Response to Reviewer #1

We would like to thank the reviewer #1 for a very thorough review with thoughtful comments and suggestions. The manuscript is now revised following the reviewer's suggestions. Here, we briefly explain how we address each of the comment. The reviewer's comments are in italic font, and our replies are in normal font.

Reviewer #1 (Remarks to the Author):

Comments on "Pacific origin of the abrupt increase in Indian Ocean heat content during the recent global surface warming hiatus" by S-K. Lee et al.

Overall comments
The "hiatus" in global warming over the last decade is a topic that has, for good reasons, received a lot of attention over the last couple of years. There are now several studies that demonstrate in a convincing way that this hiatus in global warming arises from natural variability in the tropical Pacific, in a relatively cold state over the last decade. Several studies have also shown that the relatively cold Pacific increases the ocean heat uptake locally and remotely in other regions, explaining the reduced warming of the atmosphere over this period. This paper builds from this. The increased uptake in the Pacific should lead to an increased ocean heat content in this basin over the recent decade. The authors show that this is not the case, but that the Indian Ocean upper ocean heat content, previously very stable, has soared during the same time, due to an increased transport to the Indian Ocean through the throughflow. This paper addresses an important topic, brings new insights on this topic and is in general quite convincing. I hence believe that it should eventually be published in Nature Geoscience. However, I feel that several important issues must first be addressed before doing the paper can be accepted. This probably implies doing so partly in a supplementary information section while only summarizing the most important points in the main text.

1) Previous studies have claimed that a significant fraction of the "missing heat" accounting for the hiatus may have been stored in other ocean basins (the Atlantic, the Southern Ocean) and/or in the ocean below 700m. The authors need to discuss the consistency of their results *with previous studies in greater detail. Their model indeed also allows estimating the heat content changes below 700 m and in other basins. A couple of diagnostics and additional discussion in supplementary information is needed to better resituate the current results amongst other studies.*

Reply: We appreciate this thoughtful suggestion. We have added the time series of OHC_{700} and heat budget analysis for the Atlantic and Southern Oceans and discussed them in the revised Supplementary Information (S.I.2; Fig. S2). Overall, the control experiment reasonably well captured the Atlantic and Southern OHC_{700} changes since the 1970s (Figs. 2Sa and 2Sb). In the Atlantic Ocean, the surface heat uptake increased considerably during 2003-2012 in comparison to the earlier period of 1971-2000. However, the OHC_{700} did not increase much (Fig. S2c). Instead, a large portion of the anomalous surface heat uptake was transported to the deeper ocean below 700 m ($Q_{\mathrm{ADV_V}} < 0$), consistent with previous studies (Chen and Tung, 2014; Drijfhout et al. 2014). In the Southern Ocean, the OHC_{700} did increase during 2003-2012 with a slightly higher rate than that during 1971-2000 (Figs. S2b and S2d). However, there was no significant transport of heat to the deeper ocean disagreeing with previous studies (Chen and Tung, 2014; Drijfhout et al. 2014).

For the heat content changes in the deeper ocean (i.e., 700 ~ 2000m), NODC do not provide the heat content product prior to 2005 because no reliable in-situ data is available before the Argo observations, whose spatial coverage over the global ocean reached a mature state only around 2005. NODC provides a 5-year averaged heat content product available for the pre-Argo period. However, regional inhomogeneity of the deep-water observations during the pre-Argo period is too large to use this product for basin-scale heat budget studies such as this study. Therefore, we do not think that we have reliable deep-water (700 ~ 2000m) observations to explore the heat content changes below 700 m for the study periods (1971-2012). Surely, the ensemble model simulations can provide those values. However, we do not wish to discuss pure model results, which cannot be validated. We may explore this issue in the future focusing on the Argo period (2005-present).

