Response to the comments of Reviewer 1.

This is a very well written paper describing what appears to be a very good model that will likely play an important role in climate research in coming years. The English is generally very good although there are a few quirks. I get the feeling that different parts of the paper were written by different people (not surprising in an effort of this scale) with varying command of English. The atmosphere and ocean carbon cycle sections especially could use a proofreading by a native speaker if possible. Certain symbols are used repeatedly to represent for different things. For example, q is potential vorticity on p. 315, humidity on p. 317 and "characteristic velocity" on p. 319. The ice sheet model description is lacking in specific literature references for some of the details (e.g., p. 332).

We would like first to thank the Reviewer for his very positive remarks and for his in depth evaluation of the manuscript. His comments will greatly help us to improve the manuscript.

We have carefully read the different parts of the manuscript and will propose some improvements in the revised version, in particular in the description of the atmosphere and carbon cycle models.

We considered that using two times the same symbol in completely different frameworks was not a source of confusion and we thus choose to utilize the symbol that is commonly used for the corresponding variable. However, as the reviewer find that this could cause problem for q, we have modified it, adding subscripts for all the variables for which this symbol was used except the quasi-geostrophic potential vorticity.

The main references to AGISM including all equations are Huybrechts and de Wolde (1999) and Huybrechts (2002) as mentioned in the text. Specific literature references, insofar appropriate, can be found there.

I don't understand the point of Tables 1 and 2; most of the parameters listed do not appear anywhere else in the paper. Of what use is it to tell the reader what the parameter values are when the parameters themselves are never defined or the equations in which they appear shown? This applies to some of the parameters in the other tables as well, but in Tables 1 and 2 it seems to be most of them.

We have not given the details of all the model equations and parameterizations. However, some parameters are quite standard for specialists. For instance, the Gent-McWilliams thickness diffusion coefficient is well-known for the majority of physical oceanographers and sea ice modelers should understand the meaning of first and second bulk- parameter in the sea ice rheology of Hibler (1979). Although the exact definition is not given here (but the references are given in the main text if an interested reader needs to find it), some readers and many model users may find useful to have this information in the main core of the paper. This is the reason why, for those standard parameters, the values are given in tables.

Questions about model components:

ECBilt:

What is the meaning of the third (diffusion) term on the left hand side of equation (1)? It looks like "mixing" of (relative) vorticity across gradients by unresolved small-scale processes, but why use such a high order exponent? I thought it was a typo until I checked Opsteegh et al 1998. This is the same as in Opsteegh but I couldn't find anything like this in Marshall and Molteni or Holton (I only have an older edition of Holton).

Due to the relative low horizontal resolution, we need a very scale selective diffusion to remove effectively the energy at the smallest scales that are resolved by the model. That is why the value is larger than in many other models.

CLIO:

“seawater is supposed to infiltrate the entirety of the submerged snow and to freeze there, forming a snow ice cap” (321/7-8) I find this vague, and can not really tell what is being referred to here.

In the revised version, this part will be modified ’ When the load of snow is large enough to depress the snow-ice interface under the water level, seawater in the model infiltrates the entirety of the snow layer below the ocean surface and freezes there. This snow and the frozen seawater form then a new layer (snow ice), implying in the model an increase in sea ice thickness (Fichefet and Morales Maqueda, 1997).’

“A no-slip condition is imposed on land boundaries.” (321/25) This could cause some rather strange behaviour at 3x3 degrees resolution. Wouldn’t real ice be no-slip at the coastline but be subject to deformation at scales of kilometres at most? Is there a way to parameterize this so that you don’t unrealistically prevent ice motion over huge areas of ocean?

Imposing a no-slip condition on land boundaries is classical in models although we agree that, at this resolution a free-slip might be more adapted (see for instance Hibler W.D. (1986). Ice dynamics. In, The geophysics of sea ice, N. Untersteiner (editor). NATO ASI Series Vol 146. Plenum Press, New-York, pp 577-640). Actually, there is an option in the code to choose a no-slip or a free-slip and the tests performed show that this does not affect too much the model results.

If under ice freshwater fluxes are represented as virtual salt fluxes, does this mean that in ice-covered areas it is in effect a rigid-lid model?

No, precipitation, evaporation and ocean dynamics affect the sea surface elevation below sea ice but not the ice-ocean exchanges as explained in details in Tartinville et al. (2001).

