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Interactive Comment

Interactive comment on "Estimation of the climate feedback parameter by using radiative fluxes from CERES EBAF" *by* P. Björnbom

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Subject: Observed coherent oscillations in temperature and radiative flux anomalies are also found in climate model simulations promising possible new insights into climate sensitivity

I want to thank Troy Masters (TM), also known as Troy CA, for his short comment, where he presents interesting climate simulation results that are based on the new findings from my paper as an input.

General comments

At first I want to emphasize that the scope of my paper is to report observations in the CERES EBAF data that are possibly new discoveries. In my paper I also propose a hypothetical explanation to observed phenomena based modifications of the theory in Gregory et al. (2004) and the phase plane method by Spencer and Braswell (2010). This explanation is also based on ENSO theory according to the literature. A more detailed comparison with advanced climate model simulations, as proposed by TM, is beyond the scope of my work.

For the purpose of the discussion here follows a summary of new findings in my paper especially relevant in the context of TM's reply. From Fig. 1 in my paper a change of state is observed around mid-2006. In the time interval 2003 to mid-2006 there are some oscillations in the temperature anomaly but no corresponding oscillations in the radiative flux anomaly. In the time interval mid-2006 to mid-2011, on the other hand, there are coherent (similar period) oscillations in both anomalies. The radiative flux anomaly curve is phase shifted with a lag of around seven months compared to the temperature anomaly curve. Those are possibly important new discoveries (if not already published) from CERES EBAF data reported in my paper.

According to the hypotheses proposed in my paper, the oscillations in the temperature anomaly in the second time interval, according to ENSO theory, are assumed to be controlled by heat transfer in the ocean. The coherence of the oscillations is explained by the oscillations in the radiative flux anomaly being dominated by radiative feedback and consequently described by Eq. (5) in the paper with an almost constant forcing term. Various calculations using Eq. (5) give a climate feedback parameter (CFP) value of around 6 $\mathrm{W\,m^{-2}\,K^{-1}}$. If this explanation would be valid this CFP is a measure of the ability of the planet to cool in response to increasing global surface temperature anomaly during the studied five year period and consequently in some way should be connected to the climate sensitivity.

TM reports in his short comment that he has inspected simulation results from the advanced climate model GFDL-CM3 pre-industrial control experiment. He found many five year periods with coherent oscillations in temperature and radiative flux anomalies. One example is given in Fig. 1 in TM's short comment that looks very similar to the observations for the time interval mid-2006 to mid-2011in Fig. 1. in my paper.

TM's new finding that coherent oscillations, similar to the observations reported in my paper, also occur in advanced climate model simulations is encouraging. The discovery of a climate system phenomenon that occurs in a similar way in nature and in an advanced climate model should give opportunities for new insights.

However, those new findings in fact raise more new questions than they answer:

1. Why is the value of the CFP for the climate model, calculated by applying Eq. (5) on the data in TM's Fig. 1, different (being much greater) from the long term CFP connected to the equilibrium climate sensitivity (ECS) in the same model? This question generates several further questions.

2. What is the mechanism generating the coherent oscillations in the climate model according to TM's Fig. 1? Does it agree with the hypotheses in my paper or not? This has not yet been discussed by TM (but this is an interesting future work, I presume) although such questions may be answered by studying the internal results of the simulations, for example how the various internal heat fluxes develop compared to the TOA temperature and the radiative flux anomalies. In fact such a type of analysis is probably the most important purpose of using mathematical models in science and technology, because we may reach a deeper understanding of the details of observed phenomena that are not available for studying otherwise.

3. Is it possible that there are more than one sort of mechanisms that cause those coherent oscillations? If so, it would be important to identify which mechanism is in operation before drawing conclusions from an observed or simulated coherent oscillation.

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4. Is it possible that the CFP value may change considerably when there are changes of states in the climate system? For example, in the time interval 2003 to mid-2006 according to Fig. 1 in my paper the radiative flux anomaly shows almost no change when the temperature anomaly changes. One possible explanation could be that CFP ≈ 0 during that time interval. If so, it could be possible that the long term CFP is a weighted average of the CFPs of the sequence of changing climate states. Those are of course very speculative ideas but they should be worth elaborating on by using climate models.

5. Is it also possible in advanced climate models to find climate states like the one in the time interval 2003 to mid-2006 according to Fig. 1 in my paper and may changes of state like the one in mid-2006 also be found? What are the mechanisms behind such climate states and such changes of state?

In summary, it seems to me that TM's short comment supports that the observations reported in my paper possibly are important new discoveries that could result in new insights into issues concerning climate sensitivity. It was probably the right thing to submit my paper for publication since I would have regretted if I had not done so and my observations afterward would have been published in another paper.

Specific comments

On point #2 the reason why I have not used HadCRUT4 is that my study was mostly done before HadCRUT4 was released for downloading monthly anomalies.

On point #4 I find TM's concerns of using running averages unconvincing. He does not offer much arguments to support his view. The error limits of $\pm$ 7 W m^{-2} K^{-1} seem counter-intuitive to me when compared to the data in Fig. 3 in my paper. A regression line with negative slope, which is within those error limits, should give an enormously great value of the sum of least squares far from the minimum.

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My reply on this point is the following outline of my own reasoning concerning usage of smoothed values.

As a basic assumption the net radiative flux anomaly and the surface temperature anomaly may be expressed as the sum of a mean and a random variable with zero mean. The random variables are in turn sums of various components, with and without auto-correlation:

$$\begin{aligned} N(t) &= N_m + N' \qquad (1) \\ \Delta T(t - t_{\text{lag}}) &= \Delta T_m + \Delta T' \qquad (2) \end{aligned}$$

Deviations from the mean due to measurement errors and due to using a limited set of locations for calculating global averages may be examples of random variables without auto-correlation applicable for both anomalies. When forming averages from such random variables, new random variables with less standard deviation are obtained giving a smoothing effect.

