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Discussions

Interactive comment on "Southern Hemisphere imprint for Indo–Asian summer monsoons during the last glacial period as revealed by Arabian Sea productivity records" *by* T. Caley et al.

Anonymous Referee #2

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This paper employsbromine (Br) to reconstruct surface marine productivity, which isthought to be driven by the intensity of Indian summer monsoon (ISM),in the Arabian Sea during the last glacial period. The authors then investigate the roles of southern hemisphere (SH) and northern hemisphere (NH) temperatures (based on EDML and NGRIP d18O, respectively) in the ISMchangesand emphasize upon a potential role of suborbital Antarctica temperature in suborbital-millennial ISM (and also Asian monsoon, AM) changes.They suggest (1) that during 10-16 ka, NH is thought to dominantly controlISM, and (2)that SH temperature plays an important role in ISM during the last glacial (16-40 ka), mainly based on a better correlation between Br and EDML d18O than the correlation between Br and NGRIP d18O.

Increasing researches have highlighted the role of SH climatic variations in monsoon changes at both millennialand orbital timescales(An, 2000; Cai et al., 2006;Rohling et al., 2009; An et al., 2011; Caley et al., 2013).The Br stack record in the manuscript is based on nine marine sediment cores encompassing the whole Arabian Sea. SedimentaryBr contents are assumed as a proxy of surface marine productivity changes, which is linked to ISM changes.These records allow them to make some important findings regarding the importance of suborbital SH dynamic on the ISM changes and then on the regional surface marine productivityin the past.However, I have a number of concerns about the data and their interpretationthat raise questions about the conclusions drawn. These concerns are detailedbelow.

Firstly, large Br differences existamong the cores, although Br patterns seem to be similar. These Br differences can be seen easily from the different coefficient (R2) andP values between Br records with NGRIPand EDML d18O, as listed in Table 2.These are notdiscussed and are warrant explanation in revision. In addition, they compare their records with the Br record of NIOP463, which is also from the Arabian Sea, to confirm Br association to surface productivity changes in Figure 5. Why did they exclude NIOP 463 record from the Br stacking? For age constrains, the authors only list and discuss the 14C and GEs in Table 1, but no information is provided for other cores. Age constrains for other cores should be provided.Are the age models for thesecores better than +/-5 or +/-10 kyr? If so, some sensitivity test is needed for statements in lines 13-17 on Page 9322.

Secondly, they use XRF Br counts as a proxy of surface marine productivity by its correlation with total organic carbon (TOC) from the core KS07 (Fig. 4). If so,why not directly use TOC as a proxy of surface marine productivity? The origin of Br and its preservation are not discussed. On the other hand, what is the correlation for other cores? It appears that the Br stack and TOC of the core SO90-111KL (Fig. 5) are very different. In addition, both Br and TOC are affected by sediment dilution. It is important to use 230Th-normalization, at least for some key time intervals, to be sure that sediment focusing or dilution does not affect both of the Br or TOC signals.

Thirdly, on page 9322, the authors statethat a distinct structure in the Br stack from about 22 to 16 kyr BP is not visible in NGRIP but is similar to that recorded in the EDML18O ice record (Fig. 7). This should be the key finding of the paper. Although I do not doubt that there is a distinct structure in the Br stack, the authors do not describe what kind of distinct structure and do not argue why thesesimilarities can be used to indicate that SH changes might play an important role in ISM dynamics. Furthermore, I can not follow why this distinct structureis challenging the traditional views of a strict NH control.Other factors may be important on this kind of the distinct structure, such as age control and Br sources. In particular, this distinct structureappears only duringthe AIM1 (22 to 16 kyr BP), why not during the periods of AIMs 4, 8, and 12 (Fig. 7)?Again, Fig 7B heavily relies on the age model. Are the age models better than +/-5 kyr?

Finally, and the most importantly, when they argue the pressuresystem of ISM, only the Mascarene high is considered, but not for the Indian low. As discussed by An et al. (2011), strong ISM (high cross-equatorial pressure gradient) during interglacial coinciding low NH ice volume and warm SH temperature is mainly owing to strong Indian low. Similar scenario would occur during the DO stadials. However, a weak ISM during DO stadials is shown in Figure 8b and an unchanged Indian low is shown. In fact, the NH still plays a more significant roleduring the lastdeglaciation as stated in the paper itself and in An et al. (2011), though SH temperature changes affect on the ISM during the glacial periods.

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