VECODE:

P_rˆ0 is not defined (eq 8). The definition of P_rˆmin makes sense in light of the definition of v_hat, but it only appears on the LHS of 8 and depends on a term on the RHS that is never defined.

P_r^0 is a bioclimatic parameter. This will be stated in the revised version and the value of Pr_0 will be given in Table 3.

Is temperature in the definition of GDD0 the daily mean temperature?

Yes. This will added in the revised version.

If woody residues are the fast pool (324/8), what about leaves? Do they remineralize instantly?

Yes. In accordance with litter bag experiments, the lifetime of leaf litter for a reference T of 10°C is about one year, which is comparable with an annual step of the model. Therefore, leaves are assumed to remineralize instantly.

I don’t understand how you can correct errors in productivity in semiarid regions (324/15-16) just by correcting for the vegetation fraction. Isn’t that already part of the model?

This formulation is indeed confusing. We will modify the statement as follows in the revised version: “To find out NPP per model grid cell, NPP per square meter is multiplied by vegetation fraction v, which is in most cases equal to one, and the land area. In dry subtropical regions, v is less than 1 (Eq. 8), and this helps to correct the bias in the productivity per grid cell in these regions.”

The increased allocation to woody biomass as NPP increases (324/20) presumably only applies to trees.

Structural biomass does include roots which are relevant for grasses as well.

The model was tested with an extensive data set from a limited geographical area. What are the implications of the lack of tropical and subtropical data?

Bazilevich’ dataset includes subtropical species as well. Regarding tropical trees, it is indeed assumed that allocation of NPP to green versus structural biomass is similar to allocation of temperate and boreal trees. Implications of this limitation are not that clear, in particular because physiology of tropical trees, including a role of relatively large autotrophic respiration, is not well understood.

LOCH:

I don’t see the point of defining a Redfield ratio for nitrogen when it is never otherwise mentioned. The model is clearly defined in the first paragraph of 2.4 as being a phosphorus-based one and no form of nitrogen is mentioned as a state variable.

The model is phosphorus based indeed but we need the nitrogen-to-phosphorus ratio in order to evaluate the impact of biological activity on the total alkalinity and thus on the whole carbon cycle and in particular to the atmosphere-ocean exchange of CO2.

G is the maximum grazing rate (326/11). The grazing rate is the whole of the first term on the LHS of eq (11). Similarly in (12) the r_x’s are maximum rates.

The reviewer is right. The revised version will be modified accordingly.

The Martin curve gives the distribution of POM flux not of POM (326/16) (the caption to Figure 5 also clearly implies that it is the concentration rather than the flux that is being modelled as exponential decay).

Indeed, the POM flux and not the POM concentration is actually prescribed in the model according to the Martin curve. We will modify the text and the figure legend in the revised version accordingly.

Is DOM in Redfield ratio? Normally DOM has much higher C/P ratios than biomass. I find the paper vague regarding the assumptions that were made here.

This model, similarly to other simplified models of the ocean carbon cycle (e.g. Maier-Reimer, 1993; Najjar et al., 2007), is Redfield-based. It means that POM, DOM as well as biomass composition are in Redfield ratio. Allowing for DOM in non-Redfield ratio would imply to consider different remineralization rates for P and C. Though possible in theory, it is technically demanding since it would call for the addition of several variables representing various classes of organic matter (from fresh to refractory) and the use of age tracers. It would also add to the complexity with respect to the conservation. We will add a sentence in the revised version to clarify this topic.

Maier-Reimer, E. (1993), Geochemical cycles in an ocean general circulation model. Preindustrial tracer distributions, Global Biogeochem. Cycles, 7, 645-677.

Najjar, R. G., et al. (2007), Impact of circulation on export production, dissolved organic matter, and dissolved oxygen in the ocean: Results from Phase II of the Ocean Carbon-cycle Model Intercomparison Project (OCMIP-2), Global Biogeochem. Cycles, 21, GB3007, doi:10.1029/2006GB002857.

Does the 'chemical enhancement' of air sea flux term have any discernible effect on the model output? I think this concept has fallen out of favour. We know that gas exchange is enhanced at low wind speed compared to the standard W92 parameterization but it is no longer widely accepted that this is the primary mechanism.

