Earth Syst. Dynam. Discuss., 4, C18–C24, 2013
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Earth System Dynamics Discussions

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

Pehr Björnbom

pehrbjor@kth.se

Received and published: 5 February 2013

This is my responses to the review by Troy CA (January 15th, 2013 at 11:30 am) at the Blackboard: http://rankexploits.com/musings/2013/review-paper-estimation-of-the-climate-feedback-parameter/#comment-108449

I want to thank Troy CA for his review.

General comments

The purpose of my paper is to present new concepts and interpretations by using CERES EBAF net radiative flux data. What I believe is new in this paper is the discovery and interpretation of the lag relation between temperature anomalies and radiative flux anomalies, the introduction of phase plane plots with lag in order to identify periods where non-radiative forcing is dominating the temperature changes compared to radiative forcing, and the introduction of Eq. (5) as a linear model for describing the TOA radiation flux anomalies instead of Eq. (3).

Specific comments

In point #1 Troy CA suggests investigating how the choice of radiative flux data base changes the estimated value of the climate feedback parameter. However, he does not expect much changes.

In point #2 Troy CA suggests investigating how the choice of temperature data base changes the estimated value of the climate feedback parameter. I agree with him that there may be significant differences in estimated values depending on which of the temperature data series is used. However, I consider such an investigation beyond the purpose of the present paper, which aims to present new concepts and interpretations rather than finding the most accurate value of the climate feedback parameter. Investigating how the estimated value varies due to the choice of temperature data base I want to leave to future work in the scientific community.

In point #3 Troy CA remarks that neither Forster and Gregory (2006) nor Murphy et al. (2009) are referenced in the paper. I thank him for pointing this out although I disagree with Troy CA that they use the same energy balance method as presented in my paper. The method used in my paper differs in using the concept of feedback with a time lag, as illustrated by the difference between Eq. (5) and Eq. (3), and in using phase plane plots according to Spencer and Braswell (2010), but with a time lag, for identifying the time periods when non-radiative phenomena cause the changes in net radiative flux to be dominated by feedback. However, I think that those papers in fact should be referenced and this difference be explained in a revised paper.

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In point #4 Troy CA questions the use of running averages in the paper. My view is that smoothing the data with running averages will eliminate the effect of random variations on short time scales and emphasize the variations on the time scales we are interested in. Furthermore the choice of 13 months averages would be suitable for the important comparison with the results by Gregory et al. (2004) who used annual averages in their plots and consequently eliminated the same type of random variations as in my study. The smoothed data seems to be suitable for the analysis based on Fig. 1 where the smoothed curves give remarkable information with respect to time lag and with respect to similarity of the shapes of the curves.

The five year period at end the of the studied time interval is especially analyzed by using phase plane plots. From Fig. 1 we may find direct estimates of both the time lag and the proportionality ratio that should equal the climate feedback parameter. With the scaling method used the proportionality ratio should equal the scale factor between the two y-axes.

However, the climate feedback parameter may also be estimated by linear regression of the smoothed data. This is simply an alternative method for the estimation of that parameter. The results of both methods approximately agree.

There is also a third method for estimating the climate feedback parameter from the curves in Fig. 1. Both the temperature curve and the radiative flux curve have two maxima and one minimum in the time period of 2007 to 2011. The differences in y-values between maximum and minimum give the amplitudes of the oscillations. This gives us two possibilities to estimate the climate feedback parameter from the ratio of the amplitudes. In this case we obtain 6.6 $\mathrm{W\,m^{-2}\,K^{-1}}$ and 6.2 $\mathrm{W\,m^{-2}\,K^{-1}}$.

In summary we have three lines of evidence for the estimated value of the climate feedback parameter. One evidence is from adjusting the y-axes in Fig. 1 in order to make the curves as similar as possible, one evidence from linear regression, and one evidence by calculating the ratio of the amplitudes of the two curves.

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In point #5 Troy CA has raised the question about the choice of the time period for linear regression. My view is that the time period must fulfill some criteria in order that linear regression could be justified. We should have some indications that the relationship between the temperature anomaly and the radiative flux anomaly really is approximately linear. That would be the case if the main variations in temperature anomalies are caused in another way than changes in radiative forcing since in such a case the systematic variations in the radiative flux anomaly is due to feedback.

The plotted time series in Fig. 1 in the discussion paper show a distinct change of state in the middle of 2006. Before this change of state there is not much variation in the radiative flux anomaly from 2003 and onward. The temperature anomaly shows oscillations during this time period but corresponding oscillations are absent in the radiative flux anomalies.

After mid-2006 we see a completely different pattern. The temperature anomaly continues to oscillate but with a greater amplitude than before. The radiative flux anomaly also oscillates and after scaling with $6\,\mathrm{W\,m^{-2}\,K^{-1}}$ we find that the radiative flux curve is almost similar to the temperature anomaly curve. However, the radiative flux curve is lagging the temperature curve with around seven months.