2) The results currently focus on the change in ITF transport and temperature (figure 2), but the change in Indian Ocean heat content is actually a balance between this input flow and the outflow at 34{degree sign}S. The outflow transport is arguably very close to the inflow *transport at decadal scales, but a more detailed quantification of the relative roles of transport changes and inflow-outflow temperature differences (with the contribution of each singled out) would actually strengthen the paper.*

Reply: We have added tables in Supplementary Information to show all heat budget terms for the Indian and Pacific Oceans (Tables S3 and S4), and discussed the relative role of heat transport to the Southern Ocean across 34°S on the recent Indian Ocean heat change (see S.I.5). However, since its contribution is much smaller (~ -0.07 PW) than the inter-ocean heat transport via the ITF (0.19W), we do not find that it is necessary to separate this into temperature change-driven and volume transport change-driven terms.

3) the methods section of the paper is not sufficiently documented to understand exactly how the various experiments were performed, how the heat budgets were evaluated. This information needs to be strengthened, even if this is partially done in a supplementary information section.

Reply: We think that the method section already provides adequate information to the readers about the spin-up, reference, control and three sensitivity experiments. Nevertheless, we agree that the surface flux dataset used to force these experiments can be better explained in detail. Thus, we have added a section in Supplementary Information (S.I.3) to explain in detail how the bias-corrected 20CR surface flux fields were generated. Additionally, in S.I.4, we discuss the spin-up run in detail, and in S.I.5, we explain in detail how the heat budget values shown Figure 1 were computed.

Detailed comments

L25: for some readers, it may be useful to define here that this stalled surface warming since the beginning of the century is commonly referred to as the "hiatus".

Reply: We added "(commonly referred to as the "hiatus")".

L36: Meehl et al. (2011) and references therein argue that a non-negligible portion of the "missing heat" is stored below 700 m. Drijfhout et al. (2014) argue that the heat uptake has also recently increased across several other oceanic basins (cf your text at L49-51). There *are hence other studies that argue that a non negligible part of the "missing heat" is stored in other basins and/or below 700m. Although not here at the beginning of the paper, this will have to be discussed somewhere (maybe as part of supplementary material ?).*

Reply: Please refer to our reply to your major comment (1).

L45: Is the reference right? Actually, I would argue that for the eastern Pacific, the reduction in latent heat flux should contribute as well. The change in shortwave is less straightforward because of low clouds in the eastern Pacific (in observations, colder SSTs often enhance low clouds and reduce shortwave, an effect often badly reproduced by models as shown by e.g. Guilyardi et al. J. Clim. 2009).

Reply: As suggested, we revised this sentence to point out that cold SSTs are also linked to the reduction in latent heat flux to the atmosphere. Csanady (1984) stressed that the cold SST in the tropical Pacific and Atlantic Oceans maintained by the massive equatorial upwelling is the key to the intense surface heat uptake by the tropical oceans. So, we do think that Csanady (1984) is a right reference for this sentence.

L52-54: also cf my comment about L36. It would be useful to show the contribution from other ocean basins (i.e. Atlantic and Southern Ocean) on figure 1 to support those statements (either in the main text or supplementary information).

Reply: Please refer to our reply to your major comment (1).

L61: You did not compare the upper 700m oceanic heat content to the atmospheric heat content rise expected from the unbalance at the top of the atmosphere, so you can't conclude that. A significant portion may for example lie below 700 m. You cannot check this in the observations but you can in the model.

Reply: We appreciate this thoughtful comment. Yes, we can check this using our model simulations combined with satellite observations. Available satellite data suggested that the net radiation imbalance at the top of the atmosphere was about 0.47 Wm^{-2} during 1985-2012 (Allan et al., 2014). Integrating this value for the globe, the total energy imbalance of 0.24