The chemical enhancement plays a role at very low (or nil) wind speed in warm water areas. It is true that its role is negligible in the context of global-scale climate studies. However the same version of LOCH is applied for global and regional studies (it is not used in the framework of LOVECLIM in that case). This is the reason why this term is kept here although it has a very weak effect.

It is common practice to express OBSERVED concentrations of chemical species in seawater in moles per kilogram, but in models it is normally expressed per volume. According to Table 4 nutrient and oxygen concentrations are in mol/kg but biomass is in mol/L. I find this difficult to believe. I can not imagine how it could be computationally efficient to do this while conserving mass.

In the model, under the Boussinesq approximation, mass is strictly conserved - this aspect is verified after each model change. Working units for concentrations in the model are mol/m3. In the initialization part of the model, all parameters are converted to the appropriate units. The parameter values in Table 4 were mostly derived from the literature and we prefer to keep the units from the original sources for consistency and easy reference.

Does 100% of exported CaCO3 reach the bottom (as implied on p. 328 and in the caption to Figure 5)? What is the justification for this assumption?

The totality of exported CaCO3 does indeed reach the bottom in the model as we consider that bottom dissolution is dominant and thus make the hypothesis that the dissolution in the water column can be neglected in comparison. The question as whether calcium carbonate does mainly dissolve in the water column or in the sediment has long been debated and is not fully resolved yet. Chen (1990) and other studies concluded that water column dissolution in the Pacific Ocean (the most corrosive area) represents at most 25% of the process. Newer studies (e.g. Berleson et al., 2007) point to a more important contribution of water column dissolution. However, significant discrepancies between traps measurements and the excess alkalinity method do subsist.

It should be stressed that the model is aimed at reproducing carbon exchange between the deep ocean and the surface on long timescales. In this purpose, we considered important to acknowledge the existence of chemically different environments (the Pacific is more corrosive than the Atlantic). Hence we chose to model the CaCO3 dissolution at the bottom as a kinetic chemical process. This is in contrast to most available models which compute CaCO3 dissolution using a spatially and temporally constant e-folding depth (e.g. Henderson et al., 1999; Najjar et al., 2007). A constant e-folding dissolution depth allows for water-column dissolution but does not account neither for the large spatial heterogeneity in $CaCO_3$ dissolution rates (Berelson et al., 2007) nor for their temporal evolution.

An alternative procedure would be to represent $CaCO_3$ as settling particles subject to chemically driven dissolution along the vertical. While technically feasible, this procedure would be quite CPU-demanding since pH would have to be evaluated at each grid cell. Since the model seems to perform sufficiently well under the current setting (e.g. Fig 3 in Mouchet and François, 1996) we think it is important to keep the model running fast in order to be able to address climate change over longer time period. On the other hand, a closer examination of the implications of the bottom dissolution hypothesis in LOCH is currently under progress.

Berelson, W. M., W. M. Balch, R. Najjar, R. A. Feely, C. Sabine, and K. Lee (2007). Relating estimates of $CaCO_3$ production, export, and dissolution in the water column to measurements of $CaCO_3$ rain into sediment traps and dissolution on the sea floor: A revised global carbonate budget. Global Biogeochem. Cycles, 21, GB1024, doi:10.1029/2006GB002803.

C.-T. A. Chen. Rates of Calcium Carbonate Dissolution and Organic Carbon Decomposition in the North Pacific Ocean Journal of the Oceanographical Society of Japan Vol. 46, pp. 201 to 210, 1990

G. M. Henderson, C. Heinze, R. F. Anderson, and A. M. E. Winguth (1999). Global distribution of the 230Th flux to ocean sediments constrained by GCM modeling. Deep Sea Research Part I, 46: 1861-1893

Najjar, R. G., et al. (2007), Impact of circulation on export production, dissolved organic matter, and dissolved oxygen in the ocean: Results from Phase II of the Ocean Carbon-cycle Model Intercomparison Project (OCMIP-2), Global Biogeochem. Cycles, 21, GB3007, doi:10.1029/2006GB002857.

AGISM

In equation (16) should c_p be c_i? also the subscript 'i' in the equation is given as 'ice' in the text (331/8).

No, c_p should really be c_p. It is the specific heat capacity at constant pressure for ice. Yes, the subscript for ice is always 'i'. We will change T_{ice} to T_i in the revised version.

In equation (18) ρ_w is presumably the density of water but this is not actually stated.

Yes, this is correct. We will mention that in the revised version of the manuscript.

