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January 19, 2011

Dr. Richard S. Lindzen
MIT
Dept. of Earth, Atmosphere, & Planetary Sciences
77 Massachusetts Avenue
Cambridge, MA 02139

Title: On the observational determination of climate sensitivity and its implications
Ms. No.: 2010-15738

Dear Dr. Lindzen,

The Board appreciates your cooperation in soliciting additional reviews on the paper you recently contributed to PNAS. We consulted the two experts you approved and two others selected by the Board. All four reviews (enclosed) were shared with two members of the Board before reaching a final decision. One of the Board members noted:

> All of the reviews are thoughtful assessments of the strengths and weaknesses of the manuscript in question by leading experts, so they provide valuable hints for (possibly) improving the paper…I sympathize with Rev. 4's comments who concludes that the new paper simply has to explain why the opposite conclusions from the same data set by Trenberth et al. are flawed. If that could be achieved through a major review of the current version (hopefully accounting also for other important referee remarks) then the article would provide a crucial contribution to a most relevant scientific debate.

In light of these additional critiques, the Board concurs that the current paper must be declined for publication. I am sorry we cannot be more encouraging at this time and hope the additional reviews will help in revising the work.

Sincerely,

Randy Schekman

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Editor-in-Chief

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Evaluations

Role	Suitable Quality?	Sufficient General Interest?	Conclusions Justified?	Clearly Written?	Procedures Described?	Supplemental Material Warranted?
Reviewer #1	No	Yes	No	No	No	Not Applicable
Reviewer #2	No	Yes	No	Yes	No	No
Reviewer #3	No	Yes	No	No	No	Yes
Reviewer #4	No	Yes	No	Yes	Yes	Yes

Reviewer #1

Note	Comment
Comments (Required)	This paper is a reworking of an earlier paper by the same authors published in GRL in 2009. The authors attempt to use observed correlations between radiation budget and sea surface temperatures to infer global climate sensitivity. They then compare this inferred climate sensitivity with climate model simulations, by employing the same technique they used for the observations. The paper is based on three basic untested and fundamentally flawed assumptions about global climate sensitivity: 1) Correlations observed in the tropics reflects global climate feedbacks. As justification: the authors state ' we argue that feedbacks are largely concentrated in the tropics' (see line 33 and 34 in abstract). The only argument given in the text is lines 126 and 127, where they state: " However there are good reasons to consider the tropics; for example, concentration of water vapor in the tropics". This is not an argument; more an assertion. This assertion is also radiatively wrong, for it is well known, the lower the water vapor concentration, the larger the flux changes. Since the entire paper relies on this assumption, they must prove it. I suggest they undertake the following exercise to prove their assumption: From CERES data, they should do their correlations between SST and fluxes for the whole globe; and compare it with those just for the tropics. They claim ERBE data is not good in the extra-tropics. Is this a good reason for ignoring the extra-tropics? 2) Their analyses ignore changes over land areas in the tropics. It is well known that adjustment in the atmosphere is over the entire length scale of the tropics (due to the large Rossby radius of deformation). We know,warming over the tropical pacific leads to the walker circulation on thousands of kilometers length scales; thus rising motions over the oceans (more cloudiness) in the tropics will lead to subsidence (less water vapor and clouds) over land areas. Thus, even for just the tropics, neglect of land surface temp changes and fluxes is unjustified. 3) Lastly, the authors go through convoluted arguments between forcing and feed backs. For the authors' analyses to be valid, clouds should be responding to SST and not forcing SST changes.

	They do not bother to prove it or test the validity of this assumption. Again this is an assertion, without any testable justification.

