General answers to the Editor and to the two Referees

We want to express our sincere appreciation for all the comments and suggestion from the Editor and the two Referees. We have seriously examined all the suggestions from the Editor Dr. S. J. Munchak (i.e., impact of the parallax correction, and presentation of results in a more "physical" way, with a geographical map of the statistical scores and analysis of how the algorithm performs for different types of precipitating event). The comments on the readiness and organization of the results section have been particularly appreciated. The anonymous reviewer 2 has expressed some criticism on the need of a detection algorithm and on the possibility to have consistent detections from different sensors. Well argued criticisms like these makes it possible for an author to think about the real motivations of his work, and present them in a clearer form. Finally referee 1 comments have pointed out several issues on our manuscript that have been addressed, i.e. we particularly appreciated the comments on the minimum detectable rainfall from PR, the choice of using the same dataset for training and testing and the need of a more physical analysis. Moreover we acknowledge referee 1 for the "*labor limae*" (a patient, diligent, meticulous work) on our manuscript.

In order to address all the referees and editor comments we have made substantial changes in the manuscript. In detail, we have built an independent dataset of coincident observations relative to the year 2013 and used this dataset for the testing of the algorithm. As a consequence all the results section is now relative to this new test dataset. Moreover we have removed the SSMIS radiometer on board the DMSP-F18 satellite both from the training and the test datasets because of the presence of a corrupted channel at 150 GHz. As suggested by the Editor, we have made comparisons with the results obtained calculating the Canonical Variables (CVs) in log(RR) space (where RR is the rainfall rate) finding better performances of the algorithm. Therefore, we have decided to adopt a new discrimination function based on CCA in log(RR) space. Finally we investigated the impact of the parallax corrections on the results finding a very small impact and decided not to apply any parallax correction. In order to meet the suggestion of a more physical analysis of the results we have computed a table of the statistical scores divided by rain type as observed by TRMM-PR and by the background surface. Finally, we have analyzed the impact on the algorithm of the non uniform beam filling effect.

These main changes are hereafter briefly reported:

A. **Changes in the text relative to the dataset construction and partition, and to the change of the discriminant function:**
   1. P9243 L4 changed to "AMSU/MHS radiometers in the years 2011-2013"
   2. P9243 L24: changed to "and DMSP-F17 satellites (i.e. the DMSP-F18 has the 150 GHz channel malfunctioning since February 2012)".
   3. P9246 L1 a sentence that describes the dataset partition in training and test will be inserted at the end of Section 2: " The SSMIS and AMSU/MHS datasets have been divided into a training set (which includes all data from 2011 and 2012) and a test dataset (data from 2013)".
   4. P9246 L2: a sentence describing the use of the training dataset at the beginning of section 3 will be inserted: "This section is dedicated to the training of the CCA algorithm done by using the training dataset relative to the years 2011-2012".
   5. P9247 L2; the sentence "correlation with rainfall rate (RR)" will be replaced by "correlation with the logarithm of rainfall rate log(RR)".
   6. P9249 L8: a sentence that describes the use of the test dataset at the beginning of section 4 will be inserted: "This section shows the results of the application of the algorithm to the test dataset relative to year 2013."
   7. P9270: Figure 3 will show the results on the new training SSMIS dataset.

B. **Parallax corrections**:
   Descriptions and comments will be removed after the verification that there is a very small effect on the statistical scores due to the correction of the parallax effect (See answer to the Editor, E6, and to Reviewer 1, R1.5).

C. **Results Section:**

We have changed substantially the section "4. Results". The full new results section with new figures and tables can be found in the attached document "new_results_section.pdf". The main changes are listed below:

1. Figure 4-5 will be replaced with new figures, showing the results on the test and training datasets of the CCA algorithms and the comparison with other well known screening algorithms. The comments to these figures will be consequently modified, shortened and grouped into a sub section "4.1 Discussion of Skill Scores".

2. Figure 7 will be removed and a new figure (Figure 5) has been added to section 4.1 The new figure shows the well known POD, FAR and HSS. The results are shown for rain/no-rain threshold ("truth" from PR 2A25) equal to 0.1 mm/h. Skill scores relative to CCA-GMI have been removed because they almost identical to CCA-SSMIS. A comment on this figure will be included in section "4.1 Discussion of Skill Scores".

