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Atmospheric Measurement Techniques Discussions

Interactive comment on "On the link between precipitation and the ice water path over tropical and mid-latitude regimes as derived from satellite observations" *by* Yaniv Tubul et al.

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General Comments: This manuscript relates ice water path (IWP) and rainfall rate to convection in the tropics and midlatitudes, whereas the previous published literature had focused mainly on the IWP-rain rate relationship for shallow stratiform clouds. The paper is well-written and would be of significant interest to readers of Atmospheric Measurement Techniques; however, I have significant reservations about the validity of the results because TMPA 3B42 was used as the ground validation dataset and the internal inconsistencies in the data set (it uses rain rates based on infrared and passive microwave in different places) and because the microwave retrievals may contain implicit relationships between IWP and rain rate that may compromise the significance of the results.

Specific Comments: 1. Page 4, lines 20-21: The use of TMPA 3B42 as the rainfall "ground truth" is a significant concern. 3B42 consists of rain rates retrieved from passive microwave (PMW) instruments and (where they are not available) rainfall rates from infrared (IR) brightness temperatures calibrated against PMW. Because of the relatively infrequent sampling of PMW in many locations, the IR rain rates will have a significant influence on 3B42 and IR rain rates have well-documented shortcomings because they rely on the relationship between cloud-top brightness temperature and surface rainfall rate (which is at least much more robust for convective clouds than for stratiform clouds). Furthermore, the PMW portion of the 3B42 fields is based on sensitivity to ice hydrometeors (or liquid if present in sufficient numbers) and hence some of the relationships between IWP and rain rate observed may be an artifact of the retrieval and not necessarily real. Although it would significantly reduce the amount of available validation data, using a consistent dataset such as the TRMM PR alone (which, because of its inclined orbit would occasionally intersect the Aqua orbital path) would reduce the effects of these retrieval artifacts and significantly increase confidence in the manuscript's findings 2. Page 4, line 22: 3B42 is actually 3-hourly time resolution; this is stated in lines 2-3 of page 5 but should probably be stated here instead for clarity. It might be even better to reorganize these first two paragraphs of Section 2 to completely describe the MODIS cloud products in one paragraph and 3B42 in the next. 3. Page 4, line 20: The MODIS cloud property retrievals are at the pixel level (1-km resolution) and 3B42 is at 0.25° lat / lon resolution, so what was the reason for aggregating up to 1° lat / lon resolution? 4. Page 5, line 5: These rain rates should be described as "retrieved" or "estimated", not "measured" since strictly speaking only a gauge provides a direct measurement. 5. Page 5, lines7-8: The 3B42 rainfall rates represent an average over the entire 3-hour period rather than a value at a particular point in time; therefore, interpolation is probably not recommended; rather, the Aqua overpass should be matched with whichever 3-hour window of 3B42 is coincident with it. 6. Page 9, line 9: 3B42 provides estimates of rain rate, not rain amount. 7. Page 5, lines 19-27: It might be better to introduce this information after describing the cal-

culation of the local means on line 5 of page 6 so that the purpose of the averaging is understood. 8. Page 5, lines 25-26: What is meant by the “trends” in line 25? Also, what was the basis determining what was “a good compromise between statistics [statistical significance?] and locality?” 9. Page 6, Equation (3): It would appear that when simplifying Eq. 2, the α terms drop out and all that is left is $\Lambda\beta$. Where does θ come from here (and, consequently, in Eq. 4)? 10. Page 7, lines 1-2: why were the data divided into 50 bins prior to creating the scatterplot, and what was the basis for binning? At first glance it would appear that this would make the results appear much less noisy than they really are. 11. Page 9, line 8: this relationship is nearly linear (β=1); it would be informative to test for the statistical significance of β<>1. 12. Page 9, lines 7-10 and elsewhere: it might be helpful to provide a plot or a few values showing how different the RR-IWP relationships are for typical ranges of IWP so that the significance of these differences (or similarities) between regions is clearer.

Technical Corrections: 1. Page 2, line 3: Trenberth appears to be the sole author of the paper cited here. 2. Page 3, lines 17-18: There is no entry for Thies et al. (2008) in the References. 3. Page 13, lines 23-25: Hou et al. (2014) is not cited in the body of the manuscript. 4. Page 14, lines 22-27: Lebsock et al. (2011) and Lin and Rossow (1997) are not cited in the body of the manuscript.

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