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Interactive comment on "Late quaternary temperature variability described as abrupt transitions on a $1/f$ noise background" *by* M. Rypdal and K. Rypdal

M. Rypdal and K. Rypdal

martin.rypdal@uit.no

Received and published: 14 January 2016

Response to Ditlevsen

The meaning of Eq. (1)

The growing variance with increasing scale δt for $\beta > 1$ is a natural consequence of the self-similarity of a fractional Brownian motion. However, in Eq. (1) we defined $T_{\Delta t}$ as "the mean temperature in time windows of length Δt." This is not an appropriate measure of variability on scale Δt for non-stationary time series like fractional Brownian

motions $\beta > 1$. This is because the estimated variance will depend on the length N of the data record and grows as $\sim N^{\beta-1}$. We need a definition of $T_{\Delta t}$ that measures the *range* of the variability on the longest time scales *within* a time window of length Δt The simple definition given in Lovejoy and Schertzer (2013) starts by defining the fluctuation on scale Δt as,

$$T_{\Delta t}(t) = \left| \frac{2}{\Delta t} \sum_{i=t}^{t+\Delta t/2} T(t) - \frac{2}{\Delta t} \sum_{i=t+\Delta t/2}^{t+\Delta t} T(t) \right|,$$

from which we estimate its variance as;

$$Var\left[T_{\Delta t}\right] = \frac{1}{N-\Delta t} \sum_{t=1}^{N-\Delta t} [T_{\Delta t}(t)]^2.$$

This estimator is in effect equivalent to the wavelet variance defined in Eq. (2) in our paper, since $[W(t, \Delta t)] = \sqrt{\Delta t}\, T_{\Delta t}$. We shall clarify this prior to Eq. (1) in the revision. Since it is Eq. (2) we have used as an estimator, this clarification has no consequence for the analysis and results presented.

Why 1/f is a natural scaling hypothesis

This scaling hypothesis is based on two simple observations. (i) Temperature fluctuations exist on all time scales, hence we cannot have scaling with β significantly *less than* unity. (ii) On the other hand we do not observe an infrared catastrophe, with huge fluctuations on the longest time scales. Hence β cannot be significantly *greater than* than unity. It is a crude argument, but it is only used as a motivation, not as a proof.

The AR(1) model

In the AR(1)-model the variance is not constant up to the autocorrelation time. Since $\beta = 2$ on these high frequencies the variance grows as Δt, and decreases as Δt^{-1}

for longer time scales. It is not a particularly good description of instrumental SSTs, except for the equatorial region. The $1/f$ model is better. We have a recent paper in J. Climate (Fredriksen and Rypdal, 2015) demonstrating this. In Rypdal and Rypdal (2014) we compared AR(1) and an $1/f$ model for global instrumental temperature and a 2000 yr multiproxy reconstruction, and found clear preference for $1/f$.

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We agree with the referee that AR(1) gives a better description of the Niño 34 index than a $1/f$-noise. In the equatorial Pacific the dynamics is completely dominated by the ENSO cycle. It has no meaning to fit a $1/f$ curve to the spectrum on the time scales where ENSO-dynamics dominates. The fluctuation level of the underlying $1/f$ noise on these time scales (up to about 50 yr in referee's Fig. 2) may be too low to show a $1/f$ slope at the lowest frequencies in the figure.

A spectral peak around 1470 yr?

The DO events seem to have a characteristic waiting time between stadial-interstatdal transitions, and a much less defined duration of the interstadial states. The small number of events makes it hard to reject a null hypothesis of random onsets of events. But even if the mean waiting time is 1470 yr, the waiting times have a large variance and the durations of the interstadial an even wider spread. Hence, the events look more or less like a collection of boxes of widely variable size placed approximately 1.5 ± 1.0 kyr apart. Clearly the spectrum of such a signal does not exhibit a distinguishable peak on the the frequency corresponding to the mean waiting time.

The issue of whether the stadial-interstadial and interstadial-stadial transition could or should be modeled as a stochastic two-stage process is very interesting and is the subject of a recent paper by M. Rypdal submitted to J. Climate (Rypdal, 2015).

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We have deliberately avoided to discuss that issue in the present paper, because the scaling during the stages (between the transitions) can be studied and described without knowing the right explanation for the transitions. Hence, the discussion that the referee asks for would be better suited in that paper (and in a forthcoming paper we are planning for a broader review of scaling in quaternary climate).

Aggregation of AR(1) processes?

The referee also mentions that the aggregation of two (or more) AR(1) processes may be indistinguishable from a $1/f$ process. We totally agree with that view, and we believe that this is essentially how we should look upon scaling phenomenology in climatic time series. Rather than drawing on analogies with (spatial) cascades in infinite-dimensional, more or less homogeneous systems (turbulence), which is what referee Lovejoy advocates, we prefer to view the climate system as a collection of interacting subsystems with widely different response times (AR(1) processes). However, we believe that the DO transitions and the glacial-interglacial transitions are too distinct to fit into that picture, which is the reason why we make an attempt to provide a simple stochastic description of the variability during the stages between them.

How to assess the presence of a trend?

This *is* a tricky issue, and subject to a lot of confusion. It is impossible to address without a rigorous approach to statisical hypothesis testing. The referee writes that "the $1/f$ noise assumption by nature does require there to be a trend over the whole series as part of the process..." If we try to interpret this rather inaccurate statement, we assume he refers to the fact that if a a linear function is fitted to a finite-length realization of an $1/f$ process, one will typically find a non-zero slope of that function. For a large ensemble of realizations we will find a zero mean slope, but a finite

standard deviation. If the trend estimated from the observed data set is much larger than this standard deviation, the trend is statstically significant. The open question is how to choose the exponent β t for the $1/f$-process that we use as a null model for the test. The discussion in the paper deals with this issue, and briefly discusses the difference between strong and weak tests.

A scale break in Fig. 2b, black squares?

This apparent scale break was also commented on by reviewer Lovejoy. The intention with this figure was to demonstrate the different scaling properties of 8.5 kyr long segments, one containing DO events, and another that does not. For more accurate estimates, and in particular on longer time scales, we need to consider longer data series, for which results are shown in Fig. 3. There we see that the slopes on millennial time scales and longer converge towards $\beta \approx 1$, not towards $\beta = 1.34$ or higher. However, it is true that there are higher fluctuation levels on scales shorter than about 500 yr, which appear as a scale break at this scale in stadial stages. In Rypdal (2015) this phenomenon is studied and is related to enhanced variance on these short time scales prior to stadial-interstadial transitions, suggesting that these transitions are the result of a bifurcation in a fast degree of freedom of the climate dynamical system. In Fig. 2b we made (poor) fits over a wide range of scales, including scales < 500 yr. In the revised Fig. 2 we make the fit only for scales > 500 yr, which gives much better fits and illustrates the point that segments that contain DO events exhibit spectra with $\beta \approx 1.5$, while segments without events have $\beta \approx 1$.

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Interactive comment on Earth Syst. Dynam. Discuss., 6, 2323, 2015.

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