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Interactive comment on "What was the source of the atmospheric CO_2 increase during the Holocene?" *by* Victor Brovkin et al.

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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.

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.

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Discussion paper

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

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

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

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?

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 [CH_4] variations" If their analysis of their isotope measurements is correct, then the additional/anomalous source due to geological CH_4 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 CO_2 variations to compare anomalous geological sources and sinks, representing deviations from the mean geological emissions (volcanoes, CH_4, weathering) and mean geological sinks (sediment burial). Highlighting the magnitude of a selected individual flux such as total geological CH_4 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.

3) P9. L1: The simulated net atm-to-land fluxes may be compared with the observation-based reconstruction of Elsig et al., 2009

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 CO_2 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 core CO_2 and d13C and to some extent with reconstructed CO_3^{-}. 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.

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)

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.

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 $\sim$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?

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 to rounding/integration? How do I need to add the numbers for TRAF and TRAFc to get a closed budget?

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.

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 wheathering; 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.

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?

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 Holcene 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.

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–?

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

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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 $CaCO_3$ 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).

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

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