Clim. Past Discuss., 5, S211–S216, 2009
www.clim-past-discuss.net/5/S211/2009/

Climate of the Past Discussions

Interactive comment on "Mid-Holocene regional reorganization of climate variability" *by* K. W. Wirtz et al.

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Received and published: 17 April 2009

Reply to Interactive comment of Referee 1

Referee 1 pointed out several relevant methodological problems. Most of them concern the robustness of the approach against specific assumptions or the value of control parameters. Some of the sensitivities have already been studied before submission, but were not sufficiently documented in the MS. Other sensitivities have now been calculated anew. For the forthcoming version of the MS, we envisage a more comprehensive documentation of how (in)sensitive the results of our synthesis work are with respect to specific methodological choices.

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1. **ref2:** The point of concern is a shift of individual agecontrol points that may stretch or condense a section of a series 8211; and this generally has a strong effect on spectral properties. The study depends crucially on the age models of the underlying time series. It is completely unclear, if the authors employed some quality control of the existing chronostratigraphies in order to ensure consistency among the proxy records.

We only selected for studies that had a well documented age model. Our analysis of dating points in the studied Holocene layer and dating uncertainties revealed that $>$90% of chronostratigraphies had 7–15 dated samples and dating uncertainty (σ) between 20 and 120 yr, generally increasing with age. Exceptions are, for example, ice cores with much higher precision. The variety of techniques (C^{14}, Th^{230}/U^{234}, varve chronology ..), however, hindered a more in-depth intercomparison.

We tested for the referee's hypothesis that spectral properties are highly sensitive on dating uncertainties and found the contrary: All 130 time series were split into 10 sections which were subsequently either stretched or condensed by σ=100yr, emulating a nearly maximal distortion compatible with the average error statistics. Sectional dilation/compression will produce an upper estimate of the possible stretching and condensing effect, largely overestimating dating uncertainties in particular for Upper Holocene strata. Despite some visible effects on spectra (shifts in characteristic frequencies and spectral intensities, cf. renewed figure 2 for one example, Isotopic oxygen, Soreq Cave) aging distortion turned out to be not as critical to the outcome of our study as suggested by referee 1. Only in 10.5% of cases, time distortion affected Upper/Lower Holocene switches in significant spectral peaks. Also regional patterning of mode changes turned out to be similar to the case of undisturbed records.

To conclude, at the global and regional scale spectral analysis is not sensitive against uncertainty in dating, although individual spectra can indeed be modified

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by maximal distortion of time-series. In the next version of the MS we will explain how we reached to this conclusion.

2. **ref2:** From the introduction it appears that the authors are aware of the non-stationary character of some records. However, by employing spectral analysis, which provides a time-averaged estimate, they completely ignore this aspect in their analysis. The early Holocene portion of the time series may include some well known perturbations (YD-PB transition, 8.2-ka event). Spectral analysis will then likely result in peaks at millennial-to-centennial time-scales. These peaks are an artifact resulting from the 8220;events8221; and should not be interpreted as modes.
We disagree on the first part of this argument. To reveal non-stationary (i.e. time dependend) features of time-series in a simple and robust way was the major purpose of the bootstrapping approach. Compared to other approaches like Wavelet analysis, window bootstrapping is a much simplified and, in combination with the REDFIT algorithm also more robust, methodology to detect non-stationarity (see also our reply to Ref2). It led to a consistent and aggregated description of regional trends in climate variability during the Holocene.

   Nevertheless, we found the second criticism compelling. While previously thinking that differences in the nature/origin of disruptions are sufficiently acknowledged by employing, in addition, a non-spectral analysis (counting of anomalies), we agree with referee 1 in that singular (geomorphological) events deserve for a more careful treatment. The catastrophic freshwater drainage from Lake Agassiz and its immediate effect on overturning circulation (e.g. Clarke et al, Science 2003, Kleinen et al, Science 2008) is a singular (geomorphological) event in the Holocene and should not be confounded with inherent oscillations of the climate system.

   In order to reduce the spectral ”artifacts” possibly induced by two specific singularities, we repeated our analysis after treating the time-series at the YD-PB

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transition and around the 8.2-ka event. When anomaly intensity exceeds unity in the periods 8-8.4 (as is the case in only 16% of records) and 10.8-12 kaBP, all data in the respective interval are rescaled so that anomaly intensity of the detrended time-series falls below unity. When applied to the relatively small fraction of records containing singularities, this procedure often led to more significant peaks in the Lower Holocene spectra (where the Upper Holocene part of the time-series is bootstrapped), since removal of (strong) singular events in the lower part increases regularity, enhancing the intensity of modes in the power spectrum. Changes in periodic modes particularly at Atlantic sites become more homogeneous (negative variability trend), and differences between the non-cyclic and cyclic analysis decrease (cf. Fig.7 with Fig.5-6 in old MS).

