last Holocene requires attention due to different boundary conditions during these two periods and anthropogenic CO_2 during the late Holocene (e.g., Ruddiman, 2003, 2007). Variations of solar forcing are large on a centennial time scale during the Early Holocene. Thus, the solar output effect might be enhanced since the climate system is not responded linearly (Mohtadi et al., 2016). However, due to a decrease in summer insolation and the small variation of solar forcing during the late Holocene (7–1 ka) (Berger, 1978), solar forcing might play a less important role during the late Holocene. Further studies are needed to understand the relationship between atmospheric CO_2 and solar forcing on shorter time scales during the early Holocene with more proxy records and numerical models.

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My recommendation is that paper can now be published in its current form with the understanding that further refinement of the underlying ice core records is needed with more data and that a discussion of the solar links will be borne out in the subsequent literature.

Sincerely,
Thomas Bauska

Editor's comments

Thank you for your revised paper, which I sent to two experts for re-review. Firstly I add my apologies that the process took so long: I was keen to get the opinion of both original reviewers but this took a lot longer than anticipated.

The result of the re-review is rather unsatisfying. Both reviewers want to see your data published as an important addition to the overall CO2 dataset from ice cores. However both of them felt you had not really addressed their primary concerns (1) about how robust the variations you discuss are and (2) about whether you have made a convincing case for a solar forcing of the variations.

I have therefore decided that I will in principle consider a further revision with a view to publishing the data. I have classed it as minor revision because I do not intend to ask for further review: I know what the reviewers are worried about and I will be able to judge whether you have dealt with the issues to an acceptable level. I thus want to make it clear that while this is technically classed as minor revision, I am expecting substantial changes (as outlined below), and the paper could still be rejected after the next revision if I don't see them.

Please consider all the comments made by the reviewers. I will now explain what I see as the main issues that still need to be addressed. This includes some minor issues I have noted myself. Please answer each of my comments as well as those of the reviewers.

1. Data quality. In their para 2, rev 1 makes the point that many of the individual data points show a much larger error bar than the cited analytical uncertainty. Please address this point, as it is clearly not the case that the value at a particular depth is known to within 0.87 ppm.

Sentence added in Section 3.1: ranging from 0.01 to 1.75 ppm

2. I think the issue about the very narrow error envelope shown in Fig 1 and FigS6 is about presentation. This is the envelope of 250-year averages. But because it is shown continuously it gives the impression that even centennial scale wiggles in the data are real, which is not defendible. Please deal with this at minimum by the following:

(a) On Fig 1, the caption please add "2 sigma uncertainties of the 250-year mean value, and cannot be used to interpret variations on shorter timescales" .
Added

(b) I am very concerned that the minima and maxima you later interpret might be strongly influenced by single outliers, eg at 10.8 ka and 11.1 ka. By using 250 year means you are implicitly assuming that the true concentration is rather smooth, and that the existence of data points far from the smooth line is the result of deviations caused by the enclosure process and that these are Gaussian. The existence of these outliers questions that. Please carry out (maybe in supplement) a kind of bootstrap analysis. What I mean is that you should remove outliers (e.g. any data point more than a standard 2 sigma from the line) and show what the smoothed line then looks like. If this removes any of the major deviations you subsequently interpret than this should be stated in the text and should make your interpretation more cautious.

We conducted a high-pass filtering at 1/1800 $year^{-1}$ without two single outliers at 11.08 and 10.825 ka. The trend of CO_2 data filtered by high pass filtering without 2 points at 11.8 ka and 10.825 ka is similar to the trend of original data filtered by high pass filtering at 1/1800 $year^{-1}$ (Figure 1 in this document). In addition, the magnitude of CO_2 variation at around 11 ka becomes smaller but these two records show the almost same local minima.

