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Interactive comment on "Sequential changes in ocean circulation and biological export productivity during the last glacial cycle: a model-data study" *by* Cameron M. O'Neill et al.

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CP reviewer comments #1 and author responses

AC: We thank the reviewer for their comments and the opportunity to explore some important issues in greater detail. We feel the review comments make a strong contribution to improving the quality of our work. We have followed up on the reviewer comments in detail and have attempted to address them.

We have made reference to changes to the manuscript, which are included as a supplement to the author comments, in track changes. Page and line references below

refer to locations in the revised document with track changes.

We have addressed the minor comments first, and then provided a longer discussion on the major comments.

Please note that we have changed our treatment of ocean δ13C proxy data, stemming from one of the other reviewer comments, to only include δ13C from Cibicides species of benthic foraminifera. We have also made some small changes to the parameterisation of the volcanic and weathering isotopic signatures in the model, from reviewer comments. These changes required the re-calibration of our model and re-running of the model-data experiments. The model-data results changed modestly. We have updated the figures and text (tracked in the attachment) in the manuscript, accordingly.

Minor comments

RC: "Figure 1: It is not clear, how GOC (red arrows) is split up in the part upwelling in Atlantic and Indo-Pacific Ocean."

AC: In the SCP-M simple carbon cycle box model GOC is split between a part that upwells into the subpolar Southern Ocean, and a part which transports directly into the polar Southern Ocean. This is an attempt to represent the GOC model of Talley (2013). This split is arbitrarily set at 50%. We have added this information to the caption for Figure 1 "GOC upwelling in both basins is set by default to 50% split between upwelling into the subpolar and polar Southern Ocean."

RC: "Figure 1: Does your approach focusing on changes in GOC, AMOC and export production inply, all other processes (fluxes) stay constant in time?"

AC: In the model-data experiments we allow GOC, AMOC and biological export production parameter values to vary, and we solve for them in the optimisation. The experiments include specified forcings of SST, salinity, ocean volume, polar Southern Ocean air-sea gas exchange, coral reef carbonate accumulation and cosmogenic 14C production, guided by proxy observations. Other input parameter values are held constant in the experiments.

RC: Figure 12: x-axis is wrong, eg. MIS5e is between 114-122 ka, while it has been between 118-128 ka in other figures.

AC: Thank you. We have fixed the chart.

RC: With respect to iron fertilisation you might check on Shaffer and Lambert (2018)

AC: Thank you, we have added it to the references throughout the document (P11, L22; P26, L7) and an additional sentence at P26, L8 (note the LaTeXdiff/track changes program struggles to fit reference changes onto the page):

“According to Shaffer and Lambert (2018), fertilisation of the surface ocean, and dust scattering effects on solar radiation, helped to push atmospheric CO2 into and out of their glacial minima, for example at the LGM and last glacial termination.”

RC: The fact that not one single process is needed to explain LGM-Holocene carbon cycle changes is long known, and has been called “the carbon stew” by some authors. You might want to check and discuss in more detail earlier modelling approaches on one glacial cycle (or longer), for example in Ganopolski and Brovkin (2017).

AC: Thanks. We have added to the introduction (P2, L23):

“Several studies have attempted to solve the problem of glacial-interglacial CO2 by modelling either the last glacial-interglacial cycle in its entirety, or multiple glacial-interglacial cycles (e.g. Ganopolski et al., 2010; Menviel et al., 2012; Brovkin et al., 2012; Ganopolski and Brovkin, 2017). These studies highlight the roles of orbitally-forced Northern Hemisphere ice sheets in the onset of the glacial periods, and important feedbacks from ocean circulation, carbonate chemistry and marine biological productivity throughout the glacial cycle (Ganopolski et al., 2010; Brovkin et al., 2012; Ganopolski and Brovkin, 2017). Menviel et al. (2012) modelled a range of physical and biogechemical mechanisms to deliver the full amplitude of atmospheric CO2 variation in the last glacial-interglacial cycle, using transient simulations with the Bern3D model.

According to Brovkin et al. (2012), a $\sim$50 ppm drop in atmospheric CO2 early in the last glacial cycle was caused by cooling sea surface temperatures (SST), increased Northern hemisphere ice sheet cover, and expansion of southern-sourced abyssal waters in place of North Atlantic Deep Water (NADW) formation. Ganopolski and Brovkin (2017) modelled the last four glacial cycles with orbital forcing as the singular driver of carbon cycle feedbacks. They described the "carbon stew", a feedback of combined physical and biogeochemical changes in the carbon cycle, to drive the last four glacial-interglacial cycles of atmospheric CO2.”

We have also added the following to our discussion (P28, L10):

“Ganopolski et al. (2010) and Brovkin et al. (2012) modelled cooling SST and substitution of North Atlantic Deep Water by denser waters of Antarctic origin, in the abyssal ocean, as the main drivers of falling atmospheric CO2 at the last glacial inception. Menviel et al. (2012) modelled a transient slowdown in the rate of overturning circulation in the North Atlantic across MIS 5d-5e.”

RC: Section 5.3. You might want to check on recent finding of terrestrial carbon storage from δ13C in Jeltsch-Thömmes et al. (2019).

AC: Thanks, we have added this to our discussion of the terrestrial biosphere (P33, L24):

Jeltsch-Thommes et al. (2019) estimated a glacial-interglacial change in terrestrial biosphere of 850 Pg C (median estimate; range 450 to 1250 Pg C). Jeltsch-Thommes et al. (2019) demonstrated the importance of including ocean-sediment and weathering fluxes in their modelling estimates, and suggested other studies may underestimate the full deglacial change in the terrestrial biosphere carbon stock.

RC: Figure 14: Changes in CO2 caused by changes in terrestrial NPP and carbon stocks are missing in this figure. Please add.

AC: We have incorporated the contribution of the terrestrial biosphere to the glacial

CO2 drawdown in Figure 14. We have shown the effect of the model run with- and without the terrestrial biosphere to estimate its effects, as per Ganopolski and Brovkin (2017) Figure 9b, and we have compared with their model output.

Major comments

RC: Overall recommendation

My recommendation therefore is, that the model in its present form might be a useful tool for evaluating marine processes, and might be well used together with the available marine data (apart from δ13C), but fails to give meaningfull results for the δ13C cycle. This includes atmospheric and marine δ13C. I urge the authors to get those parts out of the manuscript. If they wish to further analyse the δ13C-cycle I believe fundamental model improvements are necessary, that can not be obtained by a major revision, but by a revised model version. Besides this, shortcomings of the steady-state approach should be discussed in more detail and the unclear (wrong?) aspects of carbonate weathering and annual fluxes in/out of the simulated system (atmosphere/ocean) need to be clarified for each MIS, maybe in a table or a new figure.

AC: We've discussed the comments of the reviewer, and clarified various parts of the modelling referred to by the reviewer, in quite some detail below. We've also made some small adjustments to δ13C parameters for volcanic source carbon and silicate weathering, in the model, and incorporated those in the revised modelling results in the updated manuscript. We've clarified what our model-data results are saying about δ13C in MIS 3-5, and clarified how our model deals with carbonate weathering, with specific reference to the literature and the model code (annotations provided in the Attachments to our responses). We've also discussed features of the terrestrial biosphere in more detail. We've discussed the issues associated with MIS-averaging, revised our wording in the manuscript, and also provided a better description of the model-data results. There is an updated model code to upload with the finalised manuscript.

As a more general comment on δ13C, we demonstrated in O'Neill et al. (2019) that the model we have used, replicates the modern atmosphere and ocean δ13C data time series, and replicates the effects of anthropogenic emissions on ocean and atmospheric δ13C, including matching atmospheric data time series for the last 250 years, and GLODAPv2 data for the present, and matches Holocene data, and successfully matched LGM proxy data.

We argue that the model-data results and manuscript are best left with the δ13C material retained, however with appropriate caveats to describe the shortcomings, as laid out below.

RC: "The chosen approach of steady-state analysis combined with optimization is a way, which certainly has benefits, but also shortcomings. I believe the benefits lie in the possibility to test a great number of parameter values, and this is certainly analysed with great effort and detail and worth publishing (but see my recommendation on shortenings of certain parts below). However, there is little learned on the potential shortcomings and pitfalls, which in my view need to be discussed more deeply. I believe where this approach is falling to short is the following: By analysing only steady-state the authors miss out the opportunities to judge the results based on the timing (when do processes change leading to what results). I provide one example where the article nicely fails, producing a potenially right answer for very likely the wrong reason: One of the dominant features of atmospheric δ13C during the last glacial cycle is a drop by about 0.5‰ during MIS4. The steady-state approach now leads to the evalution of a mean value of atmospheric δ13C which does not really cover this decrease at all, it shows about a decline by about 0.2‰ from MIS5a to MIS4 (Fig 4). So, any explanation for this drop would be falling too short in the observed amplitude by 0.3‰."

AC: This is an interesting debate, and we thank the reviewer for the opportunity to explore this.

The aim of our study is to help diagnose the causes of the major changes in atmospheric CO2 during the last glacial cycle. As we identify in our manuscript, there are three particularly large, sustained falls in atmospheric CO2 between the penultimate interglacial (~125 ka) and the LGM (18-24 ka). These three major changes in atmospheric CO2 are summarised well in the literature, for example, in Kohfeld and Chase (2017). Our aim, with our model-data study, is to understand if plausible changes in ocean circulation (GOC and AMOC) and marine biological productivity can explain the major falls in atmospheric CO2. Other proxy data (e.g. δ13C) provide useful data constraints for a model-data study to help solve this problem. Our approach is to apply a model-data optimisation with a simple carbon cycle box model, to solve for major ocean carbon cycle parameter values during the last glacial-interglacial cycle, and to explain the major, non-transient, falls in atmospheric CO2. To our knowledge, this is the first time that someone has attempted a multiple proxy model-data optimisation, that is optimised against atmospheric CO2, δ13C, Δ14C, ocean δ13C, ocean Δ14C, and carbonate ion proxy data, and hard-constrained by many observational data (SST, salinity, ocean volume, sea-ice cover proxy, coral reef carbonates, atmospheric 14C production rate) for the last glacial-interglacial cycle of 130 kyr. This study is quite different and unique in this regard.

This is done in an average sense across each MIS (nine of them over the last 130 kyr), using average proxy data values for each MIS and solving for the average parameter values at each MIS over the last 130 kyr. The MIS timeframes were chosen as an accessible reference point to the scientific community and because they are also simple reference points for the major atmospheric CO2 declines in the last glacial cycle. In this way, we may not solve for maximum or minimum values in the parameters, “overshoots” and “undershoots”, within each MIS, but the changes in the average values across the last glacial-interglacial cycle. We think the article has been successful in achieving what it set out to do.

The aim of our study is not to disentangle the transient or shorter-term changes in the carbon cycle within MIS stages. Other studies (e.g. Eggleston et al., 2016) have done

that excellently for their area of focus (e.g. MIS 3-4 atmospheric δ13C), and other modelling studies have attacked this using transient simulations (e.g. Ganopolski, 2010; Menviel et al., 2012). Our study does successfully diagnose the timing of changes in major oceanic processes that drive major changes in atmospheric CO2 during the last glacial-interglacial cycle, that are hard-constrained/brutally optimised by a host of data and observations.

With regard to your comments about MIS 3-5 and our manuscript and modelling, below, we address that in more detail to clarify what it is (and isn’t) that our model-data results are telling us, and what we should have said about transient changes in our original manuscript. We clarify things quite substantially, but we are still very happy to explore the shortcomings of the approach and we have amended the manuscript with additional caveats, as explained below.

RC: Note that this δ13C feature is not rapid, it is an anomaly that has been detected from raw data by spline smoothing and is alltogether nearly 20 kyr long, however the decreasing flank falls in MIS4, the increasing flank in MIS3, thus the signal is largely smoothed out in the chosen MIS-centric analysis. The analysis of the results now comes to the conclusion that very likely changes in terrestrial carbon storage was responsible for a change in atmospheric d13Ca of ~0.2‰ (as said explaining a too little amplitude), it is furthermore said that the drop is accompanied by a 30 ppm fall in CO2 (page 12, lines 1-5), citing Hoogakker et al., 2016. I believe this is entirely wrong: The drop in CO2 happens clearly a few kyr before the drop in atmospheric δ13C, as seen in Fig. 4. Furthermore, since both CO2 and δ13C are meassured at the same samples and are both derived from gases in ice cores, this temporal offset between CO2 and atmospheric δ13C can not be explained by chronological issues. The anomalies in biosphere as documented by Hoogakker et al., 2016 all fall in line with the CO2 changes, but not with the δ13C changes, also note that Hoogakker et al., 2016 was published before the atmospheric δ13C data set of Eggleston et al. (2016). In that respect citations from Hoogakker on page 19 are also missing the correct timing: In

Hoogakker NPP drops between around 70 ka (parallel to the drop in CO2), while the δ13C drop occurs 5 ka later. Also note, that in Eggleston et al. (2016) the authors of this atmospheric δ13C record tried to make sense of it by focusing on the part in which δ13C falls, but CO2 rises again (Fig 2 in that paper) focusing on an opposite behaviour than described here.””

AC: There is a major confusion here, that we will spend some time below to help with.

There are two minor comments in our original manuscript about the terrestrial biosphere and atmospheric δ13C at MIS4. They are not conclusions of our work, nor are they a result of our model-data experiments, and thus don’t reflect any obvious or glaring deficiency in our modelling or model-data analysis that we are aware of. The two comments are just peripheral statements we made about the terrestrial biosphere in MIS 4, with a very quick look at the atmospheric δ13C data, without looking in any detail, as this excursion in the δ13C pattern is not the focus of our work. In the two short sentences in our manuscript, we made casual reference of the transient, reversing change in atmospheric δ13C across MIS 4 and MIS 3 (termed an “excursion” by Eggleston et al., 2016). We stated that the transient drop in δ13C probably reflects a weaker terrestrial biosphere, based on reconstruction of the terrestrial biosphere for the same period, by Hoogakker et al. (2016). The first of these comment is in the “Data Analysis” section, and was just the result of a quick eyeballing of the atmospheric δ13C data and another study on the terrestrial biosphere covering approximately the same time period (Hoogakker et al., 2016). You correctly point out our oversimplification of the true complexity of MIS 3-5, in our short statements.

