Response to the Interactive comment by Fortunat Joos (given in *italic* below)

The study by Victor Brovkin and colleagues is interesting. They provide results from first transient fully coupled ESM simulations covering the entire last 8000 years. This is novel and warrants publication.

We thank Fortunat Joos for his helpful and constructive comments. Indeed, transient coupled climate-carbon cycle simulation with full-scale ESM is a challenge; our runs were taking half-a-year each.

The conclusion by Brovkin et al. that shallow-water CaCO3 deposition (coral reef growth) plays a role for the late Holocene CO2 increase is similar to the conclusions from earlier studies using EMICs. A difference is that this study seems to imply that shallow water carbonate deposition is by far the most important driver for the late Holocene CO2 increase. This is a possibility, but others found additional factors such as legacy effects of earlier land carbon uptake to be equal or even more important.

We agree that the other factors, mainly legacy/memory of the deglaciation period, are relevant too. In our study, we are of course limited by (i) equilibrium assumptions of spinup and (ii) land and ocean biogeochemistry parameterizations in our model. All what we can say is that implications of excessive shallow water carbonate deposition do not violate limitations of proxies (carbonate sedimentation, carbonate ion changes).

Here below my specific comments in addition to those offered by reviewer 1.

1. Information about model drift may be helpful for the reader.

We cannot say much on the ocean biogeochemistry drift in the TRAFc experiment as there is no control experiment (without forcings) for 8,000 years, and making new simulation is not possible. The TRAF experiment was less properly initialized, as the weathering was not adjusted to changes in boundary conditions (see our response to the reviewer #1 comments), causing substantial part of surface alkalinity decreases in the TRAF experiment (Fig. 6c). This mismatch explains the initial decrease in the ocean carbon storage until 6.5 ka BP (Fig. 2a), despite of an increase in atmospheric CO_2 concentration. If TRAF would have started from an equilibrated system as TRAFc, the beginning of TRAF would have been more similar to TRAFc (close to no net co2 flux) and the uptake would have started earlier.

P4, l25-l29: I am puzzled about the large, 50%, difference in the diagnosed weathering flux between the simulations TRAF and TRAFc.

This difference is because during the spinup phase of TRAFc (as discussed in response to the referee #1) weathering rates were adjusted to match the net loss to the sediment, whereas this was not done for the spinup run of TRAF. Over 7850 years, this is large difference (2137 vs 3270 PgC in TRAF and TRAFc, respectively). On the other hand, large weathering in TRAFc is compensated by immediate surface $CaCO_3$ removal in this experiment (1224 PgC). Total alkalinity in TRAFc is affected by 3270-1224=2046 PgC weathering, which is comparable

with 2137 PgC weathering in the TRAF experiment, so that as a result net weathering in TRAFc is less than in TRAF leading to decrease in the total alkalinity. As this might be confusing, we add a comment on this in the revised manuscript. We will also add a figure with a total carbon budget (see our response below).

P3, l30: Is the ALK nudging also used during the coupled spin up 8KAFc. If not, how large is the drift in CO2? Both TRAF and TRAFc were first spin up under prescribed CO2 (260 ppm, 8KAF). The spin up is extended by an additional 100 years with an open atmosphere (simulation 8KAFc) before starting TRAFc. The weathering flux is diagnosed from the last 300 yr of the spin up. In other words, the last 200 years of 8KAF are used to diagnose the weathering for TRAF and TRAFc; the difference in the diagnosed weathering for TRAF and TRAFcarises from the other 100 years of results taken either from 8KAF or from 8KAFc. Why is there such a large difference in the diagnosed weathering flux even though 200 out of 300 years are taken from the same run? Is the model far from equilibrium? Is there a substantial model drift? Is there information from a control run available?

The alkalinity was not nudged in the spin up 8KAFc, but during the spinup procedure the alkalinity and weathering fluxes were adjusted as explained above. During the last several hundred years, the alkalinity was stable. The CO2 was fluctuating responding to the climate/weather variability was changing, but the drift in the CO2 during the last 100 years of spinup was negligible. Over such a long run, the constant weathering cannot perfectly compensate for a nonlinear evolution of the sediment.