Reviewer #2

Note	Comment
Comments (Required)	Review of Lindzen and Choi On the determination of climate sensitivity and its implications. PNAS The authors take monthly sea surface temperature (SST) averaged from 20N-20S and Outgoing Longwave Radiation (OLR=LW) and reflected Shortwave Radiation (SW) measured from a series of satellites over the period of approximately 1985 to 2009. They do some smoothing in time, and then compute the regression of shortwave and longwave radiation change at various lags on changes in SST. These regressions are then used to infer climate sensitivity. While I think such work is potentially interesting, I have two major concerns about this work. The first concern is that month-to-month variability of the tropics may have nothing to do with climate feedback processes. Although the paper acknowledges this in its introductory sections, the conclusion is greatly overstated as applying directly to CO2 driven climate change. The second and more significant concern has to do with the analysis procedure. I would advise both the author and the journal not to publish this paper as it stands. The poor state of cloud modeling in GCMs has been amply demonstrated elsewhere and the effect of this on climate sensitivity is well documented and acknowledged. The more significant result here is a claim to have demonstrated an extremely strongly negative, fast process climate feedback in the Tropics. This would be revolutionary, if it bears the test. While the stated result is dramatic, and a remarkable departure from what analysis of data and theory has so far shown, I am very concerned that further analysis will show that the result is an artifact of the data or analysis procedure. The result comes out of a multi-step statistical process. We don't really know what kind of phenomena are driving the SST and radiation budget changes, and what fraction of the total variance these changes express, since the data are heavily conditioned prior to analysis. We don't know the direction of causality - whether dynamically or stochastically driven cloud changes are forcing SST, or whether the clouds are responding to SST change. Analysis of the procedure suggests the former is true, which would make the use of the correlations to infer sensitivity demonstrably wrong, and could also explain why such a large sensitivity of OLR to SST is obtained when these methods are applied. The description of the procedures is long on philosophical discussion, but rather too spare in describing exactly what was done. Sufficient description is necessary so that another experimenter could reproduce the analysis exactly. I don't think I could reproduce the analysis based on the description given. For example, exactly how were the intervals chosen? Was there any subjectivity introduced? My major concern has to do with understanding what was done, whether this methodology is

	sound, and how to interpret the results, within the limitations imposed by using month-to-month variability to infer climate sensitivity, as it is usually defined. The inferred sensitivity of longwave emission to SST is enormous, significantly greater than that of a black body at the emission temperature of the tropics. Given that no plausible model or data analysis has ever produced anything close to this, one is inclined to think that the result comes from the methodology and not from physics. Also, the analysis is purely statistical; we have little idea what kind of phenomena is driving the correlations. Most significantly, I think, we don't know if the clouds are responding to the SST, or if the clouds are varying independent of SST and driving the SST changes. Let's concentrate on the methodology and try to infer how this result could have been obtained and whether it is applicable to a climate sensitivity estimate. To begin, consider the longwave and shortwave sensitivities obtained from the analysis, that are shown in Table 1. The longwave sensitivity is 5.3Wm-2K-1. A blackbody with the emission temperature of the tropics (258K) would have a sensitivity of 3.9 Wm-2K-1. This implies a very strongly negative feedback within the tropics that would have to be produced by a very strongly negative temperature-water vapor-cloud feedback, if it is to be interpreted as a feedback. The factor of 2 introduced to divide this feedback over the world does not obscure the fact that within the Tropics a remarkably, and perhaps implausibly, strong negative longwave feedback (relative to a blackbody baseline) is inferred, which has heretofore never been observed or produced in any model of which I am aware. This would be revolutionary if it were true. What is the mechanism for such a strong negative longwave feedback? The average longwave cloud forcing in the tropics is about 40Wm-2, so that would require about a 3.5% decrease in LWCF per degree of SST increase to produce the difference between the estimated longwave sensitivity and the black body sensitivity. Even more would be required to overcome the likely positive clear-sky water vapor feedback. The clear sky longwave sensitivity is well established from observations at about 2 Wm-2K-1, so a substantial decrease in relative humidity or high clouds would be required to get to 5.3 Wm-2K-1. A cloud forcing reduction of 5.3-2.0/40 ~ 8% decrease of LWCF per degree of tropical mean SST. Since the cloud fraction in the tropics is less than 50%, which translates into a cloud fraction change of 15 or 20% per degree of SST. That would be directly observable and someone might have noticed by now. I think cloud fraction changes with SST are quite plausible, but the magnitudes implied by this result are huge and seem unlikely to be real. So I am motivated to try to understand how this result was obtained. What is the source of the SST changes that drive the regressions? The biggest SST signals in Figure 2 seem to be related to the el Niño-Southern Oscillation (ENSO) warm events in 1987 and 1998, but if you look at the LW and SW time series in Fig. 3, these two events are either not reflected in the radiation budget quantities, or are so different in their responses as to suggest that it is not tropical mean SST that is driving the changes in radiation budget. The 1992 Pinatubo event dominates the shortwave time history, except for the apparent calibration shift in the period 1994-2000. Analysis of the procedure suggests that it is not these large changes in SST that control the correlations, but rather smaller short term changes that are the month-to-month rattle in the data. Both the SST changes and the flux changes are probably rather small in magnitude. Lines 362-390: The procedure used to compute the feedbacks is unusual and it is important to

