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Atmospheric Measurement Techniques Discussions

Interactive comment on "Detection of deterministic and probabilistic convective initiation using Himawari-8 Advanced Himawari Imager data" *by* Sanggyun Lee et al.

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The authors would like to thank the editor and the reviewers for their precious time and invaluable comments. The corresponding changes and refinements are highlighted in yellow in the revised paper. Both authors' responses and revised manuscript are attached as in a PDF file (supplemet) below. Brief responses are also found below.

Reviewer #1: I review this same paper in July/August 2016, and as it looks, only a few of my major and minor comments sent back to the authors at that time have been addressed. This paper presents the implementation of a convective initiation (CI) algorithm that operates on Himawari-8 AHI data, with applications to the Korean Peninsula. Overall, a major concern related to the requirement that journals present NEW, inno-

vative work is that this paper repeats much of the analysis methods in two papers, Mecikalski et al. (2010) and Mecikalski et al. (2015). If this journal is o.k. with the "newness" being that prior methods are developed using new (i.e. Himawari-8) data and not new methods, then the paper is not a bad presentation. But, the paper suffers from considerable grammatical problems. It need to be read by an English speaking person.

In the Mecikalski et al. (2010) paper, several Meteosat Second Generation (MSG) satellite based fields were defined outward of principal component and other information content analysis (that began with an assessment of many possible interest fields) for their value at predicting CI in the coming 1 hour. These are the results as stated in their Table 3. In the Mecikalski et al. (2015) paper, which defines the current GOES-R CI algorithm (albeit the authors have some confusion as to the authors/developers of the GOES-R CI algorithmâËŸAËĞ Tsee corrections below), the RF and LR machine learning approaches were applied to a reduced set of GOES-specific satellite predictor fields and also to NWP fields. Hence, this paper seemingly combined the Mecikalski et al. (2010) and Mecikalski et al. (2015) studies, with little new information, insights or analysis being done. I therefore again am not sure how appropriate it is to publish such a study that just re-applies already-published ideas, yet the authors have properly cited the relevant prior research and that the results are similar to these prior works. I reiterate that it may not be appropriate to publish results which are effectively _95%+ stated in other papers, based on the Methodology of Section 3, and the results in Tables 9 and 10 are also nearly the same for the RF and LR models as shown in Mecikalski et al. (2015). These authors have not done a complete analysis of all possible AHI datasets with respect to CI, which is in fact an ongoing activity in my research group today, and therefore do not shed new light on the value of all 13 infrared and the visible channels for predicting CI. I am therefore inclined to Reject this paper since it effectively duplicates prior work, while not significantly advancing our understanding.

–> Thanks for the comments. We agree that the present study used similar machine

learning approaches that were adopted in recent papers (Mecikalski et al., 2015; Han et al., 2015) to detect CI. Mecikalski et al. (2010) evaluated and analyzed Meteosat Second Generation (MSG) satellite-based interest fields using Principal Component Analysis (PCA) and Mecikalski et al. (2015) developed CI detection models based on random forest and logistic regression using GOES satellite and NWP model data. Our previous study (Han et al., 2015) also used decision trees and random forest to detect CI from COMS satellite data. However, we would like to say that the novelty of our present study when compared to the previous studies lies in the following two points: 1) Our present study is, as we know of, the first paper that evaluated Himawari-8 AHI data for CI detection. In our study, we solely focused on using AHI channel data without any ancillary data to detect CI for an operational purpose. While CI detection research has been widely conducted over US and Europe, it has had minimum exploration over Northeast Asia. This present study can contribute to the forecast and mitigation of heavy rainfall in Northeast Asia, especially during the rainy season (i.e., summer). 2) Our proposed machine learning-based approaches contain two new post processesâĂŤmajority voting and region growing, which are included in the revision. Since pixel-based CI detection is known to often result in salt-and-pepper noise and non-compact CI output, our proposed approaches include the post-processing to minimize such problems. The post-processing generally resulted in an increase of POD and a decrease of FAR.

We would like the reviewer to look at our fully revised manuscript attached. We significantly revised our manuscript according to your comments and those from the other reviewer. We improved our approaches by incorporating two post-processing techniques and added five additional validation cases (i.e., a total of 8 validation datasets) with more discussion to improve the quality of our study. Figures were updated with more clarity. Although it is not possible to directly compare our results to others' as different input and reference data were used, this present study showed good results comparable with Mecikalski et al. (2015). This implies that Himawari-8 satellite data (or future weather satellites with similar/more advanced specifications such as GOES-

R and GK-2A) can be solely used to detect CI, which enables the development of operational CI detection algorithms with high POD and low FAR. However, as shown in Mecikalski et al. (2015), model results such as convective available potential energy (CAPE), convective inhibition (CIN), and vertical shear (0-6km) can be effectively used to reduce FAR in the proposed CI detection algorithms.

If the Editor deems it appropriate to publish these results, then: (1) The authors need to carefully define all acronyms before they are used. This problem begins in the abstract and continues well into the paper. There are numerous spelling and grammatical issues (e.g., Himawari-8 is not capitalized on page 4/top, and the correct acronym is "EUMETSAT" on "EUMESAT" on page 2).

–> Thank you for your comments. We thoroughly checked acronyms and spelling problems from abstract to conclusion. A professional editing service was also used to improve the readability of the manuscript.

(3) Overall, it is not good to validate a CI algorithm using lightning observations, especially in the algorithm development stages. The authors do note the reason for poorer model performance when lightning data were used, later in the paper, as not all CI events go on to make lightning.

–> We agree that lightning observations are not appropriate to validate CI algorithms. As you said, clouds with heavy rainfall without lightning observations sometimes occurred. We added this explanation on page 12, lines 13 – 15. However, it might be useful where ground radar data are not available (e.g., ocean).

(4) Methodology/Section 3: Please remove the sentence "However, as mentioned, the GOES-R CI algorithm uses simple threshold values associated with the interest fields and the values were determined through many experimental simulations in a subjective way." First, the present GOES-R CI algorithm (Mecikalski et al. 2015) does NOT use "simple threshold values". Second, all prior research was NOT subjective in nature, but rather examined growing cumulus clouds in advance of CI with respect to physical

processes (cloud growth rates, updraft size/width, glaciation, cloud altitude, updraft longevity, etc.) as measured/observed by geostationary satellite infrared and visible datasets. Specifically, the Mecikalski et al. (2010) paper using MSG data was very focused on gaining understanding on how the interest fields behaved and subsequently how specific "threshold" values could be set, similar to how the original Mecikalski and Bedka (2006) study was performed. The way this sentence reads is that the prior work was just done without much thought, which was hardly the case.

–> Thank you for this comment. We are sorry that we thought that GOES-R CI algorithm is from National Oceanic and Atmospheric Administration (NOAA) National Environmental Satellite Data, and Information Service (NESDIS) center for satellite applications and research Algorithm Theoretical Basis Document (ATBD), Convective Initiation Version 2 (we think it is an old version), which we used in our original manuscript. We removed the sentence as suggested. We would like to say that our study is based on the research findings from the previous research papers including those the reviewer mentioned.

For more MINOR comments, I again suggest that the paper be reviewed and edited by an English-speaking person prior to acceptance.

–> We carefully proofread the manuscript several times. In addition, a professional editing service was used to improve the clarity and readability of the manuscript.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/amt-2016-308/amt-2016-308-AC1-supplement.pdf

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