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Earth System Dynamics Discussions

Interactive comment on "On the determination of the global cloud feedback from satellite measurements" *by* T. Masters

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I thank the reviewer, A. E. Dessler, for his comments on the manuscript (M12). Reviewer comments are in italics, with my replies below.

> *1. Comment: M12 uses a different surface temperature data set than the one used in Dessler 2010 (hereafter D10). This choice of the surface temperature data set makes an enormous difference and explains much of the difference between M12 and D10. This makes it harder to assign the causes of the differences between M12 and D10. So which data set is best? Given that reanalyses have more data going into them and a physics-based interpolation scheme, it seems that the reanalysis is best.*

This is particularly true in the polar regions, where the GISS and NCDC have the most trouble. I recommend that the GISS and NCDC surface temperatures be removed and replaced with the reanalysis surface temperature data sets. This will make the surface temperature data consistent with D10 and sharpen the focus of the paper on the disagreements in the flux data (which is ostensibly the focus of the paper).

Per M12 (abstract), the analysis attempts to determine the sensitivity to choices of surface temperature data as well as the radiative flux data. Nevertheless, it appears that to reproduce the smaller negative feedback (-0.57 W/^2/K) mentioned by the reviewer requires using the ERA-Interim skin temperature for Ts, rather than the near-surface air temperature (tas or t2m) typically used in diagnosing climate sensitivity/feedbacks. Table 1 shows the results using the more appropriate surface air temperatures from NCEP/NCAR and ERA-Interim reanalyses, which both show the higher magnitude negative feedbacks more in line with GISS and NCDC than the ERA-Interim skin temperature (using NCEP skin temperature yields a likely feedback of -0.86 W/m^2/K). It is a good suggestion to include these estimates from NCEP and ERA-Interim surface air temperatures (perhaps removing NCDC), but as mentioned these do not diverge much from those using the GISS or NCDC datasets, and confirm the result of M12.

2. Comment (my summary due to length): The reviewer suggests that the M12 conclusion is "too strong", and that when the first 30 months of the period are not included, and a value of +0.3 is added to account for non-cloud influences that "over most of the last decade (9/2002-6/2011), the different data sets all reveal the same thing as was found by D10: a likely positive cloud feedback."

As mentioned in #1 above, the results shown in the reviewer's comments seem to rely specifically on using the ERA-Interim skin temperature (rather than surface air temperature), along with removing the first part of the CERES record, in order to get similar results to D10. Table 1 below shows that even assuming the approximate +0.3 W/m^2/K adjustment the reviewer suggests during the 9/2002 - 6/2011 period (combined Terra + Aqua) yields a slightly negative or neutral feedback for CERES SSF1. Using EBAF (which is not recommended, see #3 below) yields a poorer correlation and a smaller negative feedback, although it is still clearly negative over the Terra period and the sign during the Aqua period depends on non-cloud influences.

As noted in M12, it is true that the CERES and ERA-Interim results are in better agreement over the Aqua period, in that neither seem to show a relationship (r^2 < 1%) between CRF and Ts. Adding an adjustment (+0.3) on top of this non-signal may result in an apparent positive feedback in some cases, but it would not seem to speak to the robustness of the result (more on this in #4), particularly when discarding a period (2/2000-8/2002) that, when included, does result in the strongest signal (r^2 = 7.6% for NCEP) and a large negative feedback. Similarly, choosing a different start date (later than 2002), where the different datasets are in even better agreement, results in more of a negative feedback (Fig. 1). M12 points out that the regressions are sensitive to the period over which they are run, which I have emphasized here in this Figure 1. If one defined the most robust result as either the period in which ERA-Interim and CERES fluxes are in best agreement (which is around 2004, at -0.5 W/m^2/K), or as the longest period with the best diagnostic (around -1.0 W/m^2/K, depending on Ts used), neither of these would reveal a similar conclusion to D10. This is not to say that we “know nothing about the cloud feedback”, but rather that the estimate depends on particular choices of data (and time periods), and strong conclusions with respect to either confirming the GCM cloud feedbacks or ruling out a significant negative feedback are not warranted.

> *3. Comment: The author should add EBAF to the analysis because EBAF has a more robust clear-sky flux product than SSF1 (I would have used EBAF in D10 had the full EBAF time series been available when that paper*

was written). My calculations show that EBAF clear-sky fluxes agree closely with reanalysis (particularly over the Aqua period), supporting the results of D10.

As mentioned in M12, the CERES SSF1 product is indeed likely better when considering interannual changes in the deseasonalized anomalies (rather than absolute flux) due to its superior calibration stability (particularly in the SW component, where the largest discrepancy exists), as well as the stability of the algorithm/metholodogy. The EBAF 2.6r product recently (Dec. 2011) corrected an error in the LW deseasonalized clear-sky anomalies resulting from the narrow-to-broadband conversion, bringing them more in line with CERES SSF1, but the difference in the SW clear-sky fluxes remains (and an apparent drift of ~ 0.4 W/m^2/decade). I do agree nonetheless that it could be beneficial to include estimates from both SSF1 and EBAF sets in tables 1 and 2, and will do so in the revised manuscript.

