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# ***Interactive comment on “The Passive microwave Neural network Precipitation Retrieval (PNPR) algorithm for AMSU/MHS observations: description and application to European case studies” by P. Sanò et al.***

## **Anonymous Referee #2**

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Review: AMT Manuscript #2014-255

This manuscript provides a description and application of the Passive microwave Neural Network Precipitation Retrieval (PNPR) algorithm applied to microwave sounder observations from AMSU and MHS. The authors provide a detailed algorithm overview in a clear and well-written manner. The methodology is sound – neural networks have been used for many atmospheric science retrieval algorithms, so I do not have any concerns related to methodology. Neural networks are only as good as their train-

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ing database, and the authors apply mesoscale model simulations to populate a large database. I am always slightly concerned about whether simulation results effectively represent the wide spectrum of precipitation events possible, but this concern should not prevent publication of this manuscript. This pathway has been employed by numerous other projects and is not considered a fundamentally flawed strategy. Radiative transfer assumptions may also introduce uncertainties, and I encourage the authors to follow this manuscript with systematic assessment of all facets of the modeling chain used in this work. The list of mostly minor comments listed below should be considered by the authors before the final manuscript is published.

Overall Rating:

Scientific significance: Good

Scientific quality: Very good.

Presentation quality: Very good.

1. Page 9352, Line 22: I suggest replacing the phrase “with some uncertainties” with “increased uncertainties”. I believe that stating “with some uncertainties” implies other categories are associated with extremely low or no uncertainties, and this representation seems unrealistic.

2. Page 9352, Line 22: The authors write “In this paper, examples of good agreement of precipitation...”. I urge the authors to also show instances where the algorithm might not perform well. This information is as important as providing evidence of good algorithm performance. The authors mention larger uncertainties associated with light precipitation, but are there any other obvious systematic deficiencies with the PNP?

3. Page 9353, Line 5: Change “through” to “thorough”

4. Page 9354, Lines 25-29: It might be worth mentioning the polarization (H/V) information gained from conical scanning instruments versus the mixed polarization signal from the cross-track scanners.

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5. Page 9355, Lines 9-14: While the scattering signal is correlated with surface precipitation, there are still large uncertainties related to the scattering signal due to unresolved microphysical issues. Recent publications (e.g., Skoronick-Jackson et al. (2013), Johnson et al. (2012), Kulie et al. (2010)) have highlighted this issue and the need to further understand how different combinations of microphysical features affects high-frequency brightness temperatures.

6. Page 9357, Lines 25-29: Is there any advantage employing a Bayesian approach for conical microwave imaging versus a neural network approach for cross-track sounders? Is the neural network approach better suited for multiple view angles, varying footprint sizes, etc. and other complicating factors associated with sounder data? This is a basic question that might be obvious to some, but not so clear for others. What is the basic reason behind the retrieval approach chosen for the two classes of microwave instruments?

7. Page 9360, Lines 4-5: The UW-NMS model reference seems dated. Are there any updates to the model since 1992? Any recent references that apply updated versions of this model? Readers might be concerned if the database is populated with this particular model versus other community models. But this issue can be resolved by including newer references where this model was applied more recently.

8. Section 2.2: The authors include a very detailed description of the NN approach used in this study. I originally thought the level of specificity was too great, but after reading the manuscript for the second time, I appreciate the level of detail provided. The main purpose of this paper was to introduce this retrieval algorithm, so I lean toward keeping the details provided in their current form.

9. Page 9364, Line 25: “constrain” should “constraint”.

10. Page 9367, Line 13: Is the  $TB < 50K$  or  $TB > 400K$  frequently applied to reject observations? Or are these criteria rarely applied?

11. Page 9371, Line 16: Is the same analysis presented in Panegrossi (2013) using the four case studies? Or was the focus of the Pannegrossi (2013) paper substantially different?

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Interactive comment on Atmos. Meas. Tech. Discuss., 7, 9351, 2014.

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