Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-70-AC1, 2021

Climate of the Past Discussions
Open Access
EGU

Interactive comment on "Long term trends in aquatic diversity, productivity and stability: a 15,800 year multidecadal diatom study from Lake Baikal, southern Siberia" *by* Anson W. Mackay et al.

Anson W. Mackay et al.

a.mackay@ucl.ac.uk

Received and published: 3 February 2021

Dr Gavin Simpson comments

1. My first comment relates to the diversity measures and how they have been handled. Hill's numbers, like other count, or count-based, measures are affected by sampling effort; all else equal, the greater the sampling effort the greater the diversity, the greater the Hill number. In palaeolimnological studies, sampling effort concerns at least two elements of the data collection and analysis process: i) the sample count,

Printer-friendly version

Discussion paper

here _300 valves; and ii) the sedimentation processes, accumulation rates, and depth-based sampling that generated the sediment slices that were counted. The authors appreciate these issues and employed methods to handle varying-effort source 1 via a bootstrap approach. . . It is unclear to what extent variation in accumulation rates might contribute to or obscure the underlying trends in diversity metrics in the core as accumulation rates are not presented alongside the time series of diversity metrics. . . A more complicated alternative might be to not use any rarefaction at all, and instead model the observed data using a regression approach, with an offset that includes both the count total and the number of years per slice. For the regression model itself, a GAM seems appropriate given the non-linear change in the metrics over time.

Authors Response âĂć Dr Simpson is essentially asking how can we account for variable SARs (which vary by a factor of 4) in interpreting Hill's numbers for species richness / diversity, and he helpfully provides a number of suggestions to consider. We agree that we should take into account SARs when presenting and discussing our diversity measures. Therefore, in the main body of the manuscript we will present new analyses for rarefied Hills diversity measures N0, N1 and N2 divided by the accumulation rate over time. âĂć Note that we have also taken the opportunity to update our age modelling using IntCal20, although specific dates over our timeframe are similar to those obtained from using IntCal13.

âĂć We have also looked at Dr Simpson's suggestion of the more complicated alternative, calculating Hill's diversity measures without randomization and using the predicted values of a GAM model specified as gam.rich1 <- gam(N0 $\sim$ s(age, k = 70), offset = log(counts*AccumRate), family = poisson, method = "REML", data = diversity). These will be presented in Supplementary Info, alongside a brief discussion of its merits and differences with data presented in the main paper.

2. The second area I wish to comment on is in the interpretation of the moving window coefficient of variation (CV) results, especially Figure 4. What is being shown in Figure 4? CV is a unit-less variable, but each plot has a label on the y-axis. In the upper panel

we see variation in the CV but most of these changes are relatively small suggesting CVs of between 9 and <3 % of the mean. Are these values biologically meaningful? Is the fact that we see CV of log(BVAR) declining in the Late Glacial and early Holocene, really an important decline? The N2 signal seems to show more variation; apart from the Late Glacial period, CV values are between 10% and 50% of the mean N2. Those numbers seem, however, at first sight to indicate that the stability of N2 is much less than log(BVAR). My comment really relates to one of whether palaeo productivity stabilised, and if so, how does that mesh with the interpretation that N2 was also stable for much of the Holocene where the variability in CV values for N2 is so much higher than for BVAR?

Authors Response âĂć Dr Simpson's concerns here relate to (i) whether the interpretations we make with respect to changing CV over time are ecologically meaningful for log(BVAR) (as variation is rather minor) and has palaeoproductivity really stabilised during the Holocene; (ii) we originally interpreted N2 as being rather stable for the Holocene, yet higher variability for N2 is counter to that interpretation.

âĂć We note that Dr Simpson doesn't object to the use of CV, but had queries about interpretations surrounding stability. We accept that CV will be a rather crude measure of stability, and we will reanalyse CV for N2 data taking into account variable SARs, as undertaken above.

3. The Kruskal Wallis analysis is non-parametric, but it is not without assumptions. The principal assumption that is certainly violated here is an assumption of independent observations, because your data are a time series. I don't know if there is a way to correct the p-values for the loss of degrees of freedom due to the dependence between samples (as is often done for regression, assuming AR noise), but if not, one might have to use a permutation or resampling/bootstrap procedure where the permutation or resampling is done in a manner that preserves for correlation structure (ruling out simple permutation and simple bootstrap resampling).

Authors Response âĂć We agree with Dr Simpson's comments and will delete the test of whether total, spring and autumn productivity, and N2 diversity were different for the three centuries immediately before and after the three Holocene climate disturbance events using the non-parametric Kruskal-Wallis one-way analysis of variance (Table 2).

