Earth Syst. Dynam. Discuss., 4, C189–C201, 2013
www.earth-syst-dynam-discuss.net/4/C189/2013/

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: 23 April 2013

Subject: Response to Anonymous Referee #2 and also final response: The interactive discussion and recent papers have convinced me that an even more thorough revision than a major one is needed.

Dear reviewer, thank you for reading my paper carefully and for your constructive criticism. I appreciate your advice that a thoroughly revised and shortened paper may be publishable in ESD.

If it is appropriate for the editors I want to submit a revised paper to ESD as a new discussion paper for ESDD instead of completing the present review process (that

would also be more convenient for me due to personal matters). The comments in this interactive discussion from the two referees and Troy Masters have convinced me that a more thorough revision is needed beyond what is normally considered a major revision.

Further justifications for that are the new papers by Dessler (2013) and Armour et al. (2012), which have appeared after the submission of my ESDD contribution. Those papers are important for my discussion of the relationship between the thermal damping rate (i. e. the climate feedback parameter, see below) and the climate sensitivity. See also my comments in the interactive discussion about those two papers.

Finally I have to think about your view that the topic is politically controversial and if that may change something in my text. In the subsequent text quotations from the referee report are followed by my comments.

General comments

> *This paper evaluates the rate of change of radiation at the top of the atmosphere (TOA) with change in surface temperature during a number of recent EL-Nino Southern Oscilllation (ENSO) events. The author shows that an approximately linear relationship is between changes in temperature and TOA radiation if the latter is lagged by 7 months. The slope is found to be 5.5 $\pm$ 0.6 $W m^{-2} K^{-1}$ which the author equates with "the climate sensitivity parameter".*

This is a correct description of facts except that I have used the term "climate feedback parameter". My use of "climate feedback parameter" originates from the textbook by Andrews (2010). Gregory et al. (2004) use the term "climate response parameter", Gregory and Forster (2008) use "climate sensitivity parameter" while Dessler (2013) has introduced the term “thermal damping rate”. I currently prefer and have recently adopted the new term “thermal damping rate”.

In my discussion paper I emphasized that I did not want to discuss the relation between the climate feedback parameter and the climate sensitivity. In the revised paper I will instead follow your advice to discuss that relation between the climate feedback parameter, from now on the thermal damping rate, and the climate sensitivity.

The subject is topical and important, but politically controversial.

I fully agree with your encouraging view that the subject is topical and important. Thank you for sharing your view that this topic is politically controversial (in fact that agrees with some recent personal experience of mine).

The relationship between TOA fluxes has been discussed by Spencer and Braswell (2010, 2011), Lindzen and Choi (2011) and Dessler (2010, 2011) amongst others, including the showing of phase plots and investigating the lags between the two variables. The main innovation here, as noted by the author seems to be the more linear nature of the phase plots obtained by using a lag of 7 months and restricting the observations to periods of strong ENSO variation, which may be interesting but not a substantial advance

I agree with your description. However, I want to add a few things. My observations also suggest at least one change of state, especially that around mid-2006, with rather different relations between changes in the temperature and the TOA radiative flux anomalies before and after that suggested change of state (the other papers mentioned have not considered such changes of state). The five year time interval after mid-2006 is characterized by coherent oscillations in the anomalies but with a time lag as illustrated in Fig. 1 in my discussion paper. Using that time lag in a lagged phase plane plot

based on Eq. (5) in the discussion paper confirms the special character of the five year period mid-2006 to mid-2011. The length of this time interval is remarkable compared to the much shorter previous observations of linear striations in phase plane plots by Spencer and Braswell.

The author’s discussion of the interpretation of their results is unclear to me, in particular regarding what relationship, if any, “the climate sensitivity parameter” derived here has on the long term equilibrium response to increases in greenhouse gases. This leaves the paper open to misinterpretation.

I hope I will be able to solve those problems in my work on the revised paper. Also see my response in the first paragraphs of this text.

The “climate sensitivity “was originally conceived as a way of characterising the simulated equilibrium global mean temperature response to changes in radiative forcing (usually doubling atmospheric carbon dioxide concentrations). The method devised by Gregory et al 2004 was created to enable modellers to estimate this parameter from short transient experiments, and the method was validated by comparing results with equilibrium studies using the same model. However, it has not been shown that estimating the climate sensitivity from shorter term predominately regional oscillations such as ENSO gives any insight to the value of long global sensitivity to increases in greenhouse gases in models, let alone the real world (see for example, Dessler, 2013).

I agree with your statements about the absence of knowledge of the relation between short term oscillations such as ENSO and various types of long term climate sensitivities. However, my view is that my observations and discussion of the coherent oscillations in temperature and radiative flux anomalies according to Fig. 1 in my discussion paper should be important in order to advance our potential understanding of any such relations, alternatively why such relations may not exist. Note also Troy Master's observation reported in his interactive short comment SC C53 and discussed by me in AC C68 that during a five year period HadCM3 also showed coherent oscillations looking rather similar to those observed by me. This shows that there is a potential for further understanding of this phenomenon by analyzing results from advanced climate models (although that is not my topic). Note also my short comments AC C90 and AC C137 comparing my results with Dessler (2013) and Armour et al. (2012), which also suggest that my results may be relevant for future work.