	consider how this is affecting the results. Instead of regressing simultaneous departures from a mean value to calculate covariance, the authors take time differences of flux and SST (e.g. DT=T(i+1)-T(i), and DF=F(i+1)-F(i), where i represents a set of time steps that the authors pick as starting and ending points) and regress onto these time differences. The objective method to select these intervals is not stated very clearly, although it is suggested that differences of greater than 0.1C is the criterion. The procedure should be stated clearly enough so that someone else could reproduce this result. The regression line is constrained to pass through zero, although if one variable had a trend and the other didn't it seems that the scatter would not pass through zero. It is not clear how these data points are chosen or whether all the data are actually used. It is unlikely that the changes over these intervals represent two states in which the SST and TOA radiation budget are equilibrated to each other. As a proposed remedy for this causality problem, the authors introduce a lag between the flux measurement and the SST measurement and compute regressions for an assortment of lags. SST(t) and Flux(t+Lag). This produces the interesting result that the regression slopes vary widely with this lag. If the lag is chosen to be zero, then the SW and LW regressions are about equal and opposite, which is what you might expect if positive SST changes were associated with a reduction in high cloud. The net radiative effect is about zero, but the increased shortwave reflection is felt as a surface heating, and the increased OLR, which cools the atmosphere, is balanced by greater subsidence, which is consistent with less cloud. The fact that the shortwave anomalies are larger if they are computed with negative lags, suggests that the reduced cloudiness might actually be driving the SST for a few months prior to the warm anomaly. If this is true, then the cloud variations are driving the SST, rather than being in equilibrium with it, as might be expected if variations in large-scale dynamics are driving the changes. If the cloud variations are driving the SST, then these data are not appropriate for computing climate feedbacks, as they are disequilibrium forced fluctuations. Also, if they are transient forcings by cloud variations, then the heat capacity of the ocean must be taken into account in computing the ratio of radiative forcing to SST change. This would make the SST changes small relative to the radiative forcing and this might explain the large values of Dflux/dTemp reported in Table 1. The lagged regressions between monthly tropical mean SST and radiation budget quantities suggest that the annual cycle may be involved in determining the covariances, but I will propose an alternative theory below under technical questions. The regressions have a peak to trough cycle across six months, suggesting an annual periodicity is involved. It would be interesting to see if the autocorrelation of the SST anomaly also shows an annual periodicity. The authors choose lags of 1 and 3 months after the SST anomaly to compute the longwave and shortwave. This produces the maximum net negative feedback. If simultaneous regressions were used the estimated LW and SW feedbacks would be equal and opposite and both large. This would be consistent with a change in tropical deep convective clouds, whose longwave and shortwave effects nearly cancel. This would give approximately a zero net radiative feedback in the current calculation. It is not clear why lagged regressions are favored, but it is clear that the two lags chosen for longwave and shortwave give the largest negative feedback. Without a better explanation, the choice of lags to focus on seems tendentious, as one could have produced a result more in accord with the common wisdom with a different choice.