I don't understand the logic of equation (20). It doesn't state whether it is C or K temperature but max(0,T) and absolute value make no sense for absolute temperature. My initial reaction was why use a term that is the same for positive and negative (Celsius) temperatures? The second term with the parentheses is much larger than the first except for temperatures near 0 so maybe the first term is just meant to be represent the melting that can happen near 0 C especially with high variance. But I think a sentence or two explaining the logic would help.

Yes, temperatures here are in °C. The reviewer is right that equation 20 represents a normal statistic to account for the variability of the daily temperature for monthly mean temperatures close to the melting point. This will be explained in more detail in the revised version.

Iceberg model

I don't understand why the drag coefficient is higher for air than water. Is this because of turbulence near the surface? "drag coefficients for water stress acting along the lower surface of the iceberg and atmospheric wind stress acting along the top surface are deemed negligibly small" (336/13) doesn't ring true to me either, but it's hard to be sure what it means as the exact geometric shape of the icebergs is not specified.

As we will specify more clearly in the revised version, the drag coefficients are adopted from a study by Smith et al. 1993, who selected drag coefficients to optimize the fit of the modeled tracks to the observed ones (see mean values in their table 3). Because optimisation of drag coefficients attempts to compensate for all the deficiencies in the model and the data at once, only general conclusions about a particular process are possible and even these must be viewed with caution (their p.44).

Coupling

I do not think the coupling among the components is sufficiently explained especially with respect to CO2. It appears that LOCH has its own zonally averaged atmospheric transport module for CO2 (Figure 1). How is the CO2 concentration over land determined? Do the land and atmosphere just use the zonal-mean values generated by the ocean (which are then further modified by the land biosphere and used in radiative transfer but not subject to advection in the atmosphere)? This seems to be the case (p. 338-339), but the description of exactly what was done here is convoluted and confusing.

This point will be explained more clearly in the revised version of the manuscript: "The atmospheric component of LOCH computes the atmospheric CO2 evolution in zonal bands which are equally spaced in latitude. A spatial interpolation procedure is thus required to transfer to LOCH the annual mean values of the CO2 fluxes between atmosphere and continents computed by VECODE. Combining the carbon fluxes from the continents and from the ocean, LOCH computes a globally averaged, annual mean atmospheric CO2 concentration which is then transmitted to ECBilt and VECODE, where it impacts on the radiative transfer and fertilization, respectively."

In the LGM experiment LOCH is turned off and atmospheric CO2 fixed at 185 ppmv. In the mid-Holocene experiment LOCH is turned off but if there is a fixed atmospheric CO2 concentration it is not stated what it is. This too requires clarification. Is atmospheric CO2 fixed in all experiments where LOCH is turned off?

Yes the atmospheric CO2 concentration is fixed in all experiments where LOCH is turned off. The value for the mid-Holocene experiment is the same as for pre-industrial conditions. This will be mentioned in the revised version of the manuscript.

I think the authors could also be more clear about what they mean by "homogenously dumped" in the North Pacific (338/9). Divided equally among grid boxes within some region? I think the region needs to be specified more precisely.

Yes, this is divided in order to have the same flux of the whole region. The North Pacific corresponds here to the ocean regions between Bering Strait and the equator.

Model evaluation:

I think that the weakness of the model in the tropics is understated (p. 344) given the subsequent emphasis this is given in qualifying the model's applicability to substantially different past and future *climates (e.g. p. 346, 347, 349). The equatorial SST is too warm and the NH precipitation maximum associated with the Intertropical Convergence Zone (ITCZ) is absent (Figures 9 and 10). But do we know for certain that the model is qualitatively reproducing the underlying physical phenomena? From Figures 9 and 10 we can not tell whether the model has an ITCZ or the model ocean has a 'cold tongue'. Many climate models have "double ITCZ" problem but this one seems to have a single maximum located near the equator. Given the way precipitation is modeled this is perhaps unsurprising: is it found in other EMICS with similar schemes? Does the model ocean have equatorial upwelling and an east-west thermocline tilt? Is the apparent lack of a 'cold tongue' just an artefact of the contouring scheme used in Figure 9, or is it a real error resulting e.g. from the simulated winds. I assume that the ocean model does reproduce the basic physical phenomena approximately, but given only the information in Figures 9 and 10 I don't think the reader can infer this with confidence. If there are other publications that address these phenomena they should be specifically referred to.*

We agree with the Reviewer that the main weaknesses of the model are related to its behavior in the tropics. This is the reason why we have clearly mentioned this shortcoming in the abstract "However, despite some improvements compared to earlier versions, several biases are still present in the model. The most serious ones are mainly located at low latitudes with an overestimation of the temperature there, a too symmetric distribution of precipitation between the two hemispheres, an overestimation of precipitation and vegetation cover in the subtropics", in section 3.1 and in the conclusions stating for instance that "the model appears well adapted to study long-term climate changes, in particular at mid- and high latitudes" and "Where the biases are strong, like in many regions at low latitudes".