2) The statement on geological methane emissions appears misleading and needs to be revised.

P8, line 28: "Geological sources of methane of the scale of 30-40 Tg/yr are pronounced in intergacials (Bock et al., 2017; Saunois et al., 2016). Although uncertainty in the geological methane source remains high, after oxidation in the atmosphere, this source would correspond to 200-300 GtC during the last 8,000 years and potentially compensate for a substantial part of the peat growth." The change in geological methane emissions (GEM) over glacial-interglacial cycle is rather small. For example, Bock et al.,2017) write: "GEMs are in fact smaller than 47 (Holocene) and 41 (LGM) Tg CH4 a–1. " and "[GEM] are not strongly variable players that could explain the observed glacial/interglacial [CH4] variations" If their analysis of their isotope measurements is correct, then the additional/anomalous source due to geological CH4 would only be 6 TgC/yr x 8,000 yr = 48 PgC over the past 8 ka. This is relatively small in comparison with the estimated peat accumulation of several hundred PgC.

In my opinion, it is appropriate for the explanation of CO2 variations to compare anomalous geological sources and sinks, representing deviations from the mean geological emissions (volcanoes, CH4, weathering) and mean geological sinks (sediment burial). Highlighting the magnitude of a selected individual flux such as total geological CH4 emissions appears misleading. It would be equally misleading to multiply the estimated weathering rate of _0.2-0.4 PgC yr with 8000 yr to get a NET source to the atmosphere of 1600 to 3200 PgC.

Our initial point was that the geological CH_4 sources have a large uncertainty, and CO_2 flux from oxidized methane is missing in the coupled model setup, but we agree with the reviewer that it is misleading to compare with total sum and not the net effect. We take this argument out of revised paper.

3) P9. L1: The simulated net atm-to-land fluxes may be compared with the observation-based

reconstruction of Elsig et al., 2009

We will add this comparison to the revised manuscript, see our reply to the reviewer #1

4) P9, l21: "On the other hand, simulations with intermediate complexity models suggested that the impact of the memory effect on Holocene carbon dynamics is rather minor (e.g., Menviel and Joos, 2012)".

This statement is not true. Please see table 4 in Menviel and Joos, 2012 for the 20 ppm CO2 increase over the last 7 ka: They attribute 10 ppm to legacy effects associated with land uptake during the transition and the early Holocene and 5 ppm to ocean-sediment interactions and only 5 ppm to coral reef buildup. Their attribution is in line with ice coreCO2 and d13C and to some extent with reconstructed CO3–. Uncertainties exist and their estimate for the coral growth is based on Vecsei and Berger which represents a low estimate.

The legacy or memory effects cannot be easily dismissed as small. For example, a substantial early Holocene carbon uptake is implied by both the d13C record (Elsig et al, 2009) as well as by reconstructions of retreating ice sheets. Such an uptake has consequences for CaCO3 compensation within the ocean and thus for the late Holocene CO2 increase. The authors may wish to revise the discussion on this topic.

Apparently, Menviel and Joos (2009) wrote "… our results indicate that coral reef growth and other shallow-water carbonate deposition are a major contributor for the atmospheric CO2 rise after 7 ka B.P.", but indeed this doesn't mean that contributions due to other processes are minor. We will revise the discussion according to the reviewer comments.

