Climate
of the Past

Discussions

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 #3 and author responses

AC: We thank the reviewer for their comments, suggestions and input into this manuscript. These comments make a strong contribution to improving the quality of our work. Please see below our responses to the individual comments.

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.

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Please note that we have changed our treatment of ocean $\delta13C$ proxy data, stemming from one of the other reviewer comments, to only include $\delta13C$ 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.

Major comments:

RC 1) The "data analysis" section 3 presents the changes in atm. CO2, d13CO2, oceanic d13C, D14C and CO3(2-) as inferred from proxy records from the LIG to the LGM. This is obviously a huge task, but which I am afraid can give rise to approximations and simplifications. I would consider seriously amending this section. How can the "increase in d13C across the glacial cycle be attributed to the growth of tundra at high latitudes"? (p12, L. 2-3).

AC: Thanks for the comment. In this instance (P12, L2-3 in the original manuscript) and throughout our manuscript, we have been a bit loose with our references to tundra, permafrost and peat, as you point out in this comment and a few below.

What we mean to refer to here is the storage of carbon by the accumulation and freezing, or burial, of peat and other soil organic matter under soil overburden, and growth of cold-climate vegetation, throughout the glacial cycle (e.g. Tarnocai et al., 2009; Ciais et al., 2012; Schneider et al., 2013; Eggleston et al., 2016; Treat et al., 2019).

We have corrected the statement on P12 L2-3, and expanded a bit, including a few more references and other possible causes of the atmospheric $\delta13C$ pattern, now at P15 L10:

"Atmospheric $\delta13C$ (Fig. 4(B)) increased by âĹij0.4‰ between the penultimate interglacial (MIS 5e) and the Holocene (MIS 1), with temporary falls at MIS 5d, MIS 4 and in

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the last glacial termination (between MIS 1 and 2). The cause of the observed increase in atmospheric δ13C across the last glacial-interglacial cycle may be the effect of accumulation and freezing, or burial in glacial sediments, of peat and other soil organic matter at the high latitudes (e.g. Tarnocai et al., 2009; Ciais et al., 2012; Schneider et al., 2013; Eggleston et al., 2016; Ganopolski and Brovkin, 2017; Treat et al., 2019). According to Treat et al. (2019), peatlands and other vegetation accumulated carbon in the relatively warm periods, and these carbon stocks were then frozen and/or buried in glacial and other sediments during the cooler periods, throughout the last glacial cycle. This buried or frozen stock of carbon persists to the present day (Tarnocai et al., 2009), although according to Ciais et al. (2012) it may be smaller now than in the LGM. Schneider et al. (2013) evaluated several possible candidates for the rising atmospheric δ13C pattern across the last glacial-interglacial cycle and could not discount any of (1) changes in the carbon isotope fluxes of carbonate weathering and sedimentation on the seafloor, (2) variations in volcanic outgassing or (3) peat and permafrost build-up throughout the last glacial-interglacial cycle.âĂĺ

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 amount 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. The reduction in atmospheric δ13C at the last glacial termination, between MIS 1 and MIS 2, coincident with a large atmospheric CO2 increase, is attributed to the release of deep-ocean carbon to the atmosphere re-

sulting from increased ocean circulation and Southern Ocean upwelling (Schmitt et al., 2012). The subsequent rebound of δ13C in the termination period and the Holocene is believed to result from terrestrial biosphere regrowth, in response to increased CO2 and carbon fertilisation (Schmitt et al., 2012; Hoogakker et al., 2016). " Other amendments to this section are shown in track changes. RC: p12, L. 11-14: How were the values for MIS3 DD14C in the Atlantic derived? From Fig. 6a, it looks like there is no data across MIS3.

AC: Thanks, this was a charting error and now the chart has been corrected to show the data for MIS 3.

RC: This is quite a shortcut to explain the deglacial D14C decrease, and maybe you want to check the references and include " increase in Southern Ocean ventilation" above anything else.

AC: P16, L4 modified to "….an acceleration in atmospheric Δ14C decline at the last glacial termination is attributed to the release of old, 14C -depleted waters from the deep ocean, due mainly to increased Southern Ocean upwelling (e.g. Sikes et al., 2000; Marchitto et al., 2007; Skinner et al., 2010; Burke and Robinson, 2012; Siani et al., 2013; Skinner et al., 2017)." RC: P14, L. 5-6: This reads like speculation.

