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Earth System Dynamics Discussions

Interactive comment on "ESD Ideas: The stochastic climate model shows that underestimated Holocene trends and variability represent two sides of the same coin" *by* Gerrit Lohmann

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Thanks for your instructive comments on the manuscript *ESD Ideas: The stochastic climate model shows that underestimated Holocene trends and variability represent two sides of the same coin* . In the following, I give answers to all the issues raised.

Answers to the comments:

1. Comment: "The key message of the article is that the underestimation, by models, of low-frequency variance could be caused by a mis-representation of non-normal modes. The article describes how non-normal modes in a forced Langevin equation amplify the low frequency variance. The conclusion of the article is that further constraints on the time-dependent climate sensitivity matrix could be obtained from the spectrum of climate fluctuations. The idea deserves to be formulated, although it would benefit some more critical examination of the frequency range over which it could be applied, because the underlying theory is linear and low-dimensional."

Answer: Indeed, the main idea behind the manuscript is to show that the underestimated local variability in the models can be reconciled with the underestimated local responses. The forced Langevin equation is the most simple dynamics to relate the spectrum with parameter λ to the local climate response to insolation forcing, again related to the damping λ. This is related to the more general fluctuation-dissipation theorem. The fluctuation-dissipation theorem relies on the assumption that the response of a system in thermodynamic equilibrium to a small applied force is the same as its response to fluctuations. Therefore, the theorem connects the linear response relaxation of a system from a prepared non-equilibrium state to its statistical fluctuation properties in equilibrium. As compared to the termination or the Early Holocene, the linear assumption for the mid-to-late Holocene trend is a valid assumption when analyzing the SST paleoclimate data (Lohmann et al., 2013). In my approach, the missing variance in the system appears naturally from the too high λ.

The second idea is that a higher dimensional system exhibits a higher variance if the underlying dynamics is non-normal. Indeed many fluid-mechanical systems extract energy out of the mean flow and show a transient amplification Trefethen et al., 1993; Farrell and Ioannou, 1996). Without changing the eigenvalues, the system can have enhanced variance in the spectrum which is due to transient growth. Therefore, the equilibrium climate sensitivity might be lower than the transient dynamics. The paragraph of this non-normal dynamics is admittedly sketchy, the two-dimensional dynamics is not explicitly worked out, but I found it instructive in this ESD ideas manuscript. In a simple 2-d system of the ocean thermohaline circulation (Stommel model), the system's response is far from normal introducing long-term fluctuations (Lohmann and Schneider, 2000).

You mention the frequency range over which the stochastic climate model can be applied. When looking at the spectra of the Holocene temperatures, the mid-to-late Holocene can be described very well by the linear model. Again, the termination, DO cycles or even the Early Holocene are not suitable and the variances may change over time (e.g., Wirtz et al., 2010; Wassenburg et al., 2016).

Action: In the revised manuscript, I will explicitly state that it is the local temperature trend as the response to latitude-varying orbital forcing which is quasi-linear for the mid-to-late Holocene.

2. Comment: "Previous investigators have indeed shown why it is not straightforward to estimate climate sensitivity by application of the fluctuation-dissipation theorem. Kirk-Davidoff (2009) provided a critique of a previous attempt by Schartz (2007) to constrain climate sensitivity from interannual variability, and Fuchs et al. (2015) as well as Cooper and Haynes (2011) provided some further technical discussion about the scope of the fluctuation-dissipation theory in atmospheric sciences. Simply put, in a simple 1-potential well system forced by Brownian motion (the Langevin equation), there is only one relaxation force in the system, which acts in a similar way at all time scales. In other words, the physical forces causing the phenomenon of relaxation (e.g.: gravitational forces in a pendulum; spring tension in a mass attached to a spring) are the same as those which determine the sensitivity to a constant forcing. This ceases to be true in the atmospheric system. Processes of relaxation at the annual time scale (which involve

geophysical fluid dynamics) involve different processes than the radiative relaxation which determines climate sensitivity."

Answer: Thanks a lot for these hints. Indeed, several articles are dealing with the FDT, but I have not seen a contribution to exploring the Holocene trends and variability. One motivation of the stochastic climate model by introducing λ in the response as well as in the fluctuation is that it provides a framework for further GCM studies. Preliminary analyses of high-resolution climate models indicate a higher local SST variance as well as a more heterogeneous, enhanced SST response to external forcing.

Action: In the revised manuscript, I will explicitly mention the goal of the FDTs. As you wrote, a comprehensive overview "would require a more systematic review, which, to be fair, is out of the scope of an “idea” paper". I will mention the benefit of simple models in guiding us to analyze comprehensive models' sensitivity and variability.

3. Comment: "Despite these reservations, it is plausible that the linear assumption expressed by the equation (1) of the article under review be indeed valid over a range of time scales greater than the interannual time scale, and hence, that the idea suggested by this author has some scope for application. However, in order to determine which range of time scales it could be, it seems necessary to provide some plausible physical interpretation of the nature of the non-diagonal terms. Indeed, the author mentions the ‘fluid dynamical context’, but what does it mean ?"

Answer: Correct. As mentioned in comments 1. and 2., the range of timescales is given by the mid-to-late Holocene. The plausible physical interpretation of the nature of the non-diagonal terms is related to the extraction of energy from a mean state / mean flow. In terms of the simple Stommel model, this is the mean ocean circulation. In fluid dynamical context, this has been discussed in terms of shear flow instabilities (e.g., Trefethen et al., 1993).

Action: In the revised manuscript, I will explicitly mention the physics of the non-normal dynamics, but will try to reduce the number of references in this direction.

4. Comment: "There is also some concern about the mathematical notations. Equation (1) is originally presented with T as vector (if bold notation is indeed supposed to indicate a vector), with λ, a scalar. What would be the components of T? If they are different climatic components (ocean, and atmosphere), then we need different relaxation time scales. Let us suppose that the original interpretation of equation (1) assumes T as a scalar, and that T becomes a vector only at the point of introducing equation (7). Then, we can legitimately consider that the different components of T correspond to different components of the climate system, in which case we would expect some non-diagonal (linear, symmetric) coupling terms. There are no such terms in matrix A. So the reader needs to infer that the system was rotated in order to get rid of the coupling terms. What is in vector Q then? The second component of Q needs to be strictly positive, in order to excite the second component of T, and finally generate the extra variance produced by the factor N. This leaves a bit too much guess work to the reader."

Answer: Sorry. The notation in (1) was meant to be for a scalar. Indeed, the vector is only introduced with (7). The vector Q is related to the variances of the individual components. In the ESD ideas manuscript, I have not specified it explicitly and normalized it to one.

Action: In the revised manuscript, I will explicitly mention Q to avoid guesswork to the reader. Furthermore, it will be clearly stated that (1) is a scalar stochastic differential equation.

5. Comment: "Assuming these questions can be answered, there is, finally, some

concern about the quality and performance of spectral estimators that would be needed to do the job of estimating A. Does the power spectrum contain enough information to constrain the non-diagonal elements of the transfer matrix? If it does, would it plausibly work given the palaeoclimate data available ?"

Answer: In the paper, the spectra in Fig. 1c are calculated analytically. For real problems, the estimation of A can be done via the POP method (Hasselmann, 1988). Then the dynamical propagator has in general a non-normal structure. The POPs can be calculated from the paleoclimate time series, which would be a logical next step. For recent climates, there exist very nice examples in the framework of (linearized) stochastically forced dynamics (e.g., Whitaker and Sardeshmukh, 1998; Kwasniok, 2004).

Action: I will try to give a short outlook in this direction.

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