In view of the above, I find the paper in its current form unsuitable for publication. It is possible that a much shorter note based on Figures 1,2 and 3a and a more careful discussion of how "the climate sensitivity parameter) can and cannot be interpreted might be publishable .

See my response in the first paragraphs of this text.

Specific comments

Title, abstract and throughout the text. etc. As noted above, a strong caveat is required if the term "the climate feedback parameter" is used on these timescales. Since the author has carefully selected out periods on ENSO variability, a more accurate title might be "Estimation of the radiative damping parameter on ENSO timescales. . . .." or "radiative damping of ENSO . . . " .

Please note that I have used a scientifically objective method for selecting the five year period especially studied. The selection procedure involved the observation of a time lag in Fig. 1 of my discussion paper and the approach to a straight line according to the lagged phase plane plot in Fig. 2b. The identification of a series of alternating ENSO episodes connected to that time period was not a part of the selection procedure but was carried out later.

I have adopted the term "thermal damping rate" and will use that in the title in the revised discussion paper. Perhaps I will use the title "On the global thermal damping rate according to CERES EBAF radiative flux data" or something like that.

Page 27 line 9 You should point out Gregory et al 2004 considered long term externally forced monotonically warming experiments in contrast to the short term quasi oscillatory variations considered here.

I intend to change that part of the text in the revised paper.

Page 27 line 18 Rather than duck the issue completely, some discussion of the difficulties in trying to relate short term observations to equilibrium climate sensitivity is needed to put the results in perspective (eg see Dessler 2013)

As explained above I will do so in the revised paper.

Page 30 line 12 If I read Spencer and Braswell correctly, this was only true for 4 out of 18 models.

Yes, you are right. Those four models are mentioned on page 30 line 9 in my paper.

Section 3. Some indication of the uncertainties in the radiation and temperature data and their impact on the results is needed- in particular I suspect the changes in the global mean temperature estimates on these short timescales are not far outside the uncertainties in estimating global mean temperature.

I will consider that for my revised paper. Please note that averaging reduces the random errors. If the error distribution would be normal without autocorrelation we would see a reduction by division with $\sqrt{13}$ in this case. The comparison of monthly anomalies with and without averaging according to Fig. 1 in this comment suggest that averaging reduces the random errors significantly although there are some autocorrelation in the temperature anomalies from stations according to Brohan et al. (2006).

Page 30 line 27 How sensitive are results to averaging period (why 13 months?) or other data manipulation?

Except my views given above I have also discussed the effect of averaging in my comment AC C68.

Some reasons for selecting 13 months are (1) Gregory et al. (2004) plotted annual averages, (2) there should not be substantial differences between 12 and 13 months averages in anomalies but it is easier to compute 13 months averages, (3) as discussed in AC C68 and above random errors are reduced by averaging over a suitable time interval.

One may object that averaging time series with autocorrelation may introduce oscillations. However, that such a random effect should produce coherent oscillations in two different time series as observed here for temperature and radiation seems very unlikely to me.

Section 5.1 I feel much of this could be relegated to supplementary material.

I will consider to do so for the revised paper.

Section 5.2 I didn't find this convincing – Gregory et al (2004) consider a sustained positive radiative forcing which gave a monotonic temperature increase, here the changes are oscillatory in both driving force (ocean circulation) and temperature.

I do not fully agree with the statement that Gregory et al. found a strictly monotonic temperature increase. Their plots using annual averages also show temperature changes influenced by unforced variability that are not strictly monotonic and may include temperature decreases (although that cannot be clearly determined in their plots).

My view is that the carbon dioxide step perturbation during an initial phase creates a radiative imbalance at the TOA. In the subsequent radiation restoration process there are no changes in the TOA radiative forcing except natural ones, if any. The temperature increases in response to the TOA radiative imbalance. But ENSO oscillations and other unforced variability will also influence the temperature and consequently influence the radiative imbalance at the TOA due to radiative feedback.

Thus, after the initial perturbation the situation is fairly similar in the cases simulated by Gregory et al. (2004) and in the natural case studied by me. During a ten year period there should be several ENSO episodes and in Gregory et al. (2004) there are a period with ten years of annual means for the $2\times CO_2$ experiment with HadCM3 in their Fig. 2. We should find at least one five year period with ENSO oscillations within this ten year period. Of course, ENSO oscillations may not be well depicted in HadCM3 so my reasoning is strictly correct only for a perfect climate model.

However, on the basis of the discussion here I think that the treatise of those issues needs an in depth makeover for my revised paper.