	Without a physical explanation for where these strong negative feedbacks are coming from, and without an acknowledgment that the results are highly uncertain and possibly not applicable at all, I would not publish this paper. Technical questions: I assume the basic data are deseasonalized monthly means of the spatially averaged quantities between 20S and 20N. Is this correct? How were the degrees of freedom estimated in calculating the uncertainties in Fig 4 and Table 1? The serial correlation is significant and will be greatly increased by the smoothing. The error bars probably should not shrink that much with smoothing as the degrees of freedom should be reduced by a factor of about three when the 3-month smoothing is applied. The low-pass and high pass filters will change the autocorrelation from that in the original data. Can one increase degrees of freedom by high pass filtering? I presume that the annual cycle was removed from any of the variables, since this was stated in the abstract (deseasonalized) but how this was done is not discussed. Given the calibration shifts and low frequency variability in the time series, it would be difficult to get a clean removal of the annual cycle, I believe. Is it the residual annual cycle dominates the results? It appears that it might, since the lagged correlations have an annual period in them (Fig. 4). On the other hand, in the next comment I point out that the filters applied constitute a band-pass filter, so that the authors are looking at variability with a particular period. Tsushima et al. (2005) did a similar analysis for the annual cycle and the global mean temperature and concluded that the longwave sensitivity was 2.0, the reflected shortwave was -1.1 and the net was 0.98 Wm-2K-1. Why are the present estimates so different? Regression on temporal derivatives of flux and SST is nearly equivalent to high passing the data and then doing regressions on that high pass filtered data. The running mean smoothing is a crude low pass filter. The two combined constitute a band-pass filter. So what the authors are analyzing is the variance passed by a 1/3, 1/3, 1/3 smoothing filter, followed by a (-1, +1) difference filter, applied to monthly data. The response function for such a filter pair is easily calculated. The running mean smoother has a sinch function frequency response, with the first zero at 0.33 cycles per month (cpm) and a phase reversal for frequencies higher than 0.33 cpm. The difference filter has a sine response on the interval zero to 0.5 cpm. Since the high pass (-1 1) filter rises to its half power point at 0.25cpm and the low pass filter cuts down to zero at 0.33, the resulting band pass filter passes more than about 20% of the variance in the window between 0.15cpm and 0.25cpm, or periods between 4-8 months. Since the input signal is probably fairly red, the peak variance might be at a lower frequency. This conditioning of the signal to be band passed, might explain the periodic nature of the covariances in Fig. 4, since a band-pass filter can make even noise input appear periodic. Tsushima, Y., A. Abe-Ouchi, et al. (2005). "Radiative damping of annual variation in global mean surface temperature: comparison between observed and simulated feedback." Clim. Dyn. 24(6): 591-597.

Reviewer #3

Note	Comment
Comments (Required)	Comments on the paper by Lindzen and Choi General: Professor Lindzen is an extremely distinguished and talented scientist, and discussion of his views in the scientific literature is welcome. However, it is important that the use of journal pages work towards a constructive and complete record, especially on matters that have already been subject to prior debate and analysis. In that regard, I feel that the major problem with the present paper is that it does not provide a sufficiently clear and systematic response to the criticisms voiced following the publication of the earlier paper by the same authors in GRL, which led to three detailed papers critiquing those findings. The abstract of the current paper refers prominently to accounting now for the 72 day precession period in the ERBE data, but this was not the major criticism of the earlier work and highlighting that in an abstract seems to me to be confusing and inappropriate. It is in the interest of both the authors and the journal to ensure that anything now published does not lead to unconstructive confusion but rather represents full clarity on the issues and how they are addressed. To that end, my major concerns are as follows: 1) The authors state that their approach tests equilibrium climate sensitivity, and they also present tables comparing the present results to sensitivities (including equilibrium climate sensitivity) of various models. While their approach may indeed be a useful test of some aspects of climate response, I do not think it is correct to refer to it as a test of equilibrium climate sensitivity, and 'apples and oranges' comparisons are important to avoid. For example, not all feedback is tropical in the long term. It is well established that snow and ice retreat influence equilibrium climate sensitivity. Some studies suggest that this effect represents a substantial feedback in the total equilibrium climate sensitivity (see e.g. Hall, J. Clim, 2004 among many others). This important effect will not be captured with the analysis approach used here and is one example of a shortcoming that is important but not acknowledged. Further, in the longer term such factors as land/sea temperature contrasts, and gradients in temperature between low and high latititudes, etc. are also expected to change, which can be expected to change the distribution/frequency/optical properties of clouds and hence the long-term equilibrium climate sensitivity as compared to shorter term responses. Changes in cloudiness at middle and high latitudes are likely to make significant contributions to equilibrium climate sensitivity, and are not captured in the current approach. Again, these phenomena will not be captured using the present approach. The paper therefore should not refer to its approach as a test of equilibrium climate sensitivity, and should avoid presenting tabular comparisons that are not appropriate. The paper would benefit by being much clearer as to what it is testing, and what the limitations of those tests could be. 2) One key criticism of earlier work by these authors is the restriction of their analysis domain to the tropical regions, because there is a great deal of exchange of heat between the tropics and higher latitudes. While it is true that much of the feedback due to e.g., water vapor changes may occur in the tropics (as highlighted by the authors here), this does not justify the idea that all the