This is simply an oversight on our part, in drafting the text and not joining the dots between the various data sources we have gathered, and our model-data results. If we look at this in a little more detail, with reference to the figures and tables in our manuscript (drawing the reviewer’s attention to them here), we can provide the following (below).

If we look at Figure 2 in the manuscript, we can see that there are dramatic changes in SST (top panel), and less dramatic changes in salinity, sea-ice proxy, sea level/ocean volume, and reef C carbon between MIS 3-5. These data are well incorporated in our model-data experiments as forcings – or constraints, or, another way of saying it - values fed into the model - they hard hard-baked into our model-data results and are influencing the results. With those forcings included, our model-data experiments solve for changes in GOC, AMOC and SO Bio across MIS 3-5, and we find important changes in these parameters across MIS 3-5 (see Figure 8 where GOC, AMOC drop and Southern Ocean biological productivity increases). Therefore, our model-data experiments, and what we should say in our manuscript, is that there are large changes in SST and other observations in the ocean during MIS 4 and MIS 3 (Figure 2 top panel), as well as the changes we estimate for GOC, AMOC and Southern Ocean biological export productivity. It is likely that the combination of these features, led to the δ13C pattern during MIS 3 and 4. We also note that Eggleston et al. (2016) posited changes in SST, ocean biological productivity, AMOC and Southern Ocean upwelling to explain the δ13C “excursion” at MIS 3-4. There are also changes in the terrestrial biosphere, but as per the reviewer comments about timing with regards of changes in the terrestrial biosphere, atmospheric CO2 and δ13C, and on closer inspection we can see that this is perhaps not a dominant driver but some background factor or simply a part of the MIS 3-4 δ13C pattern.

However, it is clear, as you point out, that our MIS-averaging approach does not capture the full extent, the overshoots and undershoots, of the changes in atmospheric δ13C across this period of MIS 3-5.

To address the reviewer comments, we simply reword the sentence in “Data analysis” you refer to (i.e. before the model results section), to better reflect the data we have used and how that data is described (e.g. Eggleston et al., 2016) and also the literature that has focussed in detail on atmospheric δ13C at MIS 3-4.

Original text: “The large drop in δ13C in MIS 4 accompanies a $\sim$30 ppm fall in CO2.

The drop in δ13C is likely caused by a reduction in the terrestrial biosphere, itself driven by the fall in CO2 (Hoogakker et al., 2016)."

We reword this and include the caveat about how our MIS-averaging does not include the full amplitude of changes within MIS 4 and 3, at P15 L23:

"The large drop in δ13C in MIS4, reverses in MIS 3 (Fig. 4(B)). This excursion in the δ13C pattern likely resulted from sequential changes in SST (cooling), AMOC, Southern Ocean upwelling and marine biological productivity (Eggleston et al., 2016). Eggleston et al. (2016) parsed the atmospheric δ13C signal into its component drivers across MIS 3-5, using a stack of proxy indicators, and highlighted the sequence of events between the end of MIS 5 and beginning of MIS 3, and their cumulative effects to deliver the full change in atmospheric δ13C. Our MIS-averaging approach fails to capture the full amplitude of the changes in atmospheric δ13C during MIS 3-5, and only captures the changes in the mean-MIS value, serving to understate the full extent of transient changes in responsible processes. In addition, the MIS-averaging approach misses the sequential timing of changes in processes within each MIS. These are limitations of our steady-state, MIS-averaging approach."

Then on P22 L24, in reference to the terrestrial biosphere:

Original text: "Notably, there is a distinct drop in NPP at MIS 4, a period where atmospheric CO2 falls by ~30 ppm (Fig. 4(A)). Falling NPP and persistent respiration of the terrestrial biosphere carbon stock during MIS 4, which releases δ13C-negative carbon to the atmo- sphere, can explain the steep drop in atmospheric δ13C during the same period (Fig. 4(B))."

We simply delete the reference to the terrestrial biosphere and atmospheric δ13C.

Plus, we have added a caveat to the discussion on limitations of the work that our MIS-averaging approach misses the full amplitude of transient changes (P34, L21):

"Our MIS time-slicing obscures details in the proxy records within MIS. For example, Yu

et al. (2013) observed a transient drop in carbonate ion concentrations in the deep Pacific Ocean during MIS 4, and there are large transient changes in atmospheric δ13C during MIS 3-4. Ganopolski et al. (2010) and Menviel et al. (2012) modelled transient collapses and rebounds in AMOC during MIS 4 (and other short-term changes in atmospheric dust supply and depth of biological nutrient remineralisation), which could have contributed to the full observed magnitude of changes in atmospheric δ13C across this period (e.g. Eggleston et al., 2016) - not captured with our MIS-averaging approach."

However, what we are getting at in our response here, is that although our MIS-averaging strategy misses the full amplitude of transient changes or "excursions" in the proxy record, this doesn't mean that we don't meaningfully capture the data signals across the glacial-interglacial cycle, in our model-data experiments, as data constraints on our model-data experiments. This is a more nuanced but very important point, that we explore in more detail in the following.

A closer look at MIS 3-5 atmospheric CO2 and δ13C and our model-data results.

Our model-data experiments at MIS 3,4 and 5, contain forcings of the model with observationally-derived SST, salinity, sea-ice proxy, sea level/ocean volume, and reef carbonate carbon fluxes. In addition to the model forcings, our MIS-averaged model-data results show a fall in GOC and AMOC, and an increase in Southern Ocean biological export productivity from MIS 5 into MIS 4. This outcome is supported by many proxy observations from the ocean for this time period including ocean carbonate ion proxy, ocean δ13C and dust records for the Southern Ocean and intense cooling in the North Atlantic Ocean (e.g. Oliver et al., 2010; Yu et al., 2016; Kohfeld and Chase, 2017). In addition, our results agree with transient modelling of the last glacial-interglacial cycle, across this period MIS 3-5. Ganopolski et al. (2010), Brovkin et al. (2012) and Menviel et al. (2012) all show a slowdown in AMOC at this time. Ganopolski and Brovkin (2017), in Figure 9(c) in their paper, model a contribution to atmospheric CO2 drawdown from dust iron-fertilisation of Southern Ocean marine biological productivity in MIS 4. Therefore, our model-data conclusions for MIS 4 are consistent with the proxy data and also transient modelling exercises for this period.

Further to the review comments, we undertook a simple reconnaissance modelling experiment to test our MIS-average model-data results, at more detailed time intervals, against the non-MIS averaged data for atmospheric CO2 and δ13C through the MIS 3-5 period, to see if they hold up.

Eggleston et al. (2016) attempted to disentangle transient changes in the atmospheric δ13C pattern during MIS 4 and MIS 3 (Heading 4.2 in their paper "Transient Changes at the Onset and End of MIS 4"). The first process they identify is iron fertilisation from dust over the Southern Ocean and a possible increase in SO biological export productivity (as above, we modelled increased SO biological export in this period too) to lower atmospheric CO2 (but this would increase atmospheric δ13C). Then, Eggleston et al. (2016) mention cooling SST (a key part of our model constraints), where they plot a global average (we model latitude bands), which would also lead to lower atmospheric CO2 as well as lower δ13C (colder ocean fractionates more δ13C). Then, they mention slowing AMOC as a minor cause of lower atmospheric CO2 and higher δ13C. Then, Eggleston et al. (2016) mention the effects of carbonate compensation and ocean alkalinity in lowering atmospheric CO2, and with minor effects on δ13C (captured in our model in MIS average).

Then, Eggleston et al. (2016) mention that Nd isotope and Pa/Th ratios in proxy data support a more pronounced slowdown in AMOC, which lasted until the end of MIS 4 (also in our model-data result for MIS 4, as discussed above and in the manuscript). Eggleston et al. (2016) discuss a weakening and shoaling of AMOC and expansion of AABW at this time, and quote the hypothesised changes to AABW and AMOC of Ferrari et al. (2014). This would have lowered atmospheric CO2 but increased atmospheric δ13C.

Then, Eggleston et al. (2016) mention that iron dust fertilisation may have reduced at the end of MIS 4 (showing the dust proxy data as evidence), leading to a drop in

SO biological productivity, which would increase CO2 and lower atmospheric δ13C, reversing the hypothesised changes in early part of MIS 4 (note our results show SO biological export productivity drops off from MIS 4 levels at MIS 3 – Figure 8 in the manuscript). At this time, SST warmed a small amount in the SO, and cooled in the North Atlantic, with presumably offsetting effects. According to Eggleston et al. (2016), quoting opal flux data, a short-term increase in SO upwelling likely led to the final drop in atmospheric δ13C to reach the trough of the δ13C pattern near the boundary if MIS 3-4 (not captured in our MIS-averaged modelling).

We forced our model with the data in Figure 2 in the manuscript, without averaging for the MIS stages, over 1kyr intervals for the period 47-75 ka. We then took our model-data results for the average parameter values across MIS 3-5 as shown in the manuscript, and profiled them to vary within each MIS according to the hypothesised changes from Eggleston et al. (2016), described above, also at 1 kyr intervals. In this way, we allow the parameters to vary within the MIS stages, but constrained to meet the MIS-average values, in their average, from our model-data experiments as shown in the manuscript.

Figure 1 below (top panel) shows model-data results compared with the proxy data for atmospheric CO2 and Figure 1 (bottom panel) shows the same for atmospheric δ13C (see figures below, at the bottom of this response document). This shows that taking the forcings for SST, salinity, sea-ice cover proxy, sea level/volume and coral reef carbonates (as per Table 2 of the manuscript), and time-profiled average MIS values for MIS 3-4-5 from the model-data experiments (taking the averages from Figure 8 in the manuscript), accounts for the full amplitude of changes in atmospheric CO2 and atmospheric δ13C across this period, the overshoots and undershoots. The time-profiled model-data results shown in Fig. 1 below also account for the MIS-averaged proxy data across MIS 3, MIS 4 and MIS 5. The model results oscillate relative to the δ13C data (Figure 1 below), due to the 1 kyr data slice intervals we have applied (we understand the δ13C data has been smoothed), but it is easy to make this a 1 year/1 second interval exercise and it will produce a smoother set of results, for future analysis (in another body of work).

Completing this analysis shown in Figure 1 below, to match the transient atmospheric CO2 and δ13C data across MIS 3-5, does not change our findings as presented in the manuscript, but actually reinforces them.

Therefore, while the model-data results we present in the manuscript do not describe fully the transient or short-term changes in the carbon cycle within each MIS, they are not inconsistent with the transient data observations – as evidenced by a 1 kyr-interval extension of our model-data results for MIS 3-5 (Figure 1), and comparison with proxy data and other modelling studies. Our model-data results show that, on average, GOC and AMOC weakened in MIS 4, and SO biology on average, was stronger, although these values fluctuated around their mean values within the MIS. We emphasise that these findings above are still peripheral to the main objective of our manuscript (major, sustained changes in atmospheric CO2 through the last glacial-interglacial cycle), although this helps shed some more light on our model-data results in the context of the reviewer comments.

RC: “The second most dramatic change in atmospheric δ13C is a sharp drop by 0.2‰ during Termination I, a time window which has been chosen to be not be included in this steady-state analysis, again missing the opportunity to use 13C to pin down responsible processes..”

AC: We disagree. We argue that the responsible processes for the major and sustained changes in atmospheric CO2 over the last glacial-interglacial cycle (e.g. SST, ocean circulation, biological export productivity, sea level, coral reefs, salinity, terrestrial biosphere) actually show themselves much more clearly, in sequence over the last 130 kyr, than the very short last glacial termination – whereby many processes were interacting in a relatively, very short period of time, and not easily untangled.

Our approach to attempting to solve for large changes in atmospheric CO2, is to study the 100 kyr lead-up to the LGM, where the large changes separate out much more clearly into unique events over 100 kyr. Many studies have attempted to answer the problem of glacial-interglacial CO2 by focussing on the LGM and Holocene periods alone (e.g. Peterson et al., 2014; Menviel et al. 2016; Muglia et al., 2018). Others may try to get at this by looking at 10-18 ka period with transient modelling, where all the changes in the carbon cycle rapidly unwound (e.g. Menviel et al., 2012; Joos et al., 2004, Ganopolski and Brovkin (2017)), but that’s not our paper. That’s almost an entirely different approach to the explicit approach of our paper which was NOT to focus on the transient termination of the last glacial maximum, which has been studied in great detail elsewhere. Our paper is focussed on the major, non-transient, drops in atmospheric CO2 in the lead-up to the last glacial maximum over 100 kyr – a much longer period that nicely shows up the sequential changes in the carbon cycle.

We have added references to the studies mentioned, to point readers in that direction if that is their area of focus, at P8 L7.

“We are interested in the LGM and Holocene as discrete periods, so our experiment time slice for MIS 2 is truncated at 18 ka, and our MIS 1 simply covers the Holocene, removing overlaps with the glacial termination. Therefore, our modelling excludes the last glacial termination (âĹij11-18 ka). The glacial termination period was highly transient, with atmospheric CO2 varying by âĹij85 ppm in <10 kyr, and large changes in carbon isotopes. Thus, it is anticipated that in a model-data reconstruction, model parameters would vary substantially for this period. Our strategy of integrating the model forward to an equilibrium state for each MIS as intervals of discrete climate and CO2, would be unsuitable when applied to the last glacial termination. Joos et al. (2004), Ganopolski et al. (2010), Menviel et al. (2012), Menviel and Joos (2012), Brovkin et al. (2012) and Ganopolski and Brovkin (2017) provide coverage of the termination period with transient simulations of the last glacial-interglacial cycle, using intermediate complexity models (more complex than our model).”

