Reply

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Our recent paper has had the desired affect of generating discussion on tropical cyclone impacts. We are honored to have received a comment from the originators of the Saffir–Simpson (SS) scale and welcome the opportunity to discuss their concerns. Simpson and Saffir (2007, hereafter Simpson and Saffir) are concerned that our proposed integrated kinetic energy (IKE)-based scales might not serve operational needs and would be difficult to communicate to the public. They also argue that conservation properties of kinetic energy make it a poor indicator of storm severity, that its calculation ignores thermodynamic contributions to intensity change, and that its calculation is too time consuming to serve operational needs.

Simpson and Saffir are absolutely correct about the need to incorporate an alternative (central pressure ranges) to maximum winds in the SS

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scale. Unfortunately, the operational use of the SS scale has evolved in the 30 plus years since its first introduction such that the winds are now interpreted to be sustained (maximum 1 min), rather than gusts as in the original use of the scale. In operations, the SS scale is now determined by maximum wind speed only, ignoring storm surge and pressure (see, e.g., www.nhc.noaa.gov/aboutsshs.shtml).

We agree completely with Simpson and Saffir that the assignment of categories should relate to the net release of energy, and that one should be "wary of individual point values of the highly variable, often ephemeral, reported max wind speeds." We showed (Powell and Reinhold 2007) that the IKE is insensitive to the maximum wind speed. IKE calculations are not time consuming. While the experimental H*Wind analyses used in the calculation of IKE are typically conducted on either a 3- or 6-h cycle, the actual calculation of IKE from a gridded analysis or from operational radii is straightforward and takes only a few seconds.

We make no attempt to relate the IKE to various complex and poorly understood processes (eyewall cycles, vertical shear, oceanic heat content, dry-air ingestion) that may contribute to changes in the structure and intensity of a tropical cyclone, but we agree that such factors should have an influence on IKE. However, we are neither advocating IKE-related measures and scales as diagnostics for such processes nor do we expect that IKE should have conservative properties; rather, we demonstrate IKE as a measure relevant to the impacts forced by wind loading (as described in building standards from the American Society of Civil Engineers) and ocean surface stress (recognized as the forcing mechanism for storm surge and waves).

Powell and Reinhold (2007) did not make "frequent references to the impact of Katrina in New Orleans." Rather, our discussion of Hurricane Katrina focuses on the Mississippi coast and the perception that Hurricane Katrina may have been interpreted by some of the public to have had a similar destructive potential as that of Hurricane Camille of 1969. This perception cost lives because even as an SS category 3 hurricane at landfall, Katrina contained twice the IKE of Camille for winds above hurricane force. Katrina has taught us that we need to improve how we communicate damage potential to the public. Incorporating the size of the damaging wind field in destructive potential scales will help to communicate how the intensity of a particular storm (based on the SS scale) translates into wind and storm surge impacts.

The SS scale has been a very valuable tool in warning people about hurricanes, but we have known for some time that the level of surge and surge-related damage is not well correlated with the maximum wind speeds at landfall. IKE-based methods could lead to more consistent warnings of damage potential both for wind and surge. Analogous to the recent adoption of an "enhanced Fujita scale" for classifying tornadoes, we view our paper as a possible foundation for an enhanced Saffir–Simpson scale.

REFERENCES

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