Also, the temperature changes are about 0.2C here (and substantial measurement uncertainty) whereas in the model studies the temperature changes are much larger and there is no measurement uncertainty.

On the other hand, in Gregory et al. (2004), figures 1 and 2, there are ten year periods plotted for HadSM3 and HadCM3 using annual averages. Five year periods towards the end of those ten year periods have less temperature changes closer to my observations.

Section 5.5 Note that the pronounced regional distribution of changes in cloud, temperature and humidity are like to be very different to those expected with a global scale warming due to increases in greenhouse gases (see for example Dessler, 2013), which make ENSO a questionable analogue for estimating climate sensitivity.

I do not fully agree with your view here. The essential points in my view is as follows.

At first I think that the present situation for the climate system undoubtedly is a global scale warming due to increases in greenhouse gases. But unforced variability, for example ENSO, has also an important impact. Those are important factors that result in changes in global temperature and the changes in the TOA radiative fluxes that we observe by satellites. With only ten to twenty years of satellite data the changes in radiative fluxes due to changing anthropogenic forcings are difficult to detect compared to the changes due to unforced variability.

The current distribution of clouds, temperature and humidity is thus part of one period of the global scale warming. During the studied CERES EBAF period the satellite data also will reflect any changes in clouds, temperature and humidity due to the gradually changing anthropogenic forcings.

I think that the thermal damping rate should have some relation to the climate sensitivity. The global thermal damping rate for a five year period dominated by ENSO oscillations of the type I have reported here may not directly show the long term climate sensitivity but it may show how the planet cools itself in response to a temperature increase during that period. The planet's ability to cool itself in the long run in response to a temperature increase should be related to the climate sensitivity in the long run. The long term cooling should be the sum of the series of short term coolings (or more exactly, the long term change of the global net radiative flux should be the sum of its changes over each separate time interval). Hence there should be some relation between short term thermal damping rates and the climate sensitivity.

As I have described in my author comment AC C137 I think that Armour et al. (2012) is very relevant for my discussion. Their theory for connecting regional thermal damping rates with the global one makes it possible to explain my observations of a lag in the net radiative flux anomaly and the high value of the thermal damping rate. I have also discussed Dessler (2013) in AC C90.

I also want to draw attention to Troy Masters' report in his short comment SC C53 that similar phenomena as observed by me may be found in HadCM3 simulations. I have discussed that in AC C68.

Thus on those issues many new thoughts have been generated by the interactive discussion. Therefore a thorough revision of the discussion of those issues is necessary for my revised paper.

You should also reference Forster and Gregory (2006), Gregory and Forster (2008) who apply the method to observational data to estimate climate sensitivity or transient climate sensitivity.

I will discuss Forster and Gregory (2006) in my revised paper. However, I disagree about Gregory and Forster (2008) because they have not used TOA radiative flux data from satellites in their plots but estimated radiative forcings.

I didn't find the original supplementary material helpful.

I will revise the supplementary material for the revised paper.

Figure 1 What is the range of uncertainty in the global mean temperature and changes in net radiation ?

See my discussion above about uncertainties. I am considering a more in depth discussion of uncertainties in the revised paper.

Figure1 Given the dominance of ENSO variations over much of this period, it would be helpful to include a plot of an ENSO index (eg the Multivariate ENSO index).

Thanks, that is a good point.

Figure 2 , 3 – it would be useful to have some indication of the time progression of the points- eg label every 6th point.

I will do so in the revised paper.

Additional References

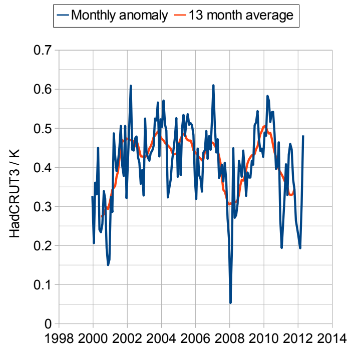
Dessler, AE, 2013. Observations of climate feedbacks over 2000-2010 and comparison to climate models. J Clim, 26, 333-342.

Forster P M and J M Gregory, 2006. The climate sensitivity and its components derived from Earth Radiation budget data. J CLim ,19, 39-52.

Gregory, J M and Forster, P M, 2008. Transient climate response estimated from radiative forcing and observed temperature change. J Geophys Res, 113,D 232105. Doi:10.1029/2008JD01045

I am going to cite the first two papers in my revised manuscript but not the third one as explained above.

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



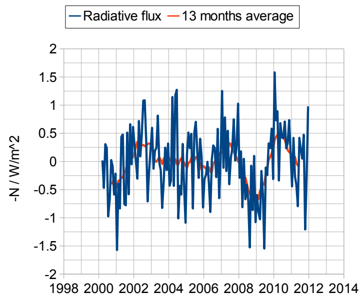


Fig. 1. Comparison of monthly anomalies and 13 months averages. Temperature (top), Net radiative flux (bottom).