

Comments on the April 4-6 2005 CBLAST meeting

by

Kerry Emanuel

Overall: CBLAST is one of the most successful and rewarding programs I have been involved with in my career. It has brought together a number of very good people working in disparate areas to tackle some of the most thorny but important problems in meteorology and oceanography. The field phase of the project, through a combination of the devoted efforts of the scientists and project managers and sheer good luck with the weather, has yielded an astonishing set of data, whose analysis we are now in the midst of. It is gratifying to hear that ONR is funding and will continue to fund the analysis of this important data set. There is a very nice combination of intellectual stimulation and practical results.

Assessment of the analysis of surface fluxes in high winds: ONR is funding a number of very different approaches to this difficult problem: laboratory experiments, direct flux measurements from aircraft in hurricanes, estimates of fluxes under hurricane eyewalls from angular momentum and enthalpy budget residuals, inferences from detailed numerical simulations of hurricanes using high resolution coupled models, and inferences from analysis of the ocean response to hurricanes.

On the subject of momentum fluxes, all these methods now seem to be converging on the result, if not the mechanisms. The laboratory, model and ocean-side work all seem to be rejecting the idea that the near linear wind dependence of the drag coefficient, deduced from low wind speed measurements by Large and Pond, can be simply extrapolated to arbitrarily high wind speed. The evidence is that the drag coefficient saturates at around the threshold of hurricane wind speed. There continues to be disagreement about the mechanism, though. There is a well developed idea, supported by laboratory work, that drag is limited by flow separation when the curvature of wave crests becomes sufficiently large. But this ignores momentum flux by spray, which other groups think becomes important at high wind speed. So the practical result is converging, but disagreement about the mechanism continues.

The assessment of enthalpy fluxes is less rosy. Spray theory and some preliminary results from the budget residual technique imply that spray becomes the dominant enthalpy transfer mechanism at high wind speed. Theory, such as it exists, implies greater transfer coefficients than are measured in sub-spray-producing winds. But Mark Donelan's superb laboratory work shows no obvious enthalpy flux enhancement up to 45 m/s. This is perhaps because 45 m/s is not quite high enough, or that he is using fresh water, or both. [Pete and Shuyi: The number 45 m/s is from my memory and should be checked with Mark.] With any luck, further refinement of the budget residual technique, applied to dropsonde and aircraft measurements made in Hurricanes Fabian and Isabel, will provide more precise estimates of the enthalpy exchange coefficient at very high wind speeds.

Shuyi Chen's modeling work incorporates ocean coupling and wave-related drag, but does not yet account for spray, with the result that the enthalpy transfer coefficient is more or less constant over the whole range of wind speeds. Nevertheless, the model produces reasonable intensity hindcasts, though it would appear that, in the very few cases presented, the wind speeds are too small relative to the pressure. With

uncertainties in the ocean coupling, the wave formulation, in the representation of spray effects, and in other parameterized processes, it is not easy to isolate the source of systematic biases or other errors. For example, the underprediction of wind speed may be owing to an exaggerated ocean temperature response or to the omission of spray. In my opinion, the community will not develop much confidence in model-based inferences about physical processes until and unless the models are run in objective forecast mode and produce consistently reasonable results, but even here, there is the potential problem of compensating errors.

Although it is conventional to formulate fluxes in terms of 10 m wind speeds, the latter are very sensitive to surface conditions and may not always be reliably produced by models. I advocate basing fluxes on gradient winds, which are much more stable, both in nature and in models, and which are more nearly external than are fixed altitude winds, which are as much a product of fluxes as their cause. But one has to consider the possibility that in the eyewall region, with extremely large horizontal gradients, the fluxes are non-local in the horizontal and depend on the radial pressure gradient through a range of radii.

Spray measurement: CBLAST demonstrated the difficulty of making measurements in the spray layer using aircraft. There seemed to be a consensus that direct measurement of spray is lacking, and this was one of the few problems with CBLAST. There were some "offline" discussions about the desirability of mounting a downward-pointing W-band radar on one of the NOAA WP-3D aircraft and flying it in rain-free, high wind regions, such as just inside the eyewall. There are a number of such radar available, and I hope that NOAA gives high priority to this in future missions.

Ocean-side measurements and measurement systems: The results of the upper ocean measurements during the CBLAST field phase are truly spectacular and will no doubt lead to great advances in understanding and predicting the upper ocean response to storms, which is in turn important for intensity prediction. These measurements are thanks to the creativity of those involved in designing the instruments and, evidently, to the great persistence of a few individuals who nursed the concept through the arcane labyrinths of the military.

The large procedural obstacles to deploying new instruments from Air Force aircraft are a great pity and no doubt discourage scientists from developing new instruments. (This should be contrasted with the state of affairs in NASA, which showers investigators interested in developing new satellite-based instruments with money and other resources.) NOAA and ONR program managers would do a great service to science and to society by streamlining this process, so that there is a clear route from the lab to field deployment. Perhaps the development of a standard wing pod or bomb bay, which developers could design to, would be the way to go. As demonstrated in CBLAST, many interesting measurements can be made from systems that are too large to be deployed through sonobuoy chutes but still small enough to be deployed from aircraft by other means.