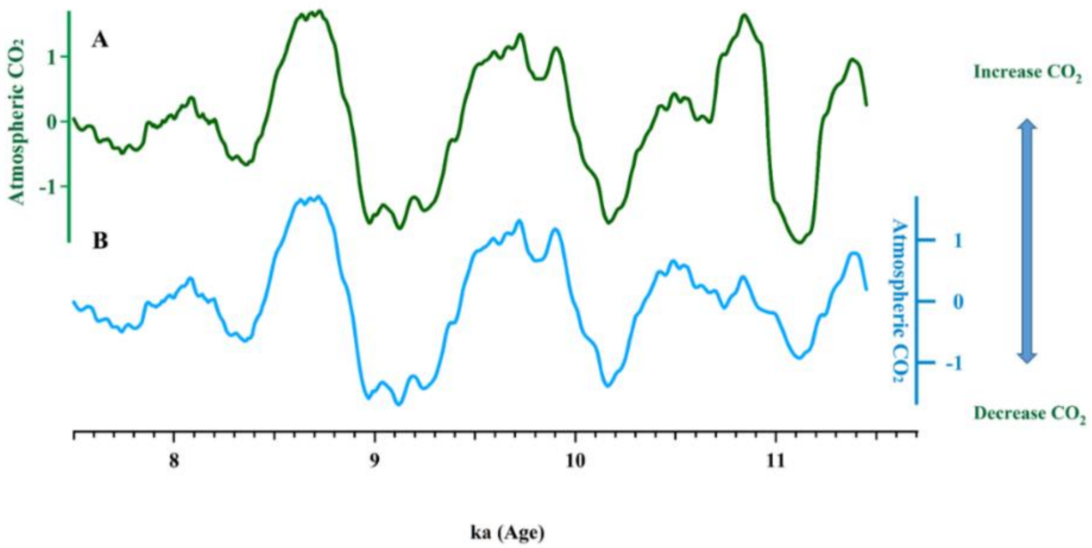

Figure R2. A. Green line indicates CO_2 original data which was filtered by high pass filtering. B. Blue line indicates CO_2 data filtered by high pass filtering without 2 points at 11.8 ka and 10.825 ka.

We also calculated the correlation coefficient between CO_2 without two outliers at 10.8 ka and 11.1 ka and climate proxies (Table R1). These results are almost similar to the results calculated with the original CO_2 record. In my opinion, these two outliers at 10.8 ka and 11.1 ka may not highly impact our interpretation. Figure R1 and Table R1 were added in the Supplement.

Table R1. Correlation between Siple Dome CO_2 record without outliers at 10.8 ka and 11.1 ka 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	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	
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CO_2 - ^{14}C production rate Marchitto et al.(2010); Reimer et al.(2004)	-0.44±0.10 (0.010)	0±148	-0.76 (<0.001)	40	-0.43 (0.005)	-0.62 (<0.001)
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Figure S6 and Table S3 were added in Supplementary Information.

(c) A small technical point. In the text it says you did 10000 MonteCarlo runs, in the caption to fig 1 it says 1000. Please correct.

The caption was revised

3. A second issue with data quality concerns the comparison with EDC and WAIS. There are some technical issues with this, as well as some opportunities missed.

(a) the text in lines 114-117 says "the CO2 offset between Dome C record and Siple Dome record is quite random (Figure 2B) because of scattering in the WAIS Divide". I assume you mean the offset between WAIS Divide and Siple Dome, please correct.

Revised

(b) It makes no sense to calculate correlations that include the major rise out of the YD. Of course all records will show correlations if they include a giant step. Please redo your correlations using only the period to 11.5 ka (the period you use in your filtered record in Fig 1).

Figure 3 shows data from 12 ka to 7 ka. However, to calculate correlations, we only used filtered data from 11.45 ka to 7.45 ka. This explanation will be written in the revised manuscript in Chap 3.3.

A sentence added to section 3.3: To calculate correlation coefficients between records, we selected data from 11.45 ka to 7.45 ka.