AC: This sentence re-worded as: (P17, L35) "There is a modest drop in abyssal Atlantic Ocean CO2-3 at MIS 5b (-13 μmol kg-1 relative to MIS 5c), which coincides with a minor drop in abyssal Atlantic Ocean δ13C (-0.19‰ and atmospheric CO2 (-14 ppm), indicating a common link. Menviel et al. (2012) modelled a transient slowdown in North Atlantic overturning circulation for this period, which could explain these features. "

RC: 2) Fit with the data: 50 umol/L as an "arbitrary standard deviation' for [CO3] is huge and represents more than the [CO3] changes (0-30 umol/L) recorded across the G-IG cycles. How much was taken for the standard deviation for d13C and D14C? It looks quite large. Figures 9-11 would gain in having a more appropriate range in the y axis. At the moment the ranges and std are large, so that it almost looks like there are

no changes from MIS5 to MIS 2.

AC: Re CO2-3. In response to this reviewer's comments, and a change to our data approach from the other reviewer comments (using only Cibicides species for δ13C), we have been able to reduce our default standard deviation for ocean CO2-3 from 50 umol kg-1 to 15 umol kg-1, a substantial improvement. The rationale for setting the CO2-3 SD at an artificial level for the weighting in our model-data optimisation is dealt with in Section 2.3.2. This is an unfortunate feature of using a box model with large boxes and applying sparse proxy data. The relatively small number of CO2-3 data points in clustered locations leaves relatively small standard deviations, giving CO2-3 a disproportionate weighting in the model-data optimisation versus the other proxies. Therefore, we overcome the issue by scaling up the CO2-3 standard deviations and applying as default across all boxes and MIS time slices.

Re δ13C and Δ14C. The standard deviations are calculated from box-averaged published proxy data and shown in the supporting information. The standard deviations look large for these box-averaged and MIS-averaged values, because the boxes in the box model are large. The ocean box δ13C standard deviation is now lower in the revised manuscript due to filtering out only Cibicides species, from the other reviewer comments.

The issue of box size and standard deviation is addressed again in the discussion of limitations of the study (P34 L7):

"However, given the large spatial coverage of the SCP-M boxes, data for large areas of the ocean are averaged, and some detail is lost. For example, in the case of the carbonate ion proxy, we apply a default estimate of standard deviation to account for the large volume of ocean covered by SCP-M's boxes relative to the proxy data locations, and to enable the normalisation of the carbonate ion proxy data in a procedure that uses the data standard deviation as a weighting. Despite this caveat, we argue that the model-data experiment results provide a good match to the data across the various

atmospheric and ocean proxies as shown in Figs 9-11."

Re Figs 9-11. The standard deviation ranges for CO2-3 and δ13C are now narrower following the improvements we have made, which improves the resolution of Figs 9-11. In addition, we have expanded y-axes where we can to help with reading the figures.

RC: 3) References: In general I find that only a few references are used over and over and sometimes not appropriately. A few additional references are included in this review. Please note the typo throughout the document in "Ridgwell".

AC: Thanks, we've now added the references suggested by the reviewer, throughout the manuscript, and we corrected the typo for Ridgwell throughout.

References added following this reviewers' comments:

Watson et al., 2000 Joos et al, 2004 Tarnocai et al, 2009 Ganopolski et al., 2010 Menviel et al., 2012 Menviel and Joos, 2012 Brovkin et al, 2012 Jaccard et al, 2013 Schneider et al., 2013 Menviel et al, 2015 Yu et al, 2016 Ganopolski and Brovkin, 2017 Lindgren et al, 2018 Mauritz et al, 2018 Yamamoto et al, 2019 Treat et al, 2019

Specific comments:

RC: 1) Abstract: The first line does not make sense. Please reformulate. L. 3 Please add "SO" in front of "biological productivity"

AC: Re-formulated as: "We conduct a model-data analysis of the marine carbon cycle to understand and quantify the drivers of atmospheric CO2 during the last glacial cycle".

Southern Ocean added to the sentence P1 L3.

RC: 2) Introduction: - L.15-19: please be more specific. Instead of "Ocean biology" you might want to refer to "iron fertilisation and its impact on nutrient utilisation", or changes in remineralisation depth (e.g. Kwon et al. 2009).

AC: text modified to (P2 L18):

"Hypotheses for an ocean biological role include the effects of iron fertilisation on biological export productivity (e.g. Martin, 1990; Watson et al., 2000; Martinez-Garcia et al., 2014), the depth of remineralisation of particulate organic carbon (POC) (e.g. Matsumoto, 2007; Kwon et al., 2009; Menviel et al., 2012), changes in the organic carbon:carbonate ("the rain ratio") or carbon:silicate constitution of marine organisms (e.g. Archer and Maier-Reimer, 1994; Harrison, 2000), and increased biological utilisation of exposed shelf-derived nutrients such as phosphorus (e.g. Menviel et al., 2012)."

