Reply

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ABSTRACT

A response is made to the comments of Michaels et al. concerning a recent study by the authors. Even after considering Michaels et al.'s comments, the authors stand behind the conclusions of the original study. In contrast to Michaels et al., who exclusively emphasize uncertainties that lead to smaller future changes, uncertainties are noted that could lead to either smaller or larger changes in future intensities of hurricanes than those summarized in the original study, with accompanying smaller or larger societal impacts.

1. Introduction

Michaels et al. (2005, hereafter MKL) recall the question of Ellsaesser: "Should we trust models or observations?" In reply we note that if we had observations of the future, we obviously would trust them more than models, but unfortunately observations of the future are not available at this time.

In this commentary, we respond to the comments of MKL, who are critical of the model and interpretation of the radiative forcing scenario used in our recent paper (Knutson and Tuleya 2004, hereafter KT04) investigating the potential impact of future climate warming on hurricane intensities.

2. Global radiative forcing scenario

MKL's assessment of the $+1\%\ yr^{-1}$ CO_2 scenario as an "unrealistically large carbon dioxide growth rate" is accurate if applied narrowly to the issue of CO_2 concentrations alone. However, in KT04 the CO_2 increase provides a surrogate for global radiative forcing from a number of sources that MKL fail to consider. When a more complete set of radiative forcing is considered, as in the set of six scenarios from the Intergovernmental Panel on Climate Change (IPCC) Special Report on Emissions Scenarios (SRES) (Houghton et al. 2001), the resulting total radiative forcing by 2100 spans a range lying both above and below the $+1\%\ yr^{-1}$ CO_2 scenario in terms of global radiative forcing,[1] as detailed below.

The $+1\%\ yr^{-1}$ CO_2 scenario used in KT04 yields CO_2 levels by 2100 that are higher than any of the six main IPCC SRES marker scenarios. However, in Houghton et al. (2001), inclusion of additional radiative forcing changes due to other greenhouse gases, sulfur emissions, organic and inorganic carbon, and climate feedbacks yields a range of global radiative forcing increase for the twenty-first century of about 2.8 to 7.7 W m^{-2} [see Fig. 9.13a, or Fig. 19 of the Technical Summary from Houghton et al. (2001)].[2] In comparison, the $+1\%\ yr^{-1}$ CO_2 scenario in KT04 corresponds to a 5.3

[1] We note that if one is interested in interpreting our results in terms of future climate change we consider it more appropriate to consider the net global mean forcing from all sources and not just greenhouse gases, although there will be some error introduced because of the different spatial structure of the various forcings.

[2] The recent analysis of Hansen and Sato (2004) and the comments of MKL focus on greenhouse gases only. Concerning greenhouse gases only, the IPCC scenarios shown in Hansen and Sato's Fig. 4 seem plausible to us over the long term, taking into account the recent multiyear fluctuations of CO_2 growth rates and that the reduction in methane growth rate is still not well understood.

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$W\ m^{-2}$ forcing increase per 100 yr, using the Houghton et al. (2001, p. 357) forcing estimate of 3.7 $W\ m^{-2}$ for a doubling of atmospheric CO_2. Thus, KT04's 1% yr^{-1} CO_2 global mean forcing scenario is close to the middle of the 2.8–7.7 $W\ m^{-2}$ range for the six IPCC scenarios, implying that the scenario is not an extreme twenty-first-century global radiative forcing scenario. In contrast, MKL propose to assume a forcing reduced by nearly a factor of 2, which would lie near the IPCC B1 scenario. The IPCC B1 scenario is still plausible, but lies at the low end of the envelope of the IPCC marker scenarios. Alternatively, one could adopt the IPCC's A1FI marker scenario, which implies a substantially stronger future radiative forcing than the +1% yr^{-1} CO_2 scenario.

