Dr. Ben Kirtman
Executive Editor
Climate Dynamics

Dear Dr. Kirtman,

We thank the three reviewers for their helpful comments and criticisms on our manuscript. Based on their comments and instructions on the model design, we have completely resigned our model experiments and revised the manuscript accordingly. We also tried our best to address all other comments from the three reviewers in the revised manuscript. Our response to the reviewer's specific comments is addressed below.

Sincerely,

Sang-Ki Lee, David Enfield and Chunzai Wang

Reviewer #1:

We thank reviewer #1 for helpful comments and for recommending publication with minor revision. Based on criticisms and comments from reviewer #2 and #3, we have completely resigned our model experiments and revised the manuscript accordingly. Our response to the reviewer's specific comments is addressed below.

The authors note an increasing trend in vertical wind shear (VWS) in the main development region (MDR) of Atlantic hurricanes in the CMIP3 ensemble A1B simulations, and conduct CAM3 experiments to show that the VWS increase is remotely forced by SST warming outside the North Atlantic. This is one of the few AMIP-type papers to isolate the cause of particular aspects of global warming. The result supports the relative SST warming argument advanced by Gabe Vecchi and his colleagues. The paper may be published as a note in Climate Dynamics after minor revision.

Major comments

1. *Fig. 6. The text and caption describes it as 200-850 hPa thickness change but I suspect that the authors have subtracted the zonal mean. To first order, tropospheric temperature displays a spatially uniform warming (e.g., Xie et al. 2010), and spatial variations are generally one order of magnitude smaller. Please clarify.*

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In the figure caption for figure 5 (old figure 6), we now have the following sentences: "Dynamic responses of the atmosphere to AGW are most prominent over high-latitudes around 50 ~ 60^{o}N with a significant amplitude in zonally averaged components (not shown). Since the main interest is tropical atmospheric dynamics around the MDR, the zonal mean components of geopotential thickness difference are removed. Note that the zonal means are not removed in VWS difference."

2. *Fig. 7 and discussion in Discussion. A1B temporal variability used to develop the regression is dominated by the trend (Fig. 1) so the degree of freedom is just one. Thus, the regression is simply meaningless, neither supporting or disproving the authors' argument. One can develop similar regressions using SST in either the MDR or the tropical Pacific, and obtains the same rising trend in VWS. I suggest deleting Fig. 7. This is just one example that some of the arguments and interpretation can be developed more carefully.*

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The regression curve is an important part of this paper because the sign and amplitude regression coefficients support our major conclusion. As discussed in our revised manuscript (section 4), the least squares equation for MDR VWS is given by MDR VWS = -2.7×MDR SST + 3.0×Tropical Indo Pacific SST. This equation confirms our main conclusions that a uniform warming of the MDR SST and Tropical Indo Pacific SST has little impact on the MDR VWS, and that the inter-basin SST difference is the most important indicator and predictor of Atlantic hurricane activity for both the 20th and 21st centuries. Therefore, we decided to keep this figure (revised figure 7) in the revised manuscript.

Minor comments.

Abstract. 1. Typo: "project" should be "projections" in Line 2. 2. "consistent with.Pacific Walker circulation" (Lines 4-5, and in the main text): the causality is rather unclear. Only later, I realized that the weakened Pacific Walker circulation is used to argue for stronger warming over the Pacific. Please reword. 3. Line 9: "accelerates" is not the right word.

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Corrected.

Lines 16-17, page 3. The cooling effect of the weakened AMOC needs references. A recent paper analyzing a multi-model ensemble of water-hosing experiments illustrates this point well (Timmermann et al. 2007, JC).

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Now, we add the following references:

Zhang R, and Delworth TL (2005) Simulated tropical response to a substantial weakening of the Atlantic thermohaline circulation. J Clim 18:1853-1860

Timmermann A, Okumura Y, An SI, Clement A, Dong B, Guilyardi E, Hu A, Jungclaus JH, Renold M, Stocker TF, Stouffer RJ, Sutton R, Xie S-P, and Yin J (2007) The influence of a weakening of the Atlantic meridional overturning circulation on ENSO. J Clim 20:4899–4919

Zhang R (2007) Anticorrelated multidecadal variations between surface and subsurface tropical North Atlantic. Geophys Res Lett 34:L12713. doi:10.1029/2007GL030225

Chiang JCH, Cheng W Bitz CM (2008) Fast teleconnections to the tropical Atlantic sector from Atlantic thermohaline adjustment. Geophys Res Lett 35:L07704. doi:10.1029/2008GL033292

Line 8, page 8. "warming of tropical Pacific" in Fig. 6 is a cooling (see major comment 1). The importance of relative SST warming for tropical convection is presented in Xie et al. (2010).

