

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

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Satellite observations of radiative fluxes and their responses to interannual variation in surface temperature are used to infer cloud feedbacks over the period 2000–2010. Cloud and non-cloud influences on the radiation budget changes (for both longwave and shortwave) are computed and compared with a previous estimate that combined the same data source with a re-analysis model of clear-sky fluxes. This paper extends previous work by demonstrating a sensitivity of the results to the methodology, timescale and temperature dataset and the work is therefore suitable for publications.

I thank the Reviewer for the many helpful and constructive comments, and am happy to hear the recommendation for publication.

C134

However, the primary result is that there is a lack of coherent relationship or consistency between cloud radiative effects and warming/cooling over interannual time-scales and so I consider it is misleading to argue prominently (in the abstract) that cloud feedback is negative and I consider that this should be revised accordingly. I also outline a number of specific suggestions and queries below.

Specific comments: 1) Abstract, line 6 - the satellite data refers to CERES so it should be made clear that the all-sky fluxes are from the same data source as the present study. 2) Abstract, line 10 - the results do not strongly suggest that negative cloud feedback is likely so I would suggest removing “resulting in a likely negative feedback”; the author indeed notes in the abstract that “there is little correlation between the changes in the CRF and surface temperatures.”

Noted - these suggested changes to the abstract have been made for the revised version.

3) p.74, line 22: since this “net cooling effect of cloud” is sometimes confused in the popular literature for negative feedback I suggest making it clear here, perhaps adding something like: “(for example whether the cooling effect becomes stronger or weaker or even changes sign to a net heating effect as the climate warms).”

I have added clarification to this line per the reviewer’s suggestion: “(that is, whether the cooling effect becomes stronger or weaker as the climate warms)” I have not included that the net cooling effect may change sign, as this seems unrealistic for the foreseeable future, since even assuming the high estimates of a positive cloud feedback would require > 10 K warming to change the ~ 20 W/m² cooling effect to a heating one.

C135

4) p.75, line 21-22: *It is unclear why the CERES SSF product "... is more stable with respect to anomalies than its Energy Balanced and Filled (EBAF) counterpart" and a line of further explanation may be warranted.*

I agree. A line of explanation has been added to the revised manuscript.

5) p.76, lines 21-25 - *I found it unclear how the well documented clear-sky sampling bias (e.g. Cess and Potter 1987, Tellus) can affect changes in cloud radiative forcing unless the clear-sky regions behave differently to the cloudy regions in terms of water vapor responses. The satellite data only samples the rare clear-sky occurrence in cloudy regions and therefore estimates lower clear-sky OLR than model calculations or microwave-derived estimates. This will only impact changes in cloud radiative forcing if this bias changes. This is not obvious and indeed model calculations show that clear-sky sampling does not appear to affect interannual anomalies in clear-sky LW (e.g. Allan et al. 2003 Q. J. Royal Meteorol. Soc.). Additionally, there is also a SW sampling bias (Erlick and Ramaswamy, 2003 JGR) relating to aerosol.*

I agree that the OLR clear-sky bias described is unlikely to significantly change, and that this is probably not going to affect the interannual anomalies. However, given that Sohn and Bennartz (2008) is the sole reference given for Dessler (2010) as to why clear-sky measurements were not used, I believe it is important to give it extra attention. Zelinka and Hartmann (2011) also use Sohn and Bennartz (2008) as justification for using kernel-modelled fluxes rather than measured fluxes.

I have included the other references mentioned by the reviewer within the revised manuscript.

6) p.78, line 14 "changes flux" → "changes in flux"
C136

Thank you for pointing this out. The change has been made for the revised manuscript.

7) p.78, line 25: *I was unsure where the 0.25 Wm⁻² adjustment came from. It is reasonable that greenhouse gas changes have increased radiative forcing by this amount over the period but what about other forcings (solar, volcanic, direct sulfate aerosols)? Will these introduce non-cloud effects on CRF? In fact, a recent paper by Kaufmann et al. (2011) PNAS, for example, shows little change in total radiative forcing since 2000.*

This section has been modified, per Reviewer 1's comments, to use the cloud-cleared OLR fluxes directly from AIRS, so this no longer applies to the revised manuscript.

8) p.80, line 4 - *please explain how the 0.16 factor is applied*

An extra line has been added to the revised manuscript explain the application of this factor: "This is applied by multiplying the factor by each month's WMGHG forcing anomaly relative to the start of the period (linearly increasing), and subtracting the result from ?CRF".