3. Figure 6 and Table 3 will be changed. The comments to this figure and table will be modified accordingly into a new subsection: "4.2 Minimum Detectable Rate".

4. New tables will be inserted in the manuscript in a new subsection "4.3 Dependence on precipitation regime".

D. Changes in the conclusions:

1. The conclusions have been updated with the new results.

Specific Answers of the authors to the Anonymous Referee 2

We acknowledge the anonymous Referee 2 for his/her deep and constructive comments and remarks.

This paper describes and validates an objective technique for deriving a rain/no-rain screening algorithm applicable to multichannel microwave brightness temperatures, irrespective of the specific sensor considered. I don't have any serious problems with their methodology; I do have longstanding philosophical reservations about the desirability of separating "screening" from "retrieval" – see additional comments below. That said, I believe this paper will be acceptable for publication if the authors can satisfactorily address the following comments:

R2.1 Throughout: The use of the adjective "novel" is reasonable in the title and once or twice in the introduction. It becomes repetitive when used every time the algorithm is mentioned (note that "novel" and "new" are not quite the same thing in English). Consider giving the algorithm a name early on so that it can be referred to unambiguously without adjectives.

We want to acknowledge the referee for this comment. We changed the references to "the novel algorithms" using the acronym CCA followed by the name of the radiometer (i.e. CCA-SSMIS, CCA-AMSU) as name of the algorithm,. We believe that this change will make the paper more readable.

R2.2 Abstract, last three lines (see also p. 9257, lines 8-10): "total amount of precipitation" seems imprecise, given that this is a detection algorithm. Perhaps "total occurrence" or "total area" or "total fraction" would be better.

We have removed the analysis on the total fraction of precipitation from the results section (see answer to the Editor E2 and R2.6 below). Therefore, in the abstract this sentence will be removed.

R2.3 p. 9240, line 3: In my opinion, achieving consistency among difference radiometers is not only NOT of primary importance, it is not even a theoretically realizable goal. The available channels and channel resolutions, which differ from radiometer to radiometer, introduce fundamental variations in the degree to which the precipitation signal can be separated from background variability, and these differences may be large over certain surface types. For example, polarization information, when

available, can be very effective in discriminating cold unpolarized precipitation from cold polarized water surfaces (including wet land). If polarization information isn't available, the ability to detect precipitation will be severely degraded irrespective of the algorithm employed.

We acknowledge the Referee 2 for this comment. We have noticed how our statements about consistency between different radiometers were oversimplifying this issue. However we believe that achieving a certain degree of consistency between the retrievals from the GPM constellation of sensors remains a priority for precipitation monitoring based on PMW products or for MW/IR combined algorithms (such as TMPA, Huffman et al. 2007)based on precipitation estimates from different sensors. In order to make our statements clearer, and to account for the Referee important remark, we have rewritten P9240 line 3-6 in the introduction:

However, the inconsistencies in the precipitation detection deriving from the use of different radiometers also might affect significantly the rainfall estimates from such a heterogeneous constellation, as well as precipitation products derived from the combination of these estimates (i.e., IR/MW merging techniques). The differences in the available channels, polarization information, spatial resolutions, and observation geometry have a strong impact on the possibility of separating the radiance due to the background surface from the signal related to precipitation. Therefore, the limits of each sensor in detecting precipitation should be carefully analyzed in order to establish the degree of consistency of the precipitation patterns (and retrievals) obtained from different radiometers. As a certain degree of coherence between different sensors can be accomplished by developing common procedures to be applied to all the different radiometers and able to detect efficiently the presence of precipitation in different environmental conditions.

And we have modified P9257 L16-23 in the conclusions:

Lots of effort is put into achieving consistency between precipitation pattern and precipitation estimates from the different sensors. Some fundamental improvements in this direction will come from the use of a common algorithm for the screening of non precipitating scenes applicable to all types of background surfaces. The CCA algorithm is an important step toward this goal, considering that it is suitable to be applied to any PMW sensor (conical and cross-track scanning) for which a long series of data coincidences with rainfall rate ground truth is available. The results show a certain level of consistency between the detection capability of CCA-SSMIS and CCA-AMSU algorithms. It is worth noting that using different thresholds and linear combinations of channels for each sensor, this consistency is reached by optimally exploiting the characteristics of each sensor. However, the available channels, polarization information, spatial resolutions, and observation geometry, which differ from radiometer to radiometer, introduce fundamental variations in the precipitation detection capabilities of each sensor posing intrinsic limitations to the level of achievable consistency.

