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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

T. Masters

tmasters@ucla.edu

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As noted by the author, I had initially posted some thoughts on another blog, to which he replied. I wanted to post a comment to include it within this particular discussion, as well as expand on a couple of those points. I will refer to the paper as B13 from here on out.

Points #1 and #2 were relatively minor, where my suggestion was to consider other TOA flux and temperature datasets to determine the robustness of the results. This is only important insofar as the methodology is actually indicative of the underlying climate feedback parameter, of which I am doubtful (for reasons discussed later). Nonetheless, the author comments that "such an investigation [is] 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." I would thus expect the paper to focus much more strongly on validating the methodology against relevant test cases where the underlying climate feedback parameter is known, such as the CMIP5 pre-industrial control experiments. Instead, the resulting value is presented in the abstract, without an attempt to see how dataset choices might affect it. Based on my experience it is relatively easy to swap the temperature/flux datasets in and out of the analysis, and I think that the surface temperature set will create a larger difference than the flux set (perhaps neither will affect much), but it should at least be done as a quick check. It appears odd to use only HadCRUTv3 when HadCRUTv4 is out.

#3 indicated other papers that should be referenced, which the author agrees with.

#4 noted my concerns about using the running averages. This greatly reduces the effective degrees of freedom, and if this is not taken into account, the standard errors produced by that regression method and reported by B13 are far too small (+/- 0.6 W/mˆ2/K). For instance, using 5 annual points rather than a running average yields a similar point estimate, but error estimates of +/- 7.03 W/mˆ2/K! With such high auto-correlation after the smoothing, those monthly values can't be considered as independent in the error estimates.

#5 and #6 deal with whether the author's method actually yields results indicative of the underlying climate feedback parameter:

Section 5.1 is where it seems B13 considers the theory behind using the lag relationship. The author seems to note that the 4xCO2 demonstration shown in Figure 4 is the wrong test for this method (and it is indeed the wrong test) due to the rapid, forced warming in one direction, as the with-lag and without-lag methods produce very similar results, whereas "with the rapidly changing see-saw type of curve according Fig. 1 we have another case where the time lag has a considerable impact and cannot be neglected in the analysis." However, B13 never goes on further to a proper test case, where it might be determined if the with-lag method correctly diagnoses the feedback parameter (and this 4xC02 experiment is actually cited as a reason why one could expect an accurate diagnosis with only 5 years of data in section 5.2, despite it clearly being the wrong test).

In fact, this method can be tested using the pre-industrial control experiments of a CMIP5 GCM, one with a reasonable simulation of ENSO, to mimic the ENSO variations over the current timespan. In this case, I use the piCtrl from the GFDL-CM3 model, trying all overlapping 5-year periods from the years 6 – 500 in that experiment (the tas value is invalid in year 5, which is why I've started after that). B13 selects the period based on the phase plane plots. . .this method is basically selecting periods with a strong relationship between T and N, and I have simulated this by selecting periods for which the R2 between T and N at some lag is > 0.70. Within each of these periods, the B13 analysis is used to determine the feedback using a) a lead-lag regression between T and N, and b) the ratio between the maximum and minimum of T and N.

From my test, there are 88 of these 5-year periods throughout the 495 years examined. The mean diagnosed "climate feedback parameter" from these periods using the B13 in GFDL-CM3 is 2.84 and 2.37 for methods (a) and (b) respectively. While it is interesting that these values are well short of what is observed from observations, the fact of the matter is that they are a very different from the "true" climate feedback parameter value of 0.75 W/mˆ2/K for that model diagnosed over longer runs (Andrews et al., 2012, GRL). It is also true that this climate feedback value actually changes over time in AOGCMS (Winton et al., 2010), but B13 should focus on showing that the diagnosed feedback parameter is actually indicative of some relevant feedback value to sensitivity on some scale. Figure 1 of this comment shows the smoothed temperature and TOA flux of that first period in the GFDL-CM3, which can be compared to Fig 1 of B13. Figure 2 of this comment shows the pdf of the diagnosed GFDL-CM3 feedback using the B13 method, along with the true value. This with-lag method indicates a far larger feedback parameter than is actually relevant for climate sensitivity. (See attached supplement for script and data to reproduce figures).

