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Author's response to reviewers' comments

Referee #1(Anonymous)

1. In the coccolith analysis used to quantify the %Fp (i.e. using image process- ing/ recognition techniques), why is no attempt made to cross-check automated counts with manual counts? The technique is outside my domain, but I am familiar enough with similar applications in speleothems and in such cases some level of comparison is made to manual counts.

Done. We (LB) counted 10 samples and graphed the results in Figure 4. They fit the automated data well.

2. It would be very useful to include uncertainties envelopes on the summer rainfall and dry-season-length anomalies shown in Figure 5.

A useful suggestion that has been undertaken. Now shown on Figure 5, the root mean square error of the prediction.

3. Could the planktic delta-18O foram series please be shown against the tropical stack from which the age scale has been derived?

Done. Shown in Figure 3.

In addition, we undertook all suggested minor editorial corrections.

Referee #2 (Martin Ziegler)

General comments

1. I do not fully agree with the conclusion that the low-latitudes are "already in the next ice age" (page 1061, line 11-12). As this study and other studies on the subject show, the low latitude monsoonal systems are dominated by precession-variations and not the 100-kyr ice age cycles of the late Pleistocene. Therefore, I would not speak of an ice age state in a proxy record that is dominated by precession variability.

We maintain this argument to stress the possible importance of the advance of the monsoon at the onset of the glaciation. However, the phrasing has been changed to be less provocative, e.g. by withdraw of the term 'ice age state'.

2. Furthermore, I think that the conclusion, that low-latitude climate variability is leading global ice volume variations (page 1046, line 14-16, page 1060 line 18-21) should be some- what modified. The monsoon intensity apparently changes during the Holocene,

at a time when the ice volume does not change yet (and also during MIS 5). Therefore the monsoon system weakens before the start of the glacial inceptions. However, the low latitude monsoon systems do not lead at glacial terminations. (Many records show that the onset of strong monsoon intensity sets in after the deglaciation, probably because extremely cold conditions in the North Atlantic during the melting phase prevented the development of a strong monsoon (see the paper by Cheng et al., 2010 Science). The lead lag relationship is complicated by the fact that the different signal are asymmetrical.)

We do not agree with the above comment. We show that in the precession band, monsoon leads d18O and ice volume. Several other studies (that we reference) also drew the same conclusion. We agree that the monsoon and global ice volume variation have an asymmetric response, and that in the deglaciation they are often in phase, but nonetheless the statistic (cross-spectral analysis) and visual inspection of the curve clearly show that, on the precession band, this is not the case.

3. The Banda Sea record and the Chinese speleothem records show the same precession phasing (a lag of approx. 3 kyrs). For the precession phase of the Asian monsoon records we have recently suggested that the monsoonal weakening during North Atlantic cold events (e.g. Heinrich events) results in this observed precession lag (Ziegler et al., 2010 QSR). It seems that these characteristic weak Asian monsoon events do not show up in the Banda Sea record, even though the productivity record seems to have a high enough resolution over the last 50 kyr to detect these events. Still, the precession phase appears to be the same. Here might be also relevant that the age model is based on tuning of a planktonic isotope record, which will be strongly influenced by local SST variability. Is the precession phase of the monsoon proxies potentially influenced by this age model?

This is a valid observation. We agree that our records do not show Heinrich events or other Dansgaard-Oeshger cycles. However, those events are clearly present in the Chinese speleothems. Nonetheless, the precession phase is similar in the two monsoon systems. This phase cannot be ascribed to an incorrect chronostratigraphy because a significant part of the record is based on 14C dating and, therefore, is independent of precession. In that part of the record, there is still synchronism between the Chinese and Banda Sea records, for example in the mid Holocene and the deglaciation (although during a strong Heinrich event).
Also, the d18O record of the Banda Sea record is not greatly affected by the monsoon signal because it does not show a strong precession pattern. Therefore, we do not think that the local SST would produce a significant bias in the d18O chronology.Thus, it is very unlikely that the synchronism observed in the precession for the two monsoon systems is a result of a chronostratigraphy error in one of the two records. Attempts to include such a complex discussion in the manuscript only detracted from the (main) focus of the paper. We therefore chose to abstain from that discussion in the final paper.

4. The method part dealing with the coccolith-based proxy is clearly outlined. However, the part on the Pollen methodology is relatively short. It would be interesting to see for example plots of the calibration datasets.

A useful suggestion that has been undertaken.

5. Some of the interpretations in the paper are drawn upon modern relationships be- tween ENSO and the Australian Winter monsoon. A recent study by Merkel et al. (2010, QSR) suggests "that glacial boundary conditions induce major modifications to ENSO teleconnections and that the "blueprint" of modern ENSO teleconnections should only

be applied with caution to glacial climate periods". Does the conclusion of Merkel and co-workers compromise the interpretations of this study in any way?

No. The model of Merkel et al. (2010) predict a glacial/interglacial variability However, the data from our records clearly show that there is a strong imprint of precession and almost no glacial/ interglacial variation..

