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Interactive comment on “Coastal precipitation formation and discharge based on TRMM observations” by R. H. Heiblum et al.

Anonymous Referee #1

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article

This paper analyzes TRMM precipitation data to investigate the impact of sea and land breezes on rainfall in the eastern Mediterranean. Possible orographic effects due to mountain ranges near the coastline are also discussed. They found a clear offshore peak of precipitation which may be explained by low-level convergence arising from a land breeze. The diurnal cycle of precipitation shows characteristic patterns changing regionally and seasonally.

I found that the paper is clearly written and the analysis is overall robust. I, however, would strongly encourage the authors to check on the reliability of the rainfall dataset they used. They adopted the TRMM 2B31 (combined PR and TMI) product to estimate the integrated hydrometeor mass (IHM) as a measure of precipitation. This product

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may or may not be adequate for a coastal rainfall diagnosis, as I describe below as my major concern. Minor comments will follow next.

Major concern:

Microwave radiometry is known to have a difficulty in rainfall retrieval near coastlines (e.g., Nesbitt et al. 2004, *J. Appl. Meteor.*, 43, 1016–1036; McCollum and Ferraro 2005, *J. Atmos. Oceanic Tech.*, 22, 498–512). This is because observed brightness temperature could be affected by warm land surface emissions when the radiometer footprint partially contains land surfaces even though the footprint center is located over ocean. In such cases, land surface emissions are often indistinguishable from warm radiation emitted by a thick layer of liquid water, and an anomalously high rain rate could be incorrectly reported. The lowest frequency channel of TMI (10.6 GHz) has an FOV size as large as $60 \times 35 \text{ km}^2$, and therefore the coastal effect could reach 60 km (or maybe even farther) away from the coast.

It is unclear if combined PR and TMI algorithms suffer from the same problem, but I'm afraid that is quite possible as far as low-frequency brightness temperatures are used for constraining path-integrated attenuation in radar measurements as done by the 2B31 algorithm. Although any coastal rainfall issue may not have been reported for the 2B31 product in the literature as the authors pointed out (line 5 on page 15677), this fact does not totally eliminate my concern, given that the 2B31 product has drawn much less attention in the first place than the individual PR (2A25) and TMI (2A12) products in existing TRMM validation studies. The authors also noted (line 6 on the same page) that they tested 2B31 data around the globe and found no consistent bias in coastal areas, but coastal rainfall biases, if exist, would have a similar magnitude across the globe (to the extent that the coastal microwave emissivity doesn't change very much from one region to another) and would not exhibit much regional contrast in any case.

In order to show that the 2B31 data don't have any such artifact, it would be crucial to

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add another figure comparable to Fig. 5 constructed with the PR rainfall (2A25) product so as to show that the present findings are insensitive to the choice of the dataset to analyze. The PR 2A25 data to be compared wouldn't necessarily have to cover the entire 13 winters but just a few winters may suffice. The TRMM PR is not affected by surface emissions (except for the lowest levels containing surface clutters) and is free from the issues mentioned above. It would be also beneficial to duplicate the diurnal cycle diagram (Figs. 7 and/or 9) using the PR 2A25 data for reference. Land surface emissions would have a clear diurnal variation due to the surface temperature variability and potentially contaminate real rainfall signals. The author commented on an unexpected afternoon peak of offshore precipitation (line 26 on page 15678), which could be possibly an artifact arising from the radiometric contamination of land surfaces to microwave brightness temperature.

Specific comments:

1. Introduction is somewhat lengthy and may better be shortened for clarity. Equation (2) and the related discussion on Page 15662, for example, can be entirely erased because the equation will never be used throughout the rest of the paper. This is more of a text book material that needs not to be repeated in journal papers.
2. page 15668, line 20: The author claims that “2B31 product tends to give improved results and lower biases compared to the PR and TMI products alone.” This is probably not true. None of the papers cited by the authors themselves (Masunaga et al., Wolff et al., Wolff and Fisher) showed that the 2B31 rainfall is *consistently* better than the other two products. By the way, the Wolff and Fisher (2008) paper is missing from the reference list.
3. Page 15672, Equation (3): A factor of 1/2 is missing from the exponential term. The Gaussian distribution is usually defined as $\exp(-((x - \mu)/\sigma)^2/2)$.



4. Page 15680, line 15: Does the TRMM detectability have something to do with the Feingold and Levin (1986) paper? The TRMM didn't exist yet in 1986.
5. Same page, line 19: "TRMM's footprint" should be "TRMM PR's footprint".

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