PW is obtained. According to the ensemble model simulations used in this study, the net heat flux into the global ocean increased from 0.15 PW during 1971-2000 to 0.23 PW during 2003-2012 (Fig. 1b and Table S2). This suggests that the net heat flux into the atmosphere (i.e., net radiation imbalance at the top of the atmosphere minus net heat flux into the global ocean) decreased from ~0.09 PW during 1971-2000 to ~0.01 PW during 2003-2012, assuming that the net radiation imbalance at the top of the atmosphere (0.47 Wm^{-2} or 0.24 PW) remained roughly the same during the two periods. The OHC_{700} in the Indian Ocean increased at a rate of about 0.07 PW during 2003-2012 (Fig. 1d and Table S2). These suggest that a significant portion of the heat missing from the atmosphere ("~ 0.07 PW × period of the hiatus" in comparison to "~0.08 PW × period of the hiatus") now resides in the upper 700m of the Indian Ocean. Therefore, we think that the sentence in question "This suggests that a significant portion of the heat missing from the atmosphere now resides in the upper 700m of the Indian Ocean, with little explanation" is not wrong. Therefore, we would like to keep this sentence in the first paragraph (Lines 36-39) and also in the main text.

L78: Some description on how the heat budget is evaluated is needed in the method section. You also need to define the regions used for the "Pacific", "Indian", etc... in figure 1 (where do you stop in latitude / longitude, etc...). This is important for interpreting you results. You may again want to show this as a map in supplementary information.

Reply: Fig. S1 is added in Supplementary Information to show the lateral boundaries of the Pacific, Atlantic, Indian and Southern Oceans. Additionally, in S.I.5, we explain in detail how the heat budget terms shown Figure 1 were computed.

L91: Another thing that can be remarked is the slight negative contribution of Qadvv, implying that some of the heat is pumped down below 700m.

Reply: We have added tables in Supplementary Information to show all heat budget terms for the Indian and Pacific Oceans (Table S3 and S4) including Q_{ADV_v}. However, since the contribution of Q_{ADV_v} is much smaller (~ -0.04 PW) than the inter-ocean heat transport via the ITF (0.19W), we do not stress this term.

L92-95: It would be nice to make the part of Qavdh associated with the Indonesian *throughflow visible on figure 1d. Also see my later comments on figure 2: I think that the volume transport at the ITF and 34{degree sign}S as well as the transport weighted temperature and slightly more detailed analysis of what drives the change in Indian Ocean OHC700 will be needed in figure 2 / supplementary information.*

Reply: As suggested, we added blue solid circles in Figure 1 to show the inter-ocean heat transport via the ITF. We also added tables in Supplementary Information to show all heat budget terms for the Indian and Pacific Oceans (Table S3 and S4), and discussed the relative role of heat transport to the Southern Ocean across 34°S on the recent Indian Ocean heat change (see S.I.5).

L97: Figure 1f shows that this heat flux increase into the ocean results from downward longwave. The Pacific cooling should contribute to decrease the upward latent and longwave heat fluxes relative to the 71-00 warmer period, which is apparently not the case from your budget. Can you explain this?

Reply: The reviewer correctly points out that both Q_{LUP} (upward longwave radiation) and Q_{LHF} (latent heat flux) over the Pacific increased slightly more during 2003-2012 than during the earlier period of 1971-2000. This may appear to be inconsistent with the La Niña-like condition in the Pacific during the 2000s. However, we would like to draw your attention to Q_{LDN} (downward long wave radiation), which was greatly increased during 2003-2012 at a rate of more than 3 times that during 1971-2000. Therefore, if we follow the global mean trend that Q_{LUP} and Q_{LHF} should increase roughly in proportion to the increase in Q_{LDN} (See Fig. 1b), the increases in Q_{LUP} and Q_{LHF} over the Pacific during 2003-2012 are relatively small compared to the large increase in Q_{LDN} during that period, thanks to the La Niña-like condition that reduced the Q_{LUP} and Q_{LHF}.

