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Comments on "Will greenhouse gas-induced warming over the next 50 years lead to higher frequency and greater intensity of hurricanes?"

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The question of anthropogenically-forced climate changes is an especially important topic with regard to possible alterations in extreme events: floods, droughts, heat waves, cold snaps and, perhaps most devastating, tropical cyclones. Bengtsson et al. (1996) (hereafter referred to as BBE) address this issue with a combination of general circulation model (GCM) runs in a doubled carbon dioxide (CO_2) mode and a control mode. However, there are two items that must be questioned, one being the validity of the downscaling technique utilized and the other being BBE's interpretation of some of the results.

The primary concern is the strong possibility of an incompatible flaw in the experimental design. BBE utilized a two-stage "downscaling" approach in their modeling work. First a low (T21) resolution coupled ocean–atmosphere GCM (Cubasch et al., 1992) was run out for a 100-year integration with an approximate 1% annual increase in CO_2. At the time of doubling of CO_2 around 60 years of integration, the resulting warmer values of sea surface temperature (SST) as well as the increased CO_2 values were applied to a much higher resolution (T106) version of the same atmospheric model for a 5-year run. For a control run, the same T106 version of the atmospheric model was also run for 5 years with climatological values of the SSTs and CO_2. As shown in Bengtsson et al. (1995), the tropical cyclone–like vortices that result in both the control and the doubled CO_2 runs do show moderately realistic resemblance to observed tropical cyclones, though the modeled storms' radius of maximum winds is nearly an order of magnitude larger than what is commonly observed (Weatherford and Gray, 1988).

The main finding in BBE is that there were substantially fewer tropical cyclones globally in the doubled CO_2 than in the control run of undisturbed CO_2 conditions. BBE attribute this startling decrease in storm numbers to a weaker intertropical convergence zone (ITCZ) and hydrological cycle with an attendant decrease in synoptic scale vorticity, low–level convergence and moisture flux and an increase in vertical wind shear — all of which should lead to a reduction in tropical cyclones.

However, for the 60 years of integration of the enhanced CO_2 low resolution coupled ocean–atmosphere GCM, there was not a decrease in the ITCZ strength and hydrological cycle, but instead a strengthening of these features (Cubasch et al., 1992) that accompanied a global warming that was on the order of 1–2°C. Indeed, a key feature for nearly all GCM simulations of the climate under an enhanced CO_2 is that increases of tropospheric water vapor along with a stronger ITCZ and hydrological cycle are *required* for any substantial global warming (Houghton et al., 1996). Thus, the results of BBE show an extremely inconsistent change when downscaling from the coarse resolution coupled GCM to the fine resolution atmospheric GCM. It seems likely that the process of downscaling from low to high resolution modeling has caused a change in the feedback processes, possibly because of changes in the

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response of the various parameterizations (particularly cumulus parameterization) to resolution changes. Because of the strong sensitivity of global warming to alterations of the ITCZ and the hydrological cycle, it is quite likely that there would have been a substantial difference in the doubled CO_2 SST field and the storm frequency if a coupled high resolution GCM had been utilized for the entire 60-year integration, instead of just the 5-year run. Do the authors feel that this reversal in the ITCZ and hydrological cycle response in the low versus high resolution GCM invalidates the results obtained?

The second comment regards the analysis of the most intense storm (which reached maximum sustained surface winds of 56.7 m s^{-1}) in the doubled CO_2 run versus the strongest storm (which reached 53.1 m s^{-1}) in the control run. BBE interprets this result to be that "given maximum favourable conditions, more powerful storms may develop" in a doubled CO_2 climate, agreeing with Emanuel's (1987) general findings that these storms will have the potential to reach more intense states. BBE insightfully noted that changes in the strongest tropical cyclones may be quite different than the changes in the mean of all of the tropical cyclones. Indeed, only a few percent of all tropical cyclones reach close to their thermodynamic potential (Merrill, 1988; DeMaria and Kaplan, 1994). However, making a broad brushed generalization regarding changes to tropical cyclones in a doubled CO_2 climate based on a sample of one out of 262 GCM–generated tropical cyclones that developed in the doubled CO_2 run would appear to be without much substantiation.

What can be done to either support or refute BBE's assertion is to examine, not just the singular strongest storm, but also the top 1, 5 and 10% of storms. It is reasonable to conclude that if the potential for stronger tropical cyclones increases, that this would affect the top few percent of storms, not just the strongest one storm. Examination of BBE's data demonstrates that the strongest 1, 5 and 10% of storms in the doubled CO_2 run were not significantly (95% confidence limit) stronger than the corresponding ones in the control run. In fact, the top 5 and 10% of storms in the control run had a larger mean than in the doubled CO_2 run, though it will not argued that this is significant. Clearly, when a non–negligible sample of cases is chosen, the suggestion of an increased intensity in the strongest tropical cyclones in their enhanced CO_2 GCM run does not appear to hold up. Do the authors agree that this new analysis with a larger sample supports the idea that the modeling runs actually suggest *no significant change* in the strongest storms between today's climate and a doubled CO_2 one?

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