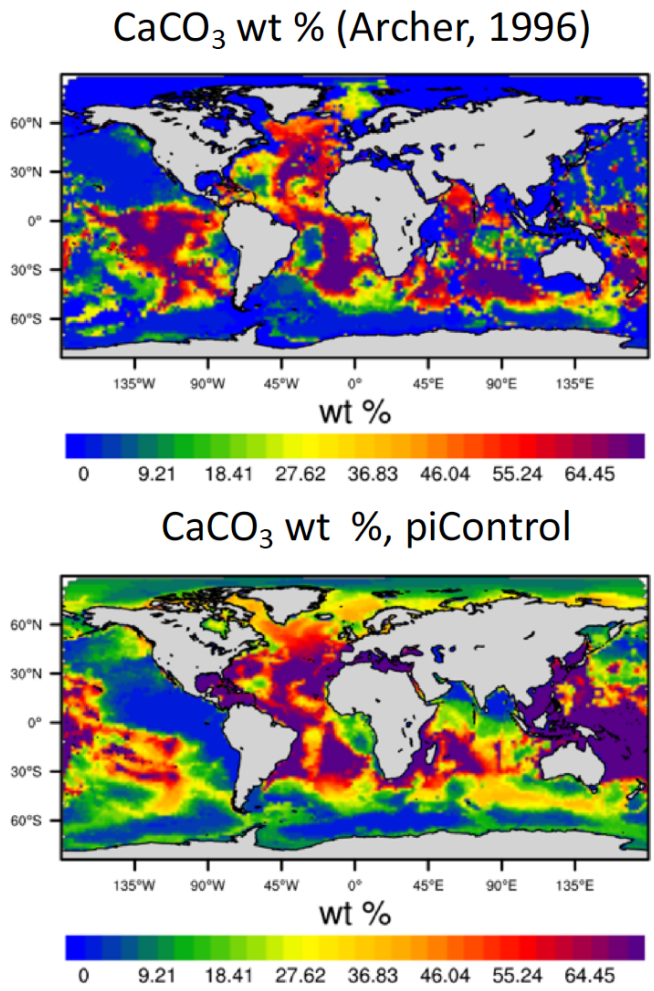
5) The cumulative shallow water CaCO3 deposition over the last 8 kyr is at the high end of available estimates. This may be discussed in the manuscript (see also point 7 below)

We agree that the cumulative shallow water $CaCO_3$ deposition as indicated in the table is confusing, as it is counterbalanced by much higher weathering, see our response to the weathering point above

FURTHER COMMENTS

1) Section 2, Could you please provide some additional information on the ocean sediment model. It would be illustrative to add a table showing the global fluxes (CaCO3, Alk, POM, opal, nutrients..) to the sediment in comparison with observational estimates.

We see the reviewer point, but think that it is more informative to compare sediment states rather than fluxes for several reasons. Particle fluxes to the sediment of POM, opal, or $CaCO_3$ reflect the respective production pattern and remineralisation/dissolution length scales (i.e. for calc the lysocline depth), thus the water column state, whereas there are no data to assess the global diffusive fluxes between sediment pore water and water column. For the $CaCO_3$ distribution, please see below the comparison of distribution in the piControl simulation of the current model version with observations. Generally, spatial state structures in our sediment model are similar to observations and driven by particle fluxes and their respective remineralization length scales in the water column (e.g. OM maxima in the tropical Pacific, opal maxima in the Southern ocean) and lysocline depth (and opal vs calc shell formation) for calcite. The general performance of the sediment model is described by Heinze et al. (1999).



