Reply

MARK D. POWELL AND ERIC W. UHLHORN

NOAA/Atlantic Oceanographic and Meteorological Laboratory/Hurricane Research Division, Miami, Florida

JEFFREY D. KEPERT

Centre for Australian Weather and Climate Research, Bureau of Meteorology, Melbourne, Victoria, Australia

(Manuscript received and in final form 30 November 2010)

1. Introduction

Franklin (2011, hereafter F11), in his comments on Powell et al. (2009, hereafter PUK), makes two main points, both concerning the issue of what factor observed flight-level reconnaissance winds in hurricanes should be multiplied by to estimate a surface wind. First, F11 offers some explanations for the differences between the earlier study of Franklin et al. (2003, hereafter FBV) and the results of PUK, and states that he believes that PUK have misrepresented FBV. We shall discuss his interpretation of the differences below, and refute the accusation of misrepresentation. Second, F11 argues that a value higher than that found by PUK is appropriate to account for undersampling of the surface wind by the measurements, and states that PUK's findings do not justify altering the current intensity estimation practice used at the NHC.

Franklin has raised these issues several times in the past, and we are glad that he has put his concerns in print so that we may have the opportunity of further discussing this important area of science and operational meteorology. We shall begin by discussing F11's two specific points. However, we are concerned that F11 has apparently overlooked what we consider to be the main point of PUK, so we shall also briefly reiterate that point. We close with a reiteration of our recommendations for operational practice.

2. Discussion

a. Reasons for differences in the surface wind factor

The main finding of PUK was the development of new maximum surface wind estimation techniques, taking into consideration along a slanted radius of maximum wind (RMW), by using unambiguous pairs of maxima from flight level (near 700 hPa) and the surface [measurements made by the Stepped Frequency Microwave Radiometer (SFMR)] along radial flight legs. This is a very important distinction that sets our results apart from those of FBV, who evaluate a surface wind factor (SWF) along a sonde trajectory, without considering the measured wind at the RMW. Therefore, the FBV estimates have two disadvantages relative to those in PUK: 1) GPS sondes will rarely sample the maximum wind at the surface (if the wind calculation is even available at the surface) because the area of maximum winds is quite small and difficult to target and 2) the flight-level wind at the time of sonde launch (or 700-hPa-level wind from the sonde profile for sondes launched above 700 hPa) is almost always less than the maximum flight-level wind speed from that radial flight leg. The first of these will lead to a lower SWF, and the second to a higher SWF, in FBV compared to those in PUK.

In the discussion on pp. 38 and 39, which F11 says we misrepresent, FBV evaluate a mean along-the-sonde-trajectory SWF in the vicinity of the eyewall as an upper bound (0.91) or a lower bound (0.88) within 3 nautical miles (n mi) of the RMW. In PUK, our reproduction of FBV's eyewall result from a larger sample of sonde data allowed us to conclude that FBV used the flight-level wind at the time of sonde launch (or 700-hPa-level wind from the sonde profile) rather than the maximum flight-level

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Corresponding author address: Dr. Mark Powell, FSU-COAPS, 2035 E. Paul Dirac Dr., 200 RM Johnson Bldg., Tallahassee, FL 32306-2840.
E-mail: mark.powell@noaa.gov

DOI: 10.1175/WAF-D-10-05054.1

wind for the particular radial leg, leading to a low bias in the denominator of their along-the-sonde-trajectory SWF.

Our method removes uncertainty from both the numerator and denominator of the SWF by using unambiguous surface and flight-level maxima along a sloping RMW for that radial. Indeed, we note that FBV were aware of one advantage of this approach, quoting from FBV: ''To convert peak 700-hPa reconnaissance winds in a tropical cyclone, what is needed is the value of (surface wind factor) along the sloping RMW.'' Perhaps this quote is the most puzzling aspect in the FBV study. Flight-level wind measurements at the RMW were available for every radial leg from which sondes were launched but FBV chose not to use them, thus guaranteeing a systematically lower denominator of their along-the-sonde-trajectory SWF.

Regarding azimuthal variation in the SWF, FBV mention a difference of 0.04 in the along-the-sonde surface wind factor between the left and right sides of the storm. Their value is smaller than PUK's 0.10, which is more consistent with theoretical arguments (Kepert 2001; Kepert and Wang 2001) and previous case studies (Kepert 2006; Schwendike and Kepert 2008). The reasons for the differences are not entirely clear but likely include that their assessment of asymmetry suffers from the limitations noted above, which were avoided by PUK.

F11 discusses the differing heights of the aircraft between FBV and PUK but acknowledges that the difference in mean heights between FBV and PUK is near negligible (<100 m). Although FBV do not specifically list aircraft flight levels beyond noting that ''most of the sondes were dropped from the 700-hPa level,'' an examination of sonde launches from missions listed in FBV's Table 1 indicates that many launch altitudes were actually well above the 700-hPa level (e.g., all sondes in FBV's Fig. 4a were launched from 4500-m altitude). The dataset from Powell et al. (2003), which used many of the same available GPS sonde profiles as FBV, comprised significant numbers of sonde launches above 3660 m (25%) and below 2777 m (25%).