4. Comment: There are many unsubstantiated claims of uncertainty throughout the paper. For example, in section 2.2.2, the authors mention the possibility of spurious trends in the reanalysis water vapor product. However, most of the calculations in this paper and in D10 are regressions against surface temperature. Because both high and low temperatures appear throughout the record, the effects of trends in the data are minimized. This is explicitly discussed and quantified in D10 with regard to potential trends in the CERES measurements. Unless the author can add something quantitative beyond "uncertainty may exist here" (which applies to everything in science), such claims should be excised. This includes, for example, the entirety of sections 2.2.2 and 2.2.4.

M12 points out a real and quantitative discrepancy existing between CERES clear-sky and ERA-Interim clear-sky observations, and the amount of uncertainty this (and other

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choices) creates is quantified in M12 tables 1 and 2. Section 2.2 discusses the possible sources of uncertainty/error in the different clear-sky set. Determining the actual error in the reanalysis forecast fluxes from each of the relevant components (temperature, water vapor, and surface albedo) is beyond the scope of this paper (and in some cases may not currently be possible). The goal of M12 is to quantify uncertainty at a high level by noting the actual difference in estimates from the various sources of radiative flux data, as seen in the two tables of M12. The reviewer notes that D10 considers the effect of potential trends in the CERES (all-sky) measurements, and seems to suggest that this may quantify the effect of component bias. But this would only be the case where the errors in these reanalysis components are not correlated with surface temperature changes.

Regarding section 2.4 (as there is no section 2.2.4 in M12), the goal of the adjustments is to remove the influence of non-cloud components on the TOA radiation budget, but M12 points out two issues – assumptions about the correctness of cloud distribution in the all-sky kernels, and known biases with the reanalysis components – that suggest they may not simply be removing these other influences. This would be less of a concern if the adjustment was small compared to dCRF/dT. However, despite the seemingly small changes in the graphed r_cld vs CRF, the aforementioned +0.3 W/m^2/K is not only of a larger magnitude than from the initial raw regression, but also inserts a signal where none was present. In the D10 regressions, the r^2 for the raw dCRF/dT is 0.3%, whereas the r^2 between the adjustments and Ts is 6.9%, such that when the adjustments are added inserted to get r_cld the resulting regression yields the D10 reported value of ~2%. Using the AIRS observed components rather than ERA-Interim, M12 finds that a negative adjustment should actually be made when accounting for CO2, temperature, and water vapour components.

> *5. Comment: A related point: there are some particularly strong claims in M12 that are completely unsupported. An example is the claim in the abstract that "Attempts to diagnose longterm cloud feedbacks in this manner*

are unlikely to be robust." Even if one accepts that there are fundamental disagreements among the data sets (which I don't, see #2 above), then this points to limitations in the data, not the method. There is nothing in the paper to suggest that the method is "not robust" — nor is "robustness" even defined. Unless the author adds some evidence that the method does not work, this claim must be scrubbed. And the rest of M12 should be reviewed for other off-hand but unsupported claims.

I agree with the reviewer that "robustness" in this context should be more explicitly framed. Per section 4 of M12, it is implicitly defined as confidence in a range that would that allow either a significant negative cloud feedback or the model spread of cloud feedbacks to be confirmed or rejected. Unfortunately, the range of uncertainty arising from sensitivity to choices of regression data and time period indeed do not allow it. However, in addition to the disagreement between datasets (and M12 considers the process of combining measured all-sky flux with reanalysis clear-sky flux to determine CRF part of the "method"), M12 points out (section 4) that even if the datasets used yielded identical results for the short-term feedback, this would still not yield strong confidence in the diagnosed value for a long-term feedback because: a) The radiative response during the short-term ENSO fluctations may be significantly different from the actual long-term cloud feedback, as D10 does an excellent job of illustrating (for example, NCAR PM1 has a long-term cloud feedback that differs from the short-term feedback by 0.93 W/m^2/K), and b) The poor correlation suggests that the cloud forcing is varying apart from global surface temperatures, and the radiative noise produced will correlate with the global surface temperatures if the lag time to the initial temperature response is less than the decorrelation time of the noise, thereby contaminating the feedback estimate. Both (a) and (b) are obstacles that can likely be overcome with a longer record, which is why M12 notes that "diagnosing a climate-scale cloud feedback in this manner will require a substantially longer time period." I agree that this should be clarified in the revised abstract as well, noting that the lack of robustness stems from the shorter time period.