(c) before interpreting very small variations in CO2 it is important to show they are robust, ie observed at different sites. As rev 2 says this will really only be tested when we have other data, but you can do more with what you have. You already dismiss the WAIS Divide data but you don't actually let the reader see the crucial comparison even for EDC-SD. Thus the reader cannot judge whether your statement "We observe that CO2 data sets from Siple Dome and Dome C share similar trends in CO2 variations" is correct. So please add a figure (I would propose in the main text (not supplement), maybe as another panel to Fig 2) in which you produce the filtered record (as in Fig 1B) for all 3 sites. When you have done this please discuss seriously how robust your findings are, and exercise an appropriate caution in the rest of the paper depending on the result.

Revised

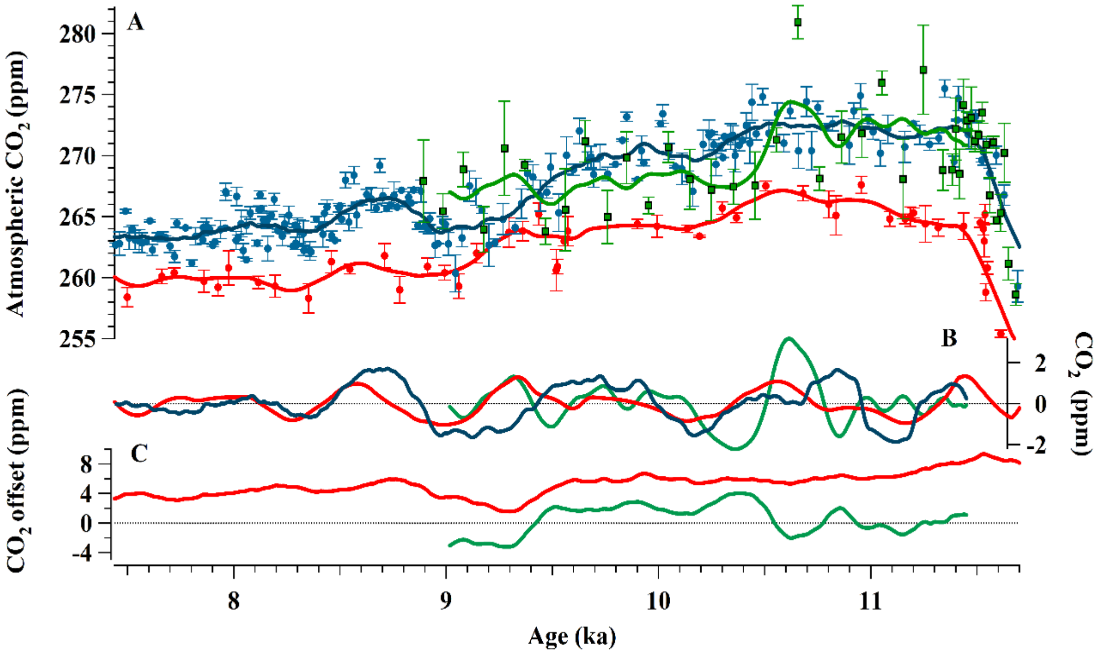

Figure R3. A. Atmospheric CO_2 records. Red dots: Atmospheric CO_2 record from Dome C ice core. Red line: 250-yr running means of atmospheric CO_2 record from Dome C ice core. Blue dots: Atmospheric CO_2 record from Siple Dome ice core. Blue line: 250-yr running means of atmospheric CO_2 record from Siple Dome ice core. Green dots: Atmospheric CO_2 record from WAIS Divide ice core. Green line: 250-yr running means of atmospheric CO_2 record from WAIS Divide ice core. B. Blue line indicates 250-yr running means of the original Siple Dome CO_2 data processed by high-pass filtering at 1/1800 yr^{-1}. Green line indicates 250-yr running means of the original WAIS Divide CO_2 data processed by high-pass filtering at 1/1800 yr^{-1}. Red line indicates 250-yr running means of the original WAIS Divide CO_2 data processed by high-pass filtering at 1/1800 yr^{-1}. C. 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.

(d) Line 164 and line 15 "The Siple Dome CO2 record shows millennial variability of ~2–6 ppm". Looking at Fig 1B, the maximum variation is clearly only 4 ppm, please correct.