To sum up, the overall effect of this treatment remains small, as quantified by less than 5% of reversals in variability trends (raw vs. treated data). Though these modifications even enhance the regional pattern in support of the main story of the MS, we propose to stick to the most simplest approach that is the analysis of raw (published) time-series data. It is important to note that this robustness only arises with the high number of proxies used in our study.

The new version will describe (in)sensitivities to singularities, possibly distorted chronologies or other treatments like normalization and detrending in more detail.

3. **ref2:** The justification of splitting the time series at 6 ka is not well justified. If such a 8220;turning point8221; in terms of the spectral character (and climate variability) does indeed exist, it should be the result of the analysis and not an input to the analysis. The study remains completely unconvincing with regard to the robustness of the timing an existence of such a turning point.
We agree that our previous description was rather short-circuited. The choice of splitting at 6ka was based on few predecessor studies (e.g. Moy-et-al2002) and operability (equal division of 12ka period). The reviewers' concern stimulated an additional analysis with a modified window approach. Instead of bootstrapping in

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either Upper or Lower Holocene, we did partial bootstrapping outside a window of 4ka length. The resulting spectrum then contains periodic modes within the 4ka interval. By moving the window from the start of the time-series to its end, the localized contribution to the power spectrum can be quantified. As the attached new figure shows, the Upper and the Lower Holocene 4ka-parts each contain a high fraction (50–70%) of periodic modes. However, during mid-Holocene, the window captures less than 30%, with a minimum at 5.5ka BP, due to discontinuity of modes. This result not only motivates the specific choice of splitting around 5.5 kyrBP but also strongly supports our notion of a global mid-Holocene change in variability modes.

4. **ref2:** The assumptions underlying the spatial clustering algorithm are not well justified: Why should one expect an exponential scaling? What is the rationale for the scale length of 1500 km? Wouldn8217;t the authors expect this scale to change if climate changes? Is it reasonable to assume that the radius is the same in all directions, given that climate zones are generally more zonally oriented?
As stated in the MS, the spatial clustering is only for visualization purposes. It therefore lacks a physical or statistical basis. Our algorithm has previously been motivated by a spatial autocorrelation analysis that revealed strong positive correlation for distances between sites below 2000km, and significant negative correlation for distances between 2000km and 4000km (will be shown in a revised MS). This turning of the sign in autocorrelation in regional event change mapping is reflected by the clustering algorithm together with the chosen value of "half influence distance" of 1500km. We argue that a kriging algorithm, for example, would produce a geostatistically even more consistent coloring of the maps; however, for the purpose of this study we deem the additional methodological complexity (in terms of new coefficients) not justified. Autocorrelation revealed no zonal effects (nearly identical for longitudinal and latitudinal distances).

5. **ref2:** In the interpretation, all proxies are considered to reflect region-specific

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properties. I wonder why the proxies are not grouped according to the dominant underlying climatic processes prior to the analysis.
This is, in principle, a valuable approach but has to be permitted by the density of records. As also explained in the MS, most proxies are related to atmospheric temperature and, to a lesser extent, to humidity. They therefore represent a wider range of climatic processes. We argue that a detailed differentiation and interpretation in terms of underlying processes (as only initiated in our MS) would indeed be a necessary second step. The first step, still, is to conclusively show that variability modes are non-stationary in a region-specific way.

6. **ref2:** The presentation regarding a potential link between cultural development and climate variability is only very cursory and not very convincing.
We agree and added a paragraph on the (potential) consequences of singular events on ancient societies (e.g. Maya, Haug 2003; Akkadian Syria, DeMenocal 2001; Tang, Yuan and Ming-Dynasties, Zhang 2008; critical discussion by Coombes 2005). In addition, we now refer more specifically to the modeling work of Wirtz & Lemmen (2003) who studied the differential effects of climate fluctuations on regional cultures (in terms of population size, migration rate and technological evolution, cf.Fig.7, op.cit.)

7. **ref2:** The language lacks precision and the flow of the presentation is not very fluent.
We are currently reworking the MS to improve clarity and continuity of language.

Kai Wirtz

Interactive comment on Clim. Past Discuss., 5, 287, 2009.