It is common in science for physical constants to be revised following new measurements and advances in measuring technology. Just as the GPS sondes fostered changes in how maximum surface winds were estimated based on FBV's publication, the SFMR has made a great improvement in our ability to estimate surface winds and provided an independent assessment of the FBV method. We do not regard the FBV method as being wrong; at the time, it was a significant advance. But it should not surprise that further advances in measurement technology and understanding have led to new methods with better properties, as in PUK. In general, we encourage the use of methods with the least bias and uncertainty.

While F11 acknowledges that FBV's 90% along-the-sonde SWF is to be applied to the peak observed 700-hPa flight-level wind speed, this is disingenuous since development of this factor never used peak flight-level winds, thus guaranteeing a high bias in the resulting surface wind estimates.

b. *Effects of undersampling*

Recently, high-resolution numerical modeling has revealed complex finescale wind structures in the vicinity of the RMW (e.g., Rotunno et al. 2009; Bryan and Rotunno 2009). These results may imply that operationally available observations have some risk of missing the strongest winds in a tropical cyclone, and this risk prompted Uhlhorn et al.'s (2010) attempt to estimate the likely magnitude of the resulting underestimation, as noted by F11.

We agree with F11 that it is possible that operational intensity estimates may in future have to take into account such processes. However, we note that this research is in its very early stages. The Uhlhorn et al. (2010) study cited by F11 was a conference talk, not an extended abstract, and the results have not yet been subject to peer review. Several factors remain to be considered, including the question of whether the numerical simulations are faithfully representing these features, and whether the amplitude of these features is the same in all storms. At this stage, we would recommend that further research be undertaken before attempts are made to account for this possible undersampling. Moreover, we would recommend that different physical factors (surface wind reduction, undersampling) be accounted for separately rather than lumped into a single factor.

We are puzzled by F11's claim that ''undersampling . . . is implicit in the formulation and operational application of the 90% adjustment.'' FBV presented averages over a large number of dropsondes. Unless some quirk of the flow causes dropsondes to preferentially sample the strongest winds, the FBV measurements would be expected to contain a mix of strong and weak near-surface wind fluctuations, and their averaging therefore would result in a mean value. Moreover, their use of the word ''implicit'' carries the meaning that FBV's results imply the existence of these features (*Oxford English Dictionary*). We do not see the logic that leads to this supposed implication and find no mention of such matters in FBV.

Finally, F11 suggests that the NHC should, and at present does, account for some undersampling of relatively small-scale strong wind features. Again, this is a puzzling statement to us. The features are relatively small in scale, and would be expected to be sampled infrequently. Yet, when what was possibly an example of such a feature was encountered during Hurricane Felix, the postseason analysis explicitly disregarded the consistent aircraft, SFMR, and dropsonde measurements in estimating the storm intensity: ‘‘Detailed data from the dropsonde suggest that the sonde and SFMR sampled a small-scale feature unrepresentative of the intensity of Felix at that time.’’ (Brennan et al. 2009).

c. Reconsideration of PUK’s recommendations

F11 closes by stating that ‘‘PUK’s findings do not justify altering the tropical cyclone intensity estimation approach recommended by FBV and currently practiced at NHC.’’ In making this statement, F11 is apparently rejecting the updated values of the slant SWF supplied by PUK. Yet, it was not these values that PUK recommended for surface wind reduction. Rather, they recommend one of the regression techniques—specifically, their Eqs. (11) and (13), depending on the particular application. F11 severely misrepresents PUK by considering only the constant SWF approach. That approach is a small, and not in our view the most important, part of that paper.

The benefits of those regression equations, as detailed by PUK, are twofold. First, they do not show a bias relative to the observations. Second, and much more importantly, they explain a much larger proportion of the variance than does a single SWF because they account for some of the effects of storm structure on the boundary layer winds. Specifically, the RMS error falls from 5.4 m s^{-1} for the 90% rule, to 3.0 m s^{-1} for PUK’s Eq. (11), as shown in PUK’s Table 7, while using the 90% rule to determine the maximum surface wind over the course of a typical reconnaissance mission shows a bias of 4.6 m s^{-1} and an RMS error of 6.4 m s^{-1} (PUK’s Table 5).

In short, PUK’s regression estimates are superior to the use of a single mean SWF since they eliminate the mean error (bias) and have smaller random error (uncertainty). It is for this reason that they are utilized, as F11 notes, in the H*Wind analysis system.

3. Conclusions

The recommendations of PUK for estimating surface winds based on reconnaissance flight-level data take advantage of unambiguous measurements of the maximum surface wind speed on a radial flight leg from the SFMR and also use the maximum flight-level wind speed to determine a slant SWF along the RMW. The SWF currently used in operational practice contains large random error uncertainty and leads to biased estimates of the surface wind due to

1) the difficulties of a GPS sonde in sampling the maximum surface wind speed and
2) the failure of the FBV study to use the maximum flight-level wind speed at the RMW in the denominator of their SWF, when the NHC practice is to apply this SWF to the maximum flight-level wind.

Finally, we reiterate that we do not recommend the use of a surface wind factor that varies only with height at the RMW, as proposed by FBV and discussed by F11. Rather, we recommend the regression techniques described by PUK, since these better account for storm dynamics and thereby lead to more accurate estimates of the surface wind speed.

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