Further, in our discussion of limitations of the study (P34 L27): “We omitted the transient last glacial termination from our analysis, a period in which atmospheric CO2 rose ~85 ppm in 8 kyr. Future model-data optimisation work could probe this period at 1 kyr intervals, or with transient, data-optimised simulations, to profile the unwinding of processes that led to the last glacial cycle CO2 drawdown."

RC: Only the long-term trend in δ13C of +0.2‰ from the penultimate interglacial to the Holocene seemed to be meaningful covered by the approach

AC: The change in δ13C across the last glacial-interglacial cycle is a bit larger than +0.2 per mil, as stated by the reviewer comment. The change in atmospheric δ13C is quoted as +0.4 per mil in the literature (e.g. Schneider et al., 2013; Eggleston et al., 2016) and is a very important feature of the last glacial-interglacial cycle of atmospheric δ13C. Noted our MIS-averaging also understates this full variation, but it is a very important long-term and sustained feature of the last glacial-interglacial cycle, so it is important that our analysis meaningfully captures this feature. As per Eggleston et al. (2016):

"Due to the lack of a complete d13C(atm) record connecting the various data sets, unanswered questions remained. Most importantly, the penultimate glacial maximum (PGM) was found to be 0.4‰ isotopically lighter in d13C(atm) than the Last Glacial Maximum (LGM), and the penultimate warm period (marine isotope stage (MIS) 5e) was also more negative in d13C(atm) by a similar amount. This is a surprisingly large difference, on the order of the changes in d13C(atm) observed during glacial terminations."

While we don't focus on the MIS 3-5 transient δ13C excursion, with better explanation provided above we can demonstrate our approach produces results that are consistent with more detailed interpretations of the transient proxy record, such as Eggleston et al. (2016).

Reviewer comments: the δ13C cycle

AC: We address the reviewer comments individually, and then provide annotated snapshots of the model code as supporting evidence in the Attachment A to our author comments.

RC: "As already seen above the steady-state approach might not be the best way to tackle atmospheric δ13C. Furthermore, for an evaluation of δ13C in general in such steady- state experiments as performed here the fluxes (e.g. as mol C/yr) and δ13C-signatures in/out of the simulated atmosphere/ocean carbon cycle are essential: atmosphere- land carbon fluxes, volcanic CO2 outgassing, weathering, and burial of organic and inorganic carbon in the sediments. Little to non of those fluxes (and δ13C-signatures) are given in the text itself. If I dig into the python source code of the model (or the description of version 1 in O'Neill et al. (2019)) I find a few information, but the source code is difficult to interpret as a non-user and some information seemed to be either misleading or wrong. An examples: Continental weathering consists of two different processes depending on the rock type that is weathered. In carbonate weathering 1 mol of CaCO3 together with 1 mol of CO2 from the atmosphere leads to the entry of 2 mol of HCO−3 into the surface ocean. In silicate weathering 2 mol of atmospheric CO2 are necessary to weather 1 mol of CaSiO3 leading again to the entry of 2 mol of HCO−3 into the surface ocean. For details see, for example Lord et al. (2016). From the description of weathering in O'Neill et al. (2019) I have the impression that the carbonate weathering is not depicted correctly (no consumption of atmospheric CO2). "

AC: Re carbon fluxes/δ13C. We have added the below table (see Figure 2/Table 1 below, at the bottom of our response) to the Supplementary Information to describe the various prescribed fluxes of C and δ13C signatures in our (revised) modelling exercise. This includes some changes from the original model/and model-data runs, from this set of review comments, and also the other reviewer comments. These changes in the model from the revised model-data experiments, will be uploaded with the final manuscript to a new Zenodo link.

Further below, we have clarified our treatment of carbonate and silicate weathering, carbonate weathering δ13C signature, and we have modified our volcanic δ13C and silicate weathering δ13C signatures in the model (incorporated in the revised manuscript model-data results provided in the revised manuscript), in response to the reviewer comments.

Re carbonate and silicate weathering fluxes.

We consulted Lord et al. (2016), as recommended by the reviewer in the RC above, and we note that the approach to carbonate weathering of Lord et al. (2016) is identical to ours, in that the activity of carbonate rock weathering simply transfers fluxes of DIC and Alk (in ratio 1:2) to the ocean via rivers, which causes a sink of CO2 to the ocean, and their treatment of silicate weathering is very similar to ours (see below, where we looked into more detail in the rock weathering model of Lord et al. (2016), which is described in detail in Colbourn et al. (2013)).

For example, in Lord et al. (2016):

“In all schemes, the terrestrial rock-weathering module calculates global fluxes of ALK and DIC from carbonate and silicate rock weathering and routes them to the coastal ocean”. And importantly, as described in in Colbourn et al. (2013), the carbonate weathering model used in Lord et al. (2016):

“Note that there is only one mole of DIC for each mole of Ca2+; this is a short-circuiting of the atmosphere based on the assumption that the atmosphere and surface ocean are well mixed on the timescales considered here. Instead of removing one mole of CO2 from the atmosphere – and by implication the ocean – and adding two moles of bicarbonate to the ocean nothing is taken from the atmosphere and one mole of bicarbonate is added to the ocean.” In addition to Lord et al. (2016), we also found our approach for carbonate and silicate weathering to be identical to a range of other studies – they are shown and referenced below. We also found our approach to δ13C in carbonate weathering, as shown by the model code as shown in the Attachment to these comments below (with line-by-line annotation) was identical to that used in Sano

and Williams (1996) and Mook (1986), the references suggested by the reviewer for us to consult (see RC below).

As pointed out by the reviewer, some confusion for the reader about carbonate and silicate weathering, is perhaps contributed from our simple, high level model description paper which glosses over some details (O’Neill et al., 2019) and perhaps non-user friendliness of the model code. We will add better descriptive text in our model code for the final model upload to this manuscript upon finalisation. We’ve provided line-by-line references to our model code in the Attachment to these responses, to help understanding.

Further on the treatment of carbonate and silicate weathering in SCP-M.

The treatment of carbonate and silicate weathering in SCP-M is described in O’Neill et al. (2019) and mainly takes into account Walker and Kasting (1992), Toggweiler (2008) and Zeebe (2012) for its basis. Walker and Kasting (1992) provides the theoretical basis for treatment of carbonate and silicate rock weathering/river fluxes in many carbon cycle models (e.g. Zeebe, 2012; Colbourn et al., 2013; Lord et al., 2016). For example, Zeebe (2012) applies to the LOSCAR carbon cycle model a simple, parameterised weathering scheme based on Walker and Kasting (1992) and the same scheme was applied in simple carbon cycle feedback modelling applied by Toggweiler (2008) and Hogg (2008). An almost identical approach, was also applied by Lenton and Britton (2006), and Colbourn et al. (2013) and Lord et al. (2016). The only difference with Lenton and Britton (2006) and Colbourn et al. (2013) from our simple model, is that they applied additional temperature and terrestrial biosphere dependencies for rock weathering.

In summary, continental silicate and carbonate rock weathering are both represented in the SCP-M model. Both supply alkalinity and carbon to the surface ocean in ratio 2:1 (e.g. more alkalinity than DIC).

The weathering equation used in the model, is as per the model documentation (O’Neill et al., 2019), and the original model code at (https://doi.org/10.5281/zenodo.1310161), and is reproduced here:

dC/dt_weath = (WSC + (WSV + WCV)AtCO2)

where WSC is a constant silicate weathering term set at 0.75x10−4 mol m−3 year−1, WSV is a variable rate of silicate weathering per unit of atmosphere CO2 (ppm), set to 0.5 mol m−3 atm−1 CO2 year−1 and WCV is the variable rate of carbonate weathering with respect to atmosphere CO2, set at 1.5-2.0 mol m−3 atm−1 CO2 year−1 (Toggweiler, 2008).

There is a slight difference between carbonate weathering versus silicate weathering, in our model, in terms of the direct consumption of CO2 from the atmosphere when weathering takes place. This direct consumption of CO2 is assumed to be fully reversed in the case of carbonate weathering, but is only partially reversed in the case of silicate weathering. The main CO2 sink activity of the carbonate weathering, is therefore is in the alkalinity fluxes to the ocean and its effects on relative pCO2 in the ocean versus the atmosphere (e.g. Colbourn et al., 2013).

Carbonate weathering

Weathering of carbonate rocks initially takes up CO2 from the atmosphere (one mol), and supplies calcium and bicarbonate ions to the ocean (an additional mol of carbon), as per the following equation:

CaCO3+H2O+CO2= Ca2++2HCO−3

Therefore, two moles of carbon and one mole of calcium enter the ocean for each mole of CaCO3 weathered. This raises ocean carbon and alkalinity by two units each. In steady state, subsequent precipitation of CaCO3 releases the same amount of CO2 back to the atmosphere that was consumed by weathering (Zeebe, 2012) – a short-term circular loop that leads to a net zero direct consumption of CO2 from the atmosphere from carbonate weathering (e.g. Colbourn et al., 2013; Lord et al., 2016). This

is described in detail in Zeebe (2012) - see Fig. 3 extract of the Zeebe (2012) schematic description of carbonate weathering.

This return of CO2 to the atmosphere (one mol of carbon, as per Fig. 3 below from Zeebe (2012)) leaves a net addition to the ocean of carbon and alkalinity from carbonate weathering in 1:2 ratio (Zeebe, 2012). The ocean carbon and alkalinity balance is later restored due to subsequent burial and CaCO3 and carbonate compensation (Zeebe, 2012).

According to Zeebe (2012):

“As a result, although the addition of Ca2+ and 2 HCO 3 increases ocean TCO2 : TA in a 2:2 ratio, on a net basis CaCO3 weathering increases ocean TCO2 : TA in a 1:2 ratio because one mole of CO2 returns to the atmosphere. If influx equals burial, carbonate weathering thus represents a zero net balance for atmospheric CO2.”

For our steady state modelling, we assume the CO2 consumed directly by the carbonate weathering process is returned to the atmosphere – a net zero of direct consumption of CO2 from the atmosphere. This is a short-circuiting of the process, but not incorrect (refer Colbourn et al. (2013) quote reproduced above, about “short-circuiting” direct atmospheric CO2 effect of carbonate weathering). Therefore, the fluxes associated with carbonate weathering are those of DIC and alkalinity into the surface ocean boxes of the model. This is the same approach applied by Toggweiler (2008), and Lenton and Britton (2006), and identical to the approach of Lord et al. (2016), and Colbourn et al. (2013). For these studies, the sink of atmospheric CO2 from carbonate weathering comes indirectly through the effects of alkalinity supplied to the surface ocean which lowers pCO2 and draw CO2 into the ocean. Some interesting quotes from those references below, with bolded parts for emphasis.

The approach for carbonate weathering in Lord et al. (2016) (the reference suggested by the reviewer), is referenced in that study to Colbourn et al. (2013), and is described in Colbourn et al. (2013) as:

“Note that there is only one mole of DIC for each mole of Ca2+; this is a short-circuiting of the atmosphere based on the assumption that the atmosphere and surface ocean are well mixed on the timescales considered here. Instead of removing one mol of CO2 from the atmosphere – and by implication the ocean – and adding two moles of bicarbonate to the ocean (as in Eq. 1), nothing is taken from the atmosphere and one mole of bicarbonate is added to the ocean.”

And “The fluxes are then used to calculate fluxes of DIC (FDIC) and Alkalinity (FAlk)”.

We note further from Colbourn et al. (2013):

“In the case of carbonate weathering there is an overall null cycle for CO2, whereas silicate weathering transfers CO2 to the Earth’s crust.”

In summary, our simple box modelling representation of carbonate weathering is consistent with the theory of carbonate chemistry, and the literature on modelling of carbonate weathering. Our calculated estimate of 10 Tmol C yr-1 from carbonate weathering supplied to the ocean at 275 ppm atmospheric CO2, is comparable to that of 12 Tmol mol C yr-1 in Morse and Mackenzie (1990) and Zeebe (2012), Archer et al. (1998), but higher than that assumed by Colbourn et al. (2013) and Lord et al. (2016) of 5 Tmol C yr-1. In those latter two studies, they simply assume an even, equal split of the fluxes of silicate and carbonate weathering in their model spin-up Fsil=Fcarb=5 Tmol C yr-1. However, Colbourn et al. (2013) quote post-spin-up, pre-industrial total flux of weathering of 12-20 mol C C yr-1, split equally between carbonate and silicate weathering (6-10 mol C yr-1 each).

We have supplied an annotated snapshot of our model code, in the Attachment A to these comments (below).

Silicate weathering

Silicate rock weathering can be described by the following chemical equation:

CaSiO3 + H2O + 2 CO2 = Ca2+ + 2HCO−3 +SiO2

Silicate rock weathering removes 2 mols of CO2 from the atmosphere for each mole of CaSiO3 weathered. The subsequent precipitation of CaCO3 in the ocean releases one mole of CO2 back to the atmosphere, with the other mole of CO2 consumed by the atmosphere, taken up in CaCO3, which may end up buried in the marine sediments (Zeebe, 2012). In steady state, over long timeframes, the silicate weathering direct consumption of atmospheric CO2 balances out volcanic emissions of CO2 (Berner et al., 1983; Zeebe and Caldeira, 2008, Zeebe, 2012). Because the steady state in the silicate weathering is achieved over a much longer timeframe (1e5-1e6 years), it is appropriate to model a direct sink of CO2 from the atmosphere associated with silicate weathering. The steady state atmosphere-ocean response to carbonate weathering only requires a relatively short timeframe, hence we can model the steady state assumption of carbonate weathering returning its direct consumption of CO2 to the atmosphere (Walker and Kasting, 1992; Lenton and Britton, 2006; Toggweiler, 2008, Zeebe, 2012).