As noted in KT04, future radiative forcing of climate is highly uncertain. This is particularly true if one considers the late twenty-first century, where forcing will be influenced by such factors as population and economic growth rates in developing and developed countries, technology changes, mitigation efforts, internal feedbacks in the climate system, and so on. This uncertainty is illustrated by the large range of global radiative forcing for 2100 in the marker scenarios mentioned above (Houghton et al. 2001). The six marker scenarios are considered equally sound by Houghton et al. (2001; see p. 18, for scenario "storylines"). While we are not experts on future radiative forcing scenarios for the late twenty-first century, we consider Houghton et al. (2001) to be a more authoritative and balanced source on this topic than the opinions cited or expressed by MKL.

In KT04, we could have presented a range of results, rescaling our results to reflect the above range of global forcing scenarios presented in Houghton et al. (2001). [The use of a standard +1% yr^{-1} CO_2 benchmark forcing scenario by the Coupled Model Intercomparison Project 2 (CMIP2) facilitates such a rescaling exercise.] The results we actually presented are representative of a scenario near the midrange of the six Houghton et al. (2001) marker scenarios, and we provided sufficient information for others to rescale our results according to their assumed scenarios. While we acknowledge in KT04 (p. 3481) that "the 1% yr^{-1} compounded CO_2 increase scenario represents an idealized greenhouse gas forcing scenario, rather than a forecast of future radiative forcing . . . . " we regard the +1% yr^{-1} CO_2 scenario as representative of a plausible future course of global mean radiative forcing, as discussed above. If MKL or others wish to dismiss such a scenario as extreme in terms of global radiative forcing, we suggest they take up their argument with IPCC, rather than criticizing individual research papers where inferences about future emission scenarios are basically consistent with IPCC's findings.

The issue of conveying the uncertainty ranges of results such as those in KT04 (or throughout climate change studies) is a challenging topic where we hope to make improvements in future studies. Our approach in KT04 of using nine separate global climate models for boundary condition input and four different versions of hurricane model physics in our simulations is intended as a step in that direction. Future emission scenarios are yet another source of uncertainty, which further research will hopefully help to narrow.

Finally, we mention a related topic that is absent from any of the discussions thus far in either MKL or KT04: radiative forcing or further climate warming beyond 2100. Five of the six IPCC radiative forcing scenarios and all six of the associated global mean temperature change estimates in Fig. 9.13 of Houghton (2001) show a continuing upward trend as of 2100. It appears very unlikely to us that the climate changes and hurricane intensity increases we discuss in our paper will have attained their maximum level by the year 2100.

3. SST–storm intensity relationships

Before discussing MKL's analysis of SST–storm intensity relationships, we note the distinction between potential intensity of tropical cyclones and actual intensity. Potential intensity (e.g., Emanuel 1988, 2000; Holland 1997) refers to an upper-limit intensity that a tropical cyclone can attain for a given set of thermodynamic conditions (SST, large-scale atmospheric temperature, and moisture) and does not consider effects of dynamical influences such as wind shear on the intensity. Actual intensity is what the tropical cyclone actually achieves under the influence of all factors, including dynamical effects. As we discussed in KT04, the idealized hurricane simulations in KT04 may be thought of as addressing the question of potential intensity.

MKL note that the correlations between SST and hurricane intensity in our simulations are higher than one obtains in their real-world analysis (e.g., their Fig. 1b). This is as expected, since our idealized experimental design does not include weather noise, wind shear effects, and other factors that can prevent storms from reaching their potential intensity. These effects, present in the real world, would be expected to lead to lower correlations between SST and actual intensity. For example, we found lower correlations between simulated intensities and SST (-0.45 to -0.58) in an earlier study (Knutson and Tuleya 1999) in which storms were simulated under conditions with synoptic weather variability. (We note that this was an earlier lower-resolution version of the hurricane model, which complicates a precise comparison.) If MKL's analysis (Fig. 1b) is an attempt to analyze potential intensity (i.e., thermodynamic upper limit on tropical cyclone intensities), it is a poorly designed attempt to do so. For example, some Atlantic seasons have as few as four to six storms total, so that MKL's methodology of using the mean of the five strongest tropical cyclones will likely mix in relatively weak storms unrepresentative of the potential intensity in seasons with relatively small numbers of storms. Is 60 kt (31 m s^{-1}) a credible estimate of the potential intensity of hurricanes for any season in the Atlantic?

