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Interactive comment on "Prediction of tropical cyclonegenesis over the South China Sea using SSM/I satellite" *by* C. Zhang et al.

Anonymous Referee #2

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GENERAL COMMENTS:

The author's attempt to use integrated precipitation rates from microwave data over developing tropical disturbances in the South China Sea (SCS) is a worthwhile research project as it might establish some characteristics sizes for future climatological studies. However, I believe that they did not provide enough evidence to establish the criterion of 3 X 10exp14 Watts as the methodology and reasoning needed to be expanded greatly before these result could be accepted. The following are some of the concerns I have, before I could accept their results:

1) The author's method of using the Illinois Listserv to obtain their sample is not a controlled procedure. These bulletins come from (I would guess) various tropical cyclone warning centers that provide forecasts and analysis in the region (e.g. JTWC, Hong

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Kong, PAGASA, Vietnam, Malaysia and maybe Darwin)...all except NHC. The authors did not explain how they could distinguish between true tropical disturbances (without a closed circulation at the surface) and those tropical depressions that may have had closed surface circulations, but for whatever reason were not yet warned on. Also, which agency (ies) did they use to determine when the system became a tropical depression: was it based on a 'warning' or on a post analysis (i.e. 'best track'). I also think it matters whether the systems pre-existed and moved into the SCS from the Philippine Sea or whether they formed totally within the South China Sea basin. Finally how many of these systems pre-existed as monsoon disturbances or depressions (systems with large circulations either at the surface or aloft, but with no distinctive surface position).

2) I would like to know what was unique to the use of the 500km radius. Did they try larger and smaller sizes with weaker results (using the same methodology)? As far as I know, 500km may be unique to a SCS system, but I would need to know if they tried any other size. In addition were these size criteria the same for all areas of the SCS basin (i.e. we often think of smaller systems forming closer to the equator (e.g. Vamei in 2001) and, of course, there is a general size difference usually found between a developing large monsoon depression versus a smaller westward moving cloud cluster (disturbance) moving in from the Philippine Sea.

3) I have a question regarding the author's major results concerning the amount of precipitation as a driving criterion as opposed to a necessary but not sufficient condition. Previous works using satellite data and size such as by Dvorak (1975), McBride and Zehr (1981) and many others have established that it is not the 'amount' of convection, but the organization of the convection that is most important...and then a requirement of a lack of persistent vertical wind shear that is most important. Do the author's believe their work contradicts these earlier studies...and how can they be sure without knowing the wind structure about each of the systems that they studied?

4) In the author's methodology (unless I misunderstood), they stated that as long as the system maintained their TLHR criterion as a mean over several days and also on the

‘latest day’, that it was considered a developing disturbance; otherwise it was a non-developing disturbance. Although I could understand the mean day over day criterion, I did not understand the ‘latest day requirement’. This appears to be an observation that ‘if’ the convection ‘goes away’, it will not form (which might be a requirement, in general, for the SCS as the size of the basin might not allow enough time for the system to ‘redevelop’ before it runs into land). I would think that the reasoning why the convection weakened to be an important factor, as well.

5) A final general point I will make concerns the relationship of the ‘center’ to the actual center of the surface circulation. Was there any attempt to ensure that for both developing and non-developing systems that if a surface circulation existed (perhaps from surface observations or QuikSCAT) that the centers of the convective center (stated by the various agencies) and the surface center were one in the same? This knowledge may help the author’s establish a physical reasoning for their results through inertial stability principles (Schubert and Hack, 1982); since unless this occurs, I do not see why the TLHR from convection would not dissipate via gravity waves, advection, mixing and friction, etc. rather than contribute directly to a surface pressure drop.

SPECIFIC COMMENTS:

These items are of lessor importance than any of the broader questions listed above:

1) The references that the authors mentioned at the beginning of the paper such as by Katsaros et al (2001) and Sharp et al (2002) are both discussions on how these authors used the confusing winds in the QuikSCAT data to establish a surface circulation and are not discussing the actual criteria for tropical cyclone genesis. 2) I was not quite sure what algorithm in the Wentz papers that the author’s were referring to. 3) I did not understand the X-axis in Figure 1. 4) References discussed in the above discussion include:

Dvorak, V. F., 1975: Tropical Cyclone intensity analysis and forecasting from satellite imagery. Mon. Wea. Rev., 103, 420-430.

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McBride, J. L. and R. Zehr, 1981: Observational Analysis of Tropical Cyclone Formation, Part II: Comparison of Non-Developing versus Developing Systems. J. Atmos. Sci. 38, 1132-1151.

Schubert, W. H. and J.J. Hack, 1982: Inertial stability and tropical cyclone development. J. Atmos. Sci., 39, 1687-1687.

TECHNICAL CORRECTIONS:

This work would have to have another thorough review of the English grammar and terminology used before it could be finalized. Although I understood most of what the authors intended, I believe there were many places where the sentence structure or use of words was not quite correct. Even the title of the paper mentions a ‘SSM/I satellite’ which I know the authors also know is actually a ‘DMSP’ satellite and the sensor is the ‘SSM/I”.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 1495, 2010.