Furthermore, in our interpretation we do not directly use ENSO variability to explain the pattern we observe; we clearly state that 'simple explanations based on spatially-homogeneous forcing linked to ENSO or ITCZ migration are therefore not necessarily valid here.' We instead stress that it is the concept of global monsoon that is valid here.

Specific comments

page 1046, line 10
"August insolation": I understand that the authors want to say that the both data sets show the same phase lag and that this lag equals the lag of August (mid?, 1st?) inso- lation with respect to precession maximum. However, it implies that August insolation is the main forcing and this is not necessarily the case.

Text amended accordingly.

line 14-16
Is it possible that the change in the proxy record precedes growth of global ice volume (see the mid-Holocene), however that such a lead is less clear at glacial terminations (see the paper by Cheng et al. on the glacial terminations, science 2010)?

Yes, as stated previously, this situation could be due to an asymmetry of global ice volume growth. However, this does not change the fact that the changes observed in the monsoon lead changes in ice volume in the precession band.

To address the reviewer's concern, we have clarified the text by adding 'increase of' before 'global ice volume', making it clearer that we specifically refer to the onset of glaciations.

page 1047, line 9:
Palaeoproductivity in the Arabian Sea appears to be decoupled from the strength of the Indian monsoon variability, even though in the modern Arabian Sea primary productivity peaks during the summer monsoon season (upwelling) (Ziegler et al, in press, Paleoceanography). This can be explained by changing boundary conditions in the past, namely a change in nutrient delivery to the surface ocean related to changes in the Atlantic Merdional Overturning Circulation (AMOC). Can such an influence (not neces- sarily the AMOC but some other boundary condition) be ruled out for the Banda Sea productivity records? Could Banda Sea productivity on long timescales be influenced by other factors that do not change on inter-annual timescales?

The productivity in the Arabian Sea shows more complex seasonal dynamics, with two seasons of high PP (winter and summer), than the Banda Sea, where only the summer PP is important. It is not clear to us what other boundary condition could be responsible for the inter-annual variability observed here (refer to Figure 2).

page 1048, line 29:

A similar link is drawn between modern and long-term ENSO variability in the past. Recently, Merkel et al. (2010, QSR) questioned the idea of persisting ENSO teleconnections through glacial-interglacial change. Does this affect the interpretations made here as well?

This question is essentially the same as an earlier one made in the general comments by this reviewer. Page 1048 Line 29 is part of the regional settings and we suggest here solely that PP can be a good marker for long term evolution of the monsoon, and possibly ENSO. The focus of the paper is not the discussion of past ENSO like conditions and problems associated therein

page 1052, section 3.4
The details of the palynological transfer functions are not presented. A common difficulty in the interpretation of Pollen records appears to be the distinction between temperature and precipitation (which is probably further complicated by the annual precipitation versus the length of the rain season). Do such potential problems compromise the interpretation of the Pollen records?

The process of developing and testing the palynological transfer functions have been described in lengthy detail in other papers (Cook and van der Kaars, 2006; van der Kaars et al., 2006) and to which we refer in this work. We added in Figure 5 the root mean square error of prediction to give an idea of the level of confidence which is achieved by this transfer function.

The intepretation of the pollen records is not compromised by problems in differentiating between temperature and precipitation seen in other geographical locations. In the lowlands of tropical Indonesia, temperature plays only a very minor part in the vegetation distribution and therefore the interpretation of the pollen data can be reliably based on knowledge of rainfall variability.

page 1053 line 5-7
What is the reason for the change in nannofossil abundance? What about the section from 15 to 35 m? Is it also poor in nannofossils?

The reason for the change in nannofossil abundance is not clear. The abundance in the lower part of the core remains low. Given the lack of clear explanation for this we have removed reference to the observation since it does not add anything interesting to, nor detract from, the present study.

line 23-25 The timeseries appears to be relatively short to analyze 100-kyr periodicity. page

This is not correct. If a strong 100-kyr cycle were present in a 150-kyr record, it would appear as the 'fundamental' cycle. This fundamental signal is not present here and thus we can conlcude that a 100-kyr cycle is not present. The 'bold' reference made to eccentricity has been changed to read 'ice volume'.

1059 line 10-15
The authors might want to refer here to a study which is currently in press in Paleo-ceanography (Ziegler et al., 2010, Paleoceanography, doi:10.1029/2009PA001884, in press). There we argue that the productivity proxies in the Arabian Sea do not nec- essarily represent the strongest summer monsoon intensity, but are instead strongly related to nutrient budget which is linked to the global ocean circulation and hence show a different precession phase.

Thank you, we have added this reference.

Figure 5
At termination II the change in the summer rainfall appears to occur before

Summer rainfall is not used extensively in this paper. Incorporating a greater discussion of this would be out of the scope of the present work.

Technical corrections

Title I would suggest instead: Past dynamics of the Australian monsoon: precession- phase and links to the global monsoon concept

Good suggestion. Done.

page 1048 line 20 larger instead of large

Correction made.

line 22 upwelling instead of upwellings

Correction made.

page 1054 line 9 Why referring to Fig. 2 here? It does not present precipitation data.

Correction made.

page 1058 line 15 should be "in anti-phase", instead of "in phase" (?) page 1061 line 8 insolation instead of isolation

Correction made.