6. Comment: A minor point: The GISS and NCDC data analyzed are anomalies. The author's archived code shows that they calculate the anomalies of these data — in other words, anomalies of anomalies — before regressing against CRF. Because the data are already anomalies, they should just be used as is. This does not make a huge difference, but if the author wants to leave these data in (which I don't recommend), this should be corrected.

On this point I disagree. The anomalies for the radiative fluxes are calculated with respect to the monthly means over the shorter period (as that is all that is available), so for a proper comparison the surface temperature anomalies must be recalculated with respect to the monthly means over the same time period.

7. Comment: Another minor point: The calculations of the AIRS clear-sky fluxes from the clear-sky kernels is clever but unnecessary. The AIRS group produces clear-sky OLR as part of their standard product. If this remains in the paper, the AIRS product should be used. I've compared the AIRS clear-sky LWOLR to the reanalysis and the agreement is quite good, further supporting D10.

I thank the reviewer for pointing this out, and agree that the AIRS modelled clear-sky OLR product should be used, rather than that modelled from the GFDL kernels.

8. Comment: Yet another minor point. It appears that the author includes changes in radiative forcing in the calculation of CRF. This is not a standard definition of CRF, so if that's indeed being done, that adjustment should be removed. It does not make a huge difference, but it would confuse someone trying to reproduce this analysis.

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I do not believe M12 includes the changes in radiative forcing in CRF (at one point M12 accounts for the changing forcing when modelling the AIRS clear-sky flux), and agree this would be incorrect if it were the case.

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Table 1. Regressions of CRF against Ts with 2.5% - 97.5% confidence interval.

Clear-Sky Flux	Ts	Time Period	dCRF/dT	r^2
CERES	NCEP	2/2000-6/2011	-1.19 ± 0.73	7.3%
CERES	ERA-Interim	2/2000-6/2011	-0.88 ± 0.75	3.9%
EBAF	NCEP	2/2000-6/2011	-0.57 ± 0.65	2.2%
ERA-Interim	ERA-Interim	2/2000-6/2011	+0.23 ± 0.77	0.3%
CERES	NCEP	9/2002-6/2011	-0.33 ± 0.89	0.6%
CERES	ERA-Interim	9/2002-6/2011	-0.31 ± 0.79	0.5%
EBAF	NCEP	9/2002-6/2011	-0.10 ± 0.86	0.1%
ERA-Interim	ERA-Interim	9/2002-6/2011	-0.10 ± 0.87	0.1%

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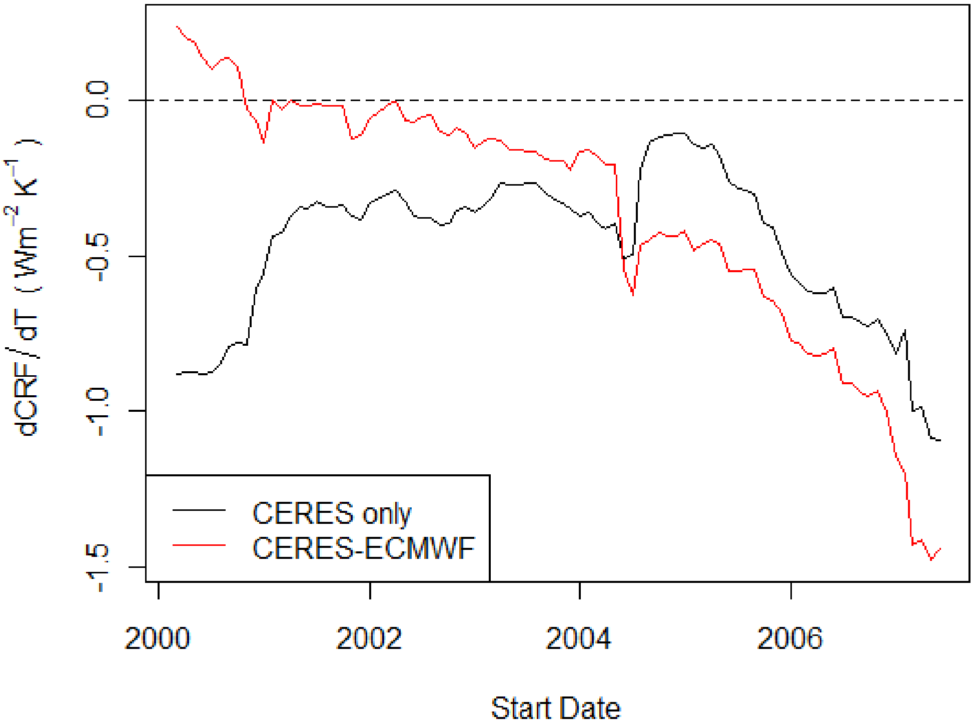

Fig. 1. The sensitivity of dCRF/dT to the start date (all regressions end in 6/2011), showing all regressions 4 years or longer. The CERES Terra product is used to derive CRF, and ECMWF ERA-Interim for Ts.

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