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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

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I want to thank the reviewer for this review that has given me the opportunity to clarify several points that may be misunderstood in my paper. In the following quotations from the referee report are followed by my comments. However, note that in one case a quotation gives my own comments from my reply to Troy CA in this interactive discussion.

General comments

The reviewer's first comment is:

> *The author addresses the important issue of estimating the climate feed-*

> *back parameter as it relates to forced climate change and explores the vital yet challenging question as to how one can best estimate the surface temperature feedback from the observational record. The problem has been longstanding, in part due to the challenges of observing the energy budget on a global scale, but also due to the fact that observed variability is largely not the result of forcing.*

I agree with the reviewer on those views.

> *Rather, interannual variability in the observed record (such as in CERES) is predominantly the result of internal climate modes such as ENSO and, over longer timescales, decadal variability. In these modes, it is possible for clouds themselves to effect surface temperature and thus the attribution of cause and effect is not so simple. Thus a major challenge exists in detangling the forced response of the climate system from the unrelated variations in the observed record.*

Again I completely agree with the views of the reviewer. As the reviewer is saying the attribution of cause and effect is not so simple. I want to emphasize though that one of the highlights of my study is the new observation of the similar shapes of phase-shifted curves in Fig. 1 in the paper in the five year interval at the end of the observation period. This observation suggests that we are seeing a radiative feedback that is lagging the surface air temperature anomaly change. The phase shift of the curves suggests a cause and effect relationship, that the radiative feedback is caused by the temperature anomaly change.

> *Unfortunately, I do not find the present work to afford much insight into this vexing challenge as it fails to recognize the dynamic and interactive nature*

of ENSO as an internal unforced mode and the hypothesis that it tacitly assumes can readily be (and has been) falsified in models.

The statement that the paper does not recognize the nature of ENSO as an internal unforced mode is simply not correct as will be clearly explained below.

The reviewer further seems to have overlooked what is new in my paper since there are no comments addressing that issue.

What I believe is new in my 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 unforced variability 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 this section the first comment of the reviewer is:

The authors propose deriving the climate feedback parameter from ENSO-related variability, yet they seem to overlook the obvious challenges in extending co-variability associated with coupled modes (i.e. ENSO) to the forced problem.

Their terminology imposes the notion that variations in radiative fluxes are driven by surface temperature variability of "non-radiative forcing connected to the ENSO" but such a perspective is inadequate to characterize either ENSO, its dynamical linkages, and its strong inherent air-sea variations that preclude a commentary on forced climate variability.

This is not a correct description of the reasoning and the concepts applied in my study. The study is using the notion that natural variability like ENSO may change the surface air temperature. This is used for interpretation of the discovery of a specific lead-lag relation between temperature anomaly changes and radiative flux anomaly changes. That ENSO may result in changes in the surface air temperature should not be controversial. Also it is generally accepted that changes in the surface air temperature causes radiative feedback. The notion that the radiative feedback may come with a lag compared to the mean global surface air temperature is based on the new discovery from observations in my study and the interpretation of those observations using a model of the net TOA radiative flux, Eq. (5) in the paper, which is a modification of the model used by Gregory et al. (2004), Eq. (3).

The great temperature oscillations due to strong ENSO phenomena suggest that great amounts of energy are added to or removed from the troposphere and the mixed ocean layer. It would be impossible for the temperature to change that much without considerable energy addition or removal. Whether this energy exchange is said to be due to non-radiative forcing, non-radiative effects or unforced variability is solely a matter of semantics.

A comprehensive study of this energy exchange has recently been carried out by Dessler (2011). He used the energy balance (1) in his paper and estimated the standard deviations of all the terms of this equation in paragraphs [7] to [11] on pages 1 and 2. The standard deviation of the term in the LHS was estimated by using a heat capacity for the climate system as well as by using Argo heat content data and amounted to around 10 $\mathrm{W\,m^{-2}}$. The standard deviations of the radiative terms in the RHS were estimated by using radiative flux data and temperature data and giving around 0.5 $\mathrm{W\,m^{-2}}$ for each term. Considering also a similar radiative forcing due to water vapor would according to Dessler give a term with a standard deviation also around 0.5 $\mathrm{W\,m^{-2}}$.

