We thank the reviewer for his constructive and insightful comments. We have considered his comments carefully and provided a response and revised manuscript.

Review of "The genesis of Typhoon Nuri as observed during the Tropical Cyclone Structure 2008 (TCS08) field experiment Part 2: Observations of the convective environment" by M. T. Montgomery and R. K. Smith

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This paper follows up on the authors' results for tropical cyclogenesis in the PREDICT project by performing the same analysis on the development of Typhoon Nuri, which was observed during TPARC/TCS08. In this paper they concentrate on a comparison of the thermodynamic environments for the first two Nuri case studies, referred to as Nuri1 and Nuri2. In a nutshell they found that the averaged environmental sounding moistened at middle levels between Nuri1 and Nuri2, warmed by about 1 K in the upper troposphere, but did not change much in the lower troposphere. These results are consistent with our 3D-Var analyses of Nuri1 and Nuri2 (see figure 1), though we see slightly greater changes in the virtual temperature profile, due probably to our slightly smaller averaging area. The paper is well written, and I recommend acceptance subject to minor revision with one caveat, which is presented below.

The caveat The authors make the following comment in their conclusions section: "Two notable thermodynamic models that have been offered previously to explain tropical cyclogenesis in general and the formation of Nuri in particular have assumed a modest cooling of the lower troposphere in the disturbance region in association with a cyclonic circulation that has a maximum in the middle troposphere. To test this thermodynamic hypothesis, we examined the change in the system-mean virtual temperature near the pouch region of the developing disturbance during the genesis phase, but found no systematic cooling in virtual temperature of the lower troposphere (below 4 km) during the genesis. We did find a small warming of the upper troposphere, though this warming is less than 1 1 K. **Neither these results nor those of the PREDICT experiment support the thermodynamic hypothesis on the scale of the recirculating pouch region.**" (Emphasis added.)

I believe that the emphasized sentence overstates the case against the thermodynamic hypothesis and needs to be tempered, for reasons discussed below. Montgomery and Smith discount the thermodynamic models (including one proposed by Raymond and Sessions 2007) because the difference in average virtual temperature in the lowest 4 km between Nuri1 and Nuri2 is negligible, whereas Raymond and Sessions assumed that a disturbed environment is both warmer at upper levels than an undisturbed environment and cooler at lower levels. However, Nuri1 was already a strong easterly wave, and comparison of the Nuri1 mean sounding with a less disturbed environment, such as that occurring in the TCS030 case (see figure 2), shows strong cooling below 4 km. Thus, as figure 3 demonstrates, the net change in the temperature profile between TCS030 and Nuri2 exhibits both upper level warming and lower level cooling.

Reply: We respectfully disagree with the reviewer. We think the reviewer is overstating our viewpoint! We are not discounting all thermodynamic models. In fact, the only thermodynamic model we discuss in this paper is that of Raymond and colleagues. From our perspective we have several concerns about this thermodynamic model that are not solely because of the small difference in average virtual potential temperature in the lowest 4 km between Nuri1 and Nuri2. We presented these concerns in our companion paper (Smith and Montgomery 2012; now in press with QJRMS). We have summarized these concerns in a new discussion section of the paper.

In our view we are not convinced that a comparison with solely the thermodynamic characteristics of TCS30 from one day is physically appropriate. Moreover, the kinematic structure of the pouch was dramatically different between TCS30 and the pre-Nuri disturbance on 14 – 15 August. One could easily argue that TCS30 failed to develop more because of the shallower and weaker pouch structure (viz., vorticity, Okubo-Weiss parameter, etc.) than the small changes in the thermodynamic temperature structure. However, this debate can not be settled here.

In the revised paper we state:

"In Smith and Montgomery (2012), we articulated a number of concerns with Raymond and Sessions' hypothesis. In a nutshell, we question the applicability of a theory for small perturbations to radiative-convective equilibrium formulated on the equator to off-equatorial tropical disturbances possessing relatively small spatial and temporal scales. In the Raymond and Sessions's model, the imposed temperature and moisture perturbations were motivated by observational data taken within a mesoscale convective vortex embryo and proximity soundings of the same mesoscale convective system. It would be fortuitous if these proximity soundings were representative of the radiative-convective-equilibrium state of the entire tropical disturbance, if such a state exists. We are not convinced that a comparison with solely the thermodynamic characteristics a non-developing disturbance on one day during the TCS08 experiment is physically defensible. It is yet to be demonstrated whether the thermodynamic control as envisaged by Raymond and Sessions is essential in a rotational environment."

