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2, S317–S321, 2006

Interactive Comment

Interactive comment on "Changes in terrestrial carbon storage during interglacials: a comparison between Eemian and Holocene" *by* G. Schurgers et al.

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The manuscript by Schurgers et al. is an interesting and well-timed paper. It presents a first multi-millennial simulation of carbon cycle dynamics using coupled atmosphere-ocean GCM, and the authors will do no harm by stressing this in the abstract. While Holocene simulations have been performed recently with intermediate complexity models (e.g., Brovkin et al., 2002, Joos et al., 2004), GCM results have more weight as these models have finer geographic resolution and include more processes, for example, planetary waves. Besides, I am not aware about any other published simulation of Eemian carbon cycle performed with geographically explicit model, so this is a clear

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novelty of the paper. The manuscript can be accepted for publication after the following remarks are taken into account.

Limitations of experimental setup

1. Initial conditions. Setting initial conditions for multi-millennial simulation of carbon cycle is a tricky issue. The carbon cycle was never in equilibrium during glacial cycles. In response to climate change, land carbon and oceanic DIC storages equilibrate in about thousand years, but oceanic carbonate compensation, a powerful regulator of atmospheric pCO2, operates on a time scale of about 5,000 years. Prescribing initial conditions from the equilibrium pre-industrial simulation, like done by Schurgers et al., results in cutoff of previous history in lysocline dynamics. For interglacial periods which start right after rapid deglaciations - associated with a release of about 500-1,000 GtC from the ocean - this approach is biased. Most likely, the ocean was a source of carbon at the beginning of interglacials, and this source is neglected in the presented simulations. This limitation should be commented in the paper.

2. Initial atmospheric CO2 is not mentioned in the section 2.3 (experimental setup). Is it 270 ppm? Also, was CO2 interactive during 1,000-yr spinup?

3. Boundary conditions. Slow carbon cycle forcings - excessive carbonate sedimentation in the ocean (Milliman, GBC, 1993), peat accumulation, glacial-interglacial changes in carbonate and silicate weathering -were ignored in the simulations by Schurgers et al. However, on millennial time scale, these small forcings can govern the pCO2 trend. The paper needs a discussion of consequences of absence of these forcings. A conclusion that “The changes in the CO2 concentration for the interglacials was shown to be caused by changes in terrestrial carbon storage in these simulations” (discussion, last para) is a direct consequence of ignorance of these slow forcings. Clearly, land carbon changes were not enough to explain the Holocene CO2 data: simulated CO2 growth was too weak (about 10 ppmv instead of 20 observed). That is why Brovkin et al (2002) used time-dependent scenario of excessive oceanic CaCO3 sedi-

CPD

2, S317–S321, 2006

mentation in their transient simulation. Without accounting for oceanic carbon source, 20-ppm increase in the pCO2 during the Holocene is not possible to explain. Alternative land source hypotheses, like Ruddiman's one about deforestation, seem to be not supported by landuse reconstructions.

Limitations of biosphere and climate models

1. LPJ version by Sitch et al. (GCB, 2003) tends to overestimate carbon storage in boreal forest region. In simulations of 126 ky and 6ky by Schurgers et al., LPJ simulates an increase in carbon storage by 20-30 kgC/m2 in latitudes above 60°N (Fig.4a, Fig. 5a). This is most likely unrealistic. Present-day data suggest a difference between biomass storage in taiga and tundra to be less than 10 kgC/m2, and a difference in litter and soil carbon is less than 10 kgC/m2 for non-wetland areas. A reason for unrealistic changes could be that modeled tree and grass fractions decrease (Fig. 7) so that bare soil fraction increases. In reality, there are some spots of bare soil in tundra, but most of land surface is covered with tundra vegetation including mosses (LPJ does not simulate bryophytes). A real transition from forest to tundra will not lead to such substantial carbon loss as simulated by the model. This affects main paper result about magnitude of land carbon changes during integlacials (about 200 GtC). Discussion of model limitations in the northern high latitudes should be added.