RC: What do you mean by composite mechanisms?

AC: we have amended this to "the aggregate effects of several mechanisms" throughout the document

RC: It would be good to also introduce the numerous modelling studies that have been done on the topic of G-IG changes in pCO2, and notably transient simulations of the G-IG trying to understand the changes in pCO2 (e.g. Ganopolski & Brovkin 2017, Menviel et al., 2012).

AC: Thanks, we have added to our 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

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model. According to Brovkin et al. (2012), a ≈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."

And also, a few lines down to explain how our approach differs (P3 L23):

"Our modelling approach differs from other model studies of the last glacial-interglacial cycle (e.g. Ganopolski et al., 2010; Menviel et al., 2012; Brovkin et al., 2012; Ganopolski and Brovkin, 2017), in that we constrain several physical processes from observations (SST, sea level, sea-ice cover, salinity, coral reef fluxes of carbon), then solve for the values of model parameters for ocean circulation and biology based on an optimisation against atmospheric and ocean proxy data. " And at P8 L14:

"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). "

RC: 3) Methods: - Variables included in the model: surely the model includes Dissolved Inorganic Carbon.

AC: yes, the model includes DIC and we have added DIC to the sentence.

RC: By "CO2", do you mean atmospheric CO2?

AC: yes, we have added "atmospheric" to the sentence at P3 L33.

RC: Does the model really includes "carbonate ions" as a prognostic tracer?

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AC: Yes. SCP-M calculates CO2-3 concentration in umol kg-1, by calculating the three species of DIC. First, pCO2 is calculated using the method of Follows et al (2006) which takes as inputs DIC, alkalinity, pH, SST, salinity and phosphorus in each box in the model. Then H2CO3, HCO3- and CO2-3 are calculated using coefficients for the solubility of CO2 (K0) and coefficients for carbonic acid of K1 and K2 using Lueker et al (2000). In the model documentation paper (O'Neill et al, 2019) the SCP-M model estimates for CO2-3 in a modern ocean setting are demonstrated to align with modern data from the ocean, using data from Key at al (2004).

We have added a summary sentence to describe this, in section 2.1 "Model description" on P4.

RC: p4, L. 2: please refer to section 2.2.1 and Figure 2.

AC: Added

RC: p7: I am very confused by the treatment of the terrestrial biosphere in the model and the paragraph L. 19-27. It reads like there is an interactive terrestrial module. But how can NPP be calculated with significance if there is no atm. Temperature or precipitation in the model?

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 use a two-box terrestrial box model scheme, presented in 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 two boxes, 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 Har-

man et al (2011), and extract is reproduced here:

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

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.

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 ($\sim$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), CO_2 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 CO_2, while the climate has more influence on the isotopic composition. Otto et al (2002) also found that the CO_2 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 CO_2 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 CO_2 is an important, or is the main driver of terrestrial biosphere productivity include Kicklighter et al (1999), Joos et al. (2001), Schimel et al. (2015), Sitch et al. (2008), Arneth et al (2017)). This view has been contested by Francois et al (1999) and 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 CO_2 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 CO_2 fertilisation, or CO_2 fertilisation + climate, were included as drivers of NPP – but noting this was not tested for the LGM.

In summary, 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 CO_2 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, which our result of 630 Pg C sits comfortably within (Prentice et al, 2011) 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.

If there is some reason to examine the terrestrial biosphere in more detail, we suggest for our study this would be done simply by a sensitivity, as applied in Menviel et al (2016) with regard to C3/C4 plants and the relative proportional influence of C3 and C4 plants on terrestrial biosphere δ13C fractionation.

We have added some text to explain that we have a simplified representation of the terrestrial biosphere employing CO_2 fertilisation, and that we don't take account of temperature and precipitation, in the methods section, P5 L24. This also includes discussion of the isotopic fractionation factor in response to one of the other reviewers:

"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 CO_2 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."

RC: Why is "tundra" discussed with such emphasis in this paragraph?.