Revised

4. The comparison with other records is OK to make (Fig 3) as long as you have caveated about how robust the variations you see are (as per my previous comments). However again please be honest and cautious. While you get reasonable correlations with 14C and 10be, it is nonetheless the case that only 2 of your 3 serious dips have an expression in your solar proxies. At 9.1k, the solar proxies are antiphased with CO2. You should mention this. Taken together with the discussion of later solar variations (around line 250 and discussed by rev 2), these should cause you to caution that the link with solar is very speculative.

Revised

Section 4 revised to : In this study, we observed that atmospheric CO_2 is highly anti-correlated with the ^{14}C production rate and ^{10}Be flux with CO_2 time lag during the early Holocene (Figure 3). However it is the case that large variations of solar forcing at ~11.1, 10.1 and 8.3 ka. The ^{14}C production rate and ^{10}Be flux are correlated with CO_2 at ~9.1 ka on submillennial time scales.

5. Please redraft section 4 and the abstract to be very cautious based on all the above. In particular the sentence "These relationships suggest that weak solar forcing changes might have impacted CO2 by changing CO2 outgassing from the Southern Ocean and the East Equatorial Pacific and terrestrial carbon storage in the Northern Hemisphere over the early Holocene" suggests you have established a mechanism which is not the case. I am OK with you making the case that there is a tentative correlation with solar forcing but in the abstract you should not go further.

Section 4 revised to: Section 4 revised to : In this study, we observed that atmospheric CO_2 is highly anti-correlated with the ^{14}C production rate and ^{10}Be flux with CO_2 time lag during the early Holocene (Figure 3). However it is the case that large variations of solar forcing at ~11.1, 10.1 and 8.3 ka. The ^{14}C production rate and ^{10}Be flux are correlated with CO_2 at ~9.1 ka on submillennial time scales.

We also check the correlation of CO2 with solar activity during the last 2,000 years on centennial time. A positive correlation between solar forcing and atmospheric CO_2 is observed during the Little Ice Age (LIA). There are two periods in which sunspots were exceedingly rare. During the Maunder sunspot minimum (1647–1715 CE), total solar irradiance (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 Dome and EPICA Dronning Maud Land (EDML) records (Monnin et al., 2004; Siegenthaler et al., 2005; Rubino et al., 2019), while WAIS divide ice record shows a decrease during this period (Ahn et al., 2012) (Figure S7 in SI). However, atmospheric CO_2 decrease drastically at ~1600 CE when total solar irradiance (TSI) shows a local maximum, which is similar to the relationship between solar forcing and atmospheric CO_2 at ~9.1 ka. To conclude, it is vague how solar forcing is related with atmospheric CO_2 variations on millennial time scales.

However, comparing the early and last Holocene requires attention due to different boundary conditions during these two periods and anthropogenic CO_2 during the late Holocene (e.g., Ruddiman, 2003, 2007). Variations of solar forcing are large on a centennial time scale during the Early Holocene. Thus, the solar output effect might be enhanced since the climate system is not responded linearly (Mohtadi et al., 2016). However, due to a decrease in summer insolation and the small variation of solar forcing the late Holocene (7–1 ka) (Berger, 1978), solar forcing might play a less important role during the late Holocene. Further studies are needed to understand the relationship between atmospheric CO_2 and solar forcing on shorter time scales during the Early Holocene with more proxy records and numerical models.

Abstract revised to:

We present a new high-resolution record of atmospheric CO_2 from the Siple Dome ice core, Antarctica over the early Holocene (11.7–7.4 ka) that quantifies natural CO_2 variability on millennial timescales under interglacial climate conditions. Atmospheric CO_2 decreased by ~10 ppm between 11.3 and 7.3 ka. The decrease was punctuated by local minima at 11.1, 10.1, 9.1 and 8.3 ka with amplitude of 2–4 ppm. Although the linkage between atmospheric CO_2 and the climate change remains uncertain due to insufficient paleoclimate records and model simulations, these variations correlate with proxies for solar forcing and local climate in the South East Atlantic polar front, East Equatorial Pacific and North Atlantic. Additional CO_2 measurements from a higher accumulation site and carbon cycle models are needed.

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