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Earth System Dynamics
Discussions

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

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Response to Cruxifix

We are not "adding signals"

The editor writes: "The very notion of adding signals is an implicit reference to a quasi-Gaussian framework that would be invalid here." One thing is that we don't see the connection between "adding signals" and "a quasi-Gaussian framework," but more

important is that we don't add signals, and our approach is not quasi-Gaussian.

What we are doing in this paper is not to "subtract the DO modes of variability before characterising the noise." What we do is to separate the glacial state into stadials and interstadials and then perform spectral analysis and fluctuation analysis of stadial state and the interstadial state separately. For the stadial state we treat the interstadial state as missing data, and for the interstadial state we treat the stadial state as missing data. There is an implicit assumption that it makes sense to characterise the two states independently, just like we believe it makes sense to characterise the glacial and interglacial states independently.

We know from the discussion we had with Lovejoy on our ESDD paper on scale breaks in the Holocene (Nielsen et al., 2015) that he does not believe such independent characterisation makes sense, and that DO events and glacial-interglacial transitions "are necessary manifestations of the multifractality." There exists, however, no convincing evidence for such a claim, which we have explained in our reply to Lovejoy and in the revised paper. The intermittency that can be derived from a multifractal cascade model does not exhibit the characteristic temperature differences observed between glacials/interglacials and stadials/interstadials, and the corresponding characteristic waiting times and durations of the various stages. In our reply to Lovejoy we illustrate the point by a multifractal analysis of all data records considered.

CWT for estimating spectral slopes?

We are somewhat confused over the editors comments about "whether the CWT is appropriate for estimating the spectral slopes in this context." We assume CWT refers to the continuous wavelet transform. We cannot find that Lovejoy has mentioned the CWT, and Ditlevsen's (quite relevant) point referred to the use of the "climacogram"

to non-stationary time series ($\beta > 1$). We have clarified this issue in our reply to Ditlevsen, and will clear up the source of the confusion in the revision.

Lovejoy's discussion about the definition of H is completely irrelevant here. His usage originates from turbulence theory, where analysis is made on spatial correlations of the velocity field. This velocity field is "non-stationary" in space, which means that structure functions can be computed directly from the field. In time-series analysis of stationary processes, one has to produce the cumulatively summed process, prior to forming the structure functions. For instance, from the white noise process one forms the cumulative process, which is the Wiener process (Brownian motion). The Wiener process is the theoretical starting point of stochastic calculus. In modern theory of long-memory stochastic processes (which is a developed branch of mathematics) the Wiener process is a self-similar process (a fractional Brownian motion) with self-similarity exponent $H = 1/2$. In this literature, the white-noise process formed by the increments of the Wiener process is said to be characterised by the Hurst exponent $H = 1/2$. In Lovejoy's terminology, white noise is characterised by the exponent $H = -1/2$. We don't want to engage in a dispute with Lovejoy about which terminology is "better." However, this is *only* a matter of notation, and Lovejoy's attempt to put some more into it (for instance to associate it with the widespread use of detrended fluctuation analysis) is a blind track. In order not to contribute to further confusion on this irrelevant issue, we drop the mention of the exponent H in the revised paper.

The testing of trend significance

The editor indicates that our discussion of tests of trend significance "needs to be expressed more carefully." We don't agree that our discussion on this point was not carefully expressed. But the issue is not trivial, and in fact requires some careful thought. We cannot make this discussion "short and easy." The only option we see to

make it more accessible is to make a long section, with examples and figures, and we believe this is outside the scope of this paper. This passage in the concluding section was placed there as a motivation for establishing β for the background noise from data that is not contaminated by the anthropogenic trend. Our discussion should at least illustrate that the testing for significance, and elimination of, the anthropogenic trend in the instrumental data is not trivial.

The relevance of Ornstein-Uhlenbeck (AR(1)) in interannual variability

This issue has been addressed in our reply to Ditlevsen.

References

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