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.
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" .
   (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.
   (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.
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.
   (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).

(c) before interpreting very small variations in CO_2 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 dismissthe 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 CO_2 data sets from Siple Dome and Dome C share similar trends in CO_2 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.

(d) Line 164 and line 15 "The Siple Dome CO_2 record shows millennial variability of ~2–6 ppm". Looking at Fig 1B, the maximum variation is clearly only 4 ppm, please correct.

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 CO_2. 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.
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 CO_2 by changing CO_2 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.