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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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This paper (hereafter M12) focuses on the question of the magnitude of the cloud feedback. This remains one of the primary uncertainties in climate science. However, M12 has several major deficiencies that need to be corrected. I detail those deficiencies here:

1. 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).

2. My biggest problem with M12 is in the framing of the conclusions. The author concludes that changing the clear-sky data set leads to different conclusions than D10, that we know nothing about the cloud feedback, and that the method used here is not "robust". However, this conclusion is too strong and an entirely different and more nuanced interpretation of the data would be appropriate.

In Fig. 1, I list dCRF/dTs for various data sets over the two periods M12 used. This is similar to Table 1 of M12, although the numbers below are different because I am using the ERA interim surface temperature data. I also emphasize here that, based on the results of D10, you must add approximately 0.3 to dCRF/dTs to get the cloud feedback (to account for forcing and non-cloud changes that affect the CRF).

In agreement with M12, I find that there are indeed differences in dCRF/dTs during the Terra time period. In particular, using Terra CERES all- and clear-sky fluxes produces a negative cloud feedback with a central value of -0.57+0.3 = -0.27 W/m2/K. Using EBAF all- and clear-sky fluxes, on the other hand, produce a positive cloud feedback with a central value of -0.08+0.3 = +0.22 W/m2/K. Using ERA interim clear-sky fluxes produces a still larger positive feedback of about +0.5 W/m2/K.

More interesting results are in the next column, which show the calculations for the Aqua period. Over this period, dCRF/dTs is reasonably consistent between the data sets. The Terra CERES cloud feedback is still lowest, but even it has a positive feedback with a magnitude of -0.05+0.3 = +0.25 W/m2/K. The other data sets are reasonably close and agree with D10 that the feedback is positive with a magnitude > +0.4

W/m2/K.

I conclude 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. Disagreements arise (primarily due to the Terra clear-sky flux) when the 30 months from 3/2000-8/2002 are brought in.

The author needs to revise the conclusion to consider the implications of the comparisons over the different periods. It is not reasonable to conclude, as M12 does, that we basically know nothing about the cloud feedback, or that the feedback is negative if you use CERES data, or that the method used is not robust. Nor is it reasonable to conclude that the results of D10 are wrong. Instead, narrower and more focused conclusions are required, with a focus on what's going on during the early part of the Terra period.

3. 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.

4. 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.

5. 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 long-term 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.

6. 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.

7. 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 LW OLR to the reanalysis and the agreement is quite good, further supporting D10.

8. 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.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 73, 2012.

Sources of data		dCRF/dTs	
All-sky flux	Clear-sky flux	TERRA period 3/00-6/11	Aqua period 9/02-6/11
Terra CERES	Terra CERES	-0.57±0.79	-0.05±0.87
EBAF	EBAF	-0.08±0.69	0.23±0.79
Terra CERES	ERA interim	0.25±0.77	0.19±0.88
EBAF	ERA interim	0.21±0.72	0.23±0.81
Aqua CERES	Aqua CERES	N/A	0.10±0.82
Aqua CERES	ERA interim	N/A	0.28±0.84

Figure 1. Units are $W/m^2/K$, and the uncertainly is $\pm 2\sigma$. The Terra CERES + ERA interim results are basically the same results as Dessler 2010. (I know this is a table and not a figure, but I couldn't figure out how to add a table to a comment)

Fig. 1.