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This part is changed to “Therefore, in this sense, the suppressed warming of the TNA increases the moist static stability and decreases the convection aloft…”. We also add the following sentence: “Note that the physical rationale provided here is consistent Xie et al. (2001) who showed the importance of regional differences in SST warming for tropical convection.”

Lines 1-2, page 10. "multidecadal signal.by fluctuations in aerosols": is there any evidence for this claim?

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Knight (2009) explored this subject extensively. He studied the Atlantic Multidecadal Oscillation (AMO) in the IPCC-AR4 climate model simulations and compared it with the observed AMO of the 20th century. He showed that internally generated multidecadal signals are all canceled out in the ensemble average of the IPCC-AR4 models. He also studied the causes for small amplitude of the remaining multidecadal signals in the IPCC-AR4 ensemble mean. He was able to pinpoint that, for example, a dip in 1880-1889 is linked to the eruption of Krakatau in 1883 and another dip in 1960-1969 is a response to the Mount Agung eruption in 1963. In the revised manuscript, Knight (2009) is cited.

Last paragraph, page 10. What shapes patterns of tropical SST warming is an important question. I suggest that the authors take a look into a new paper in press in J. Climate by Xie et al. (2010) addressing this problem. Besides AMOC and the weakened Walker circulation, there are other important factors such as the damping rate set by mean evaporation. The contrast between the strong warming in the equatorial Pacific and reduced warming in the off-equatorial MDR is most likely due to the large difference in mean evaporation and thermal damping.

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Xie et al. (2010) and their proposed mechanism are now discussed in section 4 as one potential explanation for the suppressed warming of the TNA.

Reviewer #2

We thank reviewer #2 for his/her comments and criticisms on our manuscript. Based on the comments and instructions on the model design, we have completely resigned our model experiments and revised the manuscript accordingly. We also tried our best to address all other comments from reviewer #2 in the revised manuscript. Our response to the reviewer’s specific comments is addressed below.

This paper presents results from a couple of GCM simulations aimed at explaining the increase in wind shear (and thus, presumably, decrease in hurricane activity) under global warming that is projected in IPCC AR4. I think the basic conclusions are probably correct, the arguments are reasonable, and worse papers are certainly published all the time, but this one has enough weaknesses that I do not support publication in Climate Dynamics.

The first weakness is that the ideas here are not original. Much of the discussion simply echoes that of Vecchi and Soden (2007a,b) who reached the same conclusions using the AR4 model archive data and gave essentially the same arguments. That would be ok if the authors did something new to make those arguments and conclusions stronger or clearer. They just don't quite achieve that.

What is new here is the use of targeted atmospheric model experiments in which only part of the global SST changes from the climate projections are used to force the model. The idea here is to make the attribution to relative SST stronger. The problem is that this is not done in the most careful or intelligent way, so that while the answer may basically be correct, it is not convincing.

The problem is that in these experiments, the SST in one region is set to the value from the warm climate while that in the other region is set to the value in the cooler control climate. What should be captured in these experiments, in order to make the case the authors are trying to make, is the changes in relative SST that occur as the climate warms due to changes in the spatial structure of the SST field. What is actually captured is not that, but rather the much larger changes associated with a scenario in which global warming itself occurs in only part of the domain.

To put it another way, imagine that the SST change under global warming were spatially uniform, so that relative SST did not change at all. Shear might change, or not. But let's imagine that we then did the perturbation experiments in this paper; there would be large relative SST changes imposed in those perturbation experiments, but attributing any behavior in the global warming (with uniform SST) case to those relative SST changes would be silly. Sure, if everything were linear then the impacts of those relative SST changes would cancel, but that would be a very strange way to decompose the impact of a uniform SST change.

What the authors really should do is perform perturbation experiments in which the SST field in one region (say, the Pacific) is set to the value from the warm climate from the AR4 projections, while the other region (say, the Atlantic) is set not to the value from the cooler 20th century control climate, but rather to that plus a global warming perturbation which could be estimated, for example, as the tropical or perhaps global mean SST change from the control to the warm climate. The idea is that one would turn on and off the change in the Pacific-Atlantic SST difference, rather than a much larger (and perhaps also different in spatial structure) difference that is artificially constructed by taking the warm climate value from one ocean basin and the cool climate value from the other.

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To accommodate the reviewer's criticism about the model experiment design (reviewer #3 provided similar criticism), we completely redesigned our model experiments following the instruction here. As described in section 2, now we perform four model experiments. The global ocean warming experiment (EXP_GLBW) and control experiment (EXP_CTRL) are the same as before.