So, our goal was clearly not to understate this point. However, as suggested by the reviewer, we will give more details on the model behavior in the tropics in the revised version. We will mention explicitly that the model misses NH precipitation maximum associated with the Intertropical Convergence Zone (ITCZ). This problem is indeed present in many EMICs (see for instance Fig 2 of Petoukhov V., M. Claussen, A. Berger, M. Crucifix, M. Eby, A. Eliseev, T. Fichefet, A. Ganopolski, H. Goosse, I. Kamenkovich, I. Mokhov, M. Montoya, L.A. Mysak, A. Sokolov, P. Stone, Z. Wang and A. J. Weaver, 2005. EMIC Inter-comparison Project (EMIP-CO2): Comparative analysis of EMIC simulations of climate, and of equilibrium and transient responses to atmospheric CO2 doubling. Climate Dynamics 25 363-385.)

The model has an equatorial upwelling. Although not very clear at the scale of the published Figure 13, the meridional overturning circulation displays a total upwelling in the tropics of about 50 Sv. This number will be provided in the revised version. The model reproduces reasonably well the east-west thermocline tilt and the depth of the equatorial pycnocline responds significantly to preturbations as discussed for instance in Timmermann et al. 2005. ENSO suppression due to weakening of the North Atlantic thermohaline circulation. Journal of Climate 18 (16): 3122-3139. The cold tongue is present but the east-west temperature gradient is underestimated (2.5°C in annual mean at the equator compared to more than 3.5 in observations interpolated at the model resolution). Those model characteristics will be described in the revised version and the references provided.

Similarly, in the Southern Ocean, the strength of the deep upwelling is not clear but it looks weak and this is glossed over in the text, which states a maximum of 12 Sv (345/15). Figure 13 seems to show that the upwelling of deep (e.g., > 2000 m) water is rather less than this. There are data based estimates (e.g., Karsten and Marshall 2002 JPO 32: 3315) that show substantial upwelling of deep ocean water and are not cited here (change "estimations" to "data based estimates", 345/16).

The maximum of the zonal mean cell close to Antarctica is probably not a measure that could be directly compared to observations of the deep upwelling (which also includes significant uncertainties in those data-poor regions) in coarse resolution models. First, the maximum of this cell is not a very robust diagnostic and previous simulations with LOVECLIM have shown that the value of this maximum can vary quite widely when parameters are changed without a large impact on model results. As a consequence, we hesitated to give its value in the first version of the manuscript as it could be misleading but include it as it might be interesting for some readers.

We should also notice that in the model, additional upwelling occurs in the Southern Ocean at a shallower depth, providing a total upwelling of more than 30 Sv in zonal mean, and thus a value that might appear high compared to some estimates. We of course do not claim that those 30 Sv corresponds to deep upwelling but this illustrates that only considering one cell as representative of the upwelling is not a good estimate of the total upwelling as this clearly underestimates its magnitude. We should also take into account that the model has a vertical resolution of 300m to 500m at depth so we should not expect an exact agreement on the depth of the upwelling with Karsten and Marshall estimates (who show only the top 2500 m in their figures).

Our goal in this paper is clearly not providing detailed diagnostics and sophisticated model-data comparisons but rather to describe the model components and provide the reader with a general overview of the model performance (and in this particular section the ocean circulation). A longer discussion of each element of the circulation in all the experiments (deep water formation in the North Atlantic, upwelling at depth in the North Pacific, etc.) would certainly be interesting. However, this could not be done in the present framework. This is the reason why we provide only quite-standard number for the overturning cell and some references to show that the model behaves relatively well. A more detailed discussion appears out of the scope of the present study. As suggested, we will change in the revised version “estimations” to “data based estimates” and include the additional reference of Karsten and Marshall 2002. We will also modify the text to make it clear that we do not have a perfect agreement with data-based estimates.

