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Climate of the Past Discussions

Interactive comment on "A ∼400 ka supra-Milankovitch cycle in the Na, Mg, Pb, Ni, and Co records of a ferromanganese crust from the Vityaz fracture zone, central Indian ridge" *by* R. Banerjee et al.

Anonymous Referee #2

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In this manuscript the authors present major and trace element time series obtained by EMPA analyses of a hydrogenous ferromanganese crust from the central Indian Ocean. The records were dated by a combination of 230Thex dating in the uppermost part of the crust and Co-constant flux modeling. The authors claim that there is a ∼400 kyr cycle in the records of certain elements which they ascribe to astronomical forcing of past environmental conditions in the Indian Ocean.

Newly generated paleochemical time series data and have been produced and previously unidentified cyclicities in metal abundances in ferromanganese crusts including

important implications have been found that should in my opinion be made accessible to the readership of "Climate of the Past". The current version of the manuscript is, however, not up to required standards for publication and requires a major revision. The authors need to significantly improve their interpretations and the amount of background information they provide in many places, as outlined below. In many cases the statements of the authors are not precise or highly speculative and not adequately supported by appropriate references.

The manuscript also definitely needs to be reviewed by at least one specialist on statistical analyses of time series data because I cannot judge the reliability of the statistical approaches performed by the authors!

Also the manuscript urgently requires careful proofreading by a native English speaker and rewriting in most parts because style and grammar are in very bad shape, which sometimes makes it very hard to follow what the authors actually want to say.

Introduction:

The authors need to say clearly since when the 1 mm/yr northward movement of the Indian plate occurred because clearly the emergence of the Himalayas already started about 35 million years ago. What does this exactly have to do with the Capricorn Plate?

In mentioning previous occurrences of Milankovitch cyclicities in the Indian Ocean, the authors need to precisely say which paleoenvironmental factors varied at which frequencies. It is clear that the benthic oxygen isotope record varied due to ice volume (not even mentioned in the introduction!) but other than that I am mainly aware of paleo SSTs and terrigeneous inputs (dust) which for example vary on precessional time scales together with the monsoon cyclicity. The authors need to say precisely which parameters changed on 400 kyr time scales in previous studies.

Also, which parameters changed on Milankovitch periodicities in the previous Han et al. (2003) study on a ferromanganese encrustation and which forcing factors were

responsible were responsible for these changes.

Sample and methodology:

P. 1313, line 26-27: The sentence does not make any sense. Omit!

P. 1314, line 14-15: If I understand correctly, the authors measured their elemental data at a resolution of 100 μm. Then for a reason that I don't understand they averaged the data over a mm. Why was that done? Actually, if the authors want to extract a 400 kyr cyclicity in their records, then the higher the resolution the better and 100 μm would correspond to about $\sim$10 kyr. Also, were the data with totals below 40% omitted (holes in the section) and what was the water correction? Were analyses that obviously hit detrital grains omitted? What is the precision of all the elemental analyses and was the precision the same for all elements. In table 1 with the elemental data, for all data points below 1 % abundance, the numbers need to be given with two digits after the comma.

P. 1315, line 8: Co concentration below 0.8 %!

P. 1315, line 14: Which limitations do the authors mean? Be precise!

P. 1315, line 15: Importantly, the bottom age. . . This is strong support for the chronology the authors present!

P. 1316, first paragraph: The authors did not measure the 234U activity for each 230Th measurement, but only the average. This is not very reliable, in particular in the older part of the dated section of the crust (2-8 mm).

P. 1316, line 2: . . .is the seawater-derived portion of the 230Th. . .

P. 1316, line 14-15: This statement is completely wrong and needs to be omitted. The implication that only 6 % of the 230Th water column inventory is incorporated into the crust is clearly not that the crust grew episodically but simply that most of the water column-derived 230Th is adsorbed to particles and is deposited in the pelagic

sediments and not in crusts.

P.1316: From the inset in Fig. 1 I calculate a growth rate of $\sim$14 mm/Myr for the section between 2 and 8 mm depth, which makes sense also for the development of the growth rate in the rest of the crust. 72 mm is clearly far too high!

P. 1316: The authors need to provide the Co-age equation they finally applied. In table 2 the digits after the comma for the data need to be adjusted. At the maximum 1 digit makes sense in view of the precision of the data. Are these 1 or 2 sigma errors? The 210Pb data are not discussed in the ms. and need to be omitted.

Results and discussion:

P. 1317: The Co shows a 400 kyr cyclicity, which means that the growth rate varied on a 400 kyr cycle, which points to a change in supply of Mn and/or Fe. How could that be caused?

P. 1317: It is in my opinion in no way possible to extract a 400 kyr cycle from a 1 million year record!

P. 1317: Again, as far as I can see, all those 400 kyr cyclicities are a consequence of changes in detrital inputs, so are the observed variations related to productivity and thus possibly fertilization effects of the surface waters?

P. 1318: Why would the Mg content in the crust vary with cholorophyll production? This does not make any sense at all. If you wanted to look for a potential indicator of productivity, in my opinion this could only be Ba content, or more precisely Ba flux (Ba/Co). But even then it will be difficult to relate the metal variability to particular environmental factors as for example shown by Wen et al. (1997).

Also the Mg and Na content of the crusts can not have derived from any variability in the seawater concentrations because these have definitely been constant for the past 10 million years! The variations must be dilution effects caused for example by variations in detrital fluxes.

You can in no way distinguish cosmic supply of the mentioned elements (line 27) because their cosmic supply is far too small compared with the weathering supply from the continents. This is complete nonsense.

P. 1319: All the considerations of the time series analyses have to be reviewed by a specialist, which I am not! In addition, what do the authors mean by aliasing effect?

P. 1320: Is the amplitude or variance of the 400 kyr cycle one 10th of the 100 kyr or the 41 kyr cycle?

Conclusions:

In the conclusions the age model needs to be discussed first and then the implications of the data.

Additional comment:

There are two high resolution elemental time series from nearby locations in the Indian Ocean (Frank et al., 1999) that the authors could perform their time series analyses on in order to support their conclusions by more than one record.

Additional reference:

Wen X., De Carlo E.H., and Li Y.H. (1997) Interelement relationships in ferromanganese crusts from the central Pacific Ocean: Their implications for crust genesis. Mar. Geol. 136, 277-297.

Interactive comment on Clim. Past Discuss., 6, 1311, 2010.