The warmer TNA experiment (EXP_WTNA) is designed to understand the effect of suppressed warming in the TNA. As shown in Figure 3b, the suppressed warming region of the TNA is identified as the North Atlantic region (between the equator and 40^{o}N) of its SST difference in EXP_GLBW - EXP_CTRL less than 1.75^{o}C. SSTs in the identified region of suppressed warming are increased in such a way that the MDR SST warming is equal to the EQP SST warming of 2.20^{o}C.

The cooler EQP experiment (EXP_CEQP) is designed to understand the effect of the preferential warming in the EQA. As shown in Figure 3c, SSTs in the preferential warming region of the tropical Pacific (150^{o}E-eastern coast of South America and 10^{o}S-10^{o}N), where the SST difference of EXP_GLBW - EXP_CTRL is greater than 1.95^{o}C, are decreased in such a way that the EQP SST warming is equal to the MDR SST warming of 1.64^{o}C.

EXP_WTNA - EXP_GLBW represents a warmer minus cooler TNA, while EXP_CEQP - EXP_GLBW represents a cooler minus warmer EQP. In the case of EXP_GLBW - EXP_CTRL, many forcing factors are represented including (1) global ocean warming, (2) increased greenhouse gas, (3) suppressed warming of the TNA in reference to the tropical IndoPacific warming, and (4) preferential warming of the EQP in reference to warming in the TNA and other tropical oceans. Based on the newly designed model experiments, we now conclude that (4) the preferential warming of the EQP has little impact on the MDR VWS in the IPCC-AR4 climate model simulations, and that (3) the suppressed warming of the TNA in reference to the tropical IndoPacific is the key indicator and predictor for Atlantic hurricane activity in the 21st century.

The main contribution of this paper is to provide a consistent physical rationale to Vecchi and Soden (2007a, b) and others, who suggested that Atlantic hurricanes response to the SST difference between the MDR and the other tropical ocean basins.

Reviewer #3

We thank reviewer #3 for his/her comments and criticisms on our manuscript. Based on the comments on the model design, we have completely resigned our model experiments and revised the manuscript accordingly. We also tried our best to address all other comments from reviewer #3 in the revised manuscript. Our response to the reviewer's specific comments is addressed below.

The authors investigate the controlling influence on Atlantic hurricane activity -- a very active and important research area. There is growing recognition that hurricane activity is influenced by more than local (main development region) SSTs (and aerosols). The Pacific basin is increasingly recognized as being important through its impact on the vertical wind shear in the tropical North Atlantic.

The authors reach overly broad conclusions about hurricane activity in the 21st century based on models that remain deficient in some key respects (e.g., representation of SST trends); the title and conclusions are not sufficiently nuanced to convey the latent uncertainties. I believe there is an obligation to assess the models first before using their projections to infer changes in hurricane activity. Given that MDR SSTs and VWS are influential variables, I, for one, would have first examined their distribution in the 20C3M model ensemble-mean and observations to develop confidence.

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We would like to point out that the observed MDR SST and VWS in the 20th century are largely dominated by the Atlantic Multidecadal Oscillation signal (Wang et al. 2008). However, as discussed in Knight (2009), internally generated multidecadal signals are all canceled out in the ensemble average of the IPCC-AR4 models. Therefore, the ensemble averaged MDR SST and VWS of the IPCC-AR4 models are inconsistent with observed MDR SST and VWS (Knight et al. 2009). Nevertheless, using the NCEP reanalysis data for a relatively short period of 1949-2006, Wang and Lee (2008) reported a positive trend of the MDR VWS during that period, even after removing the multidecadal signal. These points are now discussed in section 1 and section 4.

In section 4, we also attempt to explore if the suppressed warming of the TNA in the IPCC-AR4 climate model simulations is detectable from observed SST records of the 20th century, using HadISST and ERSST3. Unfortunately, we find that it is virtually impossible to cleanly separate the secular trend of observed MDR SST because the multidecadal signal of the AMO dominates over the secular trend. This point is discussed in section 4.

Wang, C. and S.-K. Lee, 2008. Global Warming and United States Landfalling Hurricanes. Geophysical Research Letters, Vol. 35, No. L02708, doi:10.1029/2007GL032396.

Wang, C., S.-K. Lee and D. B. Enfield, 2008. Atlantic Warm Pool Acting as a Link between Atlantic Multidecadal Oscillation and Atlantic Tropical Cyclone Activity. Geochem. Geophys. Geosyst., 9, Q05V03, doi:10.1029/2007GC001809. (In the special issue of "Interactions between climate and tropical cyclones on all timescales")

**	Pg 2. Para 2: The MDR VWS is "probably the most critical environmental factor for hurricane intensification." A supporting reference or brief discussion of the relative contribution of MDR SSTs and VWS in context of 20th century observations would be helpful in putting things into perspective.*

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Emanuel (1994) is now cited.