4. My final comment relates to the correlation analysis as shown in Figure 5. What type of correlation are you showing here? All the standard correlation coefficients are bounded -1, 1 but the plot shows values >1. I suspect what you're showing is the coefficients from a linear regression through the scatter of productivity and diversity data in the 1000 year moving windows? This needs to be fully explained in the methods. Putting an informative label on the y-axis would also help the reader understand this figure. Another problem with Figure 5 is the use of the p values encoded as colours on the plot. It's impossible to tell what the p-values are at the low end, where the interest lies, because of the colour scheme used. Also, you (or the reader) are at risk of making a massive multiple comparisons mistake here. If you are going to use the p values then you would need to correct them for multiple comparisons (i.e. for as many tests as the number of data points shown) using the false discovery rate to adjust the p values.

5. Then you could use a binary indicator perhaps to show which values remain significant. Other questions remain with this correlation analysis: is a linear model in a moving window a good fit to the data?; if relying on the p values, how biased are these because neither the response nor the covariate are independent; if this is a linear regression, which variable played the role of a dependent and independent variables?

Authors Response âĂć These comments were perhaps the most substantial for us to deal with as they go to the heart of what we are trying to get out from our data. As pointed out, one of the problems of the moving window analysis was that it was highly subjective to the size of the window, and Dr Simpson was right in that sometimes a linear model might not be the best model to describe the data.

âĂć We have decided instead to take a different approach to investigate productivity-

diversity relationships over time, which is hypothesis driven and, we believe, more robust. Through independent palaeoclimate studies, we have identified three periods of climate change in southern Siberia over the past 15,000 years. A period of rapid warming (the Bølling-Allerød), cooling (the Younger Dryas) and a period of relative stability (mid-Holocene). Using a non-linear approach, we will test productivity-diversity relationships.

6. Regarding the principal curves, how confident are you that the data are well represented by a single gradient? The eigenvalues of the CA that you used for the starting curve would be a guide as to whether there is remaining structure in the data on axes 2 and higher. PrCs are really useful when there is a strong single gradient in the data, but when there are secondary gradients my experience is that they are much less useful and can get stuck in some weird solutions that don't make ecological sense. You can get an idea of this by looking at the complexity of the smoothing splines fitted to individual taxa; if these are not simple linear, monotonic, or unimodal curves than it is more likely that the PrC is being asked to do too much and is a good sign of problems with the fit. I mention this because of the very rapid changes in the PrC scores, which can happen when the PrC itself is too complex; it would be useful for the reader to see the 2d ordination with the PrC superimposed on it, perhaps in the supplementary materials?

Authors Response âĂć As suggested by Dr Simpson, we checked the result of the CA. The variation capture by axis 1 was only explaining ∼4% of the total inertia/variation. The first four axes seem relevant (in total explaining a little bit more than 11% of the total variation), so based on this actually PrC might not be a very good choice in this case.

âĂć Comments from Reviewer 1, Point #5 have already persuaded us that we need to reanalyse our data based on separating out planktonic taxa from benthic taxa, as multiple gradients indicated by the PrCs may be, for example, reflective of the different habitats.

7. Finally, throughout there are few attempts to quantify the uncertainties in the quantities you estimate, interpret, discuss, and present to the reader, or to compare the observed results with appropriate null models of no change/trend. This makes it difficult to gauge the overall support of your interpretation that comes from the data and the analyses

Authors Response âĂć We think through a more focussed, hypotheses-driven approach our interpretations that come from the data and the analyses will be more robust. We will ensure that uncertainties are considered throughout.

8. Minor Comments- L189: there's an extra parenthesis before "De'Ath"; - L215: here and throughout, the superscripts in your units appear to have gone missing, perhaps during the conversion to PDF?; - L224: here and throughout, the way you present the Hill's numbers changes through out the manuscript. Sometimes the "N" is in italics, sometimes not, and sometimes the number is in italics and sometimes not; - L243: the RËĘ2 should be upper case R and the 2 is not superscript; - L294: delete "as" between "declined" and "rapidly" or change the sentence to indicate what BVAR changed as rapidly as; - L501 add "zone" after "photic"

Authors Response âĂć These have all been taken account of. For information, all Hill's numbers are given with N italicised, and the following number not, i.e. N0, N1 and N2 (as per Felde et al. 2016).

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-70, 2020.

Printer-friendly version

Discussion paper