

## ***Interactive comment on “Modeling geologically abrupt climate changes in the Miocene” by B. J. Haupt and D. Seidov***

### **Anonymous Referee #3**

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This manuscript explore the hypothesis that in the Miocene, "that some of those warming episodes at least partially might have been caused by dynamics of the emerging Antarctic Ice Sheet, which, in turn, might have caused strong changes of sea surface salinity in the Miocene Southern Ocean. The results suggest that relatively small and geologically short-lived changes in freshwater balance in the Southern Ocean could have significantly contributed to at least two prominent warming episodes in the Miocene."

Strangely the manuscript doesn't actually provide any convincing proxy evidence of sudden warmings that need explaining (one paper is on sea level rise and the other is on the Cretaceous), so the whole enterprise starts off poorly. Other reviewers and commentators have remarked on the poor grounding in paleoclimate data and I concur

with them. The authors need to start from the beginning and properly (a) justify their experiment, (b) carry that logic through their experimental framework, and (c) show that their hypothesis is supported or refuted by data.

In the absence of that effort this study has little merit as a pure modelling sensitivity study. If one evaluates its merits as a sensitivity study, the argument boils down to this. "If we add arbitrarily large amounts of freshwater to high latitudes, it affects the meridional overturning circulation. Sometimes. We're not quite sure why its only sometimes, but we can't be bothered to figure it out".

One does not need to run a numerical model, coupled or otherwise to reach that same conclusion, so what value do the simulations have? It certainly adds nothing as a gedanken experiment. The main argument, from my reading is that stopping southern ocean deep water formation can stop "heat piracy" and thus warm the Southern Ocean, which is a fine idea, but the implementation as a set of numerical experiments needs work. Is the scenario they are exploring even roughly reasonable? Is it in agreement with all the available proxy records? Does the timing (i.e. sequence of events) make sense?

The modelling framework also has weaknesses at the technical level. While the authors claim that it is coupled, in reality it is two models that are run uncoupled and the results are passed iteratively to each other. This may or may not be an issue, it is difficult to evaluate because the validation experiment is in an unpublished masters thesis. Furthermore, numerical hosing experiments performed in the North Atlantic exhibit great sensitivity exists to the location of the hosing and its magnitude and how the water is added (right along a continental margin or spread out over several grid cells of the ocean). This may be important for this study, but is unexplored.

The modelling itself is therefore weak, but could be saved by having a more direct connection with proxies and perhaps with more accurate boundary conditions (the reconstruction looks poor in the Tethyan region). Any improved paper should deal with

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benthic oxygen isotopic records, mg/ca records, SST records, ice volume records and attempt to develop a testable hypothesis and then actually attempt to test the hypothesis. This hypothesis must also include an argument for what is causing the freshwater pulses, what their magnitude might be and where they might be located. Given that even massive hosing in published North Atlantic experiments only has an impact for hundreds of years, the authors must also explain why hosing is at all relevant to the warmings that last for much longer than that. This should also include constraints imposed by mass balance of the ice sheets (and the benthic oxygen isotope record). This is a large set of tasks that will take time and care and lead to a longer and better manuscript; I hope the authors will attempt the undertaking.

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Interactive comment on Clim. Past Discuss., 6, 2687, 2010.

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