Huffman, George J., et al. "The TRMM multisatellite precipitation analysis (TMPA): Quasi-global, multiyear, combined-sensor precipitation estimates at fine scales." *Journal of Hydrometeorology* 8.1 (2007): 38-55.

R2.4 p. 9241, lines 1-2: It has been too common over the past decades, in my opinion, for algorithm developers to strive to "separate the problem of identifying precipitating areas from the problem of estimating the intensity of the rainfall." I have never accepted that there is a valid justification for seeing these as two distinct problems to be solved separately. On the contrary, the determination of rain vs. no-rain is nothing more and nothing less than that of determining whether the rain intensity is greater than zero. If the intensity retrieval algorithm can't do that with adequate skill on its own, then it probably isn't doing a very good job with other rain rates either. A retrieval algorithm that needs to be protected from a wrong determination of rain/no-rain by a separate screening algorithm is clearly failing to optimally utilize the available information; otherwise it should be able do exactly as well as the best screening algorithm applied to the same channels.

We thank the referee for this comment. We agree, , the problem of detection of precipitation is part of the more general problem of intensity retrieval and it is related to the ability of separating the noise due to the background surface from the signal related to precipitation, especially when the signal to noise ratio is low.

However, there are still many practical reasons why the determination of rain vs. no-rain can be treated as a distinct problem from estimating precipitation:

1- In Bayesian – physically based estimation algorithms (e.g. GPROF, UW and CDRD) the adoption of a screening procedure drastically reduces the size of the a priori database, improving the computational time (we notice how this problem was solved brilliantly by other authors using pre-compiled tables, i.e. in the UW algorithm).
2- If the estimation algorithm makes use of simulations, a screening procedure based on observations may take into account quantities (such as effect of polarization of radiation caused by clouds) that are still not well simulated by radiative transfer models.
3- In semi arid regions in which drought is a serious issue, estimating the time of consecutive days of non-rain is probably more important than estimating the intensity of precipitation.
4- Over noisy background surfaces (such as arid regions) most algorithms (including GPROF v7) do provide any estimate of precipitation, because of the high ratio of false alarms.

The CCA approach, which gives acceptable results in detecting precipitation, could be further exploited and developed for precipitation intensity retrievals.

R2.5 p. 9242, line 5: The authors cite Petty (2013) but do not mention the two subsequent papers that actually implement and validate a TMI retrieval algorithm based on the conceptual framework laid out in that first paper. Some of the methods and findings of the later papers might be relevant here.
Petty, G. W., and K. Li, 2013: Improved Passive Microwave Retrievals of Rain Rate over Land and Ocean. Part I: Algorithm Description. J. Atmos. Ocean. Tech., 30, 2493-2508.
Petty, G.W., and K. Li, 2013: Improved passive microwave retrievals of rain rate over land and ocean. Part II: Validation and intercomparison. J. Atmos. Ocean. Tech., 30, 2509-2526.
We agree, we will insert the references to these papers and change the manuscript as follows, p. 9242, line 5:
The approach of Petty (2013) has been implemented and validated in two subsequent papers (Petty and Li 2013a, Petty and Li 2013b), in this work we have modified and adapted the approach to the problem of precipitation detection using a simpler surface classification and without considering the linearization of the Tbs into pseudochannels.

R2.6 p. 9255, lines 1-3: I don't understand this part. I would think that a "perfect" algorithm applied to pixels misclassified as raining by the screening algorithm would not retrieve "the mean value of precipitation rate over the full data set" but rather something more like "the mean value of precipitation rate over the full data set minus those pixels correctly classified as raining," which is presumably a much lower value. In other words, one wouldn't expect that the PDF of rain rates for misclassified pixels is the same as the climatological PDF for all pixels, because higher rain rates are less likely to be misclassified.
We noticed how this section of the manuscript was unclear, and the definition of FPF questionable (see also Answer to Editor comment E2). Therefore, we have decided to remove the discussion and Figure 7. The discussion of a new figure (which has become Figure 5), where the standard POD, FAR and HSS for all algorithms are presented has been included in section "4.1 Discussion of skill scores".