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Climate of the Past Discussions

Interactive comment on "Robustness of the Atlantic-Pacific flow reversal in the early miocene" *by* E. Bernsen and H. A. Dijkstra

Anonymous Referee #1

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In three previous modelling studies by the same senior author it has been argued that a flow reversal through the Panama Seaway occurred sometime between 30 and 20 Ma ago in response to the widening of the Drake Passage and closing of the Tethys Seaway (Omta and Dijkstra, 2003; von der Heydt and Dijkstra, 2005; von der Heydt and Dijkstra, 2006). However, none of these studies was really convincing due to the use of flawed paleogeography and highly idealized model setups including a flat-bottom ocean with a globally constant depth of 5000 m. In the present manuscript the authors aim at studying the "robustness" of this result by carrying out a series of sensitivity analyses with a new method to compute equilibrium solutions of the global ocean model MOM4. The authors conlude that the Panama flow reversal is "a very robust feature".

Regrettably, the present study is not much more convincing than the previous ones.

Major problems are:

1) The paleogeographies (more specifically the land-sea distributions) for the Miocene/Oligocene simulations are the same as in the previous studies (Omta and Dijkstra, 2003; von der Heydt and Dijkstra, 2005; von der Heydt and Dijkstra, 2006) and hence are strongly flawed. Some examples: An open India-Eurasia passage during the Oligocene (the passage closed during the Eocene), two "Bering Straits" in the Oligocene and one open Bering Strait in the Miocene (bio- and chronostratigraphic evidence clearly suggests an opening of the Bering Strait between 5 and 5.5 Ma ago; Gladenkov et al., 2002, Palaeo3). Moreover, which part of the Miocene is considered here? The Tethys (note the correct spelling of "Tethys"!) connection to the Indian Ocean closed after ∼15 Ma (Roegl, 1999; Harzhauser and Piller, 2007).

2) Keeping temperature and salinity fixed in the sensitivity studies does not help testing the "robustness" of the Panama flow reversal since the potential effect of a changing density field on baroclinicity (in combination with bottom relief) is completely ignored.

3) Sill depths in the Drake Passage and the Tethys Seaway are varied individually (while the other sill remains absent). This implies that at one important gateway the bathymetry is always unrealistic. It would be more instructive to consider reasonable combinations of sill depths. Moreover, no sill is implemented in the Panama Strait. Why??

4) If we took the results at face value, the study shows that a depth of the Tethys Seaway below ∼1500 m would be associated with an eastward (i.e. into the Atlantic) Panama Strait throughflow (Fig. 6). However, it is likely that the Tethys Seaway was actually shallower than 1500 m in the Oligocene. This would imply that the flow through Panama Strait was always eastward, disproving the notion of a flow reversal in the course of the Oligocene/Miocene.

Given these shortcomings, I do not recommend publication of the manuscript in Climate of the Past. On the other hand, the new method based on Jacobian Free Newton

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Krylov techniques to compute equilibrium solutions of an ocean general circulation model and its application to paleomodelling problems is innovative. Therefore, I would be willing to review a substantially revised manuscript in which the above mentioned problems have been taken into account. In particular, this would imply re-doing the model runs with more realistic paleogeography.

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