

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

Anonymous Referee #1

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Note: 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, innovative 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.

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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 algorithm—see 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.

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).

(3) Overall, it is not good to validate a CI algorithm using lightning observations, espe-

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cially 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.

(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.

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

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