The standard deviation of the term accounting for the exchange of heat to and from the troposphere and the mixed ocean layer was estimated from the standard deviations from the other terms. Dessler found that this standard deviation is approximately equal to the standard deviation of the LHS of Eq. (1) in his paper, i. e. around 10 $\mathrm{W\,m^{-2}}$. Dessler in paragraph [11] concludes that a radiative forcing from clouds could only explain a few percent of the surface temperature changes. A thought total radiative forcing including water vapor thus could only explain a further few percent. Dessler is writing that this is consistent with work showing that heating of the surface and atmosphere during ENSO comes from ocean heat transport (Trenberth, 2002, 2010).

I agree with Dessler's view. My interpretations of the observations are firmly founded on this view of how the surface and atmosphere are heated during ENSO. Together with the similarity of curves in Fig. 1 and the phase plane plot in Fig. 2b in my paper, Dessler's calculation gives further support of the interpretation that radiative feedback due to temperature changes dominates over changes in radiative forcing during the five year period mid-2006 to mid-2011 where data suggests that the temperature oscillations are driven by a series of alternating ENSO events.

> *This issue has been discussed at length in some of the works cited by the author, yet the implications of these discussions seem to have been overlooked in this work. The author has misread the main points of Trenberth et al. and Dessler et al., which are quite explicit in saying that one cannot gain insight into the climate feedback parameter from ENSO.*

This is not a correct description of what is said in the paper. My view, as explained in the paper, is that one may gain insight into the climate feedback parameter by studying the TOA radiative fluxes with methods based on principles similar to those used by Gregory et al. (2004).

My view about Trenberth et al. (2010) is that they acknowledge the use of satellite radiative flux data for studying radiative feedback by using linear regression using the same linear model of the TOA radiative flux as used by Gregory et al. (2004), Eq. (3)

in my paper. They present this model as Eq. (1) in their paper. The only essential difference from Eq. (3) is the last term in their case that is representing random variations that by me and Gregory et al. are supposed to cancel when using annual averages.

In paragraph [12] on the second page of their paper Trenberth et al. emphasize that simple correlation analysis is preferred for determining the radiative feedback parameter according to their Eq. (1). It is obvious that this is based on their assumption that the dominating effect determining the radiative flux changes is radiative feedback due to temperature changes that are much greater than the changes in radiative forcing (in view of my discovery from the recent CERES EBAF data such an assumption needs independent verification since it may be valid in some time intervals but not in other).

Trenberth et al. (2010) also emphasize problems with advanced climate models that are important in this context. In page four of their paper, paragraph [23], they are saying that it is not controversial to state that a more realistic simulation of ENSO events remains a high priority for model developers. My view is that as a consequence one must be cautious with conclusions from comparisons between observations and models in this context.

It is obvious from the same paragraph that their view is that the radiative feedback caused by temperature changes in connection with ENSO may be the result of complex interactions including changes in circulation, atmospheric stability and changes in clouds which are not well depicted in models. This agrees with my views and similar ideas from Stephens (2005) are used in my paper as a possible explanation for the discovered lead-lag relation between the temperature anomaly and the radiative flux anomaly.

As to Dessler (2010) his work also is based on the theoretical framework resulting in Eq. (3) as described in my paper. In case of studying cloud feedback the climate feedback parameter is developed as a sum of different parts, each of which are considered representing one type of feedback. Dessler wants to determine the cloud feedback parameter and for that purpose the radiative flux is also developed as a sum with the idea of identifying the part of the radiative flux that is depending on clouds. The value of the cloud feedback parameter then is determined by linear regression on the cloud related radiative flux anomalies as a linear function of the temperature anomalies (also in this case there is an assumption about a dominating role of radiative feedback compared to radiative forcing that needs an independent verification in view of my discovery, except that a time lag should also be considered).