Therefore, relative to carbonate weathering, there is an additional step applied with silicate weathering. To account for the unit of CO2 consumed directly from the atmosphere in silicate weathering that is not returned (one more unit than carbonate weathering, as per Zeebe, 2012), and using the approach of Toggweiler (2008), we also subtract an amount equal to a unit of silicate weathering directly from the atmosphere. This is the same approach of Zeebe (2012) who applies a doubling of the molar flux of silicate weathering (to replicate two mols of CO2 initially drawn from the atmosphere), and that of Toggweiler who subtracts a flux of CO2 directly from the atmosphere (but no direct consumption of CO2 in the case of carbonate weathering) to account for the additional unit of CO2 consumed by silicate weathering (when compared with carbonate weathering). This flux is subtracted directly from Atmospheric CO2 in SCP-M as referenced in the model equation above (and described in the code in Attachment A to these comments). This flux, subtracted from the atmosphere, negates the effects on atmospheric CO2 of the units of C added to the ocean by the silicate weathering flux of C. Volcanic CO2 emissions are set equal to the amount of CO2 taken directly from the atmosphere by silicate weathering, to reflect the long-term offset of volcanic emissions by silicate weathering (Walker and Kasting, 1992; Archer et al., 1998, Toggweiler, 2008; Zeebe, 2012, Colbourn et al., 2013; Brault et al., 2017).

As described in Walker and Kasting (1992), Toggweiler (2008), Zeebe (2012) Brault et al. (2017), Colbourn et al. (2013, 2015) and Lord et al. (2016), in steady state the silicate weathering flux feedback for CO2 matches the volcanic CO2 emissions, which we have set in SCP-M. Note, for anthropogenic scenarios we separate volcanic emissions from weathering flux, because the silicate weathering feedback under the forcing of atmospheric CO2, is expected to increase at a greater rate than volcanic emissions (volcanic emissions do not respond to anthropogenic emissions of CO2).

Our calculation for silicate weathering yields a flux of carbon to the oceans 6.3 T mol C yr-1 at 275 ppm atmospheric CO2. Our volcanic emissions rate is set to this figure, which is in good agreement with Lord et al. (2016) who set their volcanic C flux at 5.6 Tmol yr-1 to balance the silicate weathering component.

RC: Furthermore, from the python code I learned that weathering (probably meaning carbonate weathering, since in silicate weathering all CO2 comes from the atmosphere with its δ13C-signature) has a δ13C-signature of $-6.9‰$ similarly as volcanic CO2. While the volcanic δ13C seems to be in the expected range (although on the lower side) I believe the weathering δ13C-signature is wrong, since carbonate rocks have a typical δ13C-signature of about +1-2‰ see for example Sano and Williams (1996); Mook (1986).

AC: Re carbonate weathering. The δ13C of carbonate weathering in our model is not -6.90 per mil, as stated in the reviewer comment above, but it is 0 per mil, via our application of the reference standard value for δ13C (the Pee Dee Belemnite) = 0. This feature is shown clearly in the annotated excerpt of the model code in the Attachment A. 0 per mil is the identical value for carbonate weathering used in the first reference provided by the reviewer (Sano and Williams, 1986), and precisely in the middle of the

range (+/- 1 per mil) used in the second reference provided by reviewer (Mook, 1986). We have added text to our model code to make this more obvious (for final upload).

With regards to silicate weathering δ13C. In the SCP-M model the δ13C of silicate weathering CO2 drawdown was originally set at -6.90 per mil, which is the same as the volcanic δ13C we had assumed. This approach was consistent with offsetting volcanic CO2 emissions with silicate weathering (Zeebe, 2012; Toggweiler, 2008, Lord et al., 2016; Colbourn et al., 2013, 2015; Walker and Kasting, 1991). This is a simplification with regards to the δ13C, and therefore we have changed this, and now applied the atmospheric δ13C signature output from the model to the silicate weathering flux (this is now updated in our model results/re-runs).

As per the reviewer comments we have now set the δ13C of the direct consumption of CO2 by silicate weathering, to take atmospheric δ13C value. This is a modest change, however, as atmospheric δ13C is in the range -6.3-7 per mil in the last glacial-interglacial cycle, and we had initially assumed a fixed value of -6.90 per mil.

We also note the reviewer comment that our assumption of -6.90 per mil for volcanic CO2 emissions is at the low end of literature estimates. We have modified this to -4.5 (compared with -4.0 in Zeebe, 2012).

In summary, the changes we have incorporated in the final set of model runs for this manuscript, guided by the reviewer comments:

-We have changed the δ13C of silicate weathering direct consumption of CO2 from the atmosphere, to the atmospheric δ13C signature outputted from model at each time step, as suggested in the reviewer comments (previously it was set as -6.90 per mil).

-We have adjusted our δ13C of volcanic emissions from -6.90 to -4.50 per mil, which is more of a “middle of the range” value.

-We have tidied up the model code description of carbonate weathering and its δ13C (for upload to the Zenodo repository upon finalisation of the manuscript).

RC: I also do not understand how their approach with not explicitly considering terrestrial carbon change (terrestrial carbon to my understanding is covered as externally to the atmosphere/ocean system, fluxes in/out of it prescribed by optimization) covers changes in C3 vs C4 photosynthesis (which have a significantly different isotopic fractionation) on glacial/interglacial timescales (Collatz et al., 1998; Köhler and Fischer, 2004) which leads to differences in the mean terrestrial δ13C and therefore also the changes in the δ13C-cycle as a whole (Kaplan et al., 2002).

AC: Thanks for the comment. Our response is broken in two parts 1) the terrestrial biosphere and 2) C3 vs C4 photosynthesis.

In summary, the terrestrial biosphere is explicitly considered in our modelling. It is two boxes within the carbon cycle box model we have used. It is not prescribed by optimisation.

We have decided not to assess C3 versus C4 photosynthesis and its effects on δ13C fractionation.

We discuss both of these points in more detail below.

Terrestrial biosphere in SCP-M

The terrestrial biosphere is treated in SCP-M as two boxes that exchange carbon with the atmosphere based on fluxes of net primary productivity (NPP) (carbon in) and respiration (carbon out). It is part of the carbon cycle that includes the terrestrial biosphere-atmosphere-ocean-sediments-volcanoes etc. Our box model applies a simple representation of the terrestrial biosphere, whereby biological productivity responds to carbon fertilisation. Therefore, CO2 is the driver of terrestrial biosphere productivity in this model. We apply the two-box terrestrial box model scheme of Harman et al. (2011). The inputs are starting estimates of net primary productivity (NPP), the terrestrial biosphere carbon stock, plant respiration rate and atmospheric CO2. The approach of Harman et al. (2011) is to split the terrestrial biosphere into a

fast-response (grasslands and grassy components of savannah systems) and a slow-response (woody trees) component. In this model, the productivity is mostly focussed on the plants/grasses component.

The formula is shown in the model documentation paper (O'Neill et al., 2019) and Harman et al. (2011), and is reproduced here:

$\text{dAtCO2/dt} = -\text{NpreRP}[1+\beta \text{LN(AtCO2)}] + \text{Cstock/k} + \text{Dforest}$

Where Npre is NPP at a reference pre-industrial level of atmospheric CO2, RP is a parameter to split NPP between short-term terrestrial biosphere carbon stock and the longer term stock (Cstock1 and Cstock2). B is a parameter with a value typically in the range 0.4-0.8 (Harman et al., 2011). Cstock is the carbon stock in each terrestrial biosphere box, k is the respiration timeframe for each box. Dforest is the prescribed rate of deforestation emissions for present day simulations and projections. A terrestrial biosphere fractionation factor is applied for the carbon isotopes.

This flux out of the atmosphere feeds into the two terrestrial biosphere stocks of carbon (Cstock1 and Cstock2), and the boxes lose carbon to the atmosphere by respiration, as per the equation above. This differential equation for NPP, respiration, and the net flux into and out of the terrestrial biosphere (increase or decrease in the terrestrial biosphere carbon stock), solves at each time step of the model, taking the model's output of CO2 and then calculating the NPP, respiration, Cstock1 and Cstock2 and calculating a new atmospheric CO2. The time step of the model is one year, with 10,000 years for each model-data simulation.

Harman et al. (2011) model the terrestrial biosphere primarily as a function of atmospheric CO2. They also incorporate an optional temperature dependency. This is the same approach used in the simplest 4Box terrestrial biosphere module of the Bern Simple Carbon Model (Strassman and Joos, 2018; Seigenthaler and Joos, 1992; Kicklighter et al., 1999; Meyer et al., 1999), and described by Enting (1994) – although we understand that there are various terrestrial biosphere modules applied with the Bern models, and most are more complex. As far as we can discern, the simple carbon fertilisation approach is also used in Jelstch-Thommes et al. (2019), which also applies the simplest 4Box terrestrial biosphere of the simple Bern model.

There are other possible drivers of the NPP – temperature, precipitation, soil nutrient levels. In the context of our simple carbon cycle model, we are mainly interested in CO2. We don't model atmospheric temperature, and if we were to try to incorporate atmospheric temperature as a driver of terrestrial biosphere, we would also need to incorporate it for terrestrial weathering. There is a limit to how much detail we want to include in the model given we are conducting many simulations (∼80,000) in our model-data optimisations across the MIS of the last glacial-interglacial cycle.

We do note that there are studies devoted to determining whether the CO2 fertilisation effect or climate is the dominant control on terrestrial biosphere NPP and the size of the terrestrial biosphere carbon stock. According to Hoogakker et al. (2016), CO2 fertilization, rather than climate, is the primary driver of lower glacial net primary productivity by the terrestrial biosphere, accounting for around 85% of the reduction in global NPP at the LGM. Kaplan et al. (2002) also concluded that over glacial-interglacial timescales, global terrestrial carbon storage is controlled primarily by atmospheric CO2, while the climate has more influence on the isotopic composition. Otto et al. (2002) also found that the CO2 fertilization effect is mostly responsible for the total increase in vegetation and soil carbon stocks since the last glacial maximum. Kohler et al. (2010) prioritised CO2 fertilisation as the driver of terrestrial biosphere in their "control" main simulation scenario for glacial-interglacial cycles over the last 740 kyr, but also ran scenarios with a climatic driver for the terrestrial biosphere to estimate the effects of "fast" climate changes on atmospheric δ13C. Other studies arguing that atmospheric CO2 is an important, or is the main driver of terrestrial biosphere productivity include Kicklighter et al. (1999), Joos et al. (2004), Schimel et al. (2015), Sitch et al. (2008), Arneth et al. (2017). This view has been contested by van der Sleen et al. (2015).

Given we don't model the atmospheric temperature or precipitation, we saw limited

additional benefit to introduce them into our model of the terrestrial biosphere, although it would not be difficult to do this. Finally, given that CO2 and atmospheric temperature co-vary closely, across glacial cycles, it seems of limited benefit to split these effects out in our simple carbon cycle modelling exercise. For example, Meyer et al. (1999) found similar results for modelling carbon uptake in the terrestrial biosphere whether only CO2 fertilisation, or CO2 fertilisation + climate, were included as drivers of NPP – but noting this was not tested for the LGM.

Our aim is not to contribute new findings on the terrestrial biosphere, but we present the behaviour of the terrestrial biosphere in our manuscript to confirm that our exhaustively multi-proxy constrained model-data output is consistent with the range of literature estimates of variations in the terrestrial biosphere in the last glacial-interglacial cycle and LGM-Holocene period, and we show this. For example, our experiment shows a change in the terrestrial biosphere carbon stock of +630 PgC between the MIS 2 (LGM) and MIS 1 (Holocene) period. This compares with other estimates of +540 PgC (Brovkin et al., 2007), +∼820-850 PgC (Joos et al., 2004) – with the majority by CO2 fertilisation, ∼+500 PgC (Kohler et al., 2010), +∼500 PgC (Brovkin et al., 2012), +850 PgC (Jeltsch-Thommes et al., 2019), +511 +/- 289 PgC (Peterson et al., 2014), +378 +/- 88 PgC (Menviel et al., 2016). Another estimate of the LGM-Holocene terrestrial biosphere change is 550-694 Pg C (Prentice et al., 2011), which our result of 630 Pg C sits comfortably within. Our estimate is actually towards the upper end of the literature ranges, suggesting if anything we could exaggerate the effects of the terrestrial biosphere from the LGM to the Holocene period, with perhaps little to gain by splitting out temperature and precipitation effects. If did, we would probably also need to consider other important features such as soil nutrients and local humidity. While we have a simple, but explicit two-box representation of the terrestrial biosphere, we don't believe that this detracts from our model-data results, as shown in Figures 9-11 and Figure 12 specifically for the terrestrial biosphere.

C3 and C4 photosynthesis.

In summary, our model exercise doesn't take account of C3 versus C4 photosynthesis in the terrestrial biosphere, or consider its effects on the δ13C signature of the terrestrial biosphere. In response to the reviewer comments, we looked into this in more detail to see if we can improve our modelling – noting that it is very easy to update the model code for something like this. For example, we re-ran the model-data experiments as part of one of the other reviewer comments, so could easily incorporate more detail for the terrestrial biosphere, such as C3%/C4% variation in δ13C.

Our approach was to understand and quantify the references provided by the reviewer, review approaches by other modelling exercises for the glacial-interglacial cycle of last 130 kyr, and decide whether we should re-run the modelling with an alternative treatment of the terrestrial biosphere to cater for C3%/C4% and δ13C. As part of investigation, we also constructed the C3/C4 model of Collatz et al. (1998)/Kohler and Fischer (2004) in a python module that easily fits into the carbon cycle box model, to evaluate whether it would improve our modelling (described below and attached to these comments).