For an alternative view to MKL's Fig. 1b concerning SST–tropical cyclone intensity relationships, we invite readers to examine Fig. 1 of DeMaria and Kaplan (1994a) for the Atlantic basin, Fig. 4 of Whitney and Hobgood (1997) for the NE Pacific basin, or Figs. 1 and 2 of Baik and Paek (1998) for the northwest Pacific basin. For each basin, these show an apparent empirical upper bound on tropical cyclone (TC) intensity that generally increases with increasing (geographically coincident) SST, in contrast to MKL's Fig. 1b, which was constructed using basin-scale, seasonal-mean SST anomalies.

In KT04 we cite the work of Emanuel (2000), who finds a statistical relationship between the potential intensity (related to SST) and actual intensity such that once a storm reaches minimal hurricane intensity it has an equal probability of reaching any intensity between minimal hurricane intensity and its potential intensity. Presumably the processes that lead to this spread are the ones that would also reduce statistical correlations between SST and intensity in analyses such as MKL's Fig. 1b. Emanuel's analysis also demonstrates, contrary to the views of MKL, that potential intensity theory is relevant to actual intensities experienced in the real world.

MKL make no mention of KT04's comparisons showing similar intensity sensitivity results for model simulations and the potential intensity theories when applied to the same large-scale environments. While this general agreement does not prove that either our model or the theories are correct in their sensitivity to climate change conditions, it indicates that the KT04 model's sensitivity results are at least plausible in light of an independent methodology (i.e., potential intensity theory).

Despite the problems with the conception of MLK's Fig. 1b mentioned above, the figure does show some interesting, though noisy, empirical results. When MKL comment on the "overly strong relationship" or "correspondence" between SST and hurricane intensity in our study, they are referring to correlation or percent variance explained between these variables. A more useful measure of the sensitivity of hurricane intensity to SST (when a statistically significant correlation is found, as in MKL) is the slope of the regression lines between the variables. From that viewpoint, their Fig. 1b shows what appears to be a statistically significant ($p = 0.009$) positive slope of about 25 kt $°C^{-1}$, or in percentage terms over 25% increase in wind speed per degree Celsius. In our experiments, we find a wind speed change of 5.8% for an SST change of 1.75°C, or in percentage terms 3.3% $°C^{-1}$. Thus, the sensitivity (slope) shown by MKL is stronger by more than a factor of 7 than we simulate in our climate change experiments. However, rather than indicating that KT04's results need to be scaled up by a factor of 7, we speculate that such empirical extrapolations based on present day SST intensity variations do not adequately capture effects of the enhanced upper-tropospheric warming that climate models predict will occur with greenhouse gas–induced warming. One must consider details of the atmospheric temperature profile changes, as KT04 and the potential intensity theories have done, to account for this effect.

We noted in KT04 that there is uncertainty concerning future changes in atmospheric temperature profiles and other factors such as vertical wind shear, which could affect storm intensities. In contrast to MKL, who evidently believe that these factors will change in such as way as to oppose future SST-driven intensity increases, we note that these factors could change in ways that either reduce or enhance the increases of intensities that we simulate. Uncertainties such as these are a "two-edged sword"—not the panacea envisioned by MKL.

4. GFDL model intensity simulation skill

MKL contend that the model used in KT04 has no intensity forecasting skill and therefore is of limited utility in studies of future climate change impacts on hurricane intensity. In doing so, they fail to recognize the important distinction between the operational hurricane forecasting problem (a classical initial value problem) and the boundary value problem addressed in KT04, where one is concerned with the maximum hurricane intensity that is possible for a given set of large-scale environmental conditions (i.e., a climatological or statistical distribution of maximum intensities).

In the operational initial value problem, forecast performance measures can be affected by factors other than model error that are not relevant to the boundary value problem being addressed, including the quality of initial data (observations) available for the forecast, and the procedure used to incorporate this data into the initial condition for the model forecast. In an earlier paper (Knutson et al. 1998) we provided an example (Figs. 1a,b) of an evaluation of simulated hurricane intensities in the "boundary value" context. [Note that this is based on an earlier version of the Geophysical Fluid Dynamics Laboratory (GFDL) hurricane model.] The analysis in Knutson et al. (1998) compares the geographical distribution of maximum intensities produced in a simulated sample of northwest Pacific typhoon case studies (that included synoptic weather variability) to the observed geographical distribution of maximum intensities over a 22-yr period. The comparison shows that the spatial distribution and magnitude of "climatological maximum" tropical cyclone intensities produced by the GFDL model are fairly realistic compared to observations. Such a test is useful in the context of the boundary value problem we seek to address in KT04.