B13 notes that "such lead-lag relations are also found in the results from advanced climate models", so it is seems that the paper should check whether these lag results properly diagnose the underlying climate feedback parameters in those models. There is some discussion related to the potential change in this short-term response versus multi-decadal scales, which is important, but again, it should be examined more in-depth on what this value diagnosed by the B13 method actually represents.

I am sympathetic to the results generally indicating a larger lagged response in observations than models, which is an interesting phenomenon and has been examined by others as well. It seems to me that Spencer and Braswell (2011), Lindzen and Choi (2011), and Dessler (2011) have already noted this lead-lag relationship, and that most are in agreement that these observations show larger responses at these lag times than most models. The question is whether this is indicative of some relevant climate feedback parameter, a limitation in ENSO simulations, or merely an artifact of applying a zero-dimensional model during the ENSO fluctuations. Unfortunately, I don't see much in this paper that adequately addresses that concern or validates the method, so I'm not sure I see what additional insight it provides.

Please also note the supplement to this comment:
http://www.earth-syst-dynam-discuss.net/4/C53/2013/esdd-4-C53-2013-supplement.zip

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

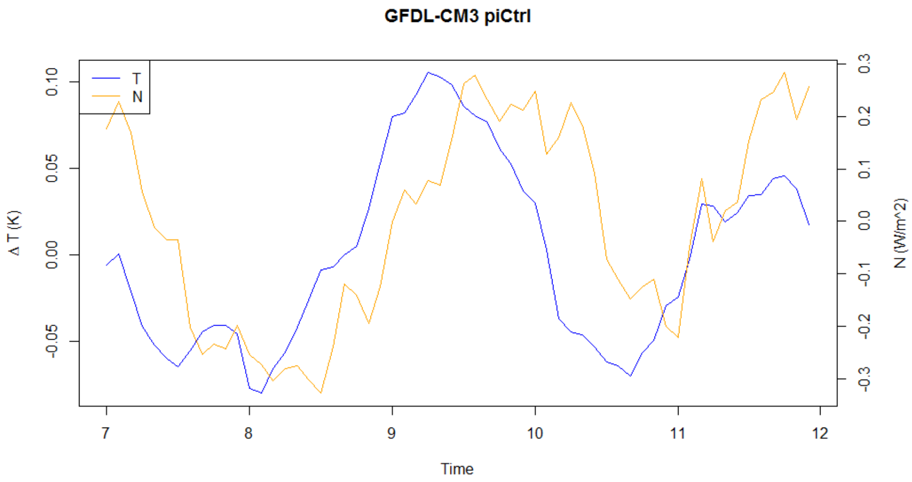

Fig. 1. 5 years of smoothed tas (T, blue) and rtmt (N, orange) from the GFDL-CM3 pre-industrial control experiment.

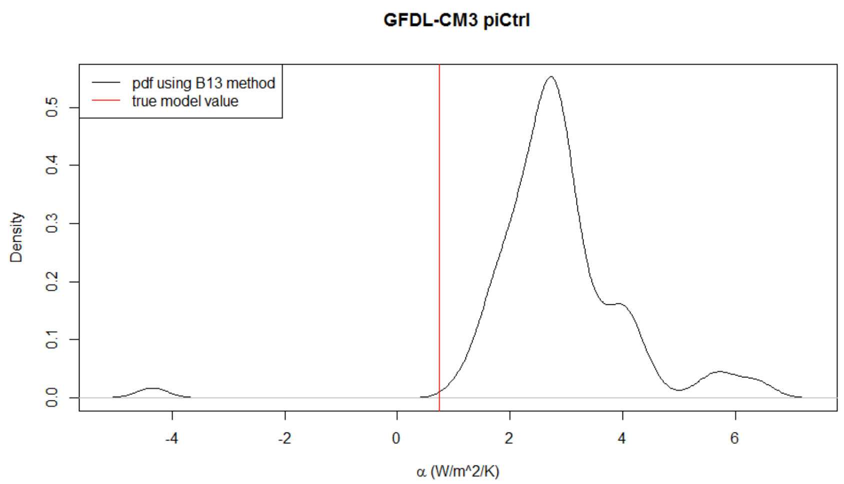

Fig. 2. PDF of diagnosed climate feedback parameter using B13 method on 500 years of GFDL-CM3 piCtrl data (black), along with the true climate feedback parameter value (red line).