Kohler and Fischer (2004), suggested reading by the reviewer, in their excellent paper do make a very good point about C3/C4 photosynthesis in the context of glacial-interglacial δ13C, that is worth reproducing here as a summary:

“Oceanic inorganic carbon is becoming 0.4 heavier during the G/IG transition, which is in good agreement with both modelling studies and data constraints (Curry et al., 1988; Duplessy et al., 1988; Michel et al., 1995). It should be noted that 85% of this calculated oceanic change in δ13C can be explained by the increase in the terrestrial carbon stock and only the missing fraction of 15% by changes in the abundance of the two photosynthetic pathways. Thus, uncertainties in the current knowledge on C3/C4 plant distribution during the LGM are of minor importance for the overall simulation results."

Collatz et al. (1998)/Kohler and Fischer (2004) modelling approach

The Collatz et al. (1998) approach to modelling C3 vs C4 %, is based on the estimation of a “cross-over temperature” for dominance of C4 or C3 plants. Above the cross-over temperature, C4 plants are favoured. Below the cross-over temperature, C3 plants are favoured. Collatz et al. (1998) derived a simple equation for the cross-over temperature of C3 vs C4. The cross-over temperature exhibits a positive relationship with atmospheric CO2. Therefore, as CO2 goes up, the cross-over “hurdle” temperature for C4 dominance also increases, so C4% has a negative relationship with CO2. While increasing temperatures may favour C4 plants, if CO2 was also increasing, this would tip the advantage back towards C3 plants. The cross-over temperature calculation of Collatz et al. (1998) is shown as:

T50(degC) =(10/lnQ10)ln(pO2(1 + 0.5αC3/αC4)/(0.8 x pCO2 x s25(αC3/αC4 $-$ 1))) + 25

Where T50 is the crossover temperature for C4 and C3 dominance, where aC3 is the “intrinsic quantum yield for C3 photosynthesis” and pi is the leaf internal pCO2, assumed to be equal to 0.8 x atmospheric pCO2. s25 is the value of s at 25°C and Q10 is the relative change in s for a change in temperature.

s is defined as:

s=2600Q10ˆ(Tx-25/10)

where Q10 is the relative change in s for a 10°C change in temperature.

To analyse C4%, Kohler and Fisher (2004) extended the Collatz et al. (1998) equation and provide a simple set of equations to estimate C4% and C3% between the glacial and interglacial periods, using the change in temperature relative to changes in the cross-over temperature between the two periods :

C4%=C4%* x (1$-\alpha$C3/C4 x (ΔT$-\Delta$T50))

C3%=C3%* x (1+αC3/C4 x (ΔT$-\Delta$T50))

We reconstructed this C3%/C4% model of Collatz et al. (1998)/Kohler and Fischer (2004), to investigate the reviewer comments.

We have uploaded the Python script and data for last glacial-interglacial cycle atmospheric CO2 and temperature, at https://zenodo.org/record/3889704#.XuH3Ji1L0_U.

We use the cross-over temperature calculation of Collatz et al. (1998), the C4% model of Kohler and Fischer (2004), and estimate an average terrestrial biosphere δ13C using the C4% and C3% output from this model and estimates of δ13C for C4 and C3 plants.

To test our simple model works, we satisfy the estimate of T50 of 22 deg C at atmospheric pCO2 of 350 ppm from Collatz et al. (1998), and, as per Kohler and Fischer (2004) Figure 4, ~18 deg C at atmospheric pCO2 of ~280 ppm, and ~11 deg C for atmospheric pCO2 of ~190 ppm. We forced our version of the C4% model of Kohler and Fischer (2004)/Collatz et al. (1998) with atmospheric temperature and CO2 through the last glacial-interglacial cycle (Figure 3 below). The atmospheric temperature data of Jouzel et al. (2007) is derived from Antarctic ice cores, so it likely overstates the amplitude global average temperature cooling during the glacial period. Jouzel et al. (2007) show peak cooling of ~11 degrees C, which is greater than global estimates in the range 3-6 deg C (Schneider von Deimling et al., 2006a; Holden et al., 2009; Schmittner et al., 2011; Annan and Hargreaves, 2013). We take an intermediate average global LGM cooling of 4.5 degrees, and scale the profile of Jouzel et al. (2007) to the average global amplitude of cooling of 4.5 deg C for the LGM, which is the middle of the range of global estimates. This is a simplification, but appropriate for our reconnaissance exercise. We also apply the last glacial-interglacial cycle atmospheric CO2 data of Bereiter et al. (2015) – Figure 3 below.

In terms of what starting values to use for δ13C for the C3 and C4 plants, we note a huge variation in the possible values to use for δ13C of C3 plants, and also note a large variation in the estimates for average δ13C of the terrestrial biosphere applied in terrestrial biosphere and carbon cycle modelling exercises for the last glacial-interglacial

cycle. We discuss in more detail below, but flag that natural variation in the average values assumed for δ13C fractionation of the terrestrial biosphere, and variation in δ13C values assumed between modelling studies, greatly outweigh the posited variation in δ13C fractionation from C4% vs C3%.

Carbon cycle modelling exercises show a large range (e.g. Brovkin et al., 2002 (-16 per mil), Menviel et al. (2016) (-23.3 per mil), Jelstch-Thommes et al. (-24 per mil), and the study of Kohler and Fisher applied an average of -16 per mil (C3 -19 per mil, C4 -5 per mil). For this simple exercise, we take the starting average δ13C for terrestrial biosphere taken from Jeltsch-Thommes et al. (2019) (this text was a suggested reference by the reviewer) of -24 per mil, and back out the average starting C3 and C4 δ13C assuming the PI value of C4% of 20% applied in Kohler and Fischer (2004) (the reference suggested by the reviewer). This yields a starting δ13C for C3 plants of -27 per mil, and -14 per mil for C4 plants. For comparison, Kohn et al. (2010) provided a range of δ13C estimates for C3 plants of -20 to -37 per mil, with a global average of -27 per mil. O'Leary et al. (1988) provided a synthesis of global data of -27.1 per mil for C3 plants and -13 per mil for C4 plants.

We model C4% to vary from the preindustrial starting estimate of 20% (Kohler and Fischer, 2004), up to an average of 25% during the LGM (Figure 4) (see figures below, at the bottom of this response document). We model average δ13C for the terrestrial biosphere to vary between the range -24.2-23.6 per mil during the last glacial-interglacial cycle, a variation of 0.6 per mil (Figure 4 below).

Our estimated C4% from using the Collatz et al. (1998) equation (25% as per Fig. 4) is a little higher than Kohler and Fischer (2004) (24%) and this likely reflects differing atmospheric CO2 and temperature assumptions. For example, Kohler and Fischer (2004) take average northern hemisphere average temp change of -5 degrees, and southern of -8 degrees. We have inputted a global average change of -4.5 degrees C as per the literature range of 3-6 degrees C cooling (Fig. 4 below). However, there must be something else being applied by Kohler and Fischer (2004) to achieve their

LGM “target” C4% of 30-33%.

The approach of Kohler and Fischer (2004) was to establish a target variation of C4% between the LGM and the PI and then to see what parameterisations of their model runs could reach that target. Our estimate of LGM C4% is of 25% is far below the “targeted” C4% of 30-33% from Kohler and Fischer (2004). Their study found that varying the C4% amplitude in the Collatz et al. (1998) C3/C4% share model could increase C4% from 20% to 24%, but increasing the grassland succession amplitude increased the C4% up to 42%, a much bigger change than the C3/C4% share model alone. Furthermore, according to Huang et al. (2001), local moisture conditions might be even more important than any temperature or CO2 effects on C3/C4%.

The grassland succession factor is an equation contributed by Kohler and Fischer (2004) to estimate the effects of changes in the tree-line (the divide between where trees and grasses grow) as a function of changes in temperature, between the LGM and PI. According to Kohler and Fisher (2004), this is the main driver for the C4% change and change in the terrestrial biosphere δ13C fractionation, perhaps not the temperature and CO2-dependant equation of Collatz et al. (1998). Kaplan et al. (2002) posit something different again, that the major driver of changed terrestrial biosphere δ13C discrimination since the LGM is retreating ice sheets, with an additional or ancillary role for C3/C4 plant substitution.

Our estimated change in terrestrial biosphere δ13C fractionation of $\sim$+0.6 per mil, is below the estimate from Kohler and Fischer of 1.3 per mil, and that reflects that they include the grassland succession factors in their LGM-PI analysis. The offset in assumed δ13C fractionation between C3 and C4 of -13 per mil (-27 per mil less -14 per mil) is very similar to their chosen -14 per mil (-19 per mil less -5 per mil), suggesting that the differences reflect the use of another factor outside C3/C4%, the grassland succession factors, to drive their results.

Beyond the simple exploratory attempt above, modelling highly uncertain grassland

succession factors, or ice sheet retreat/advance, or localised moisture and temperature changes, to try and explain uncertain changes in C3% vs C4%, for which the starting values themselves could fall within huge ranges of uncertainty, looks beyond the scope of our study.

We note that, with regard to the estimates of C4% used by Kohler and Fisher to create “targets” for pre-industrial and LGM periods, Kohler and Fisher (2004) say the following:

“NPP and fC4 for the LGM are based on modelling studies only and, thus, represent only weak indicators which were only used for uncertainty estimates.” And furthermore, on P16:

“However, because the constraints on NPP and the fraction of C4 plants were based on only a few mostly modelling studies, we merely interpret those as a model evaluation.” These findings underscore the uncertainty of estimates for quantifying C4/C3 and therefore δ13C of the terrestrial biosphere. This uncertainty is amplified in the actual estimates of δ13C for C3 and C4 plants, as we discuss below.

Fig. 5 below is reproduced from Kohn (2010), and shows the range in δ13C fractionation for C3 plants alone, which spans -20 to -37 per mil, and is impacted by many factors including temperature, precipitation, and effects of canopies and new growth.

Furthermore, more recently, Kohn (2016) attempted to estimate the change in δ13C for C3 plants from the LGM to modern day, based on atmospheric CO2, and also to quantify the effects of precipitation on C3 plant δ13C. This shows the variation in C3 δ13C discrimination itself, is even bigger than the posited effect of C3/C4% (see Fig. 6 below extract from Kohn (2016)

To model C3 and C4 δ13C properly, there are other important effects in C3 plants (on their own), that would need to be taken into account. For example, Francois et al. (1999) point out that changes in the δ13C fractionation from a changing C4% were partially offset by changes in the opposite sign in the fractionation of C3 plants due to the modification of the intercellular CO2 pressure within their leaves.

Peer group/modelling approaches

In exploring this issue of C3 vs C4% and δ13C further, and to benchmark our work against the peers who are modelling and analysing the last glacial-interglacial cycle (0-130 ka), we investigated the literature. C3 versus C4 fraction in photosynthesising plants is not discussed much in the literature of modelling of the last 130 kyr glacial-interglacial cycle of carbon. We couldn't find any reference to C3/C4 photosynthesis and δ13C in Eggleston et al. (2016), who contributed the atmospheric δ13C data we used in our model-data analysis. We don't find any mention of C3/C4 photosynthesis and its effects on δ13C in any of Ganopolski et al. 2010, Brovkin et al. 2012, Ganopolski and Brovkin, 2017; Kohfeld and Chase, 2017.

Brovkin et al. (2002) simply state, with reference to their CLIMBER-2 model of the last glacial-interglacial cycle:

"Most of the carbon (ca. 85%) is allocated to the C3 photosynthesis pathway and the remaining carbon (15%) to the C4 pathway. The globally averaged δ13C fractionation factor for terrestrial biosphere is 0.984." (-16 per mil).

We find no reference to any changes for glacial interglacial C3 and C4 and terrestrial biosphere δ13C modelled in Brovkin et al. (2007, 2012), or Ganopolski (2010, 2017).

The transient modelling of the last glacial-interglacial cycle, undertaken by Menviel et al. (2012b), does not mention C3 and C4 photosynthesis, or its effects on δ13C fractionation.

We note that Kohler et al. (2010), mention the parameterisation of C4% in the terrestrial biosphere in their 740 kyr transient simulations with the BICYCLE model. In their control simulation (CTRL) they had a representation of the terrestrial biosphere that emphasised CO2 fertilisation as the dominant control on terrestrial biosphere NPP, and limited or no change (hard to tell from reading) in C4% on the glacial-interglacial

δ13C of the terrestrial biosphere. There is an extended scenario TB+ which emphasises climate as the driver of the terrestrial biosphere, faster response of NPP/terrestrial biosphere and parameterises higher C4% in the LGM (and associated change in the δ13C of the terrestrial biosphere), leading to a combined small effect on deep Pacific δ13C of 0.1 per mil.

However, in discussing the all-important drivers of the changes in atmospheric pCO2, δ13C and deep Indo-Pacific δ13C, and mean ocean δ13C, for termination I, as listed in Kohler et al. (2010) Table 3, C4% and terrestrial δ13C changes are not mentioned. The features listed by Kohler et al. (2010) as the drivers are: lower ocean temperatures, smaller terrestrial carbon storage, lower sea level, weaker NADW formation, enhanced marine export production, larger sea ice cover (gas exchange), higher Southern Ocean stratification.

There is a little more discussion of the C4% and terrestrial biosphere in terms of the LGM and Holocene, which unfortunately is only a small fraction of our 130 kyr period of interest.