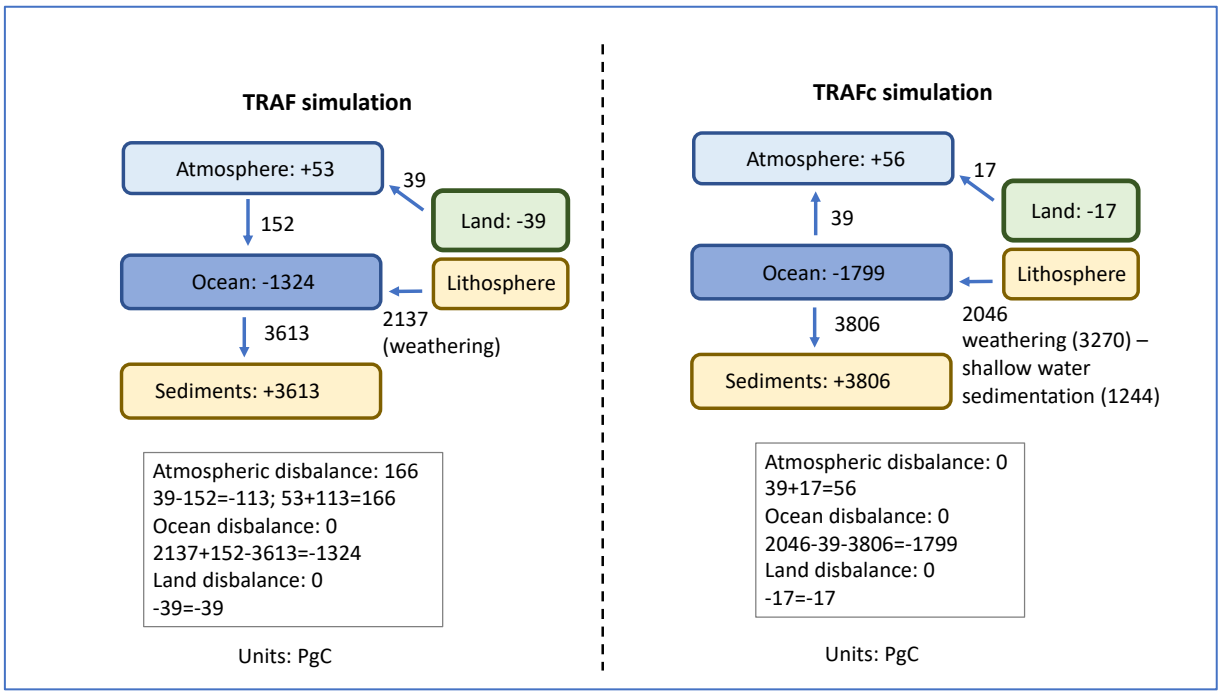
Comparison of $CaCO_3$ in coretop of marine sediments: data (top), model (bottom). Data are from Archer, D., Global Biogeochemical Cycles 10(1): 159-174, 1996.

2) Table 1: Additional explanation may be needed to understand table 1. It would be great if a sign convention is selected that allows to simply add up all the numbers to get to a _0 overall budget.
This illustrative table provides the cumulative C changes in PgC in the atm., ocean, land, and sediments. It also provides the weathering input, but I miss the corresponding flux from sediments to the lithosphere. I guess this is included in the sediments? Is the CaCO3 removed at the surface added to the sediment pool? I guess this is not the case?

In MPI-ESM, lithosphere is not an explicit component of the carbon cycle. According to the HAMOCC sediment model (Heinze et al, 1999), the sediment layer flux from the active layer (12 cm) to deeper layers is effectively a flux to lithosphere as deeper layers cannot be dissolved (constitute an ultimate loss). We do not report this flux to lithosphere explicitly. The CaCO3 removed at the surface is not added to the sediment pool. What is ultimately lost to the sediment is compensated for by the constant weathering fluxes.

The budget seems not to add up exactly. For simulation TRAF, I get 2303 PgC in the atm-land-ocean-sediment versus 2137+152= 2289 PgC; are this minor 14 Pg difference to rounding/integration? How do I need to add the numbers for TRAF and TRAFc to get a closed budget?

Indeed, simulation TRAF doesn't have a closed carbon budget, while it is closed for TFAFc. To illustrate how the budget needs to be calculated, we prepared a figure below which might be added to the revised manuscript.



Additionally, it may be illustrative to show anomalies in ocean-to-sediment flux for CaCO3 and POC in Table 5. It is not clear whether the POC flux to sediment remained roughly constant or not.

Indeed, the POC flux to sediments is not constant; as the weathering is constant, nutrient loss to the sediment is not properly compensated for the POC fluxes to the sediment. We will discuss this in the revised paper.

