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Interactive comment on "Spatio-temporal variations of nitrogen isotopic records in the Arabian Sea" *by* S.-J. Kao et al.

Anonymous Referee #2

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The manuscript written by Kao and coll. tackle the difficult problem of diagenesis and its effect on sedimentary nitrogen isotopes. Recently, this question has been intensely reviewed by Robinson et al. (2012), but here Kao et al. focus on the Arabian Sea adding a new sedimentary nitrogen isotope record spanning the last 35 ka from the south-eastern Arabian Sea off India. By compiling and comparing nitrogen isotopes on nitrates, particles and surface sediment from the northern and the southern Arabian Sea, they show that the differences in sedimentary d15N between the northern denitrification-influenced and the southern Arabian Sea can be better explained by the water depth effect (besides already shown by Robinson et al. in 2012). The interest of the ms of Kao et al. is that the authors applied a correction to account for the water depth bias. Not surprisingly, all the records then show similar high values during the Holocene and similar low values during the last glacial period, but a different trend

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during the late Holocene that the authors attribute to a strengthening of the summer monsoon and associated increase in water column denitrification in the northern AS. I do not have many comments to address on this ms that should be published after minor to moderate corrections/revisions. In spite of evident English mistakes that must be corrected before publication (but noteI am not a native English speaker and then cannot help!), the article is easy to read and focused on its main objectives. My comments concern especially the new record from the SE Arabian Sea which is unfortunately not enough deeply discussed in the ms. My first comment concerns the stratigraphy. Even the stratigraphy of core SK177/11 is well constrained by 7 AMS 14C, the d15N record is very different from the two other records from the southern part of the Arabian Sea (Fig. 8a; cores NIOP 905 and SO42-74KL). Is this difference only the result of an age offset due to different methods of chronology or does it reflect a peculiar dynamics off SW India. Besides, more details concerning especially the oceanography and climatology (nutrients, production, water masses, currents) of this region would be then helpful to better constrain the dynamics of the region. For instance, are the d15N variations just a matter of denitrification versus nitrogen fixation? Maps showing nitrate dynamics off SW India (concentration, utilization) would be helpful. You cannot say that the d15N low at 13 ka occurs during the YD event which is younger (Fig. 3 and text page 8720, lines 15). Anyway, this low should be in phase with those centered during the YD of cores NIOP 905 and SO42-74KL (Fig. 8a). Please clarify. C/N ratio and d13Corg (Fig 3 and 4) are clear indications that organic matter is pristine autochthonous (planktonic) material irrespectively of the climatic period. However, I would suggest the authors to plot the C/N profiles in Fig. 3. Moreover, the authors noticed that “An abrupt decrease in d13C was observed in concert with the dramatic decrease in d15Nbulk at the start of deglaciation”, and that “A sharp decrease of d13CTOC in SK177/11 at the start of deglaciation (Fig. 3b) may indicate a rapid change of physical circulation had occurred in characteristics of the intermediate water flowing into the AS”. They should also notice that the d15N and d13Corg profiles mirror each other. It might be important and interesting to discuss these observations in more details. What do the authors mean

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by a rapid change of physical circulation in characteristics of the intermediate water flowing into the AS? In the core of the ms, the way the authors made the corrections to remove the bias due to water depth is not clear. Please improve. My last comment concerns the choice of the authors to reject in their compilation the record of Pichevin et al. (GBC, 2007) from the NE Arabian Sea (Kao et al., page 8725, lines 14-15), arguing that it might be influenced by terrigenous input. This assumption contradicts the interpretations of Pichevin et al (2007). The authors should integrate the record of Pichevin in their comparison.

Minor comments : Refs : Mollenhauer et al. instead of Mullenhauer et al. Fig. 8a: I would suggest the authors to separate in two different graphs the 3 cores from the southern part of the Arabian Sea from the northern cores (including Pichevin's core). The figure would be then more readable.

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