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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the diagnosis of climate sensitivity using observations of fluctuations" *by* D. Kirk-Davidoff

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Comments on "On the diagnosis of climate sensitivity using observations of fluctuation" by D. Kirk-Davidoff (ACPD-12409-12434).

In 1975 C. Leith expressed an idea of using Fluctuation-Dissipation Theorem (FDT) to estimate climate system response onto external forcing. The main advantage of this approach is due to the fact that such an analysis can be based on the observational data providing pure ("model-free") estimate for the climate sensitivity.

In this paper Author reviews the procedure of evaluating climate sensitivity suggested by Schwartz (2007). Using the data from the IPCC model runs and results obtained with (very) simple climate model he comes to the conclusions that "The FDT is poorly suited to the evaluation of model sensitivity in practice" mainly because "much longer climate records than exist for the real world are required". The nature of the paper is indeed close to the recently submitted works of (Knutti et.al., 2008) and (Foster et.al.2008).

I think the study is well motivated and the results are interesting and well supported, however some minor but important corrections should be made to improve the paper. I recommend this paper for publishing after addressing my comments below.

1. General comment. I think that the section 1.1. should contain more discussion about why FDT should (or should not) work for the climate system with references of the papers cited below.

In Section 1.1. the author basically says that for a linear system with time-independent operator and white-noise Gaussian forcing (1)

$$dx/dt = Bx + \zeta + f \quad (1)$$

one can calculate its response operator according to (2) or (4)

$$B = 1/\tau \ln C(\tau)C(0)^{-1} \quad (2)$$

$$-B^{-1} = \int C(\tau)C(0)^{-1} d\tau \quad (4).$$

Obviously climate system is not a linear dynamical-stochastical system. Why should we believe the theory based on such an inaccurate assumption? Why any climate sensitivity estimate based on this linear theory can be any better than the analysis made with the help of the modern GCM? It is true that Penland, Sardeshmukh, Newman and colleagues showed that the linear system (1) with the proper choice of a linear

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operator can produce quite accurate predictions for weather statistics (comparable in quality to the predictions made with GCMs). However for the sensitivity analysis this is not enough - even a small inaccuracy in the system operator B can lead to dramatic changes for B^{-1} (and for system sensitivity). The author is also skeptical about the linear hypothesis (1) saying "...this realationship might not be so simple in real world".

Meanwhile, FDT is valid for nonlinear systems as well. Indeed, in (Dymnikov, Gritsun, 2005) and (Majda et.al., 2005) it was explained that (4) can be obtained for nonlinear system with stationary gaussian PDF using a theory of the Fokker-Plank equation. For the systems which PDF is a quasi-Gaussian distribution equation (4) will become an approximation for the response operator. The quality of this approximation depends on the system (see Majda et.al, 2005; Abramov, Majda, 2007). Numerical studies of (Dymnikov, Gritsun, 1999), (Gritsun, Dymnikov, Branstator, 2002) and (Gritsun, Branstator, 2007) demonstrated that FDT in the form of (4) provides very accurate approximation for the system response operator (and system sensitivity) of the barotropic, 2level QG and atmospheric general circulation models. In the same time, (A) they apply formula (4) for complete (or large enough) model phase space and (B) all models in these studies have stationary statistics (i.e. were used with perpetual January boundary conditions).

What about climate system? It is impossible to calculate (4) in full dimension for climate system (because its dimension is huge and there is not enough data to correctly invert $C(0)$). Also the climate system does not have any stationary statistics (the solar forcing is time dependent).

One way to deal with (A) is to consider an external forcing only from the space of the leading EOFs of $C(0)$. This greatly reduces the dimensionality of the system and the amount of the data required. One may also speculate that the atmosphere has more or less stationary statistics in winter (or in summer) and apply (4) for winter (summer) data. Using these ideas (Dymnikov and Gritsun, 2005) calculated operators (4) for both AGCM and NCEP/NCAR reanalysis winter data (in the space of 30 leading 3D

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EOFs of $C(0)$) and showed that these operators have some common basic features. Note that the CO2 forcing does not belong to the space of the leading EOFs of $C(0)$ and (Dymnikov and Gritsun, 2005) approach may not be applied to the problem of the climate system response onto CO2 forcing (and climate sensitivity in the definition of the current paper).