AC: Thanks for picking up on this. We have substantially revised this paragraph as follows (P10 L25):

"The terrestrial biosphere module in SCP-M does not explicitly represent the carbon stored in buried peat, permafrost and also cold-climate vegetation that may have expanded its footprint in the glaciation, such as tundra biomes (e.g. Tarnocai et al., 2009; Ciais et al., 2012; Schneider et al., 2013; Eggleston et al., 2016; Ganopolski and Brovkin, 2017; Treat et al., 2019). The freezing and burial of organic matter across the glacial cycle may significantly imprint the terrestrial biosphere CO2 size and δ13C signature (Tarnocai et al., 2009; Ciais et al., 2012; Schneider et al., 2013; Eggleston et al., 2016; Ganopolski and Brovkin, 2017; Mauritz et al., 2018; Treat et al., 2019). Schneider et al. (2013) and Eggleston et al. (2016) both observed a permanent increase in atmospheric δ13C during the last glacial cycle, of âĽij0.4‰ and attributed its cause likely due to soil storage of carbon in peatlands which were buried or frozen as permafrost as the glacial cycle progressed. Ganopolski and Brovkin (2017) incorporated permafrost, peat, and buried carbon into their transient simulations of the last four glacial- interglacial cycles, observing that these features dampened the amplitude of glacial-interglacial variations in terrestrial biosphere carbon stock, in the CLIMBER-2 model. As a crude measure to account for this counter-CO2 cycle storage of carbon in the terrestrial biosphere and frozen soils, we force the terrestrial biosphere productivity parameter in SCP-M in the range âĽij+5-10 PgC yr-1, increasing into the LGM (MIS 2), and maintained in the Holocene (MIS 1). We maintain the forcing of the terrestrial biosphere in the Holocene, as the posited effects of buried peat and permafrost storage of carbon on atmospheric CO2 and δ13C during the lead-up and into the LGM, were likely not fully reversed after the glacial termination (Tarnocai et al., 2009; Eggle-

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ston et al., 2016; Mauritz et al., 2018; Treat et al., 2019), and were partially or wholly replaced by other soil stocks of carbon (e.g. Lindgren et al., 2018). SCP-M calculates net primary productivity (NPP) using this productivity input parameter, as a function of carbon fertilisation (Harman et al., 2011)."

RC: Tundra is not an "inert" carbon pool

AC: we've modified the sentence as per above excerpt to refer to carbon stored in frozen peat, permafrost soils.

RC: and I don't think "permafrost" is a vegetation type

AC: We've modified this sentence as per above excerpt, to remove the reference to permafrost as a vegetation type.

RC: What is "pre-carbon fertilisation"?

AC: This is just the Npre in the equation for NPP from the model documentation, reproduced above. We can refer to this as "undisturbed" (by CO2) NPP. The equations for NPP takes an input value Npre, which is subsequently varied due to any change in atmospheric CO2. This is our model representation of CO2 fertilisation of the terrestrial biosphere.

RC: p8: what is the point of Table 1 if all the values of GOC, AMOC, biology are the same? It would be interesting to mention the PI control values though.

AC: Thanks, we've consolidated Table 1 to show the MIS model-data experiment ranges and the PI control values.

RC: - p10-11: The 'depth issue" should also be discussed in 2.3.1 and 2.3.2.

AC: Re 2.3.1 – there is a much greater coverage of δ13C and Δ14C data for the ocean boxes so we have not applied a default weighting for those data in our model-data optimisation. For CO2-3, a problem presents because there are only 1 or 2 data points in some boxes, and they are clustered near the box boundary, so we end up with

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unrepresentative data for some boxes for CO2-3. So, we applied a larger weighting for CO2-3 data, as discussed in 2.3.2.

4) Discussion:

RC: p20, L. 3-6: It is not what the simulations tell you, but the proxy data!

AC: We've removed this reference to the modelling and replaced with reference to the proxy data shown in Figure 4 (P23, L7).

RC: p21, L. 1-2: This is wrong → you are forcing your model with SST, Sea-ice. . .. so all these factors contribute to the pCO2 decrease. The experiments show that changes in oceanic circulation and SO biological productivity also contribute to that pCO2 decrease.

AC: We have reworded this sentence to list the full set of changes modelled (P24 L7)

RC: Please take into consideration that G-IG pCO2 changes have been previously successfully simulated with models of intermediate complexity (e.g. e.g. Ganopolski & Brovkin 2017, Menviel et al., 2012) and box models.

AC: We have added a sentence at the start of the discussion to reference these studies (P23, L5) and they are referenced throughput the Discussion.

RC: p21, L. 3-4: I don't understand the meaning

AC: This sentence has been reworded (P24 L6).

RC: p21, L. 7: Might want to check Piotrowski et al., 2008, Yu et al., 2016. (Nat. Geo).