L102-109: I feel that figure 2 and its discussion need more details (maybe with some as supplementary material). Ideally, one would like to understand in more detail how the Qadvh bar of figure 1d is controlled by the throughflow transport. I guess that, to first order, the ITF transport is roughly balanced by the outflow at 34{degree sign}S (it would be nice to show that), so that the OHC700 within the Indian Ocean is controlled by the intensity of (both) transports and the difference in weighted temperature between the ITF and 34{degree sign}S *outflow. I feel that a slightly more quantitative analysis of the relative importance of transport variations and in/out flow weighted temperature variations in controlling the Indian Ocean OHC700 (maybe partly as supplementary information) would be useful. The IO warming stems from both increased volume transport and increased inflow temperatures (cf figure 2c, both being the result of the deeper thermocline in the western Pacific seen on figure 3) but you can quantify this more precisely in the model. But figure 3 also suggests warmer outflow temperatures at 34{degree sign}S, which may partially compensate that. A more detailed quantitative analysis of that is needed. Please summarize the essential results in the main text and provide a bit more discussion and figures as supplementary information.*

Reply: Please refer to our reply to your major comment (2).

L115-117: I would move that sentence to the beginning of the paragraph.

Reply: This paragraph is now moved to the beginning of the paragraph.

L129: It would help to also show the zonally averaged curl on figure 3.

Reply: We think that it is very straightforward to infer the sign of the wind stress curl from the simple shapes of the zonally averaged zonal wind stresses shown in Figure 3b. Therefore, the zonally averaged wind stress curl is not included in Figure 3b.

L140-143: It would be helpful to have the values from the reference experiment as well on figure S1 and to have a slightly expanded discussion of those experiments in the SI. Actually, those experiments suggest that PAC winds are responsible for the bulk of the OHC changes in the IO and that IND winds, whereas non-negligible, have a much smaller effect.

Reply: The heat budget values from the control and reference experiments are shown in Table S2, S3, S4, S5 and S6 in Supplementary Information (S.I.5). We have also added a section in Supplementary Information (S.I.6) to further discuss the results from the three sensitivity experiments. In that discussion, it is pointed out that the inter-ocean heat transport via the ITF was increased more in EXP_PAC than in EXP_IND. Note that Figure S1 is now moved to the main manuscript (now Fig. 4).

L188: Not very clear. I went to ref 26 and did not find the exaplanations clearer. Does this mean that every year of this spinup forcing is randomly selected amongst 20CR years? If so, this will create a shock due to the brutal change in forcing at every year change. So I imagine that this spinup actually uses climatological forcing + a random year of high frequency forcing, is that right? Do I misunderstand something here?

Reply: We added S.I.4 to discuss the spin-up run in detail. The reviewer is correct that every year of the spinup forcing is randomly selected amongst 20CR years between 1948-1979 as discussed in the method section. It is a common practice to spin up an ocean model with a seasonally varying climatological surface flux data set. However, recent studies have suggested (and also from our own experience) that weather noise (linked to for example winter storms) and interannual-frequency surface forcing (linked to the North Atlantic Oscillation for example) are also important in shaping the mean state of the global ocean (Kirtman et al., 2012). Our spin-up method with randomly selected forcing years, which was first used in Lee et al. (2011; ref 26), conserves the total variance of the surface flux fields without introducing spurious long-term variability. Therefore, we think that our method is an effective way to spinup any ocean model (either global or regional) or ocean-sea ice coupled models. We have not found any evidence that the discontinuity at the beginning of calendar years (due to interannual variability) causes any adverse effect during the spin-up run.

Reference:
Kirtman, B.P., Bitz, C., Bryan, F., Collins, W., Dennis, J., Hearn, N., Kinter III, J.L., Loft, R., Rousset, C., Siqueira, L., Stan, C., Tomas, R., Vertenstein, M., 2012. Impact of ocean model resolution on CCSM climate simulations. Clim. Dyn. 39, 1303-1328.

L193-196: this heat flux correction methodology is insufficiently described. Please provide more details, potentially in a supplementary information section.

Reply: We have added a section in Supplementary Information (S.I.3) to explain in detail how the bias-corrected 20CR surface flux fields were generated, including the heat flux correction methodology.