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Climate of the Past Discussions

Interactive comment on "Fingerprints of changes in the terrestrial carbon cycle in response to large reorganizations in ocean circulation" *by* A. Bozbiyik et al.

Anonymous Referee #3

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Review of Fingerprints of changes in the terrestrial carbon cycle in response to large reorganizations in ocean circulation

The manuscript reports a suite of coupled atmosphere/ocean/carbon cycle model simulations focusing on the earth system impacts of catastrophic freshwater discharge to the surface ocean, as occurred during Heinrich events. The simulations represent the effects of discharge to the north Atlantic or southern ocean on the thermohaline circulation, resultant perturbations of oceanic heat transport and atmospheric processes (e.g., shifts in ITCZ position), and finally the impacts on biotic carbon cycle processes on the continents. Two major conclusions are reached: 1) that carbon cycle changes

associated with freshwater discharge are dominated by changes in vegetation and soil C stocks in South America, and 2) that freshwater discharge to sites of downwelling in the northern and southern oceans produce spatially distributed climatic and carbon cycle changes of opposite sign.

In my opinion the study is a useful attempt to probe coupled systems behavior in response to a well defined forcing that is highly relevant to understanding recent examples of abrupt paleoclimatic change. There are some omissions from the model, such as dynamic vegetation response, which may or may not be important given the timescales considered here. . .nonetheless these issues are well disclosed and as long as they are kept in mind the study is of value. A more significant problem, and my primary concern with the paper, is the relevance of the model boundary conditions to the paleoclimate events under consideration. The simulations were carried out under preindustrial modern conditions, but the results are taken to be representative of the effects that would be expected during the heart of the last glacial period and last deglaciation. In the conclusions section it is acknowledged that due to this discrepancy the standing state of the vegetation was different than in the model simulation, and that this may modulate the magnitude of any carbon cycle changes. But more than this the biome distributions and climate tolerances were different in many regions, global pCO2 levels were 40 – 80 ppmv lower than in the simulation, and global temperature and precipitation distributions were different. To what extent, then, is it appropriate to assert that the coupled system behavior under these very different boundary conditions can be approximated from the simulations? The authors provide some support for this extrapolation in their comparison to paleoclimate records (though this becomes somewhat of a circular argument given the way the records are used to distinguish among freshwater discharge locations), but in general I feel this diminishes the strength of the study. Perhaps simulations using pre-Holocene boundary conditions are an obvious next step, and could lend additional weight to the interesting set of potential mechanisms identified here.

Specific and technical points:

Page 1815, Lines 8-14: Please clarify these statements. Also “stimulating” not “simulating”

Page 1823, Lines 23 – 28: This is quite vague…what is the expected atmosphere/ocean partitioning? You’ve drastically slowed the export of excess carbon to the deep ocean via downwelling? This shuts off a large part of your oceanic carbon sink, leaving you with approximately equal uptake capacity in the atmosphere and surface ocean. Indeed this is what you see at the global scale – subequal increases in the marine and atmospheric carbon stocks w/ a slight lag reflecting transfer from the atmosphere to ocean. Is the marine increase localized in the surface ocean?

Page 1824, Lines 3-6: I’m not sure that this statement is quite right…see (Beer et al., 2010) for example…many of your ‘big change’ ecosystems are probably not precipitation-limited today.

Page 1826, Line 5: Your model doesn’t include dynamic vegetation, so the origin of this statement is unclear. Please clarify or re-cast this statement.

Page 1827, Line 11: The table only shows results from 1 model, so it doesn’t really illustrate the point made here.

Page 1828, Lines 16-18: This statement is not straight forward and invokes a mechanism that is entirely distinct from the shift in ITCZ position discussed throughout the rest of the manuscript. This should be discussed in the body of the ms or the conclusion adjusted to focus on the mechanisms examined here.

Table 3 caption: Specify that the model results are derived from the 1.0 NA case.

Page 1844, Caption 2: “statistical significance is more than 1 sigma (more than 67% confidence) according to the Student’s t-Test” confuses concepts. Please improve the description of the statistical measure, e.g., “…ensemble mean was different from zero at the 67% confidence level (Student’s t-Test).” Same in figures 5, 6 and 8.

Beer, C., Reichstein, M., Tomelleri, E. and al., E., 2010. Terrestrial gross carbon dioxide uptake: Global distribution and covariation with climate. Science, 329: 834-838. doi:10.1126/science.1184984.

Interactive comment on Clim. Past Discuss., 6, 1811, 2010.