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Interactive Comment

# *Interactive comment on* "Modelling ocean circulation, climate and oxygen isotopes in the ocean over the last 120 000 years" *by* R. Marsh et al.

**Anonymous Referee #1**

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General comment

The paper of Marsh et al. presents an attempt to constrain still poorly known magnitude of iceberg discharges associated with Heinrich and Antarctic IRD events by using a climate model of intermediate complexity with a rudimentary 18O module. The author claim that their results provide a further support for their hypothesis (Rohling et al. 2004) about considerable contribution of the Antarctic ice sheet to sea level variations on millennial time scale. However in a view of serious methodological flaw I do not believe that in its present form this paper can be published in Climate of the Past.

1. Model

Compared to the Roche et al (2004), 18O module in GENIE-1 is a very crude one. It is known that the first order effect on oceanic 18O during the glacial cycles is the sea level change. As it was already noted by the 2nd reviewer, prescribed forcing for the 18O model is inconsistent with the reconstructed sea level changes. For example, Siddall et al. (2003) reconstruction implies sea level -80m at 60KyBP (Fig. 3), while 18O in water at the same time implies a twice larger sea level drop (Fig. 10). For the surface 18O changes in atmospheric hydrological cycle are also extremely important but they are totally ignored in the GENIE-1 model. Thus the authors try to constrain freshwater flux associated with Heinrich events using a subtle, third-order effect, but not properly accounting for the first order effect and completely ignoring the second-order effect. Such a problem is likely to be ill-posed and it is not surprising that results are inconclusive. Although the authors cited Roche et al. (2004), they failed to mention one important fact. Roche et al (2004) using a much more advanced 18O model and numerous data from the North Atlantic, estimated a total magnitude of sea level rise during HE4 event as 1-3 m, i.e. one order of magnitude less than that prescribed by Marsh et al. I seriously doubt whether benthic 18O recode from Iberian margin alone can be considered as a reliable constraint on the magnitude and duration of Heinrich events (two other cores are located in the opposite hemisphere). It is also worrisome that the model does not simulated AABW formation for the present day conditions (Fig. 7) and does not simulate intrusion of AABW water into the Atlantic during the whole glacial cycle except for the short episodes associated with HE (Fig. 8b). This is in odd with paleoceanographic data (13C) which clearly show that the deep Atlantic was filled with AABW water during the whole glacial age. The failure of the model to simulate presence of southern water masses in the deep Atlantic may be crucial for correct representation of 18O in Iberian margin core. Indeed, Fig. 11a shows very strong warmings associated with the periods of shutdown of the thermohaline circulation, while more realistic models (e.g. Manabe and Stouffer, 1997; Fluckiger et al., 2006) show that temperature response to a shutdown of THC is restricted to the upper 1 km of the ocean. Furthermore, as it is clearly seen in Fig. 6, the model is



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unable to simulate Dansgaard-Oeschger cycles, the major millennial climate variability during the glacial age. This implies that the glacial THC in the model is much too stable compared to reality and hence cannot be used to constrain the magnitude of real meltwater pulses.

2. Antarctic scenario

While the Northern Hemisphere meltwater scenarios are at least at the upper end of empirical estimates (Hemming, 2004), Antarctic meltwater scenario (WWP-3) is completely ad hoc and unrealistic. Indeed, the total mass loss of the Antarctic Ice Sheet between 60 and 52 KyBP in MWP-3 corresponds to about 75 m in sea level equivalent which is the total volume of the glacial Antarctic Ice Sheet. Even the present day accumulation rate over the Antarctic Ice Sheet (assuming complete cessation of ice calving into the ocean) would compensate only a half of this loss. Under the glacial conditions accumulation rate was twice smaller than at present, which would imply that Antarctic ice sheet would lost 3/4 of its volume between 60 and 52 KyBP. However Antarctic ice cores from very different locations (Byrd, Vostok, Dome C, etc.) present no evidences for such drastic changes in the extent and elevation of Antarctic ice sheet. Even more problematic is the fact that Siddall's sea level record (Fig 3) shows only 10 m sea level rise during the same period that can be readily explained by the melting of the Northern Hemisphere ice sheets due to increasing summer insolation. Wherer than 75 m of sea level rise implied in MWP-3 scenario?

3. Analysis

It is clear that the motivation for this work was to provide a further support for the hypothesis about significant Antarctic contribution to sea level variations on millennial time scale. However, whatever the motivation is, the authors must correctly and objectively present their results. They wrote in the abstract

"During the period of simulation corresponding to Marine Isotope Stage 3, the best agreement between the simulated oxygen isotope record in the North Atlantic and



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core measurements is found in the experiment that includes MWPs around Antarctica as well as into the North Atlantic. This challenges previous assumptions about the dominant role of northern ice sheets in glacial sea-level variability"

However, to be objective they have to add that in two southern locations which are much more close to Antarctica, an agreement with observations is the worst for MWP-3 among all scenarios both for the whole glacial cycle and MIS-3. This fact completely undermines their overoptimistic statement. When taken together with inadequacy of their model and the fact that scenario for Antarctic MWPs is unrealistic, no ground remains for the statements given in the conclusions:

"This result supports both the evidence reviewed by Hemming (2004) for two different types or classes of Heinrich event, and the assertion of Rohling et al. (2004) that Antarctic ice sheets may have contributed substantially to glacial sea-level variability".