For example, Joos et al. (2004) modelled a change in terrestrial biosphere δ13C between the LGM and Holocene of 0.5 per mil. However, they observed the following:

"Changes in the mean terrestrial isotopic signature have a minor impact on the modeled changes in δ13C of DIC.……….The estimated oceanic δ13C shift is 0.05% smaller than in the standard case, if the land biosphere-atmosphere δ13C difference is kept at the Holocene value of -17 per mil." Menviel et al. (2012a) provided an interesting quote and the following caveat with their modelling of the last glacial termination and Holocene:

"A caveat is that a constant atmosphere-land isotopic fractionation factor is applied in the inverse approach by Elsig et al. [2009] and in this study, therefore not taking into account any relative changes in the occurrence of C3/C4 plants and other influences on fractionation. However, using the LPJ-DGVM vegetation model, Joos et al. [2004]

found that changes in fractionation and C3/C4 plant abundance due to climate and CO2 changes lead to a decrease in δ13C signature of the terrestrial biosphere of about 0.5 per mil from the early Holocene (10 ka B.P.) to pre-industrial times. A 0.5 permil decrease in biosphere δ13C translates into an atmospheric δ13C decrease of about 0.02 permil. This suggests that changes in the atmosphere-land isotopic fractionation have a small influence on the results presented above. “

We note another paper relevant to our manuscript, by Menviel et al. (2016) and focussed on the LGM (18-24 ka), made brief mention of C3/C4, and described that they undertook a sensitivity of -0.7 per mil and +0.5 mil around their average estimate of -23.3 per mil δ13C for the terrestrial biosphere, but the modelling results of that sensitivity are not discussed further in the paper. That type of sensitivity is pretty easy to undertake for analysing only the LGM and the Holocene, as any studies on C3 vs C4 (Kohler and Fisher, 2004; Kaplan et al. 2002, Francois et al., 1999; Joos et al., 2004) have looked at this time period – even though they produce uncertain estimates for the % C3 vs C4 and therefore δ13C fractionation factor. It is a much more difficult proposition to come up with values for a sensitivity for the last glacial-interglacial cycle in its entirety (130,000 years), but that may be an interesting piece of work on its own – future work.

Studies focussed on the terrestrial biosphere

We note the references that focussed specifically on the terrestrial biosphere in detail as the major focus of their work, in the early 2000’s, or example those provided by the reviewer (e.g. Collatz et al., 1998; Kaplan et al., 2002, Kohler and Fisher, 2004), and another (e.g. Francois et al., 1999), focused only for the Last Glacial Maximum and PI/modern periods None of them examined the last glacial-interglacial cycle which was ~130 kyrs in duration. All of these studies above, to our understanding, produced uncertain results.

A recent study devoted to analysing the terrestrial biosphere in detail/major focus

(Jeltsche-Thommes et al. (2019) - suggested by the reviewer), does not mention this feature C3 vs C4%. Jeltsche-Thommes (2019), in their study focussed on the terrestrial biosphere from the last glacial maximum to the Holocene, simply state:

“The δ13C signature of terrestrial carbon is set to -24 ‰.” (at the top of page 856).

We wondered whether we can contribute something important here with regard to C3%/C4% and the terrestrial biosphere δ13C that has not been considered by any of our peer group of model-data analysis of the last glacial-interglacial cycle.

In summary, there are studies that focussed specifically on the terrestrial biosphere, using dedicated vegetation models. We see that these studies had great detail for the terrestrial biosphere, but were very light on detail for other features of the carbon cycle (ocean circulation and biology, volcanism, weathering, the effects of calcium carbonate compensation). In reviewing these papers, and consistent with our prior understanding, there is not great confidence on quantifying the change in C3 and C4 proportions during the LGM and Holocene, and this is particularly worse during the time period we have analysed up to 130 ka. The papers of Collatz et al. (1998), Kaplan et al. (2002), Kohler and Fisher (2002), all focus on the period LGM-present. There is no coverage of the last glacial cycle 130-20 ka, which is the focus of our study. Furthermore, studies that do focus on the last glacial-interglacial cycle of atmospheric CO2, eg Brovkin, Ganopoloski, do not mention C3 versus C4 fractionation in their papers – making difficult any comparison. We even note that a paper we have referenced in our manuscript, Hoogakker et al. (2016), a paper devoted entirely to the terrestrial biosphere in the last glacial-interglacial cycle, does not address C3 versus C4 plant composition.

As shown above, it is actually an easy process to add the C3 and C4 equations of Collatz et al. (1999) and Kohler and Fischer (2004), and also a temperature dependency for NPP, as we have shown above and with the attached code (Attachment B). We could do this and then re-do the simulations as an appendix or addendum (or a sensitivity).

We could even just apply a sensitivity on the δ13C of terrestrial biosphere of +1/-1 per mil change between LGM and Holocene. However, that's a straightforward exercise for the LGM and Holocene comparison, but it would involve us trying to fit the uncertain LGM-Holocene changes back for the entire last glacial cycle, which is another highly uncertain exercise. We note all of the studies referenced in the reviewer comments and described here, considered C3 vs C4 only for the LGM to Holocene-modern period, but we've explicitly looked at the lead-up to the LGM over the period from 130 ka. We would not like to try to extrapolate changes in C3 v C4 for the LGM over the entire last glacial-interglacial cycle, and implementing the Collatz et al. (1998)/Kohler and Fischer (2004) module would not help us much in that regard as it only explains less than half of the change δ13C of terrestrial biosphere from the LGM (the rest explained by changes in grassland vs forest succession).

There is huge uncertainty around average δ13C factors for plants, and that extends even further to C3 and C4 δ13C, and their possible respective shares and variations. The indicated changes of 0.3-1.8 per mil terrestrial biosphere δ13C between the Holocene and LGM, from the literature described above, are very minor compared to the absolute uncertainties and range in δ13C of the terrestrial biosphere itself.

Summary on terrestrial biosphere and C3 vs C4 photosynthesis

We investigated these topics enthusiastically, based on the reviewer's comments. We're very confident, based on our assessment of the papers above, that our model results will not change by much at all, and the paper conclusions by nothing at all, by varying our approach to the terrestrial biosphere (equally for rock weathering as discussed above). If the CP Journal Editors and the reviewer feel greatly compelled that we need to modify our modelling approach, we certainly can (these would not be major model revisions, only minor adjustments). Our preferred approach, is to simply add a caveat that our model-data experiments don't consider the effects of C3/C4 photosynthesis on δ13C fractionation of the terrestrial biosphere.

We reconstructed this C3%/C4% model of Collatz et al. (1998)/Kohler and Fischer (2004), to investigate the reviewer comments. We have uploaded the Python script of the simple model and data for last glacial-interglacial cycle atmospheric CO2 and temperature, at https://zenodo.org/record/3889704#.XuH3Ji1L0_U.

Amendments to the manuscript

We have added the following text to the model description (P5 L24):

"The terrestrial biosphere is represented in SCP-M as a stock of carbon that fluxes with the atmosphere, governed by parameters for net primary productivity (NPP) and respiration. In SCP-M, NPP is calculated as a function of carbon fertilisation, which increases NPP as atmospheric CO2 rises via a simple logarithmic relationship, using the model of Harman et al. (2011). This is a simplified approach, which omits the contribution of temperature and precipitation on NPP. Other, more complex models of the carbon cycle applied to glacial-interglacial cycles have a more detailed treatment of the terrestrial biosphere, including climate dependencies (e.g. Brovkin et al., 2002; Menviel et al., 2012). A number of studies emphasise the role of atmospheric CO2 as the driver of terrestrial biosphere NPP on glacial-interglacial cycles (Kaplan et al., 2002; Otto et al., 2002; Joos et al., 2004; Hoogakker et al., 2016), although other studies cast doubt on the relative importance of atmospheric CO2 versus temperature and precipitation (Francois et al., 1999; van de Sleen et al., 2015).

The isotopic fractionation behaviour of the terrestrial biosphere may also vary on glacial-interglacial timeframes. This has been studied for the LGM, Holocene and the present day (e.g. Collatz et al., 1998; Francois et al., 1999; Kaplan et al., 2002; Kohler and Fischer, 2004; Joos et al., 2004; Kohn, 2016). The variation in isotopic fractionation within the terrestrial biosphere reflects changes in the relative proportions of plants with the C3 and C4 photosynthetic pathways, but also strong variations within the same photosynthetic pathways themselves (Francois et al., 1999; Kohn, 2010; Schubert and Jahren, 2012; Kohn, 2016). The drivers for these changes include relative sea level and exposed land surface area (Francois et al., 1999), global tree-line extent (Kohler and Fischer, 2004), atmospheric temperature and CO2 (Collatz et al., 1998; Francois et al., 1999; Kohler and Fischer, 2004; Kohn, 2010; Schubert and Jahren, 2012), global and localised precipitation and humidity (Huang et al., 2001; Kohn, 2010; Schubert and Jahren, 2012; Kohn, 2016), and also changes in the intercellular CO2 pressure in the leaves of C3 plants (Francois et al., 1999).

Estimated changes in average terrestrial biosphere δ13C signature between the LGM and the Holocene fall in the range -0.3-1.8‰ (less negative δ13C signature in the LGM), with further changes estimated from the onset of the Holocene to the pre-industrial, and even greater changes to the present day (due to rising atmospheric CO2). This feature has been covered in detail within studies that focussed on the terrestrial biosphere between the LGM and Holocene, but less so in modelling and model-data studies of the last glacial-interglacial cycle. Menviel et al. (2016) provided a sensitivity of -0.7+0.5‰ around an average LGM value of -23.3‰ for the LGM, based on previous modelling of the LGM-Holocene timeframe by Joos et al. (2004). Another modelling study (Menviel and Joos, 2012), assessed the variation in LGM-Holocene δ13C of the terrestrial biosphere to be a minor factor and it was omitted. Kohler and Fischer (2004) assessed the changing δ13C signature of plants between the LGM and Holocene to be a minor factor in setting δ13C of marine DIC, compared to the change in the absolute size of the terrestrial biosphere across this period.

Given the uncertainty around the starting estimates of δ13C, the uncertain LGM-Holocene changes, the large number of potential drivers, and the further uncertainty in extrapolating the posited LGM-Holocene changes back for the preceding 100 kyr, and the modest changes relative to the average δ13C signature (and the very large range in, for example, present day estimates of C3 plant δ13C (Kohn, 2010, 2016), we omit this feature with the caveat that there is added uncertainty in our terrestrial biosphere results with respect of the δ13C signature applied. We apply an average δ13C signature of -23‰ similar to values assumed by Menviel et al. (2016) and Jeltsch-Thommes

et al. (2019) (23.3‰ -24‰ respectively), but more negative than assumed in Brovkin et al. (2002), Kohler and Fischer (2004) and Joos et al. (2004) (-16-(-17)‰. Our aim is not to contribute new findings of the terrestrial biosphere, but to ensure that the simple representation of the terrestrial biosphere in SCP-M provides the appropriate feedbacks to our (exhaustive) glacial-interglacial cycle model-data optimisation experiments, that are in line with published estimates."

We have also updated the discussion of our model results for the terrestrial biosphere, to provide a bit more detail and some additional references (Section 5.3), plus an additional caveat in the "advantages and limitations section" (P34, L18).

"Furthermore, we apply a simple representation of the terrestrial biosphere in our model-data experiments, relying primarily on atmospheric CO2 as the driver for NPP. This approach provided reasonable results for the terrestrial biosphere carbon stock and NPP, on the whole, but may miss some detail in the terrestrial biosphere during the last glacial-interglacial cycle."

Future work could enhance this set of modelling results with more detail in the terrestrial biosphere. For example, the modelling values for ocean circulation and biology derived here, could be used to solve for the optimal data-matching values for C3 and C4 plant productivity, with separate δ13C-fractionation factors, to help inform that area of study.

Attachment A

Carbonate rock weathering in SCP-M

The reviewer mentioned the model code. In terms of the model equations and model code, the flux of carbon to the ocean from carbonate weathering is set in our model by the following equation (please see O'Neill et al. (2019) and the annotated model code snapshot below):

RVCARB=WCARB*AtCO2 (1)

Where WCARB is a weathering parameter with respect of atmospheric CO2 and is set at 1.5-2.0 mol C/m3/atmosphere. At 275 ppm atmospheric CO2, this is a flux of 10 x1012 mol C annum. (for comparison, this flux is 12 x 1012 mol C annum in Morse and Mackenzie (1990), Zeebe (2012) and Archer et al. (1998), and 14.9 Tmol C annum in Toggweiler (2008). This flux of carbon is added to the low latitude surface box of the model (as per Toggweiler (2008), Zeebe (2012), Hogg (2008)), and alkalinity is added in the ratio ALK:DIC 2:1 (as per Toggweiler (2008), Zeebe (2012), Colbourn et al.,2013) by multiplying RVCARB by 2.0 to create the river flux of alkalinity to the ocean surface boxes. This 2:1 flux of alkalinity:carbon reflects that the initial one mol of CO2 consumed by the carbonate weathering equation, has been returned to the atmosphere (the DIC proportion of 1 is 2 mols less one mol returned to the atmosphere) as per Zeebe (2012) and Lenton and Britton (2006).

The fluxes of DIC and Alk from carbonate weathering are added to the ocean via the river fluxes of C and Alk (see below). This lowers pCO2 in the ocean surface box and therefore draws CO2 from the atmosphere into the ocean, a net sink of CO2 from carbonate rock weathering. We do not subtract a mol of CO2 directly from the atmosphere in our equation for atmospheric CO2, as for the time scale modelled ∼10 kyr, we are taking the short-cut of assuming the CO2 taken up directly from the atmosphere from carbonate weathering, is released back to the atmosphere upon precipitation of CaCO3 into the ocean (Zeebe (2012), Toggweiler (2008)). Carbonate weathering is therefore a flux of carbon and alkalinity to the surface ocean via a river flux, leading to lowering of pCO2 in the surface ocean box and subsequent drawdown of CO2 from the atmosphere. An almost identical approach to ours, was applied by Lenton and Britton (2006), a paper devoted to the study of rock weathering as a sink of atmospheric CO2. The only difference is that Lenton and Britton (2006) applied an additional temperature and terrestrial biosphere dependency on weathering.