3) Figure 3 and P6, line 8/9 "Carbon sedimentation is high in upwelling zones, mainly in coastal areas and the tropical Pacific, and that causes strong accumulation patterns." It may be instructive to show fluxes to the sediment as diagnosed at the end of the spin up and anomalies relative to this initial flux. It is my interpretation - I may be wrong - that figure 3 shows changes in DIC plus the integrated ocean-to-sediment flux. If this is true, this may be a bit misleading as the change in DIC reflects a real change in store, while the accumulated transfer to sediment may be too a large extent balanced by weathering; then the actual change in sediment/lithosphere is smaller. If my interpretation is wrong, then the spatial gradients in the ocean sink/source shown in Fig. 3 may be better explained.

We thank reviewer for this comment; indeed, we subtracted weathering then constructed this figure. We will explain this better in the revised paper.

4) P6, l26: "Natural changes in vegetation and tree cover are most pronounced for the time slice around 1 CE" Do you mean there are large changes at 1 CE or rather 1CE minus 6 kyr BCE?

We mean 1 CE minus 6 kyr BCE, will clarify this in the paper.

5) P7, l11: “The simulated increase in land carbon storage before 2000 CE and decrease afterwards is consistent with the changes in atmospheric d13CO2 (Schmitt et al., 2012).” The original Holocene data are given in Elsig et al, 2009 and the outcome should be compared with the reconstructed air-land fluxes presented by Elsig et al.

We will compare with fluxes from Elsig et al. (2009), see response to the comments of the referee #1.

6) P 7, l19: “The global CaCO3 export from surface to aphotic layer increases by about 5% between 6000 and 2000 BCE in both TRAF and TRAFc simulations and returns to the 6000 BCE level by the end of the simulation.” Could you please provide an explanation for this change in CaCO3 export. Is POM export also declining or is the rain ratio changing in response to changes in surface CO2/CO3–?

Both, CACO3 and POC fluxes to sediments are not constant; as the weathering is fixes, losses to the sediment are not properly compensated. This also leads to changes in the rain ratio. We will discuss this in the revised paper.

7) P7, l23: “Accounting for 7850 years of experimental length, the required excessive carbonate sedimentation in the shallow waters would be 3 Tmol/yr or at the lower bound of estimates of 3.35 to 12 Tmol/yr CaCO3 accumulation proposed by Vecsei and Berger (2004) and Opdyke and Walker (1992).”
I am confused here. Dividing the 1224 PgC of excessive CaCO3 deposition given in Tab 1 by 7850 yr and by 12g/mol, yields 12 Tmol/yr and not 3 Tmol/yr. I think it would be illustrative to compare the cumulative surface CaCO3 removal of 1224 PgC with the cumulative estimates given by Vecsei and Berger for the last 8 ka. This number is likely around 300 PgC; (Vecsei and Berger suggest a cumulative deposition of 378 PgC over the last 14 kyr).

We agree that this could cause a confusion. Here, we meant a difference between TRAF and TRAFc experiments. It is caused by counteracting of CaCO3 removal by higher weathering flux at the surface; see a comment to the weathering point above. As seen in the Table 2 missing in the manuscript (see response to the point 8 below), additional CaCO3 removal accounting for extra weathering in TRAFc is much smaller, about 3 Tmol/yr.

8) P8, line 10: Table 2 seems missing in the MS?

We apologize as in the last-minute update, we forgot to add the table to the paper draft. We provide the table below. It is useful for comparison of carbon fluxes between recent synthesis of data by Cartapanis et al (2018) and model experiments.

Table 2. Model-data comparison of average carbon fluxes from 6000 BCE to 1850 CE, Tmol yr^{-1}

Source	$CaCO_3$ burial	Surface $CaCO_3$ removal	C_{org} burial	Volcanic outgassing[1]	Weathering	Ocean, water column losses
Data[2], average (min - max)	23.3 (11 - 38)	0	18.3 (6 - 58)	9.2 (4 - 15)	11.7 (9 - 19)	20.8[3]
TRAF	27.9	0	10.5	0	24.6	13.8
TRAFc	29.1	13	11.3	0	34.7	18.7

1 Including aerial volcanic CO_2 outgassing

2 Pre-industrial fluxes according to the Figure 1 in Cartapanis et al. (2018)

3 Minus volcanic outgassing