In the present paper (as well as in Schwartz (2007)) the response operator is calculated using detrended global mean surface air temperature using (2). I see at least several reasons of why it gives incorrect values for GCM's sensitivities.

1) FDT suggests calculating (4) in "true" full dimensional phase space of the model. Dramatic reduction of the phase space to single global mean surface temperature can (and will) completely change sensitivity characteristics of the response operator. So why should it correctly predict the system response onto CO2 forcing? (I believe that this can be the major source of the problem).

2) FDT in the form of (4) requires existence of the stationary PDF. Is the "detrending procedure" a legitimate solution for the system nonstationarity? May be the use of the more or less stationary winter data would give better results? Or may be FDT in the form of (2) and (4) is not valid for such a system at all?

3) May be "correct" formula (4) will give better results than "linear" (2)?

4) I completely agree with the author that the lack of the available data can be the major reason of bad performance of (Schwartz, 2007) algorithm either.

2. Minor comments.

2-1. Introduction. Definition of the sensitivity as "a surface temperature response to CO2 doubling" should be given somewhere in the Introduction.

2-2. Section 1, Page 12411, Lines 10-15. The list of references is incomplete. There are many other studies devoted to the use of FDT in atmospheric science (see References 1-6).

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2-3. Section 1, Page 12411, Lines 16-20. Explicit discussion on how the amount of available data affect the results of the FDT based method can be found in several above mentioned papers.

2-4. Section 2, Page 12415, Lines 22-23. There are no "blue" and "red" curves on Fig 1A.

2-5. Section 2, Page 12416, Lines 1-4. It looks like the "decorrelation time scale" and "relaxation time constant" are the same? One should use single name for it.

2-6. Section 2, Page 12416, Lines 4-7. Am I correct that Fig 1B shows the strong sensitivity of the "relaxation time constant" with respect to the lag? This is an indication that the linear system (1) is not a good approximation for the GCMs.

2-7. Section 2, Page 12417, Lines 4-6. Linear theory (1) does not assume any linearization so it should work the same way for all forcings.

2-8. Section 2. Page 12417, Lines 9-15. "The response" in 1), 2) and 3) should probably be "The response of the global mean surface temperature"?

2-9. Section 3, Page 12421, Lines 5-6. What is the "equilibrium climate sensitivity" mentioned here (i.e. it should be different from the one defined on page 12417...)?

2-10. Section 3, Page 12417, Lines 11-12. What "temperature" is mentioned here? T_s?

2-11. Section 3. Page 12417, Lines 11-18. Is the "adjustment time scale" the same as "response time", "adjustment" and "time required for the temperature to reach 90% of its equilibrium"? One should use single name for it (including captions for Fig.5).

2-12. Section 5. Page 12424, Lines 16-19. I think the conclusion N1 (i.e. "The FDT is poorly situated to the evaluation of model sensitivity in practice") sounds too strong at least because A) in general the "sensitivity" is not just a response of the global mean SST onto CO2 doubling, B) we can get reliable multidimensional response operators

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for modern GCM with FDT (see (Gritsun, Branstator, 2007)). The statement 1) from the abstract looks much better.

References:

1. Gritsun, A. and V. Dymnikov, 1999: Barotropic atmosphere response to small external actions. theory and numerical experiments. Atmos. Ocean Phys., 35 (5), 511-525.

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3. Dymnikov, V. and A. Gritsun, 2005: Current problems in the mathematical theory of climate. Izv. Acad. Sci. USSR, Atmos. Oceanic Phys., 41, 294-314.

4. Majda, A., R. Abramov, and M. Grote, 2005: Information Theory and Stochastics for Multiscale Nonlinear Systems, CRM Monograph Series of Centre de Recherches Mathematiques, Universite de Montreal, Vol. 25. American Mathematical Society, ISBN 0-8218-3843-1.

5. Gritsun, A. and G. Branstator, 2007: Climate response using a three-dimensional operator based on the fluctuation-dissipation theorem. J. Atmos. Sci., 64, 2558-2575.

6. Abramov, R. and A. Majda, 2007: Blended response algorithms for linear fluctuation-dissipation for complex nonlinear dynamical systems. Nonlinearity, 20, 2793-2821.

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