We consulted Lord et al. (2016) as suggested in the reviewer comments. Lord et al. (2016) use the cGenie model to estimate weathering feedbacks from atmospheric

CO2 emissions. Lord et al. (2016) is a paper that is devoted to the feedback of rock weathering on atmospheric CO2. The treatment of carbonate weathering, in terms of setting this simply as fluxes of DIC and Alk in the ratio of 1:2 to the surface ocean box, is identical to ours. Where they differ, is because they are looking in much more detail at the effects of terrestrial rock weathering, they also explore other dependencies for rock weathering, such as temperature, terrestrial biosphere productivity and run-off rates. Ours has an atmospheric CO2 dependency, as per Zeebe (2012), Toggweiler (2008), Walker and Kasting (1992).

Silicate rock weathering in SCP-M

The treatment of silicate weathering in the SCP-M model is:

RVSIL=(BSIL+WSIL*AtCO2) (2)

Where BSIL is a constant weathering rate of 0.75 e-4 mol/m3/yr (Toggweiler, 2008), and WSIL is a rate varying with atmospheric CO2, set at 0.5 mol/m3/atmosphere as per Toggweiler (2008). For atmospheric CO2 of 275 ppm, this is a weathering flux of 5.7 x 1012 mol C annum (5 x 1012 mol in Zeebe (2012) and 5.63 x 1012 mol annum in Toggweiler (2008)).

The silicate and carbonate weathering fluxes of carbon, are added to the surface ocean boxes of the box model. Alkalinity is also added, in a ratio of 2:1 to the carbon fluxes (Sarmiento and Gruber (2006), Toggweiler (2008), Zeebe (2012)).

However, there is an additional step applied with silicate weathering. To account for the unit of CO2 consumed directly from the atmosphere in silicate weathering (one more unit than carbonate weathering, as per Zeebe, 2012), and using the approach of Toggweiler (2008), we also subtract an amount equal to a unit of silicate weathering directly from the atmosphere. This is the same approach of Zeebe (2012) who applies a doubling of the flux of silicate weathering, and that of Toggweiler who subtracts a flux of CO2 directly from the atmosphere to account for the additional unit of CO2 consumed by silicate weathering (when compared with carbonate weathering). This flux is subtracted directly from Atmospheric CO2 in SCP-M. This flux subtracted from the atmosphere negates the effects on atmospheric CO2 of the units of C added to the ocean by the silicate weathering flux of C. The effect of the more alkaline ocean (alk:C is 2:1 in the silicate weathering flux) is to draw down the volcanic emissions of CO2. Volcanic CO2 emissions are set equal to the amount of CO2 taken directly from the atmosphere by silicate weathering, to reflect the long-term offset of volcanic emissions by silicate weathering (Walker and Kasting, 1992; Archer et al., 1998, Toggweiler, 2008; Zeebe, 2012, Brault et al., 2017). In Walker and Kasting, 1992; Toggweiler, 2008; Zeebe, 2012; Brault et al., 2007, volcanic emissions are also set to the silicate weathering drawdown of CO2.

As described in Walker and Kasting, 1992; Toggweiler, 2008; Zeebe, 2012; Brault et al., 2007, Colbourn et al. (2013, 2015); Lord et al. (2016), in steady state the silicate weathering flux feedback for CO2 matches the volcanic CO2 emissions, which we have set in SCP-M. Note, for anthropogenic scenarios we separate weathering flux from volcanic emissions, as it is clearly a non-steady state simulation, and the silicate weathering feedback, under the forcing of atmospheric CO2, is expected to increase at a greater rate than volcanic emissions.

We note that Zeebe (2012) implements the scheme slightly differently to ours, by subtracting fluxes of carbonate and silicate weathering from the atmosphere, but by doubling the silicate flux to account for the net removal of CO2 from the atmosphere (balanced by volcanic emissions). In Zeebe (2012) when CO2 is returned to the atmosphere from precipitation of CaCO3 in the ocean surface boxes, there is a net zero direct flux of CO2 from the atmosphere from carbonate weathering and a direct flux of CO2 from the atmosphere of 1 mol from silicate weathering.

The SCP-M model code for carbonate and silicate weathering

Below is the description and extract of the original model code presented in O'Neill

et al. (2019) as referenced by the reviewer. A revised model code, incorporating the changes described in this response, will be uploaded with the final manuscript.

Fig. 7 (see figures below) first model code extract annotations:

Line 418 shows the equation (1) above, where the carbonate rock weathering (RVCARB) is calculated from atmospheric CO2 with the WCARB parameter.

Line 419 shows the equation (X) above where the silicate rock weathering (RVSIL) is calculated from atmospheric CO2 and a constant.

Line 420 the silicate weathering amount to be directly subtracted from the atmosphere, as described above, “weaths”, is identified.

Line 423 Volcanic emissions is set to equal “weaths”, the direct (net) amount of CO2 taken from the atmosphere by silicate weathering, as described above.

In line 425-428 there is the option to apply an input value for volcanic emissions instead of setting it to equal silicate weathering. This is for the model runs with analysis of anthropogenic emissions/short time frames and is switched off for our experiments.

In Line 431 the net effect of volcanic emissions and silicate weathering on atmospheric CO2 is calculated

In Line 432 the above terrestrial fluxes of carbon can be disabled by a switch (for sensitivities and model testing) via “TerrestrialGeo” (1 is on, 0 = off).

In line 435 the δ13C of silicate weathering drawdown of CO2 from the atmosphere is set to the hardwired value of -6.90. In the revised model code it is now set to atmospheric δ13C within each model time step.

In line 436 the δ13C for silicate weathering direct atmospheric CO2 flux and volcanic emissions of CO2 are applied to their fluxes of carbon and converted to molar concentrations in the atmosphere. (we have now amended the volcanic emissions δ13C to a value of -4.5 per mil).

In line 437 the terrestrial δ13C fluxes can be switched on or off (for model testing or sensitivity) via “TerrestrialGeo” (1 is on, 0 = off).

In line 438 the radiocarbon content (zero, dead) of volcanic emissions and weathering fluxes is applied.

In line 442 both RVCARB and RVSIL fluxes of carbon are added to the surface ocean box via river flux.

In line 443 alkalinity flux is added to the surface box in ration 2:1, leading to a lowering in pCO2 in the surface box and a drop in atmospheric CO2.

Fig. 8 below, second model code extract line by line decscriptions:

In line 475 ocean δ13C is calculated. The river flux of C (derived from weathering) is introduced to the surface ocean box with a δ13C of the standard value “Sstand” (δ13C=0) as discussed above. The dissolution of marine carbonates also introduces carbon with the standard value for δ13C (δ13C = 0) to the ocean boxes.

In line 481 the net fluxes of volcanic emissions and silicate weathering drawdown of CO2 are added to the equation for atmospheric CO2.

In line 484 the δ13C of net fluxes of volcanic emissions and silicate weathering drawdown of CO2 are added to the equation for atmospheric δ13C.

The confusion with the reviewer likely comes from our comment in the model code in line 416 “# As per Toggweiler (2008) only silicate weathering is a sink of CO2 from the atmosphere”. We will delete this statement as it is a poor descriptor.

In addition, we should modify the following comment “# Weathering of carbonate rocks is a source of carbon to the low latitude surface ocean via rivers” with “…source of carbon and alkalinity” Therefore, it is indeed the case in SCP-M that both carbonate and silicate weathering ultimately work as sinks of atmospheric CO2 by altering the surface boxes’ alkalinity.

The third code extract below (Fig. 9) shows the values chosen for weathering input parameters, as described in the text above. At line 321, weathδ13C is the value that was applied to silicate weathering, NOT carbonate weathering as assumed by the reviewer.

Attachment B

Collatz/Kohler and Fischer terrestrial biosphere C3%/C4% python script and last glacial-interglacial cycles of atmospheric CO2 and temperature data, constructed for this author response:

https://zenodo.org/record/3889704#.XuIDri1L0_V

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Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2019-146/cp-2019-146-AC3-supplement.pdf

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Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2019-146, 2019.

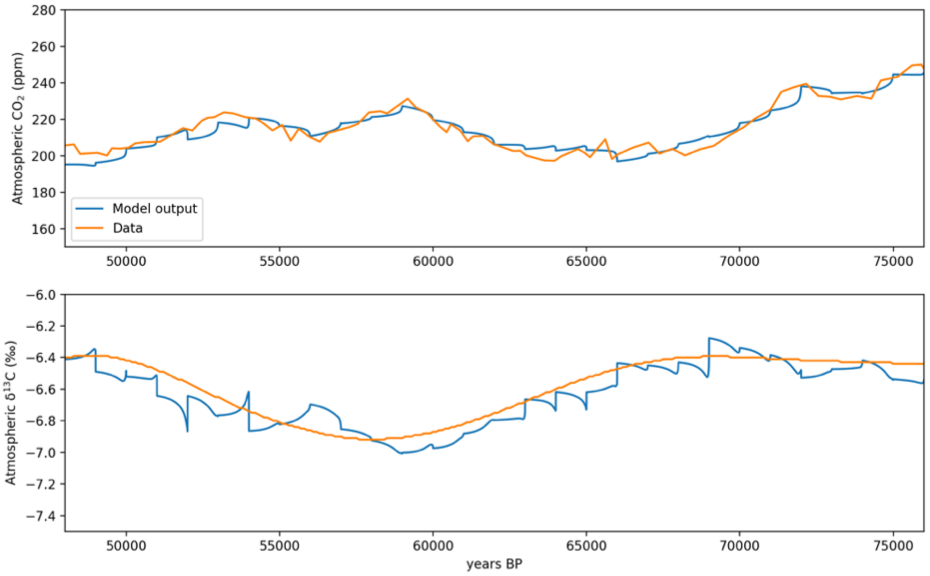


Fig. 1. 1kyr-interval model results for MIS 3-5 compared to proxy data for atmospheric CO2 (top panel) and atmospheric δ13C (bottom panel), using non MIS-averaged model inputs from the manuscript

Parameter (units)	**Value**
Terrestrial biosphere δ^{13}C (‰)	-23
Marine biological productivity δ^{13}C (‰)	-19
Carbonate weathering DIC flux δ^{13}C (‰)	0
Silicate weathering CO$_2$ flux δ^{13}C (‰)	Atmosphere δ^{13}C
Volcanic CO$_2$ δ^{13}C (‰)	-4.5
Marine carbonate δ^{13}C (‰)	0
Air-sea δ^{13}C fractionation factors	0.9989-0.999
Air-sea D14C fractionation factors	0.98-0.998
Volcanic CO$_2$ emissions (mol (GtC) yr^{-1})	6 x 10^{12} (0.1)
Carbonate weathering flux of C (mol m^{-3} yr^{-1})	1.5
Silicate base weathering flux of C (mol m^{-3} yr-1)	7.5 x 10^{-3}
Silicate weathering slope with respect of atmospheric CO$_2$ (m^{-3} yr^{-1})	0.7
Calculated carbonate weathering flux of C at 275 ppm atmospheric CO$_2$ (Tmol yr^{-1})	10
Calculated carbonate weathering flux of C at 190 ppm atmospheric CO$_2$ (Tmol yr^{-1})	7
Calculated silicate weathering flux of C at 275 ppm atmospheric CO$_2$ (Tmol yr^{-1})	6
Calculated silicate weathering flux of C at 190 ppm atmospheric CO$_2$ (Tmol yr^{-1})	5
Terrestrial NPP interglacial base rate (PgC yr^{-1})	66

Fig. 2. Table 1: parameterisation of various fluxes of C and δ13C in the modelling experiments

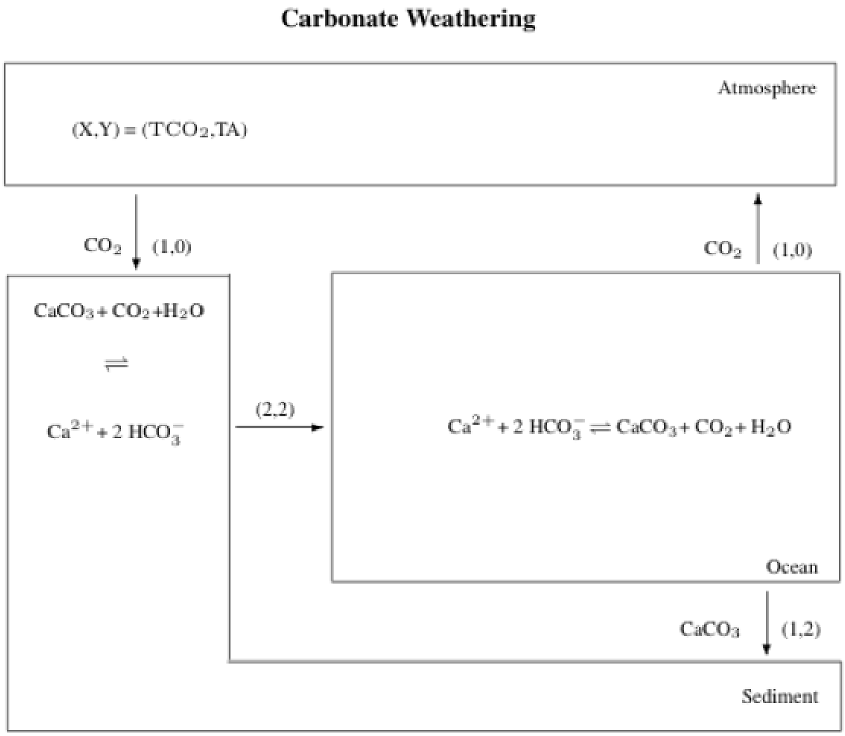


Fig. 3. Extract of the Zeebe (2012) schematic description of carbonate weathering