Dessler's view of how cloud feedback works is of interest in this context. On page 1523, third column, he writes that the cloud feedback refers to the net effect of clouds on radiation as the planet warms. The net effect of clouds is a reduction of the TOA net incoming energy of around 20 $\mathrm{W\,m^{-2}}$ and the changes of this incoming energy due to surface temperature changes is the cloud feedback. Furthermore Dessler is saying that if the cloud changes are further reducing (increasing) the net incoming energy as the climate warms this will offset some of (amplify) the warming resulting in a negative (positive) cloud feedback. Again a complex feedback mechanism is described, which should need time for spreading over the planet, justifying a hypothesis involving a time lag in the radiative feedback.

> *The text also repeats the exposed flaws in earlier works, such as in Spencer's for example, by alluding to clouds, and ENSO itself, as a forcing rather than a feedback.*

This is not correct. The text of the paper clearly emphasizes that it is a radiative feedback due to temperature changes that is studied. Both the time series plots in Fig. 1 and the phase plane plot in Fig. 2b give substantial evidence that during the five year period that is especially studied, the linear model equation (5) is satisfied:

$$N(t) = F - \alpha \Delta T \left(t - t_{\mathrm{lag}}\right)$$

The reason why the temperature anomaly time series and the net radiative flux anomaly time series exhibit proportional variations according to Fig. 1 and that the phase plane curve in Fig. 2b approaches a straight line during this five year period is that the unforced variations in the second term in the RHS in that equation dominates over the radiatively forced variations in the first term. This five year period is characterized by a radiative forcing F that is not varying very much compared to the unforced radiative feedback caused by the temperature changes.

This interpretation of mine can hardly be described as "alluding to clouds, and ENSO itself, as a forcing rather than a feedback".

Further explanation of my views on this matter is found in the following quote from my reply to Troy CA:

> *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), $N(t) = F - \alpha \Delta T \left(t - t_{lag}\right)$, 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 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.

The reviewer's next comment is:

Finally, the author is also willing to pose an hypothesis (that the regression between temperature and radiative fluxes is indicative of the feedback parameters) without carrying out the obvious tests that are readily performed in the realm of models (and have been done in other works).

This is not correct. My study is firmly based on a climate model study and on the corresponding methods developed by Gregory et al. (2004). The observations and their

interpretations have been comprehensively compared with climate model simulation results in Gregory et al. This concerns for example the question if a length of the time interval of five years would be suitable for estimating the value of the climate feedback parameter. Also the question if the observed lead-lag relation would be compatible with the climate models used by Gregory et al. is discussed. Those comparisons are based on the fact that during the relaxation process after a carbon dioxide forcing step experiment we get a pure radiative feedback caused by surface temperature changes. That is analogous conditions to what we are observing in the five year period mid-2006 to mid-2011 except that the temperature changes vary more rapidly than in the relaxation process.

It should also be noted that my results suggest that studies comparing climate feedback from observations with results from advanced climate models should consider that the climate system may change state suddenly as observed in Fig. 1 in the paper around mid-2006. The time periods before and after mid-2006 show very different relationships between temperature anomaly changes and radiative flux anomaly changes. According to my interpretation only the time interval after mid-2006 is characterized by unforced variability that dominates the temperature changes giving a linear response in the radiative flux. If such changes of state are not considered in the analysis of radiative feedback, for example if linear regression would be used over the whole CERES EBAF time interval, the results of comparing satellite radiative flux observations with climate models are likely to be flawed.

As the above discussion of Trenberth et al. (2010) suggests one should be cautious with conclusions from comparisons of observations and climate models in the context of ENSO. ENSO events are not well depicted in climate models. In order to get a deeper understanding of such feedback results and also to relate them 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. However, this is beyond the scope of my work because I have not sufficient insight into advanced climate models to do such work skilfully. Different researchers may have different skills and sharing results by publishing them is a normal way of getting the most out of the differing skills.

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