9) p.80, line 5 - *there may also be other non-cloud SW effects such as small volcanic eruptions (Solomon et al. 2011 Science) and changes in aerosol emission (e.g. Kaufmann et al. 2011 PNAS). These are probably not represented in ERA Interim either but this caveat should also be mentioned.*

The revision now includes mention of these effects / references.

10) p.81 - *discussion: it is interesting that another analysis using AIRS and CERES data also finds a negative LW cloud feedback although this is balanced by positive SW cloud feedback so the overall implied cloud feedback*

is positive (Zelinka and Hartmann (2011) JGR); recent analysis of MISR data are also suggestive of a negative LW cloud radiative “feedback” due to a decline in cloud altitude since 2000 (Davies and Molloy, 2012 GRL) although this is predominantly manifest as a decline in cloud altitude during the 2008 La Nina cold event. It is of course important to stress that there is little physical evidence for feedback over this time-scale and changes in ocean temperature may be leading atmospheric response over some regions and cloud radiative effects altering ocean temperature over others (depending upon the depth of the mixed layer which varies strongly by region) (e.g. Dessler 2011 GRL; Spencer and Braswell, 2008; Lloyd et al. 2011 Clim Dyn). Recent estimates of ocean heat content changes also seem to correspond with CERES measurements although there is a substantial observational uncertainty (Loeb et al. 2012 Nature Geosciences) and it is probably more likely that ocean changes are leading top of atmosphere flux changes over ENSO cycles as argued by Dessler (2011) GRL. Fast adjustments to forcings and regional changes in SST may be operating in addition to slower feedbacks in response to warming/cooling (e.g. Andrews et al. 2010 GRL; Lloyd et al. 2011 Clim. Dyn) as alluded to in the text.

I agree with many of the reviewer’s points, and have included discussion of the Zelinka and Hartmann (2011) and Davies and Molloy (2012) results.

11) Figure 2 - it would be useful to show the time-series of surface temperature and also to include the HadCRUT and ERA Interim data dataset in the comparison to demonstrate the sensitivity to dataset used. The correspondence between clear-sky flux estimates (from CERES, ECMWF and AIRS) is remarkable. Does this indicate that the difference between CERES/ECMWF and CERES-only feedbacks originate mostly in the SW or

C138

are subtleties of the trends in clear-sky LW important?

Per Reviewer 1’s comments, and as suggested in point #14 below, I have replaced the NCDC temperatures with ERA Interim temperatures. I have added another panel to figure 2 which now includes surface temperatures as well. To answer Reviewer 2’s question, these small subtleties appear to make a difference of $\sim 0.35 \text{ W/m}^2/\text{K}$ depending on the surface temperature set used, although the larger discrepancy exists in the early part of the SW component, where the correspondence is not quite as good.

12) *Since the interannual relationships are being studied, rather than long-term feedbacks, it may be informative to de-trend the datasets. This would also remove any spurious long-term drifts.*

Due to the different ENSO states between the start and finish of the period, and the fact that a linear trend is greatly affected by the highs and lows caused by these states, de-trending the data is unlikely to effectively remove the long-term drift and may actually introduce a bias. For example, calculating a simple linear trend of the Terra CRF produces an apparent $0.69 \text{ W/m}^2/\text{decade}$ trend, which well exceeds the expected drift of the SSF1 degree product and is almost entirely the result of the large dip in 2010 CRF (presumably) associated with that El Nino.

13) *The difference between Terra and Aqua periods are interesting and this should be mentioned explicitly. Does it suggest a problem with the early Terra/CERES record?*

I have added another figure to the revised manuscript, which illustrates the sensitivity of the estimate to the time period over which the regressions are run. Both the ERA Interim and Terra results are sensitive to this start date, which is expected given the lack of concrete relationship between CRF and T_s over the period.

14) Table 1 - The comparison with Dessler (2010) is not clear since different surface temperature records are used. I suggest switching from NCDC to ERA Interim surface temperature as this will provide a consistent comparison with Dessler (2010) while also demonstrating any sensitivity to the dataset used (is ECMWF the same as ERAInterim?).

As mentioned above, the revised manuscript will use ERA Interim temperatures rather than NCDC. ECMWF is indeed the same as ERA Interim, and have thus changed the “ERA-Interim” reference to ECMWF in that table.

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