2. Climate model biases. Of course, climate simulated by the climate model is far from perfect. To what degree the cold bias in northern high latitudes is responsible for a strong change in carbon storage in this region? This should get more attention in the paper.

Complications in land carbon analysis in feedback presence

A negative feedback between land carbon storage and atmospheric CO2 complicates analysis of simulations with interactive carbon cycle. Terrestrial carbon storage increases with a growth in the atmospheric CO2. If simulated CO2 growth is less than observed - such as in the Holocene simulation by Schurgers et al. - then land carbon changes are stronger relatively to a simulation with CO2 prescribed from observations. Thus, biases in simulated CO2 trends (too strong CO2 changes during Eemian and too weak CO2 changes during Holocene) artificially smooth a difference between Eemian and Holocene changes in carbon cycle. To some extent, this affects the main paper statement that net increases for Eemian and Holocene were rather similar (about 200 GtC). If reconstructed CO2 data were used for initial conditions, then Eemian carbon changes would be stronger than in the Holocene. This seems to be in line with stronger insolation forcing during the Eemian.

Comparison of atmospheric CO2 dynamics during Eemian and Holocene

A general outcome from the study is that atmospheric CO2 rises in both Eemian and Holocene due to release of land carbon storage. However, Vostok ice core showed that CO2 was fluctuating around a level of 270 ppm during 128-114 kyr BP. This is different from the Holocene dynamics when CO2 was growing up from 260 to 280 ppm. What could be a reason for this difference and why this is not captured well by the model? This VERY interesting question should be addressed in the paper.

Title of the manuscript

The title does not reflect a key point that land vegetation and atmospheric CO2 were fully interactive in the transient simulations. “Dynamics of terrestrial biosphere, climate and atmospheric CO2 during Eemian and Holocene” or “Interactions between terrestrial biosphere, climate, and atmospheric CO2 during Eemian and Holocene” would be a more appropriate title.

Other comments

Page 451, line 12. Brovkin et al. (2002) indeed found a land carbon release of 90 GtC, but this was not enough to explain 20-ppm CO2 rise. They used external carbonate sedimentation forcing to drive CO2, so a decrease in oceanic alkalinity was an important driver in their simulation, like in the one by Joos et al. (2004). See discussion of

CPD

2, S317–S321, 2006

Interactive Comment

slow forcings above.

Page 453, Biome descriptions. A threshold for forest biomes is set to tree cover of 0.8 (Table 1). Since LPJ is using 0.95 as a maximum tree cover fraction, forests are in a narrow range of 0.8-0.95 for tree fraction. As a result, temperate areas with tree cover less than 0.8 are set to temperate grasslands. That means, for example, that temperate/boreal regions with 70% of trees are shown as grassland (or tundra) on vegetation maps (Fig. 2). This is rather counter-intuitive because herbs are not dominant in this case. Of course, this is done just for presentation purpose, but a threshold for forest biomes deserves explicit mentioning during discussion of Fig. 2.

Page 454, line 1: “with the soil temperature from the atmosphere model.” Soil temperature is usually simulated not in atmospheric but in land surface model. Saying “from land surface module of ECHAM3” is more correct.

Page 454, last para: This introductory para is not necessary and can be omitted.

Page 455, line 9: “0.015x10**6 km3 yr-1” is difficult to read. Common units in global hydrology are thousand km3. Besides, relative changes are more important to mention here than absolute values.

Page 455, line 23: “the control run”. Term “model run” is a jargon, “model simulation” is more appropriate.

Page 456, line 10: vegetation data -> vegetation reconstructions

Page 457, line 21: “the increase of the atmospheric CO2 concentration about 40 PgC”. Correct unit for the atmospheric CO2 concentration is ppm, not Gt.

Page 475, Fig. 3a: Please show here Vostok ice core data for the Eemian and discuss data-model comparison in the text. See also a comment above.

Interactive comment on Clim. Past Discuss., 2, 449, 2006.