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Geoscientific
Model Development
Discussions

Interactive comment on "Description of the Earth system model of intermediate complexity LOVECLIM version 1.2" *by* H. Goosse et al.

Anonymous Referee #1

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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).

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.

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).

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.

"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?

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?

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.

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

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

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

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?

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.

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 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).

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.

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.

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.

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?

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).

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

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.

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.

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.

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?

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.

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 modelled 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.

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).

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.

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 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.

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).

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

p. 315 "which was using a flat bottom" which used a flat bottom

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

p. 316 "the law of the perfect gases" the ideal gas law

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)

p. 320 change "baroclinic one" to "baroclinic mode"

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

p. 320 "20 levels along the vertical" 20 levels in the vertical

p. 322 "presents very small diffusion" has very small diffusion

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 ...

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

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)

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

p. 327 "Alkalinity and dissolved organic carbon" inorganic

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.

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

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

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

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

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

p. 337 "sea state Ss state"

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

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

p. 342 GCM not defined

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 > $\sim$30N

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

Figures

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

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

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?

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

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.

Interactive comment on Geosci. Model Dev. Discuss., 3, 309, 2010.