Nonlin. Processes Geophys. Discuss., 1, C360–C363, 2014
www.nonlin-processes-geophys-discuss.net/1/C360/2014/
© Author(s) 2014. This work is distributed under
the Creative Commons Attribute 3.0 License.

Nonlinear Processes in Geophysics
Discussions
Open Access

Interactive comment on "Estimation of sedimentary proxy records together with associated uncertainty" *by* B. Goswami et al.

Anonymous Referee #2

Received and published: 26 July 2014

General comments:

The manuscript by Goswami et al. deals with an important yet often overseen problem - namely how the dating uncertainties affect the interpretation of paleoclimatic proxy records. The work highlights how the nature (variability) of the proxy signal itself adds to the final uncertainty of the age model – also an issue rarely realized by researchers working with proxy datasets. The text is exceptionally well structured and neatly written. The authors provide a step-by-step algorithm estimating the probability of a data point having a certain calendar age. I am looking forward to an open source, easy to use program, which can assist me with such a task. The only problem I see is the choice of exemplary data for a dataset provided by the authors (Lonar Lake data). It appears, that the authors do not feel at ease with XRF measurements and their arguments

could have been much stronger if applied to quantitative instead of qualitative data. My comments concerning problems of XRF data (actually never even mentioned as such in the text!) are listed below together with specific comments. I like the proposed approach and appreciate the elegant simplicity of its presentation. I would like to see this work published. However, it requires some changes from its present state – either providing a different example or a solid clarification for used one.

Specific comments:

Page 1025, line 3: change 'allow us to investigate' to 'allow us investigating'

Page 1025, line 15; page 1038, lines 11-13: how do the authors know what exactly Ca and Al stand for? They take for granted that Ca represents groundwater inflow and Al surface erosion – and this is the first time ever these data are reported. I assume that for a proof of concept it is better to use a dataset that is settled and has already been published. At least, the authors should explain in full what their interpretation of XRF data is based on.

Page 1026, line 4: change 'unobservable climatic variables' to 'past physical variables that cannot be directly measured'

Page 1027, line 3: add 'ever' after 'first'

Page 1030, line 22: add '(age reversals)' after 'outlying values'

Page 1030, line 25: Please do not mix 'age' and 'date'. 'Age' is an interval while 'date' is an exact time point in the past. Even if informally speaking 'radiocarbon date' is fine, in order to get the exact point in the past you need to calibrate it – and here the problem you deal with in this paper starts... I suggest changing 'radiocarbon dates' to 'radiocarbon ages' or 'radiocarbon dating points'.

Page 1037, line 17: if DWF stands for 'depth-spanning weight function' do you really need 'the weight function DWF'?

Paragraph 3.1: I realize that the authors use here a synthetic record but why given as " X ‰ " ? Presumably, it is supposed to represent a synthetic δ18O record (as suggested by Fig 4) – then please state it clearly. Or else, just call the synthetic dataset "Proxy value" to avoid confusion. Adding a temporal resolution of synthetic data set would also helpful.

Paragraph 3.2: The authors admit that they use unpublished data and without spending a word on how they where actually measured they take for granted what the data stand for and how to interpret them. This choice is the weakest point of the manuscript and has further consequences for their argumentation. Additionally, it shows the authors' poor understanding of data they are working with – a pretty ironic turn in their paper as they are trying to help paleoclimatologists to understand the dangers lurking behind incautious application of age models. Please see my comments below.

Page 1040, line 6-7: what do the authors mean by 'proxy measurement error were in order of parts per million'? Unless they mean a 'dating error' (and just made a tipping mistake) there is a problem. Similarly, page 1045, line 19-20: 'the proxy measurement error is already at the instrumental limits of precision'. I recon - from the unit used (cps, counts per second) – the authors are using XRF (X-Ray Fluorescence) measurements. XRF scanners provide the bulk intensities of major elements (e.g. Al, Si, K, Ca, Ti, Fe). The data are given as e.g. Ca area, which refers to the intensity of the element, measured as mentioned in cps or alternatively total counts. Traditionally XRF data are reported as 'XRF Ca-intensity (cps)' or 'XRF Al-intensity (cps)'... etc... Now, coming back to the original question, how do the authors come from cps to ‰ XRF measurements are qualitative – therefore it is extremely difficult (though not impossible) to estimate the error of the measurement. One of the more tedious methods is scanning the same core (or epoxy block for that matter) several times – still this will provide again only qualitative information. Preparing additional discreet set of samples and running a quantitative measurement on e.g. ICP-OES or ICP-MS will be the most time consuming but also most appropriate way to calibrate XRF data. Nowadays, XRF

data are mostly used as a 'quick & dirty' method to have a first look into the composition of the sediment and for identification of interesting intervals – basically first screening. E.g.: Are there any cycles, or abrupt changes? What is the amplitude of change, shape of the curve? Are there any similarities to reference records like ice cores? This is the reason why the error of the measurement is not as crucial as for quantitative measurements. Moreover, depending on the instrument used to measure the profile (AVAATECH, Itrax, etc...) and its settings, intensities of single elements might be too low to provide a reliable signal. Presented Al intensity seems to me too low to be really significant. I believe that by their nature XRF data are not an easy substance for statistic approaches (given the difficulties in estimating XRF measurement uncertainties), and in particular the Al data chosen by the authors are suboptimal to illustrate the proposed method. If the authors insist on using them – please provide all the limitations of the XRF measurement (similarly as you they did for their own method) and necessary background information to make sure you are not interpreting noise.

Page 1046, lines 16-20: here my comment boils down to the nature of XRF data. Yes, the variability of Al is indeed higher than that of Ca, at the same time the counts (cps) are very much lower – a strong indicator to treat the data with caution. If compared on the same scale Al will show as good as no changes at all so unless there is a good argument for using Al intensity this dataset is simply not convincing and the authors should employ a more robust/significant proxy.

Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 1023, 2014.