	responses to that feedback must be similarly tropical in their distribution. I see no reason to believe that transfer of heat from the tropics to higher latitudes could not change the distributions of cloudiness at higher latitudes, and I could not see that the authors' approach accounts for this or similar possibilities. I did not find the statement that this leads to a factor of two being added to the equation but not influencing the result to be understandable. The authors address this point too briefly for their explanation to be clear to this reader. Further the matter is presented only in the supplement. Given the importance of this issue and its having been the focus of several previous criticisms, a more complete explanation that fully illustrates what they have done in response and what difference it makes would need to be provided in the main body of the paper. It is too fundamental to leave to a supplement. I was also curious regarding the restriction of attention to tropical oceans only; responses over land would also seem to be of interest if a quantitative analysis is the goal. 3) Another key criticism of their earlier work that the authors have not addressed sufficiently in my view is the fact that some model runs, including several in AMIP using prescribed SSTs, do not all include the appropriate forcings, including (but not limited to) that associated with volcanic aerosol. This point was made well in responses to the earlier paper but is not fully addressed here. In the absence of such forcings, the energy balance in these models is incomplete and cannot constitute a test of climate response. The authors argue that they account for this with leads and lags, but this does not seem to me to be correct. The most obvious example of this occurs in the period of the Pinatubo eruption, when volcanic aerosols led to a large change in shortwave radiation (see, e.g., the authors' own figure S5). Volcanic forcing is not included in many of the AMIP runs (and is not in some of the CMIP runs either), so it is not surprising that the shortwave changes are inconsistent with those measured. Surely in such situations, the approach used cannot represent a test of climate response since the energy budget in those simulations is not fully consistent. I would not be surprised if the longer term (e.g. decadal) changes in other forcings such as greenhouse gases or anthropogenic aerosols (including e.g those from biomass burning, which may vary quite a bit in the short term and long term) were also important, and I did not find the authors' explanation regarding why this doesn't matter to be clear. . It may also be noteworthy that several papers have emphasized how volcanoes can influence ocean heat uptake for many decades after an eruption, but of course that effect cannot be included in prognostic models that don't include the forcing; what happens in the AMIP runs may also be limited since only the SSTs are prescribed (with significant uncertainties), and a complete representation of ocean heat flow will be lacking. Without a fully consistent energy budget, I don't think an appropriate test of climate responses can be performed. The authors need to explain more fully why this isn't necessary in their view, and how they interpret e.g, the most prominent example of the Pinatubo period, without it. 4) Another key criticism of their earlier work was the approach taken to determining error bars, smoothing, and 'lags', and the extent to which choices made result in tests that may not be representative of climate sensitivity. This doesn't mean that the authors' tests are not of some interest, but if the results are strongly dependent upon smoothing and lags, then what is being tested may very well not be indicative of the responses of interest for climate change. If, as is stated, high frequency variations make it difficult to identify periods of warming or cooling, the interpretation of the outcome of a smoothed analysis seems to this reader to be quite ambiguous; it may just be 'noise' but it also may be real, and representative of the real climate response that

	is of interest here (I believe this point was made well in responses to the earlier paper). So if the authors insist on this, I feel they would need to present a series of results demonstrating what happens with different choices more clearly, rather than continuing to emphasize a particular choice. Further, a particular criticism was that it is not appropriate to apply different smoothings, or different lags, to the longwave than the shortwave data, due to the need for a fully consistent energy budget. But the present paper seems to continue that practice (page 11, lines 237-238). I don't see how that can be appropriate, since it must create inconsistencies. In short, I did not find the authors' explanation of how they addressed earlier criticisms on data handling (smoothing and leads and lags) to be clear. The statements made near the end of the paper regarding uncertainties in the approach are welcome, but this needs to be moved up, and a fuller discussion presented.