This suggests that the radiative flux changes are responses to the temperature changes but that the response comes with a lag of around seven months. This interpretation is further supported by using a phase plane plot with lag and using the way of reasoning introduced by Spencer and Braswell (2010).

That is why I have chosen the five year period mid-2006 to mid-2011 for the linear regression with Eq. (5), $N(t) = F - \alpha \Delta T\left(t - t_{\mathrm{lag}}\right)$, as the linear model with seven months lag. I claim that I have found substantial evidence that a linear model according to Eq. (5) is valid in that time period. In such a case a linear regression should give a value of the climate feedback parameter that is the proportionality ratio between the temperature change and the radiative flux change due to feedback. This proportionality

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ratio also should be equal to the scale factor between y-axes in Fig. 1 when the two curves have been adjusted to be as similar as possible and it should also equal the ratio of the amplitudes of the two curves. As discussed above the results from those three methods agree.

A further consequence of our reasoning is that it is not correct to make a linear regression on the time period 2003 to mid-2006 or a time interval including that period. In that period Eq. (5), whatever time lag is used, cannot be treated as a linear model since the radiative flux changes due to feedback appear to be systematically contaminated by radiative forcing of some unknown kind (cf. Spencer and Braswell 2010). Thus a linear regression would not give a reasonable result for the climate feedback parameter for that time interval or for a longer time period including that interval.

In point #6 as well as point #5 Troy CA discusses the question if the non-radiative effects on temperature variations due to ENSO are at all useful for diagnosing feedback, which is a fundamental assumption for my and several other studies. My view is that it is worth studying feedback phenomena even over such fairly short time periods when satellite data are available. However, in order to relate such feedback results to feedback on longer timescales probably considerable progress is needed in understanding of ENSO phenomena and other natural variations through applying and developing advanced climate models.

Troy CA mentions a paper by Lin et al. (2011) as an argument against feedback studies over such fairly short intervals. However, I do not agree that one may conclude something about ENSO from the results of Lin et al. (2010,2011). Their results seem to need a reconsideration before being useful.

An interesting aspect of their papers is that they write that they reproduced the results of Spencer and Braswell (2010). They analyzed net radiative flux data applying the phase plane method using the same model for TOA net radiative flux anomaly as used by Gregory et al. (2004) and by Spencer and Braswell (Eq. (3) in the discussion paper):

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$$N = F - \alpha \Delta T$$

Lin et al. found basically the same value of $\alpha = 6$ W m^{-2} K^{-1} as Spencer and Braswell.

They proposed that this model should be extended with a term that has to do with the memory of the climate system. This term is based on the integral average of the temperature anomaly for a memory time t_m. Their improved model is:

$$N = F - f_S \Delta T - \frac{f_m}{t_m} \int_{t-t_m}^{t} \Delta T dt$$

When the memory time $t_m \to 0$ that equation is transformed into the original model used by Spencer and Braswell:

$$N = F - (f_S + f_m) \Delta T$$

Consequently the following equation is valid for the coefficients:

$$\alpha = f_S + f_m$$

In their further analysis Lin et al. assumed that the result of Spencer and Braswell is valid for an analysis assuming no memory effect. According to that assumption $\alpha = f_S + f_m = 6$. However, they used $f_S = \alpha = 6$ instead of using the equation $\alpha = f_S + f_m = 6$ in their analysis, giving results which are not compatible with their intended assumptions.

Another interesting aspect is that the model used by Lin et al. has a clear relation to the model with lagged feedback used by me. According to the first mean value theorem of integration we have:

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$$\frac{1}{b-a}\int_a^b f(t)dt = f(c) \quad \text{where} \quad b \geq c \geq a$$

Thus

$$\frac{1}{t_m}\int_{t-t_m}^{t} \Delta T dt = \Delta T(t - t_{\text{lag}}) \quad \text{where} \quad t_m \geq t_{\text{lag}} \geq 0$$

As a consequence the model of Lin et al. may also be written:

$$N = F - f_S \Delta T - f_m \Delta T(t - t_{\text{lag}})$$

This suggests that the results from my study approximately agrees with the model of Lin et al. with $f_S = 0$ and $f_m \approx 6$ W m^{-2} K^{-1}. This result differs significantly from the results of Lin et al. Perhaps this comparison with the method of Lin et al. should be included in a revised paper?

Anyway, simple energy balance considerations suggest that the great temperature changes during strong ENSO oscillations are likely dominated by non-radiative effects, as assumed in my study. Dessler (2011) has made a comprehensive analysis of that energy balance issue supporting such an assumption.

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

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