4. Bibliography

The paper contains numerous inaccurate citations (see below).

Specific comments

Page 659, line 2. "physical definition" of abrupt climate changes is given not in Rahmstorf (2000) but in Rahmstorf (2001).

Page 659, line 3. "northern hemisphere temperature abruptly shifted by 6-10 C in mid to high latitudes". I wonder where the authors find this number. They are not given in Rahmstorf (2003) and in Broecker's Table 3 these numbers are given only for Greenland temperature. However, for Greenland these numbers are not up-to-date because recent studies based on 15N and 40Ar isotopes demonstrated that Greenland temperature changes associated with D-O events were as large as 10-15 C. For the rest of high and middle northern hemisphere latitudes reliable data about millennial scale temperature changes are simply absent.

Page 659, line 6. "The dominant periodicity often about 1500". For the most of glacial cycle D-O oscillations do not reveal any periodicity. A very weak 1500 kyr periodicity is only seen over part of MIS-3. 3000 and 4500 years time intervals discussed in Alley et al. (2001) are not the periods but time intervals between individual D-O events.

Page 659, line 30. (Cutler et al., 2003), corroborated with independent sea level records, have shown that sea level may have varied by as much as 30 m". Cutler et al. (2003) discussed sea level variations at the orbital time scales. This is a completely different story. On the orbital time scales sea level have varied (without any doubt) as much as 120 m!

Page 659, line 29. "Variation of sea level with a timing history similar to Antarctic climate fluctuations". Again, which sea level variations - orbital or millennial?

Page 660, line 10. "suggesting the global importance of seasonality in ice sheet mass balance". It is unclear what the authors would like to say here.

Page 660, line 10. " Fluckiger et al. (2006) use model simulations to demonstrate how modest freshwater input to the whole Atlantic Ě can lead to abrupt reorganisation of the global ocean circulation and sufficient regional sea-level rise Ě to destabilise the Laurentide ice sheet" Firstly, Fluckiger et al (2006) (or anybody else) didn't show that 0.5-1 m regional sea level rise associated with shutdown of THC is sufficient to destabilize Laurentide ice sheet. They merely speculated. Secondly, the "modest" freshwater flux used in Fluckiger (0.2-0.4 Sv) would result in the global sea level rise by 2-4 meters over 100 years which is much more than the sea level rise associated with the reorganization of THC.

Page 660, line 23. "Episodic break-up of the northern ice sheets is suggested by "Heinrich events"". I do not understand this sentence.

Page 661, line 3. "While some IRD layers are linked to iceberg calving from circum-Atlantic ice sheets that are driven by D-O climate cyclesĚ" How D-O events can drive iceberg calving?

Page 661, line 4. “Ĕ there are some suggestions that Heinrich events might occur independently of D-O cycles (Marshall and Koutnik, 2006)”. Suggestion that Heinrich events represent internal osciallations of the Laurentide ice sheet has been made already in 1993 by MacAyeal, and since then it was confirmed by a number of modeling studies (Payne, 1995; Marshall and Clarke, 1997; Calov et al., 2002). I do not think it is a good manner to cite only the most recent publications.

Page 662, line 22. “The Heinrich mode provides information on the mechanisms which affect the THC” Which information?

Page 662, line 25. Broecker et al. (1985) and (1988) did not consider Heinrich events as a key forcing of THC because Heinrich events were not discovered at that time. In particular, in the first paper it was proposed that a weakening of the glacial thermohaline circulation was a result of a melting of the ice sheets during strong mode of the THC. Thus meltwater pulse was considered as a part of internal feedback rather then an external forcing for D-O oscillations.

Page 663, line 24. “The Antarctic ice sheet is often regarded as highly stable (Ganopolski and Rahmstorf, 2001), but recent studies may suggest otherwise”. This statement is incorrect. Firstly, the instability of the West Antarctic Ice sheet is frequently discussed in scientific and popular literature over the last 15 years. Secondly, Ganopolski and Rahmstorf (2001) have not even mentioned the Antarctic Ice sheet.

Page 663, line 27 and page 664, line 2. Reference to Clark et al. (2002, Nature) and Weaver (2003) in this context is incorrect. The first paper is just a review and presents no modeling results supporting Antarctic contribution to sea level rise. The second one is based on an assumption of the Antarctic Ice Sheet instability rather than proves this instability. In fact, the modeling results in favor of Antarctic origin of MWP-1 were presented in another Clark’s paper (Clark et al., 2002, Science). Reference to Stocker and Johnsen (2003) is also misleading. This paper presents no modeling (or any other) evidences for the Antarctic contribution to sea level variations on millennial time scale.

Page 664, line 14. What does it mean “equilibrium fractionation”?

Page 665, line 13. “To our knowledge, this is the first time that a model of such complexity has been used to simulate a full glacial-interglacial cycle. Previously, experiments of such length have been restricted to somewhat lower complexity.” I wonder whether the authors are aware about a series of very important studies performed by Tarasov and Peltier who used a geographically explicit energy balanced model coupled to a slab ocean model and a high resolution ice sheet model. Although their ocean model is simpler than that in GENIE-1, the use of interactive ice sheets makes their modeling works of , a comparable (if not superior) complexity, especially when the simulations of the glacial cycle are concerned.

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Interactive comment on Clim. Past Discuss., 2, 657, 2006.