To put this in perspective, the premise of the Raymond-Sessions calculation was to determine the change in convective response which occurs in going from an environment in radiative-convective equilibrium to one which is highly disturbed. As noted above, the warming in the upper troposphere and cooling in the lower troposphere assumed by Raymond and Sessions are comparable to that seen between TCS030 and Nuri2 in figure3. The mean tropical environment (which differs little from the undisturbed environment since the fractional areal coverage of strong disturbances is small) is likely to be close to radiative-convective equilibrium, as the tropics as a whole exports only a small fraction of incoming solar energy to higher latitudes. It is therefore more appropriate to compare the Raymond-Sessions results with the Nuri2 - TCS030 difference than with the Nuri2 - Nuri1 difference.

Reply: The reviewer's statements raise more questions to us than we are capable of answering with clarity. This is partly because there appears to be a level of imprecision in quantifying the closeness to radiative-convective equilibrium. Our question is: Can one really regard a

temperature dipole perturbation of plus-minus 1K as characterizing a "highly disturbed" environment? Another question is: What metric does the reviewer use to define "close to radiative-convective equilibrium"? Does the reviewer define "closeness" in terms of temperature, moisture and cloud fraction, and if so, over what space and time scales? To what extent are garden-variety tropical disturbances in a state of close radiative-convective equilibrium according to this metric?
Finally, as discussed above and noted in the revised text, we are not convinced that TCS30 is an appropriate comparison.

To show that TCS030 is appropriate as a benchmark for an undisturbed tropical environment, we compare the virtual temperature profiles of TCS030 and Hagupit2 in figure 4. Hagupit developed into a typhoon about a week after the Hagupit2 mission. However, for most of that week, the pre-Hagupit disturbance drifted to the west as a weak wave without developing, making it more like TCS030 than Nuri. With the exception of a thin layer near the surface, the Hagupit2 profile is actually warmer than TCS030 through most of the observed depth range (see figure 5). Only below about 1:5 km is Hagupit2 cooler than TCS030. Thus, the mean temperature of the Hagupit2 sounding below 4 km is quite close to that of TCS030. In contrast, Nuri1 is considerably cooler than Hagupit2 in the lowest 4 km. The evidence at hand thus favors the hypothesis that the Nuri1 environment was significantly cooler than the undisturbed environment at low levels. Another interesting example of cooling at low levels in a tropical cyclone environment is given by Mapes and Houze (1995).

Reply: The above argument starts with the presumption that the Hagupit disturbance did not develop for about one week's time because it did not possess a cooler temperature perturbation in the low troposphere than TCS30. In our view this argument is overstated and oversimplistic because, as shown by Bell and Montgomery (2010, Geophysical Research Letters), the Hagupit disturbance was under the influence of an upper-level cold low disturbance and significant vertical shear during and a few days after the aircraft missions. To suggest that Hagupit developed only when the cold temperature perturbation appeared with no recognition of the vertical shear seems to us to be a large stretch of the imagination!

Reference:

Sheared deep vortical convection in pre-depression Hagupit during TCS08, *Geophys. Res. Lett., 37, L06802, doi:10.1029/2009GL042313, 2010*: 2010, M. M. Bell, and M. T. Montgomery

Montgomery and Smith correctly point out that virtual temperature is more closely related to buoyancy than temperature. However, figures 1-5 show that the virtual temperature correction is small in comparison with differences in temperature profiles between different cases.

If there is more variability in the temperature profiles between different cases, wouldn't this caution against selecting a particular disturbance as a reference ?

Minor comments

1. The sounding for Nuri4 should probably be cut off at 3 km in figure 5, since, as the authors note, the deep soundings in this case are unrepresentative of the storm core.

2. The same is probably true of Nuri3, as it is unlikely that the few soundings the C130 made before having to return to Guam are representative.

Reply to 1 & 2: In the original ACPD version, we already pointed out the few C130 soundings on Nuri3 and Nuri4 above 3 km. Rather than remove data from Figures, we have modified our original comments noting the limited sampling during Nuri3 and Nuri4.

In section 3 we have modified the pertinent text as follows:

"As a reminder, there were only nine soundings above 3 km in Nuri3 and only two soundings above 3 km in Nuri4. The former were dropped en route to the tropical storm and the latter were dropped en route to and from the typhoon and may not be representative of the core region of the system."

References

Mapes, B., and R. A. Houze, Jr., 1995: Diabatic divergence profiles in western Pacific mesoscale convective systems. J. Atmos. Sci., 52, 1807-1828.

Raymond, D. J. and Sessions, S. L.: Evolution of convection during tropical cyclogenesis, Geophys. Res. Lett., 34, L06811, doi:10.1029/2006GL028607, 2007.

Raymond, D. J., Sessions, S. L. and Lopez, C. L.: Thermodynamics of Tropical Cyclogenesis in the Northwest Pacific, J. Geophys. Res., 116, D18101, doi:10.1029/2011JD015624, 2011.