**	Fig. 1: Can the observed distribution be also plotted in the same panels (using a right scale) for the sub-periods for which data is available? This will allow at the very least for comparison of trends, etc.*

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Wang and Lee (2008) reported a positive trend in the observed MDR VWS during the period of 1949-2006. This paper is now cited in section 1. Additionally, we now add some discussions in section 4 on the observed secular trend in the MDR SST and tropical IndoPacific SST using HadISST and ERSST3.

* *SST -trends (Fig. 2 and Pg. 3, para 2): The authors make much of the fact that the IPCC-AR4 ensemble-mean SSTs exhibit smaller warming trend in TNA than in the tropical Pacific. Their conclusion regarding decreased hurricane activity, in fact, rests squarely on this feature. But how confident are we about the representation of this feature in IPCC projections? Although it is a difficult question, it needs to be tackled if the paper is to be taken seriously. One could for instance compare the observed 20th Century trends with those in the 20C3M ensemble-mean. As best as I recall, the tropical central-eastern Pacific has a cooling-trend in the 20th Century as opposed to a weak warming one in the MDR region (Guan and Nigam 2008); Karnauskas et al. 2009 also find the zonal SST-gradient to strengthen in the 20th Century, i.e., an absence of warming trend in the central-eastern tropical Pacific. The SST-cooling in this region appears consistent with the "ocean dynamical thermostat" Clement et al. (1996). The observed SST-trends are thus in some contrast to the IPCC ones (Fig. 2), although the latter are for a 2-century long period. Some discussion of this issue is necessary in the interest of analysis credibility.*
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Regional warming trend in the 20th century is indeed a highly debated issue. We discuss in section 4 about the difficulties in cleanly separating the secular trend of observed MDR SST from the multidecadal signal of the AMO.

The main point of this paper is to show that suppressed warming of the TNA in reference to the tropical IndoPacific causes a secular increase in the MDR VWS. Therefore, we think that it is a realm of the future studies to explore why the IPCC-AR4 climate model simulations project a suppressed warming in the TNA or how reliable that projection is. We provide two potential explanations. The first explanation is the weakening Atlantic thermohaline circulation, which is consistent with the suppressed warming of the TNA. The second explanation is a greater evaporative cooling response in the TNA suggested by Leloup and Clement (2009) and Xie et al. (2010). We now discuss this point in section 4.

* *Experimental design: The EXP_LOW and EXP_ROW integrations have elevated CO2 but control-period SSTs in the Pacific and Atlantic basins, respectively. This must lead to radiational imbalance at the surface as well in the atmospheric column; the latter is concerning since the column will cool down in response to reduced upward long wave (and sensible and latent) heat flux, with stabilization in the Tropics (on account of compensatory sinking). I am afraid I am not comfortable with this experiment design, especially since Figs. 4b and 4c don't add up to 5b, even after leaving some room for nonlinearity. I suspect Figs. 4b and c are showing not just the basin SST effects but also the large-scale dynamical effects of tropospheric sinking (induced by radiational cooling of the column over the OTHER basin) which, in turn, is generated from the discord between CTL SSTs and the overlying CO2-rich troposphere there. This additional effect confounds subsequent analysis. The sinking is, of course, not able to fully offset*

radiational cooling, as indicated by the lower thickness over much of the Pacific in 6b, and over much of the Atlantic in 6c.

** The mirror image response over the Tropical-subtropical basin is thus guaranteed in Figs. 6b-c and Figs. 7b-c. For instance, in the LOW experiment, tropical Atlantic will have more convection and rainfall because of warmer SSTs. In the ROW case, tropical Atlantic will be under sinking motions because of the disconnect between CTL SSTs and the overlying CO2 rich atmosphere. Thus convection is suppressed and you see a mirror image in rainfall and circulation; NOT surprising.*

In view of the above experimental design, it is impossible to ascribe the VWS signal over TNA in the IPCC-AR4 runs to one or the other basin, since both are activated -- one inadvertently. I am afraid the authors will have to devise a more suitable experimental strategy, and resubmit their analysis.
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To accommodate the reviewer's criticism about the model experiment design (reviewer #2 provided similar criticism), we completely redesigned our model experiments. As described in section 2, now we perform four model experiments. The global ocean warming experiment (EXP_GLBW) and control experiment (EXP_CTRL) are the same as before.

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