With regard to ocean anthropogenic heat content trends, it is asserted that “this is a standard model bias that is discussed in detail in Loutre et al. (2010)” (346/3). But this is a paper “in preparation” by the same group! If this is a “standard” model bias surely there is a reference in the existing literature.

We wanted to give the most recent reference to this point. The paper of Loutre et al. (2010) is now submitted to Climate of the Past and the discussion paper is thus freely available: Loutre, M.F., A. Mouchet, T. Fichefet, H. Goosse, H. Goelzer, P. Huybrechts, 2010. Evaluating model performance with various parameter sets using observations over the last centuries, Clim. Past Discuss., 6, 711–765, 2010 www.clim-past-discuss.net/6/711/2010/. The updated reference will be given if the revised version of the manuscript.

For the past millennium simulation, the assertion that some cooling trends are less than previous results “mainly due to the weaker solar forcing applied” is vague and needs some explanation. Why is the forcing different? Does “weaker” refer to the mean insolation or the amplitude of the variability? Similarly, the “small decrease in CO2 concentration between the periods 1200–1400 and 1700–1800 suggested by the observations” is given no context of literature or figure/table references, and it isn’t obvious in Figure 16. In the mid-Holocene experiment, it is not stated what the “increase of” summer air temperature (347/13) is relative to (although it is in the figure caption).

The most recent estimates of solar forcing have smaller amplitudes than the ones proposed a few years ago. This is the reason why the changes are weaker here. The reference value for the total solar irradiance has not changed, only the amplitude of the fluctuations is reduced. This will be discussed more clearly in the revised version by including the following sentences in the section devoted to the description of the forcing “The total solar irradiance changes have been scaled to provide an increase of 1 W m-2 between the Maunder minimum (late 17th century) and the late 20th century. This roughly corresponds to a threefold reduction in amplitude compared to some previous simulations conducted with the model (e.g., Goosse et al., 2005) but is in better agreement with recent reassessments (Lean et al., 2002; Foukal et al., 2006).” We will also modify the section 3.3 to clearly specify that it is the variations of solar irradiance that are weaker.

The decrease in CO2 concentration is apparent in Figure 16 but it is indeed small. It is discussed in more details, for instance, in the paper of (Frank DC, Esper J, Raible CC, Buntgen U, Trouet V, Stocker B, Joos F. Ensemble reconstruction constraints on the global carbon cycle sensitivity to climate. Nature 463, 527, 2010). The reference will be given in the revised version of the manuscript.

The changes for the mid-Holocene experiment are related to the present-day conditions. This will be mentioned in the revised version of the manuscript.

The conclusion states that “A deeper analysis was performed using previous versions of the model for all the experiments presented here.” (349/17), but no references are given.

The references were given in the introduction (end of page 313, beginning of page 314). We do not think that it is necessary to repeat them here but we will refer to the introduction at this stage in the revised version of the manuscript.

Some details:

The term ’oceanic’ should be used with care. Oceanographers generally use it to indicate open ocean vs coastal or continental shelf environments. In most cases “ocean” is more appropriate, e.g., “ocean component”, “ocean model”, “ocean carbon cycle”, “ocean grid”. I counted at least 16 separate occurrences of “oceanic”, none of which are really appropriate in my view. Similarly (although perhaps appearing opposite), one normally refers to the “dynamical core” of a model rather than “dynamic” (e.g., p. 315).

All the occurrence of the term oceanic will be replaced and we will used dynamical core as suggested in the revised version.

p. 312 “a full 3-D representation of the system” seems like an overstatement with only 3 vertical layers

We will replace *“a full 3-D representation of the system”* by *“a 3-D representation of the system”* in the revised version.

p. 315 “which was using a flat bottom” which used a flat bottom

This will be modified in the revised version.

p. 315 the “perfect gas constant” ideal gas constant (note that R is incorrectly represented as a superscript in eq. 1)

This will be modified in the revised version.

p. 316 “the law of the perfect gases” the ideal gas law

This will be modified in the revised version.

p. 316 “Above 500 hPa, the atmosphere is supposed to be completely dry” I can’t tell if this refers to a model assumption or an assertion about the real world (see also p. 318 line 5)

This is a model assumption. This will be mentioned clearly in the revised version.

