Referee #1:

1) Criticism by Pielke & Christy: Hansen et al. (2005) estimate of ocean heat storage (0.85 W/m**2) is different from the Willis et al. (2004) estimate (0.62 W/m**2).

Reviewer Response: Hansen et al. (2005) actually explained how they derived this estimate in their paper and explain this again in their response. The discrepancy is associated with warming that occurred at depths exceeding 750 m in the model (the Willis et al. estimates only extends down to 750 m). It is clear to me that the claim by Pielke & Christy is simply not valid and it appears to me that they did not carefully read the Hansen et al. (2005) paper. In addition, the Willis et al. and Levitus et al. (2005) estimates of ocean heat content for 1993-2003 are very similar.

2) Criticism by Pielke & Christy: Hansen et al. (2005) only examined the 1993-2003 decade which they claim is not representative.

Reviewer Response: In both their original paper and in their response to Pielke and Christy, Hansen et al. make it very clear why they chose the 1993-2003 decade. Reasons include: "Improved definitions of Earth's energy balance is possible for the past decade. First, the predicted energy imbalance due to increasing GHGs has grown to 0.85 W/m**2 +/-0.15 W/m**2, and the past decade has been uninterrupted by any large volcanic eruption. Second, more complete ocean temperature data are available, including more profiling floats and precise satellite altimetry that permits improved estimates in data-sparse regions." There is significant interdecadal variability of ocean heat content as shown by Levitus et al. (2005) which Hansen et al. (and other scientists) suggest may be related to sampling problems. I do not agree with this suggestion but Hansen et al. have been very open about this and readers can judge for themselves. This criticism by Pielke & Christy is not a very serious one, no one is "hiding" anything.

Also, Hansen et al. showed the net radiation at the top of the atmosphere for 1880-2003 (based on their model). Levitus et al. (2001; 2005) have shown that the ocean heat content dominated earth's radiation balance for the past 50 years.

3) Criticism by Pielke & Christy: Hansen et al.(2005) ignores radiative forcings (e.g., biogeochemical effects of increasing CO2) other than those used in the model simulation.

Reviewer Response: I am not aware of any estimates that any modeler can use for this forcing. Most models ignore some type of forcing and/or process. If this criticism were valid then no one would ever do science! This because there are always uncertainties, models are never complete, etc.

4) Criticism by Pielke & Christy: The value of radiative forcing used for the indirect aerosol effect is used to "fit" the model results to observations.

Reviewer Response: This is a serious accusation that does not appear to have any basis in fact. If Pielke and Christy want to be taken seriously on this accusation then, at a minimum, they need to clearly state what value of the forcing should be used and how they think Hansen et al. are using this estimate of the indirect aerosol to fit the model to the data.

5) Criticism by Pielke & Christy: Discrepancy between Willis et al. (2004) and Hansen et al. (2005) on current warming rate.

Reviewer Response: This discrepancy occurs because Willis et al. are referring to a substantial warming that occurs during the 1970s in the Levitus et al. (2005) estimate that Hansen et al (2005) suggest may be inaccurate due to lack of data. I already expressed my disagreement with Hansen in point 2 above and I note that Hansen has been "open" about this problem.

6) Criticism by Pielke & Christy: Hansen et al. (2005) statement of where ocean heat content is important differs from Willis et al. (2004).

Reviewer Response. This difference occurs because Willis et al. and Hansen et al. have simply emphasized different aspects of the changes on model and observed ocean heat content in their respective papers.

Referee #2:

The exchange is not worthy of publication. In fact, I do not understand why P&C even wrote their piece in the first place. They continually destroy whatever point they had in mind by noting Hansen 'did it right', e.g. deciding and stating the global radiative imbalance from Willis et al is what Hansen gets; by quoting the Willis et al description of distribution of ocean heating and calling it a disconnect…did they even look at Hansen's Fig 3? To this last point, Willis is a coauthor with Hansen (as P&C note). Willis would surely not be involved in a contradictory study. And it appears to me he was not.

None of the participants in this pathetic exchange seem to have the slightest clue about the large decadal noise that exists in the oceans and some ocean models. If they did they would not make the comments and calculations they do. A decadal of data and analysis leave no room, after natural variability in the ocean is considered, to make statements about global warming issues. Further, the sparse nature of the ocean observations makes statements about 'global ocean warming' highly unrelaibale. The interpolations done by Levitus have been shown to lead to potentially misleading conclusions. The use of the altimeter by Willis et al looks good but is likely to miss any baroclinic signals in the upper ocean that might impact the estimates of heat content in the upper 750m.

Suggest this exchange not be published in Science.