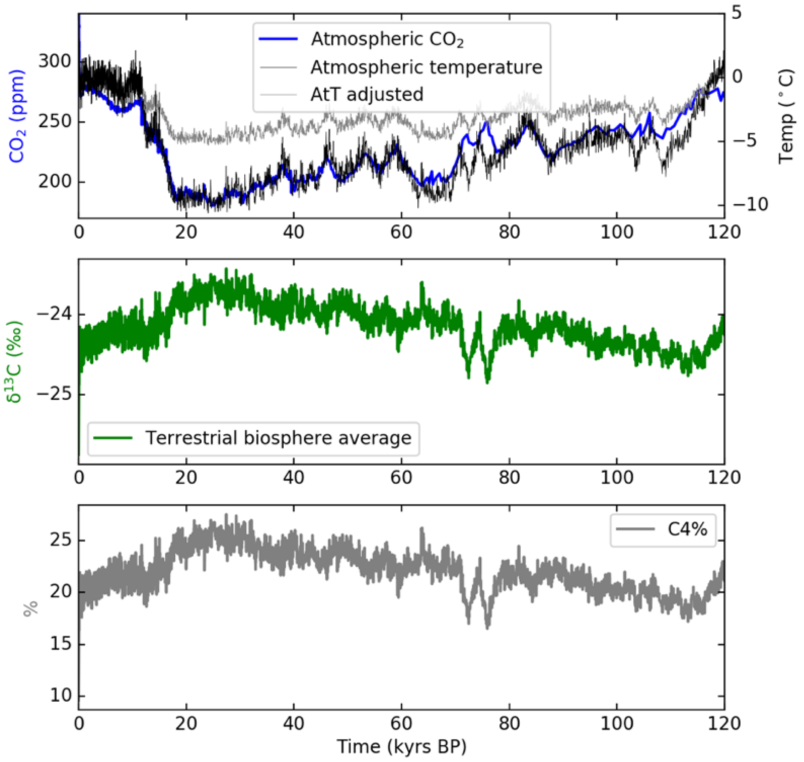


Fig. 4. Modelling of the share of C4 photosynthetic plants (C4%) (bottom panel) and average terrestrial biosphere δ13C fractionation factor (middle panel) as a function of atmospheric CO2 and temperature

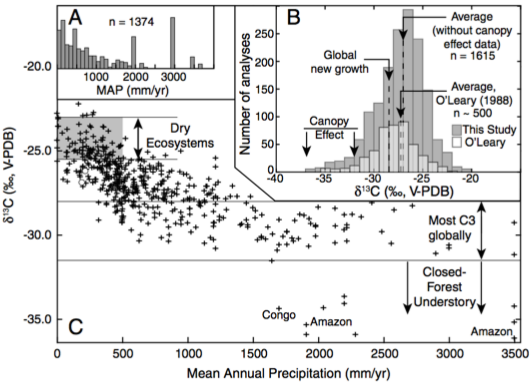


Fig. 1. (*A*) Histogram of MAP values for isotopically charact.erized C3 plants, showing emphasis on relatively arid ecosystems (MAP ≤ 500 mm/yr) and tropical rainforests (spikes at MAP ~ 2,000, 3,000 mm/yr). (*B*) Histogram of δ^{13}C values of modern C3 plants. Data compiled in this study average −27.0‰, excluding analyses from the understory of closed-canopy forests. Estimated global average composition, based on global trends in precipitation and vegetation, is approximately −28.5‰, significantly lower than typically assumed. An accurate average δ^{13}C value for C3 plants is needed for accurate models of carbon fluxes, atmospheric CO_2 compositions, and soil organic matter. (*C*) δ^{13}C values vs. MAP showing increasing δ^{13}C with aridity. Data sources are listed in *SI Text*. White dots are average compositions of data from a large collection made in a single month during a wet year (35).

Fig. 5. The Figure below is reproduced from Kohn (2010), and shows the range in δ13C fractionation for C3 plants alone, which spans -20 to -37 per mil, and is impacted by many factors including temp, precip

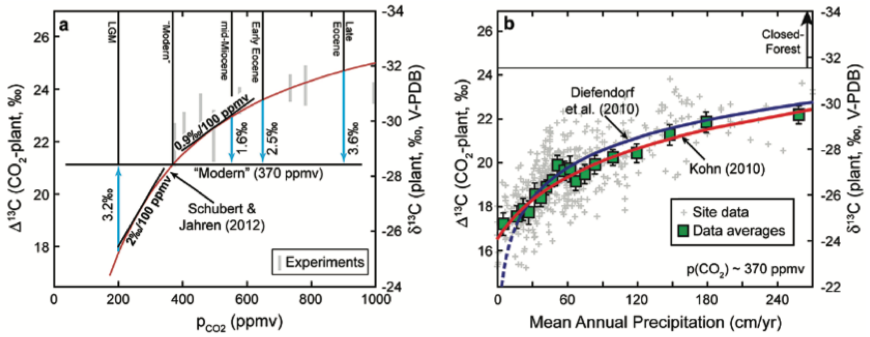
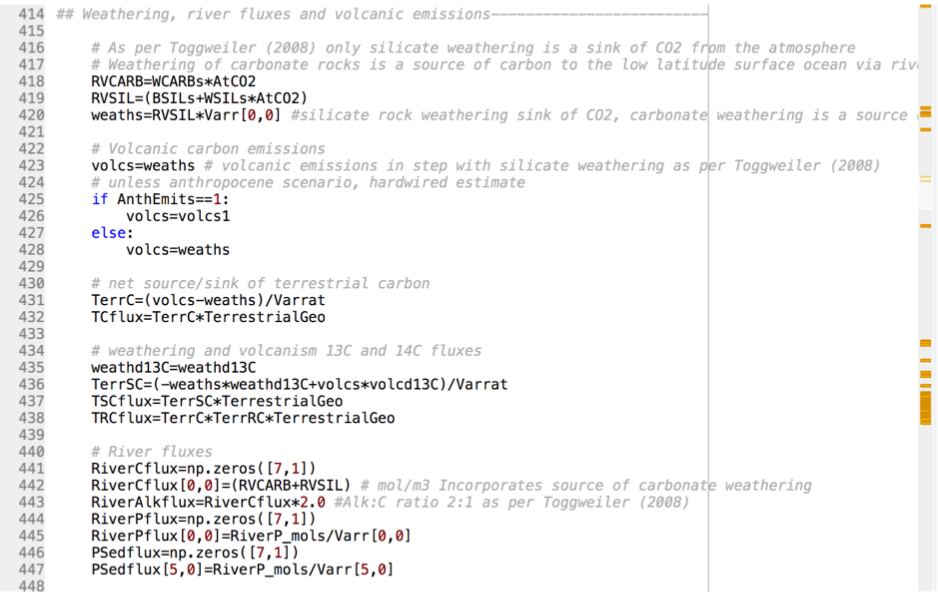


Figure 1 Proposed models for factors that influence δ^{13}C of C3 plants. **(a)** p_{CO2}. Differences are illustrated between geological conditions vs. AD 2000 (p_{CO2} = 370 ppmv, average δ^{13}C = -28.5 for C3 biomass). LGM = Last Glacial Maximum. Note inverse relationship between δ^{13}C and Δ^{13}C. Experiments are for above-ground biomass (Schubert and Jahren, 2012), shifted to fit preferred curve. **(b)** Mean annual precipitation (data and data averages from Kohn, 2010).

Fig. 6. Extract from Kohn (2016) showing the range d13C of C3 plants and effects of pCO2 and precipitation



```
## Weathering, river fluxes and volcanic emissions------------------------

    # As per Toggweiler (2008) only silicate weathering is a sink of CO2 from the atmosphere
    # Weathering of carbonate rocks is a source of carbon to the low latitude surface ocean via riv
    RVCARB=WCARBs*AtCO2
    RVSIL=(BSILs+WSILs*AtCO2)
    weaths=RVSIL*Varr[0,0] #silicate rock weathering sink of CO2, carbonate weathering is a source

    # Volcanic carbon emissions
    volcs=weaths # volcanic emissions in step with silicate weathering as per Toggweiler (2008)
    # unless anthropocene scenario, hardwired estimate
    if AnthEmits==1:
        volcs=volcs1
    else:
        volcs=weaths

    # net source/sink of terrestrial carbon
    TerrC=(volcs-weaths)/Varrat
    TCflux=TerrC*TerrestrialGeo

    # weathering and volcanism 13C and 14C fluxes
    weathd13C=weathd13C
    TerrSC=(-weaths*weathd13C+volcs*volcd13C)/Varrat
    TSCflux=TerrSC*TerrestrialGeo
    TRCflux=TerrC*TerrRC*TerrestrialGeo

    # River fluxes
    RiverCflux=np.zeros([7,1])
    RiverCflux[0,0]=(RVCARB+RVSIL) # mol/m3 Incorporates source of carbonate weathering
    RiverAlkflux=RiverCflux*2.0 #Alk:C ratio 2:1 as per Toggweiler (2008)
    RiverPflux=np.zeros([7,1])
    RiverPflux[0,0]=RiverP_mols/Varr[0,0]
    PSedflux=np.zeros([7,1])
    PSedflux[5,0]=RiverP_mols/Varr[5,0]
```

Fig. 7. Original model code extract showing silicate and carbonate weathering equations (see line by line descriptions in Attachment A)

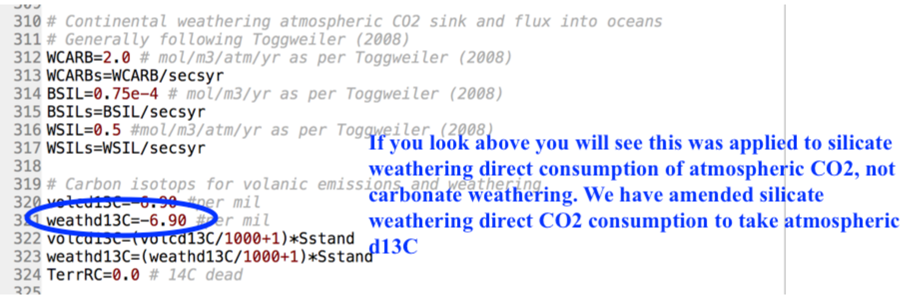
```
## Step forward model calculations-----------------------------------------

    # Model equations

    # Ocean boxes
    Parr = Parr +dt*secsyr*(np.dot(PhysMat, Parr)+BioP+RiverPflux*Rivers-PSedflux*Rivers)
    Carr = Carr + dt*secsyr*(np.dot(PhysMat, Carr)+BioC+cflux+NetCflux+RiverCflux*Rivers)
    Alkarr = Alkarr + dt*secsyr*(np.dot(PhysMat, Alkarr)+NetAlkflux+RiverAlkflux*Rivers)
    Fearr = Fearr + dt*secsyr*(np.dot(PhysMat, Fearr)+BioFe)
    Siarr = Siarr + dt*secsyr*(np.dot(PhysMat, Siarr)+BioSi)
    Oarr = Oarr + dt*secsyr*(np.dot(PhysMat, Oarr)+BioO)
    SCarr = SCarr+ dt*secsyr*(np.dot(PhysMat, SCarr)+BioSC+Scflux+NetCflux*Sstand+
                              RiverCflux*Sstand*Rivers)
    SCratio=SCarr/Carr
    RCarr = RCarr + dt*secsyr*(np.dot(PhysMat, RCarr)+BioRC+Rcflux+NetCflux*(RCarr/Carr)-(RCD1*R

    # Atmosphere
    AtCO2 = AtCO2 + dt*secsyr*(Atcflux+TCflux-((CFert-Respire)/Varrat)+
                               ((AnthEmit+DeforestC)/Varrat)*AnthEmits)
    pCO2a=np.append(pCO2,AtCO2) # create an array of all pCO2
    SCAt = SCAt+dt*secsyr*(AtSCflux+TSCflux-((CFert-Respire)/Varrat)*TerrBioSC*
                           TerrestrialBios+(AnthSC1+DeforestSC)/Varrat
                           *AnthEmits)
    RCAt = RCAt+dt*secsyr*(AtRCflux+RCS1At/Varrat-RCD1At*RCAt*Varrat/Varrat-TRCflux-((CFert-Resp
                           *TerrestrialBios+((AnthRC1+DeforestRC)/Varrat)*AnthEmits+(Bomb14C/Var
```

Fig. 8. Original model code extract showing application of reference standard d13C (0 per mil) to weathering fluxes to the ocean (see line by line descriptions in Attachment A)



```
# Continental weathering atmospheric CO2 sink and flux into oceans
# Generally following Toggweiler (2008)
WCARB=2.0 # mol/m3/atm/yr as per Toggweiler (2008)
WCARBs=WCARB/secsyr
BSIL=0.75e-4 # mol/m3/yr as per Toggweiler (2008)
BSILs=BSIL/secsyr
WSIL=0.5 #mol/m3/atm/yr as per Toggweiler (2008)
WSILs=WSIL/secsyr

# Carbon isotops for volanic emissions and weathering
volcd13C=-6.90 #per mil
weathd13C=-6.90 #per mil
volcd13C=(volcd13C/1000+1)*Sstand
weathd13C=(weathd13C/1000+1)*Sstand
TerrRC=0.0 # 14C dead
```

If you look above you will see this was applied to silicate weathering direct consumption of atmospheric CO2, not carbonate weathering. We have amended silicate weathering direct CO2 consumption to take atmospheric d13C

Fig. 9. Original model code showing the d13C signature (-6.90 per mil) originally applied to silicate weathering