The paper submitted by Liebrand et al presents an interesting new set of high-resolution benthic $\delta^{18}O$ and $\delta^{13}C$ data from the South Atlantic ODP Site 1264, spanning the early Miocene. The careful study of their spectral content, their comparison with other records available over this time interval and the decomposition of the $\delta^{18}O$ record into ice volume and temperature signals using an inverse modeling approach make it possible to bring new insights into the episodes of expansion of Antarctic (and Greenland for M1) ice sheets in response to orbital forcing. The paper is short and focused, well illustrated. It will clearly deserve publication in Climate of the Past after a few corrections or improvements have been performed.

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Here are a few specific problems, questions or suggestions that the authors should address to improve the manuscript.

1/ The time resolution of the Site 1264 record is not indicated in the manuscript. From the amount of samples studied (1754) and the time interval covered by the record (4.8 Myr, spaning from 23.7 to 18.9 Ma), this resolution appears to be around 3 kyr in average.
It is important that the readers have this number in mind since it is above the Niqyst limit for the precession cyclicity. This strengthen the idea that the lack of a strong precession signature in the Site 1264 record is likely due to a poor precession imprint on the deepwater $\delta^{18}O$ and temperature, and not a problem associated to a too low resolution.

2/ Until Figure 7 and the sentence from line 4-page 2749 (« .. on which the latter two periods are close within the age estimates of the Mi-1a and Mi-1aa episodes »), it was not perfectly clear to me whether the four major ice sheet growth episodes discussed in the manuscript had been solely determined based on the high-resolution, Site 1264 $\delta^{18}O$ record, or if the authors focused on specific ice building episodes that seemed to fit stratigraphically with those already recognized and labeled in previous works (the so-called « Mi » episodes). May be a sentence should be added somewhere at the start of the discussion to make it unambiguously clear that the study is fully self-supported by the analysis of Site 1264 data.

3/ Page 2746 (stable isotope stratigraphy).
Over the entire time interval discussed, Site 1264 (and 1090) $\delta^{18}O$ record is about ~ 0.5% heavier than the benthic oxygen records at the two Ceara Rise sites (figure 3). Yet, Zachos et al (2001) pointed out the existence of a ~ 0.4‰ difference in the average $\delta^{18}O$ values before and after Mi-1 at the Ceara Rise sites ; a shift which – according to the authors - is not recorded at Site 1264. Reading these elements, I was puzzled by the fact that the general $\delta^{18}O$ offset between Site 1264 and the two Ceara Rise sites could remain apparently unchanged before and after Mi-1…
Looking at the Ceara Rise records, it appears actually that the « 0.4‰ shift » indicated by Zachos et al does not correspond to an overall shift in the $\delta^{18}O$ values, but to the occurrence – after the Mi-1 event - of more « glacial » episodes with heavier $\delta^{18}O$ values, while the peak « interglacial » episodes retain $\delta^{18}O$ values that are relatively similar to the pre-Mi-1, $\delta^{18}O$ base line. In other words, the average shift in $\delta^{18}O$ at Ceara Rise is actually indicative of an increased variability with enhanced glacial conditions after Mi-1 … an evolution which - from what I can see in the figures presented in the manuscript- is also apparent from the Site ODP 1264 record.
Thus, to me, as far as this ~ 0.4‰ $\delta^{18}O$ shift prior/after Mi-1 is concerned, there might not be a striking difference between the evolution of the $\delta^{18}O$ records at Ceara Rise and in the

Southern Ocean. This implies that the authors shouldn't need to suggest potential explanations to deal with it (i.e. changes in abyssal circulation, flow reversal through the Panamanian Seaway).

4/ Page 2747 (and suppl. material). I must admit that I've always been rather skeptical about inverse modeling of $\delta^{18}O$ for old periods where there is not much quantitative control possible. Having said that, I recognize that the bulk ($\delta^{18}O$, $\delta^{13}C$) data presented in the paper convey enough temporal and spectral pieces of information to support the major conclusions reached by the authors, even if flaws can exist in the inverse modeling.
Recent studies dealing with carefully dated benthic $\delta^{18}O$ records covering the last deglaciation have revealed that there exist important diachronisms (up to several thousand years) in the temporal evolution recorded at remote sites (or located at different water depths), due to oceanic circulation and the complex interplay of deep sea temperature and $\delta^{18}O$ effects (i.e. Skinner and Shackleton, 2006). Given these recent developments, I wonder to which extent the fact that the modeled temperature component represents a global value for all oceans (instead of representing the true temperature signal of the water mass at Site ODP 1264) can have an impact on the conclusions reached by the authors. In particular, it is puzzling to notice that inverse modeling of Site 1264 data suggests that ice-sheet growth precedes (~7 kyr) northern hemisphere polar cooling, a result which is in contrast to previous findings using the same modeling approach (Bintanja and Van de Val, 2008).

5/ The interval of high benthic $\delta^{18}O$ that takes place in 400kyr cycle 52 (around 20.7 Ma) is not far from showing the same characteristics than the four major ice sheet growth episodes discussed in the manuscript. The inverse modeling suggests an as important (and long-lasting) Southern Hemisphere ice sheet extension over this interval, and – although it is not as pronounced as for the other four episodes – the wavelet analysis also suggests that this ice building event is followed by an interval of increased (near) ~100 kyr spectral power (i.e. Figure 4).
If this interval could be interpreted as an additional « major ice sheet growth episode », then all the major ice building episodes recognized from Site 1264 record would be two 400kyr cycles apart, suggesting that the sequence of events is even more regular than concluded by the authors.

6/ As clearly seen from the data, in the sequence of events to and from a major ice expansion event, the increase of ice volume during a low amplitude eccentricity interval is directly followed by an episode of high amplitude, ~ 100kyr variability.

- Could this enhanced 100 kyr variability reflect an increased instability associated to the size reached by the ice sheet during the preceding growth episode?
- Obviously, during these intervals of high amplitude, -100 kyr variability, major ice retreats take place every 100 kyr (!). Couldn't the occurrence of these large amplitude retreats help to explain why the ice sheets are not adequately pre-conditioned to enter a major growth episode at the next node of the 400 kyr cycle? (It's like seeing the succession of events under a different perspective. The authors tend to put the emphasis on how can a large ice sheet finally build-up (i.e. merging of several ice-sheets), whereas I'd rather put the emphasis on why the large ice sheets cannot build up at every 400kyr node..).