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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A characterization of thermal structure and conditions for overshooting of tropical and extratropical cyclones with GPS radio occultation" *by* R. Biondi et al.

Anonymous Referee #3

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There are 5 issues that I have flagged as requiring major revisions. I do not think any of these issues will be particularly difficult to address, but they should be resolved before this paper is published. The 5 issues are as follows:

1. Overshooting is defined by equations 2 and 3, but the variables on the left-hand sides of those equations are not defined. (What is Hcoldest_std? What is Hcoldest_std+1?) After the equation, it says that these variables are "considered to be" some things, but those are not definitions. The best I can do is to interpret this as sloppiness and assume that both should be Hcoldest. Then, when Hcoldest satisfies equation 2, I can "consider it to be indicative of" one thing, and when Hcoldest satisfies

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equation 3, I can "consider it to be indicative of" another thing.

2. Assuming my interpretation is correct, equation 2 defines an overshooting event as the height of the minimum temperature anomaly being higher than the mean tropopause height plus one s.d. of the tropopause height. (This s.d. is ambiguous in the text, but I assume this is the s.d. of the year-to-year monthly means at that location. This should be clarified.) Defined in this way, we might expect "overshooting" to be detected in 16% of all cases, even without tropical cyclones (if the tropopause heights are normally distributed, then the tail outside 1 s.d. contains 16% of the probability). Equation 3 is perhaps better, but it is difficult for me to quantify the affect of the RO measurement error. (As a side note, the uncertainty of "0.996 km" is silly. Is the uncertainty really known to 3 significant digits? Even if yes, is that 4 meters so important that we could not call this 1 km?) In the end, though, the precise definition of overshooting might not matter a great deal if the goal is to compare relative overshooting frequencies between basins and different categories of TCs.

3. I am encouraged to see events referred to as "possible overshooting", which emphasizes the fact that these may not be true overshooting events. But, I am still left with some unease over the uncertainty as to what these events are. It was Romps and Kuang who noted the possibility that large-scale lifting of the tropopause by TCs – as opposed to convective overshoots – might be responsible for the anomalously cold temperatures. Can RO be used to distinguish between these two possibilities, perhaps when used in concert with some other instrument?

4. Part of the methodology was unclear to me. RO measurements are associated with a TC if they occur "in a time window of 6 hours and a space window of 600 km" with respect to the TC center. Why is such a strange criterion used? At the time of the RO measurement, the TC center is physically located somewhere. Why not associate an RO measurement with a TC if it is within a certain distance of the TC center at the time of the RO measurement? It seems it would be simple to linearly interpolate the TC positions to the time of the RO measurement, thereby requiring only a distance

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threshold. I do not think this will have much impact on the results – in 6 hours with a 5-m/s translation speed, the TC would move 100 km, which is small compared to 600 km – but the criterion is strange enough that I got hung up on it as I was reading.

5. Finally, what is the horizontal footprint of the RO measurements? This information is necessary for me to understand whether these measurements could be sampling an individual cloud updraft, a collection of updrafts, or some average on the scale of the entire TC. Also, it would be helpful to give the vertical resolution.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 29395, 2014.