

Interactive comment on “Re-examining the 4.2 ka BP event in foraminifer isotope records from the Indus River delta in the Arabian Sea” by Alena Giesche et al.

L. Giosan

lgiosan@whoi.edu

Received and published: 2 November 2018

Comment on Giesche et al. by L. Giosan (Woods Hole Oceanographic Institution) and K. Thirumalai (Brown University):

Giesche et al. present a valuable new dataset of planktonic foraminifer isotopic time series from core 63KA in the Arabian Sea. The authors briefly mention our recent paper on a similar topic (Giosan et al., 2018, *Climate of the Past*, in press). Giesche et al. expand on the original study by Staubwasser et al. (2003), but similar to this previous work, the new data exhibit low signal-to-noise ratio in a very complex coastal region. We argue that a more conservative interpretation is required to take into account this.

[Printer-friendly version](#)

[Discussion paper](#)



Addressing uncertainties is needed to convincingly show that salinity signals, which are indicative of a “4.2 ka event”, or any such millennial/centennial events in the late Holocene for that matter, are detectable in the foram 18O (and 13C) composition in this region.

Surface water masses in the NE Arabian Sea at core 63KA location may be affected by (a) advection of waters from NW Arabian Sea that have a variable upwelling-modified composition; (b) fluvial discharge from the Indus but also from River Hub that is proximal to the core (figure 1 in Giesche et al. supplementary materials); (c) changes in winter to summer rain and snow/ice meltwater with variable isotopic signal that feed the Indus; (d) deep winter mixing bringing Arabian Sea High Salinity Water Mass (ASWHSW) to the surface. All these potential sources and/or modifiers affect the isotopic signal in planktonic forams. For example, ASWHSW mixing would increase the salinity and decrease the temperature of surface waters.

The dynamics of these waters masses is also complex near the coast. For example, the effect of the Indus freshwater plume at the core location is uncertain as summer coastal circulation is directed in the opposite direction along the coast of India. In fact, this is obvious in the modern salinity map provided by the authors (figure 1 in Giesche et al. supplementary materials) where the change in signal at the core location is close to none between summer and winter (< 0.2 psu). If anything, River Hub discharge could affect the salinity at the core site more than the Indus (same figure).

Given this complexity, despite any statistical tests, we argue that interpreting a signal of 0.04-0.07‰ as “significant” or “weakly significant” when intra-sample standard deviation is on the order of 0.12‰ is misleading. Smoothing of the signal and ulterior correlation at a subjectively-chosen window is bound to produce some degree of significance even in random data. The fact that there is no significant correlation in sample-to-sample comparison of the same species (*G. ruber*) at different size fractions is unsettling and should be taken as a warning signal.

[Printer-friendly version](#)[Discussion paper](#)

Are the proxies chosen by Giesche et al. appropriate in these conditions to the task of reconstructing the summer and winter monsoons? We argue that the authors do not make a convincing case for this. First, their indicator for winter mixing, *N. dutertrei*, does not preferentially live in winter. Assuming that the limited sediment trap data cited by the authors is correct, the summer peak abundance in *N. dutertrei* is as important quantitatively as the winter peak due to its more extended temporal range (4 months compared to 1-2 months in winter). Thus isotopic signals in this species will be a mixed summer-winter ^{18}O and not appropriate for detecting a winter monsoon signal. Furthermore, interpretation of ^{13}C values in this and other planktonic species is too simplistic given their known problems (e.g., possible shift in habitat, vital effects). Such problems are not discussed in the paper and interpretation is not even supported in the only cited reference (encyclopedia entry by Lynch-Stieglitz, 2006).

In these conditions it is not productive to extend further our analysis of the paper as all interpretation and conclusions are vitiated by inappropriate basic assumptions. We urge the authors to consider a more conservative approach in interpreting this new data. It is evident to us that solving the salinity signal using forams in this region needs a more sophisticated approach (e.g., Ba/Ca in planktonics; temperature correction from Mg/Ca measurements, etc.). The alkenone-based SST estimates from Doose-Rolinski et al. can only be used to understand a qualitative indicative range of cooling as we now know that the high temperature plateau of the alkenone method limits its usefulness given the high SSTs in the region.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-104>, 2018.

Printer-friendly version

Discussion paper