Even though the operational intensity forecast performance of a model is not the most appropriate test for determining a model's suitability for the boundary value problem addressed in KT04, it is important to clarify some misconceptions found in MKL. MKL state that the GFDL hurricane model used in KT04 exhibited no skill for intensity prediction during the last two hurricane seasons (2002 and 2003, presumably). In contrast, at the recent 59th Interdepartmental Hurricane Conference, J. L. Franklin of the National Hurricane Center presented verification statistics for the 2004 season demonstrating that both GFDL operational models (GFDI and GFDN) had significant skill in the Atlantic and eastern Pacific relative to climatology and persistence benchmarks (see online at http://www.ofcm.gov/ihc05/Presentations/01%20session1/s1-03franklin.ppt). Perhaps more importantly, as we state in KT04, we use a higher-resolution version of the GFDL model in KT04 than the 2004 operational version, so the skill of the KT04 model for intensity forecasting actually has not yet been fully evaluated in an operational setting, as one might mistakenly conclude based on the statements of MKL. A high-resolution version of the GFDL model closer to the one used in KT04 is being used operationally for the current (2005) hurricane season. To date, this model continues to demonstrate skill, based on over 200 cases in the Atlantic (M. Bender 2005, personal communication).

[3] Interestingly, the Statistical Hurricane Intensity Prediction Scheme (SHIPS; DeMaria and Kaplan 1994b) uses an SST measure as one of its primary predictors of intensification.

In KT04, we devote an entire section to the issue of vertical wind shear and note that our study does not explicitly address the complex issue of vertical wind shear. This is in part because operational forecasting experience indicates that the GFDL operational model performs poorly under sheared conditions for intensity prediction but has shown value as an intensity predictor when the shear is low. By avoiding the vertical wind shear problem in KT04, we were attempting to avoid using the model in situations where it is known to have simulation problems based on its operational performance.

In short, the GFDL hurricane model, while not perfect, is an appropriate model to use for the study we conducted simulating the impact of climate change on hurricane intensities, contrary to the views of MKL.

5. Conclusions

MKL propose to adopt what appears to be a plausible but low-end scenario of future radiative forcing, whereas Houghton et al. (2001) indicates that even stronger radiative forcing scenarios than we use in KT04 are also plausible. MKL present a flawed SST–intensity regression analysis comparing correlations of real-world intensities versus SST with idealized model correlations where no synoptic weather variability is present. Interestingly, their noisy regression (slope) results hint at a much greater sensitivity of hurricane intensity to SST than our simulations. We believe such a high sensitivity (slope) is likely unrepresentative of what to expect for future climate change, as it is not supported by our simulation results. MKL contend that the KT04 hurricane model has little or no utility for the climate change/hurricane intensity problem because it does not show useful skill in operational hurricane intensity prediction. Aside from not using the same model as KT04 for their assessment, they ignore effects that limited observations and initialization issues have on operational initial value forecast performance—sources of forecast error that are irrelevant in our idealized "boundary forcing" experiments. They also ignore the correspondence of our sensitivity results with potential intensity theory and our earlier work showing a reasonable simulated climatological distribution of intensities for the northwest Pacific basin.

MKL's manner of "including uncertainties" leads them to conclude that "the influence of atmospheric composition changes on future hurricane intensities will be undetectable in the foreseeable future and in fact may never be manifest." In KT04, we concluded that "CO_2-induced tropical cyclone intensity changes . . . will probably not be detectable for decades to come . . . "

Given that we state this in our paper, MKL's comments in the final paragraph about detectability seem puzzling. We also concluded that "if the frequency of tropical cyclones remains the same over the coming century, a greenhouse gas–induced warming may lead to a gradually increasing risk in the occurrence of highly destructive category-5 storms." After considering the comments of MKL, we stand by our conclusions in KT04. If global radiative forcing proceeds along the path of the "low-end" IPCC B1 scenario (or the scenario proposed by MKL), the rate at which we would expect hurricane intensities to change would be considerably slower than cited in KT04. On the other hand, a high-end IPCC scenario such as A1FI would lead to an even faster rate of change than cited in KT04.