We now assume that the mean of the net radiative flux anomaly with a lag is a function of the mean of the surface air temperature anomaly and a radiative forcing:

$$\begin{aligned} N_m(t) &= f(F(t), \Delta T_m(t - t_{\text{lag}})) \qquad (3) \\ N_{m0}(t) &= f(F(t), 0) \qquad (4) \end{aligned}$$

Equation (3) shows the net radiative flux anomaly with a lag for a given forcing and a given change of temperature anomaly. Equation (4) is the net radiative flux anomaly for the same forcing in case the temperature would not have changed.

A basic assumption here is that there is no relationship between the random variables.

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The functional relationship between the net radiative flux anomaly and the surface temperature anomaly is only found between the means.

The function in the right hand side of Eq. (3) may be Taylor expanded as a function of the temperature anomaly:

$$f(F, \Delta T_m) = f(F, 0) + \left(\frac{\partial f}{\partial \Delta T_m}\right)_{\Delta T_m = 0} \Delta T_m + e_{1s} \tag{5}$$

The third term on the RHS is a systematic error. This gives the following equation where the third term also is a systematic error:

$$N_m(t) = N_{m0}(t) + \left(\frac{\partial f}{\partial \Delta T_m}\right)_{\Delta T_m = 0} \Delta T_m(t - t_{\text{lag}}) + e_s \tag{6}$$

However, $N_{m0}(t)$ is the net radiative flux anomaly without any radiative feedback. Thus by definition we should have $N_{m0}(t) = F(t)$.

$$N_m(t) = F(t) + \left(\frac{\partial f}{\partial \Delta T_m}\right)_{\Delta T_m = 0} \Delta T_m(t - t_{\text{lag}}) = F(t) - \alpha \Delta T_m(t - t_{\text{lag}}) + e_s \tag{7}$$

where we have defined $\alpha = -\left(\frac{\partial f}{\partial \Delta T_m}\right)_{\Delta T_m = 0}$.

However, the observations give us the measured monthly anomalies N and ΔT. By combining Eq. (7) with Eqs. (1) and (2) we obtain:

$$N(t) = F(t) - \alpha \Delta T(t - t_{\text{lag}}) + N' - \alpha \Delta T' + e_s \tag{8}$$

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This equation has two random terms while the last term is a systematic error. The random terms will cause errors when we estimate α. What if we take averages of the two sides? Then we get:

$$N_{\mathrm{av}}(t) = F_{\mathrm{av}}(t) - \alpha\Delta T_{\mathrm{av}}(t - t_{\mathrm{lag}}) + N'_{\mathrm{av}} - \alpha\Delta T'_{\mathrm{av}} + (e_s)_{\mathrm{av}} \quad (9)$$

The random variables are averaged. Because averaged random variables without auto-correlation have less standard deviation we should expect less error in estimating α from Eq. (9) instead from Eq. (8).

The deviation of the linear model from the data points in the linear regression is both due to random errors around the regression line and due to systematic errors. The systematic errors may be due to a fundamental deviation from linearity between the means. Perhaps also auto-correlation in the anomalies may contribute to the systematic error.

Anyway, it is clearly seen in Fig. 3 in my paper that there are both a random error and a systematic error. Consequently, the residuals are not random variables with zero mean. To my knowledge the standard error from the regression in such a case cannot be used to calculate confidence limits. That is why I only reported tvice the standard error as error limits without giving any confidence level.

Auto-correlation in the anomalies is an important issue when analyzing anomalies as time series, especially for calculating linear trends, where it is assumed that there are some auto-regression models describing the physics, i. e. the linear trend does not correspond to an assumed physical model. In our case we assume as a hypothesis that a linear relationship between two variables (and none of them is the time), based on a Taylor expansion, is approximately valid as a model of the physics. That is a different case.

In the part referring to point #5 and point #6 TM seems to preconceive that the $4{\times}\mathrm{CO}_2$

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demonstration according to Fig. 4 in my paper was a test case for the with-lag method. However, the purpose of that demonstration was to show that the method of Gregory et al. (2004) is not sensitive to a lag. As a consequence, if the CFP found by the with-lag method would be the real long term CFP (which according to the discussion above may not be so) its value should agree with the value from the method by Gregory et al. This should perhaps be better emphasized in a revised paper.

I think that such a test of the with-lag method as proposed by TM must be done using advanced climate models as discussed in the general comments above. I do not consider it possible using a simple Energy Balance Model. I find it most likely that only an advanced climate model with a fairly good skill in simulating ENSO oscillations is necessary in order to make any progress. Such a study may not only be limited in studying archived simulation results but may also include doing new simulations with modified parametrizations. My own skill and resources to do such studies is much too limited to reach a result of sufficient quality and I do not consider such a study within the scope of my work.

Finally TM notes that Spencer and Braswell (2011), Lindzen and Choi (2011) and Dessler (2011) have also noted lead-lag relationships in the CERES EBAF data, which I also have mentioned in the paper. However, none of them have noted the change of state in mid-2006 and the coherent oscillations in the time interval mid-2006 to mid-2011. Nor they have introduced phase plane plots with lag for analyzing such phenomena. It is also possible that my result may explain why Lindzen and Choi (2011) got their result. Those aspects of my paper should therefore be novel.

Interactive comment on Earth Syst. Dynam. Discuss., 4, 25, 2013.