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## ***Interactive comment on “Dynamics of ~100-kyr glacial cycles during the early Miocene” by D. Liebrand et al.***

**Anonymous Referee #1**

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In the manuscript submitted to “Climate of the Past Discussions”, D. Liebrand et al. present an impressive high-resolution benthic foraminiferal isotope record from ODP Site 1264 (Walvis Ridge, SE Atlantic Ocean), which closely tracks climate evolution over the latest Oligocene to early Miocene (23.7-18.9 Ma). The authors use a set of 1-D ice sheet models to deconvolve the temperature and ice volume components in the  $\delta^{18}\text{O}$  signal, and conclude that Antarctic ice build-ups occurred during short episodes of low eccentricity forcing. The authors further argue that long-term ice sheet expansion was controlled by a non linear mechanism (such as merging of discrete ice-sheets), whereas ice-sheet dynamics became highly sensitive to the 100-kyr eccentricity forcing during termination phases. This is an interesting and challenging paper, which will potentially provide a valuable contribution for understanding the main processes controlling climate evolution across the late Oligocene/early Miocene. How-

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ever, I would suggest that the authors critically re-evaluate some of the results and interpretations and revise their manuscript before it can be accepted for publication in "Climate of the Past Discussions". My main criticisms are twofold: 1) some sections of the text are rather cryptic and lack relevant information (see details below). These shortcomings could be easily remedied during revision. 2) I am not entirely convinced by the interpretations derived from the modelling results in relation to ice sheet expansion. Firstly, I wonder how applicable the modelling technique employed is for the late Oligocene-early Miocene, when climate boundary conditions were quite different from today's (gateway configuration, water mass distribution, unknown composition for Antarctic ice  $\delta^{18}\text{O}$ , etc.). Although most of these parameters remain poorly known, the authors appear quite uncritical about various alternative scenarios. Secondly, I am puzzled by the fact that local oceanography is not really discussed. The water depth of Site 1264 is relatively shallow (2505 m) in contrast to the deeper records shown in Fig. 3. Could the variability exhibited by the benthic signals in Site 1264 relate also to local water masses and not just to global climate? For instance, the Site 1090  $\delta^{18}\text{O}$  signal does not show the prominent 100 kyr cyclicity after 23 Ma displayed in Site 1264 (Figure 3), although the resolution of the two data set appears quite similar. As the model cannot resolve individual water masses and/or oceans (see Page 2747, Lines 22-24), the significance of the modelling results needs careful evaluation at the intermediate water depth of Site 1264. Please find below detailed comments regarding various aspects of the manuscript

Abstract Concise and clearly written. Page 2742, Line 14: use lower cap for "Supports".

Introduction Page 2743, Lines 4-23: future and past verb tenses mixed in this section, please make consistent.

Section 2 Page 2744, Lines 3-12: too sketchy and general, please provide relevant information concerning site, cores and samples: 1. was a splice available for sampling? 2. what drilling tool was used? 3. how complete was core recovery? 4. explain why Site 1264 is uniquely situated to record major oceanographic changes (Lines 9-11)

Lines 11-12: last sentence is out of place, as you address this topic in subsection 2.2.

Section 3 Page, 2745, Lines 4-6: please explain why samples were sieved into  $>37$ ,  $>65$  and  $>125 \mu\text{m}$  fractions for foraminiferal analysis, and specify from which size fractions benthic foraminifers were picked for isotope analysis. It is quite unusual to use benthic foraminifers from small size fractions ( $<250 \mu\text{m}$ ) for isotope analysis. Correct identification of *Cibicidoides* species is difficult in these smaller size fractions. Correct identification is quite critical because *Cibicidoides* species show different isotope signals. Additionally, isotope values may differ in juvenile tests, which may be common in smaller size fractions. Page, 2745, Lines 24-25: the reproducibility quoted for duplicate measurements is quite low. Could this be due to erroneous identification of *Cibicidoides* species and/or picking of juvenile tests? Please also specify cleaning methods: for instance were tests cracked and sonicated prior to analysis? Any infill may bias the isotope signals (for instance presence of coccoliths in chambers would impart a "surface" signal to the benthic measurement). Pages 2745-2746, Lines 25 and below: could this unexplained offset be due to misidentification of *Cibicidoides mundulus*? Were all samples picked by the same person? This large offset is extremely puzzling, especially because the lower resolution record measured at UF shows no offset!

Section 5 The discussion should be expanded to include a more critical evaluation of modelling results: please discuss the applicability of the modelling technique for a far distant time interval with markedly different climate boundary conditions and the possible influence of local oceanography on the isotope signals.

Figure 7 I do not fully understand the relevance of Figure 7. In Section 5 (Page 2749, Lines 6-8), Figure 7 is briefly mentioned to support age estimates for Mi events. However, the position of Mi events is quite debatable, as these were originally determined in low resolution isotope data sets. Figure 7 shows that this terminology is rather confusing, as the placement of events appears quite arbitrary in the various isotope records shown. Please note misspelling of Kerguelen Plateau in Figure caption.

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