AC: We have picked up the citation of Yu et al (2016) in reference to AMOC in the MIS 4, a little further down in the manuscript (P29 L28). We have added a reference to Piotrowski et al (2009) in the same place (P29 L29).

We have also added the Piotrowski et al (2009) δ13C data to our dataset and cited it in the manuscript (Table 2).

RC: p21, L. 10 –p22, L. 5: This section really has to be discussed in light of all the work that has been done on the impact of iron fertilisation in the Southern Ocean. Some work on the topic: Watson et al., 2000, Nature; Jaccard et al., 2013, Science; Yamamoto et al., 2019, Climate of the Past;

AC: Text added (P31 L2):

"Our finding of increased biological productivity, while mostly constrained to MIS 2 and MIS 4, and a modest contributor to the overall glacial CO2 drawdown, corroborates proxy data (e.g. Martinez-Garcia et al., 2014; Lambert et al., 2015; Kohfeld and Chase, 2017) and recent model-data exercises (e.g. Menviel et al., 2016; Muglia et al., 2018; Khatiwala, 2019). Martin (1990) pioneered the "iron hypothesis", which invoked the increased supply of continent-borne dusts to the Southern Ocean in glacial periods. Increased dust supply stimulated more plankton productivity where plankton were bio-limited in nutrients supplied in the dust, such as iron (Martin, 1990). Since then, the iron hypothesis has retained an important place in the debate over glacial-interglacial cycles of CO2. Watson et al. (2000) took experimental data on the effects of iron supply on plankton productivity in the Southern Ocean (Boyd, 2000) and applied this to a carbon cycle model across glacial- interglacial cycles. Their modelling, informed by the ocean experiment data, suggested that variations in the Southern Ocean iron supply and plankton productivity could account for large (âĹij40 ppm) swings in atmospheric CO2, with peak activity in the last glacial cycle at MIS 2 and MIS 4. Debate has continued over the magnitude of the contribution of Southern Ocean biological productivity to the glacial CO2 drawdown. According to Kohfeld et al. (2005), based on sediment data, the Southern Ocean biological productivity mechanism could account for no more than half of the glacial CO2 drawdown. Others emphasise that Southern Ocean biological export productivity fluxes may have been weaker in the LGM, in absolute terms, but that with weaker Southern Ocean upwelling, the iron-enhanced productivity contributed to a stronger biological pump of carbon and was a major contributor to the LGM CO2 drawdown (Jaccard et al., 2013; Martinez-Garcia et al., 2014; Yamamoto et al., 2019).

" RC: p22, L. 18: "sea-ice cover"

AC: Thanks, corrected

RC: p23, L. 1-12: Figure 13 is interesting but care has to be taken here given the large size of the "boxes". This should at least be discussed in light of previous modelling studies on the subject (e.g. Menviel et al., 2015, GBC).

AC: This figure has changed from the original manuscript due to a change in our data method for δ13C, stemming from the other reviewer comments. We are now only using Cibicides species δ13C data, and we re-ran our model-data experiments. There are only slight variations to our model-data results. However, a narrower spread of standard deviations of the δ13C data necessitates us to change this Figure. We do think it's an important figure that provides some insights into our model, the results in this manuscript and how they might differ from other studies that simply rely on qualitative and simple statistical analysis of proxy data (without models).

Text added P29 L3:

"These observations from Fig. 13 could be exaggerated in SCP-M due to the large size of its ocean boxes and therefore relatively large spread of δ13C values and standard deviations for each box. In addition, this experiment may reflect idiosyncrasies in the SCP-M model design and its simple parameterisation of ocean circulation and mixing. A finer resolution model may show a greater sensitivity of the ocean box δ13C to variations in ocean circulation. Menviel et al. (2015) analysed the sensitivity of ocean and atmospheric δ13C to variations in NADW, AABW and North Pacific Deep Water (NPDW) formation rates, in the context of rapid changes in atmospheric δ13C and CO2 observed during the last glacial termination. Their modelling, using the more spatially-detailed LOVECLIM and Bern3D models, showed modest but location-dependent sensitivities of ocean δ13C to slowing ocean circulation, and particular sensitivity to AABW. These models are much higher resolution and show greater sensitivity of δ13C to ocean circulation over depth intervals not differentiated in the SCP-M boxes,

but also quite a variation across the LOVECLIM and Bern3D models. However, our simple experiment illustrated in Fig. 13 does highlight the potential for important changes in the ocean during glacial-interglacial periods to go unnoticed, when focussed on one set of ocean proxy data and without validation by modelling."

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