p. 320 change “baroclinic one” to “baroclinic mode”

This will be modified in the revised version.

p. 320 change “a realistic bathymetry compatible with the resolution” to “a realistic bathymetry (within the limits of the model resolution)”

This will be modified in the revised version.

p. 320 “20 levels along the vertical” 20 levels in the vertical

This will be modified in the revised version.

p. 322 “presents very small diffusion” has very small diffusion

This will be modified in the revised version.

p. 322 “The interest of employing this elaborate scheme is that, for a coarse resolution grid such as the one used here, it allows to determine the location of the ice edge ...” The advantage of employing this elaborate scheme is that, for a coarse resolution grid such as is used here, it determines the location of the ice edge ...

This will be modified in the revised version.

p. 323 I don’t think the P_r’s in eq 7 need to be in ()s

We agree but we consider that this avoids confusion with superscripts so we prefer to keep it this way.

p. 325 “LOCH intents at reproducing the export production (i.e. flux of organic carbon to the deep ocean)” LOCH attempts to reproduce export production (i.e. flux of organic carbon out of the surface ocean)

This will be modified in the revised version.

p. 325 “The LOCH biological module should hence not be understood” therefore

This will be modified in the revised version.

p. 327 “Alkalinity and dissolved organic carbon” inorganic

This will be modified in the revised version.

p. 331 “Longitudinal deviatoric stresses” I have no idea what ’deviatoric’ means and can not suggest an alternative, but I don’t think this is a real word. “horizontal stress deviators” could possibly be “horizontal stress anomalies” but I don’t know enough about this type of modelling to be sure.

A deviatoric stress is the stress from which the hydrostatic (actually cryostatic in this case) component has been subtracted (i.e, it is the difference between the normal stress and the hydrostatic stress). This is standard terminology in continuum mechanics. We will add this additional explanation in the revised version of the manuscript.

p. 331 Glen/Glenn (Glen on p. 354)

Glen is the correct spelling. This will be corrected in the revised version.

p. 332 "an exponential Arrhenius equation" Not sure this an appropriate term. In chemistry "Arrhenius equation" has a specific meaning. It does not to my knowledge apply to all equations of similar mathematical form

The equation is the same one than known in chemistry. The ice hardness (rate factor) depends on temperature in an exponential way, with the other parameters being a constant A, E the activation energy, and R the ideal gas constant.

p. 334 "daily cycle and random weather fluctuations" delete "random"

This will be modified in the revised version.

p. 334 "therefore does not escape to the ocean" run off?

This will be modified in the revised version.

p. 335 add degree symbol to "sin(45)" and "cos(45)"

This will be modified in the revised version.

p. 337 "sea state Ss state"

This will be modified in the revised version.

p. 337 "the magnitude of air velocity" the wind speed

This will be modified in the revised version.

p. 341 "land fraction covered by ice and orography" orography and land fraction covered

This will be modified in the revised version.

p. 342 GCM not defined

It is defined in the introduction (third line).

p. 344 "between the cold eastern part of the Atlantic compared to the warmer western part" isn't this backwards? western half is colder for latitudes > ↑30N

Yes, this will be corrected in the revised version.

p. 345 "the Greenland-Norwegian Sea as well as in the Labrador Sea" the Greenland- Norwegian Sea and the Labrador Sea

This will be modified in the revised version.

Figures

Figure 2 - the meanings of the symbols should be detailed in the caption

This will be modified in the revised version.

Figure 5 caption change “Fluxes toward sediments” to “Fluxes to sediments” and “phosphorous” to “phosphorus” (see also notes on LOCH above)

This will be modified in the revised version.

In Figure 7 there are no colour bars and no numbers on the contours. Also the Greenland plot seems to indicate that the ice sheet doesn’t reach the coastline anywhere. What time period is this for?

For illustrative purposes, we display on the model grid the observed surface elevation of the present-day ice sheets. Contour lines over the ice sheets are for every 250 m of elevation, major ones for every 1000 m are shown in thick. This will be added to the caption in the revised version.

Figure 11: why number the panels abcd and then not refer to these panels in the caption?

This will be modified in the revised version.

Figure 19: I think the effects on vegetation (347/21) are interesting and a map of the vegetation differences (e.g., total terrestrial biomass carbon) should be shown too.

We will include in the revised version a figure showing the changes in total vegetation cover between mid-Holocene and present-day.