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Interactive comment on "Climate and marine biogeochemistry during the Holocene from transient model simulations" *by* Joachim Segschneider et al.

Anonymous Referee #1

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This manuscript describes the results of a transient simulations with a comprehensive atmosphere-ocean-sea ice biogeochemistry model over the past 9.5 kyr of the current interglacial. A major finding is a large change in the oxygen minimum zones over the Holocene. The authors also discuss how marine changes may alter atmospheric CO2 concentration. Such long transient simulations are rare and as such this manuscript should be published after the following comments have been addressed.

1) The role of model drift should be discussed a bit more in depth. A ctrl run of 2 kyr seems somewhat short and there may be still some long-term adjustments going on. Can the authors exclude that model drift leads to the changes in Pacific OMZ,

overturning, water mass age and related biogeochemical changes? If I understand it correctly, SST in the CTRL is expected to change by 0.3 K from 9.5 to 6.5 kyr BP which is substantial compared to the signal in the transient run.

2) The biogeochemistry model is run offline. How is convection and diffusion and related tracer transport treated? Are water mass age in online and offline model versions the same?

3) The vertical resolution is 31 layer in the ocean. How many layers are below the mixed layer and what is the range in layer thickness. I suspect that layer thickness in the deep may be very large which may be problematic for deep Pacific oxygen?

4) While the representation of physical processes and of atmospheric dynamic is advanced in the online model compared to the current crop of Earth System Models of Intermediate Complexity (typically used to explore the carbon-climate system over the Holocene), major biogeochemical processes are neglected which likely affects results. These processes should be explicitly mentioned and their potential influence on results discussed. It is know that shallow water carbonate deposition changed over the Holocene (Vecsei and Berger, 2004) . Ocean-sediment interactions and changes in weathering and their legacy effects are missing. How is the freshwater input into the North Atlantic for the 8.2 kyr event represented? What is the role of changes in land biosphere carbon inventory? Is there an influence from volcanic eruptions (Huybers and Langmuir, 2009) In this respect, insight from earlier ocean modelling studies on Holocene CO2 evolution are relevant (e.g.; (Brovkin et al., 2016;Menviel and Joos, 2012;Ridgwell et al., 2003;Broecker et al., 2001))

5) Unfortunately, atmospheric CO2 was prescribed in the biogeochemistry module of the model. This makes the interpretation of air-sea carbon fluxes very difficult and it is difficult if not impossible to quantify how the atmospheric CO2 would evolve in the coupled model system with free atmospheric CO2. I find Figure 7 to be highly misleading. It is fine to show the time-integrated air-sea flux, but to equate this with

atm. CO2 changes is not possible.

In case the ocean would be in steady-state in terms of circulation, prescribing atmospheric CO2 to the ocean would lead to an outgassing when atmospheric CO2 is decreasing (9.5 to 7 kyr BP) and to an ingassing of carbon into the ocean when atm. CO2 is increasing. The simulated air-sea carbon flux under varying climate must be interpreted in the context of this atm. concentration driven flux. There is no change in the ocean's carbon inventory in the first few millennia despite a decrease in the prescribed CO2. The model ocean would lose carbon to the atmosphere if atm. CO2 would be freely evolving. The model ocean takes up carbon during the time of the atm. CO2 rise (7 to 4.5 kyr). Is this uptake driven by the prescribed concentration or is a concentration driven uptake partly mitigated by the modelled changes? I have the impression that the uptake is rather small for the magnitude of the CO2 increase. In the late Holocene, atm. CO2 varies little and the ocean is loosing CO2. So I guess overall the climate driven ocean signal is likely an outgassing of CO2 to the atmosphere over the entire Holocene. Unfortunately, this is hard to verify with the current setup.

Figure 7 should be deleted and the cumulative air-sea flux should be shown in Figure 6. The interpretation of the cumulative air-sea flux must be revised.

6) It would be appreciated if changes in O2 and other tracers would be a bit better attributed to underlying processes. There is hardly any quantitative attribution of O2 changes to mechanisms. How large is the influence of solubility changes versus changes in remineralization/water mass age? It would be good to separate solubility-driven changes and changes in –AOU, i.e. biologically-mediated changes. Do the authors also find the conventional anti-correlation between solubility-driven and oxygen-utilization-driven changes in the OMZ (Bopp et al., 2017)?

How does stratification change and what is its role for O2 and BGC evolution?

7) P.24, l2: export production is the wrong metric to judge the efficiency of the biological pump. What matters for atmospheric CO2 is nutrient utilization. The interpretation

given here is flawed.

Further comments 1) P4, 2. Para: Other transient simulations include those from the Trace21kyr experiment by NCAR and as mentioned above there are quite a few EMIC studies available that are worth mentioning.

2) P9: Are changes in ice albedo be taken into account? Please mention here that solar TSI and volcanic forcing is neglected

3) P9 The Indermühle CO2 data have been update and the chronology adjusted. See e.g. Elsig et al.. Which specific CO2 data and age-scale is used to force the model?

4) P12: 1 para would better fit in method section

5) P12, l21: delete "again"

6) P12, l25; the drift appears not "modest"; please delete modest.

7) P13, line 1-2: It seems that the whole signal may be explained by drift?

8) P13, sec 3.1.2 It would be useful also to discuss Indo-Pacific overturning here.

9) P14: line 18 to 21: How does this increase in water mass age from 1500 to 1800 yr relate to model drift? What is driving this change in Indo-Pacific water mass age and overturing circulation? How do the age change compare in the online and offline model version?

10) P18, line 28: What is the role of saturation/solubility changes?

11) P20, discussion: The authors should say something here and in the results about the SST evolution at different seasons. See for example discussion on Holocene temperature evolution by (Liu et al., 2014;Samartin et al., 2017).

12) How is the control extrapolated in Figure 5?

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