MKL's comments serve as a reminder of emission scenario uncertainties underlying KT04's analysis (due to uncertainties in projections of population growth, economic growth, mitigation efforts, etc.). In contrast to MKL, who exclusively emphasize uncertainties that lead to smaller future changes, we have noted uncertainties that could lead to either smaller or larger changes in future intensities of hurricanes than those summarized in KT04, with accompanying smaller or larger societal impacts.

Note added in proof: After this Reply went to press, two observational studies were published (Emmanuel 2005; Webster et al. 2005), both providing new observational evidence for possible emerging long-term upward trends in tropical cyclone intensity measures, correlated with rising tropical SSTs. Emanuel's study reports a much higher sensitivity of tropical cyclone intensities to SST changes in the past that we simulate for future SST changes. Interestingly, his results are closer to the high sensitivity inferred from MKL's Fig. 1b. Investigation of these important issues is continuing.

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REFERENCES

Baik, J.-J., and J.-S. Paek, 1998: A climatology of sea surface temperature and the maximum intensity of western North Pacific tropical cyclones. *J. Meteor. Soc. Japan,* **76,** 129–137.

DeMaria, M., and J. Kaplan, 1994a: Sea surface temperature and the maximum intensity of Atlantic tropical cyclones. *J. Climate,* **7,** 1324–1334.

——, and ——, 1994b: A statistical hurricane intensity prediction scheme (SHIPS) for the Atlantic basin. *Wea. Forecasting,* **9,** 209–220.

Emanuel, K. A., 1988: The maximum intensity of hurricanes. *J. Atmos. Sci.,* **45,** 1143–1155.

——, 2000: A statistical analysis of tropical cyclone intensity. *Mon. Wea. Rev.,* **128,** 1139–1152.

——, 2005: Increasing destructiveness of tropical cyclones over the past 30 years. *Nature,* **436,** 686–688.

Hansen, J., and M. Sato, 2004: Greenhouse gas growth rates. *Proc. Natl. Acad. Sci. USA,* **101,** 16 109–16 114.

Holland, G. J., 1997: The maximum potential intensity of tropical cyclones. *J. Atmos. Sci.,* **54,** 2519–2541.

Houghton, J. T., Y. Ding, D. J. Griggs, M. Noguer, P. J. van der Linden, X. Dai, K. Maskell, and C. A. Johnson, Eds., 2001: *Climate Change 2001: The Scientific Basis.* Cambridge University Press, 881 pp.

Knutson, T. R., and R. E. Tuleya, 1999: Increased hurricane intensities with CO_2-induced warming as simulated using the GFDL hurricane prediction system. *Climate Dyn.,* **15,** 503–519.

——, and ——, 2004: Impact of CO_2-induced warming on simulated hurricane intensity and precipitation: Sensitivity to the choice of climate model and convective parameterization. *J. Climate,* **17,** 3477–3495.

——, ——, and Y. Kurihara, 1998: Simulated increase of hurricane intensities in a CO_2-warmed climate. *Science,* **279,** 1018–1020.

Michaels, P. J., P. C. Knappenberger, and C. Landsea, 2005: Comments on "Impacts of CO_2-induced warming on simulated hurricane intensity and precipitation: Sensitivity to the choice of climate model and convective scheme." *J. Climate,* **18,** 5179–5182.

Webster, P. J., G. J. Holland, J. A. Curry, and H.-R. Chang, 2005: Changes in tropical cyclone number, duration, and intensity in a warming environment. *Science,* **309,** 1844–1846.

Whitney, L. D., and J. S. Hobgood, 1997: The relationship between sea surface temperature and maximum intensities of tropical cyclones in the eastern North Pacific Ocean. *J. Climate,* **10,** 2921–2930.