Reviewer #4

Note	Comment
Comments (Required)	1) If the paper were properly revised, it would meet the top 10% category. 2) The climate feedback parameter is of general interest. 3) I answered no, because the exact same data have been used by others to get an opposing answer and I do not see any discussion or evidence as to why one is correct and the other is not. The responses to the questions should be evident in the review. Detailed comments: This paper uses ERBE and CERES SW and LW fluxes and observed sea surface temperature variations in the tropics to evaluate the overall radiative feedback of the climate to SST changes. The resulting feedback fractions and slopes are compared to those computed from 11 climate model datasets for the same time period and to CMIP results. LW and SW slopes are computed separately using only those intervals having changes of 0.1°C or greater. Because this paper is response to the criticisms of Trenberth et al. (2010), it attempts correct past errors in methodology and data use. It performs a smoothing and lag study to determine the highest correlations between the changes in flux and SST, settling on lags of 1 and 3 months for LW and SW, respectively. The study finds that the slopes from the observations and models differ significantly with the former yielding values that are much less sensitive than observations. Moreover, the analysis shows that the model sensitivities are generally different from those in the IPCC AR4 report, unless one considers the confidence intervals of sensitivities computed in this study. The implications are that the climate sensitivities, i.e., feedbacks, are highly exaggerated by the models and, hence, their predicted warmings are too large. General comments Trying to understand the feedback of the Earth-atmosphere system to radiative forcings from observations has been going on for a long time and remains difficult. This paper continues in that vein and, as far as I am concerned, shows that observations and model calculations are different.

	Whether the subject analysis provides a true estimate of the radiative feedback parameter remains unclear. Trenberth et al. (2010) performed a very similar analysis and got the opposite result. Why are the two analyses of the same data so different? That is the big question here. While the specific comments bring up some issues related to that question, it is clear that this paper provides no insight. Why can the two papers arrive at such divergent answers? I would love to see that question resolved satisfactorily. Both cannot be right. Perhaps, both are wrong. But to go beyond Trenberth et al. and LC09, this paper has to address that question and argue why Trenberth is wrong and the current analysis is correct. Otherwise, we are left with two completely opposing analyses of a common dataset and no discussion as to why one is correct and the other is not. I am glad to see that this paper was redone, but I think it could be a lot better. I would recommend it for publication after major revisions that address the main concern above and the specific issues below. Specific comments *Lines 64-68:* This paragraph implies that what will be tested here is cloud feedbacks. Cloud feedbacks are, indeed, considered highly uncertain, but a recent paper (Dessler, *Science*, 2010) has shown that observations suggest that the feedbacks from clouds are more likely to be positive than negative. There should be some recognition of the more explicit analysis by Dessler. *Lines 124-150:* I do not find the argument for using only the tropics particularly convincing. I understand the desire to keep the sampling to a minimum and trying to maintain control of the forcing by seeking areas that are mostly covered by water. (By the way, I cannot find a definition of "tropics" in the text, **a glaring omission**. I will assume that it is the same as for LC09.) The argument discussed in the SI that the relative humidity is low in the extratropics and not the tropics is fine in an average sense, but, for two seasons of each year, the humidity in many parts of the extratropics can be quite high for long periods. A dry summer in one hemisphere or another would surely have some feedback repercussions. The same goes for the argument that cloud feedbacks are confined to the tropics. Droughts and extreme rainfall are quite common in the extratropics and they result from extremes in temperature, humidity, and clouds. All of those factors would contribute to the feedbacks. It seems that the authors could test their assumptions about using the tropics by analyzing the CERES and/or ERBS and the model data to at least 60° latitude. Yes, there is some sunlight at 60° all during winter, just not a whole lot. But it is balanced by the larger amount in the opposite hemisphere. They could do the same thing for the models. If it cannot be tested positively one way or another, without relying on broad, somewhat handwaving ("we believe"), arguments, then the idea that the authors are computing a global feedback parameter is based on a poorly understood assumption. *Line 172 & Figure 2:* Although, some of the endpoints were shifted appropriately since LC09, this plot suggests some residual cherry picking that was shown by Trenberth et al. (2010). There

	is a change exceeding 0.1°C in the 1985-86 period that was not used. Again, in 2008. The cooling in 1998 was dropped because there were no flux data? Why were the available flux data used in LC09 for this period, but not now? There should be an explanation. And, the data for the available months of 1998 should have been included for the relevant endpoints. What happens if the endpoints are changed by a month? *Lines 186-196:* Again the discussion above about using only the tropics is applicable. Is the simple model of LCH01 valid? *Lines 252-253*: The values at the same lag times should also be compared regardless of the peak correlations. Does it make any difference in the AMIP models whether there is a lag or not? Minor comments *Line 186:* “Quality” and “are” disagree. *Line 186:* Is there some evidence that CERES data are better in the tropics than elsewhere? If anything, they would be worse in terms of spatial sampling because there are small gaps between each orbit.